UTRECHT II: CHARTING THE FUTURE OF PARAPSYCHOLOGY

PROCEEDINGS OF AN INTERNATIONAL CONFERENCE

HELD IN UTRECHT, THE NETHERLANDS

OCTOBER 16-18, 2008

Edited by

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PARAPSYCHOLOGY FOUNDATION, INC.
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HET JOHAN BORGMANFONDS FOUNDATION
THE NETHERLANDS

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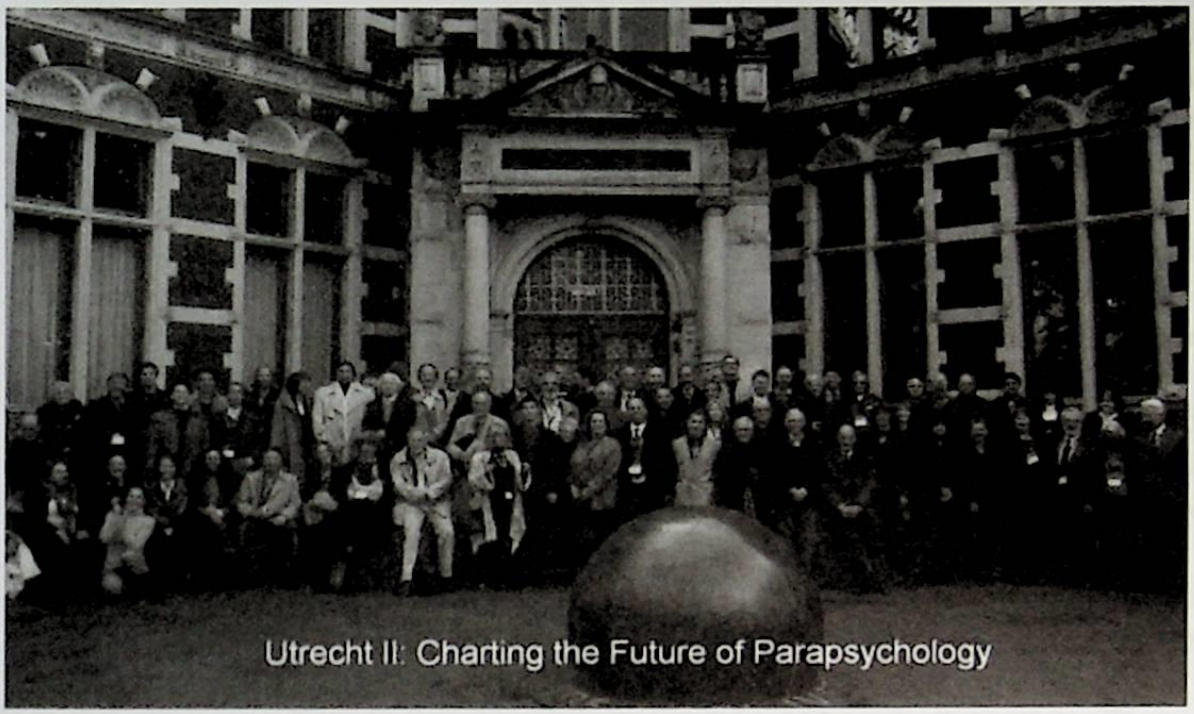
This conference is dedicated to Eileen J. Garrett, Frances P. Bolton and Johan Borgman, along with researchers the world over — past, present, and future — who share their determination to unravel the mysteries of psychic functioning



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CONTENTS

OPENING REMARKS xii
Wim Kramer, Hugo Verbrugh, Eileen Coly, Lisette Coly, Chris Roe

SESSION ONE: MEASURES OF PSI — 'What do we know?' & 'Where are we going?'

CONSCIOUSNESS AND PSI (CAN CONSCIOUSNESS BE REAL?)
Roger Nelson 1

PRESENTIMENT RESEARCH: PAST, PRESENT, AND FUTURE
Eva Lobach 22

SEARCHING FOR NEURONAL MARKERS OF PSI: A SUMMARY OF THREE STUDIES MEASURING ELECTROPHYSIOLOGY IN DISTANT PARTICIPANTS
Thilo Hinterberger 46

GENERAL DISCUSSION 63

SESSION TWO: EXPRESSIONS OF PSI IN LIFE — 'What do we know?' & 'Where are we going?'

DREAMS AND ESP
Robert L. Van De Castle 72

FIELD INVESTIGATIONS OF HAUNTINGS AND POLTERGEISTS
Fátima Regina Machado 115

PERSONALITY VARIABLES IN SPONTANEOUS PSI RESEARCH: CONTEXTUALIZING THE BOUNDARY CONSTRUCT IN ITS RELATIONSHIP TO SPONTANEOUS PSI PHENOMENA
Christine Simmonds-Moore 151

GENERAL DISCUSSION 216

INVITED ADDRESSES

FACING THE CHALLENGES OF PARAPSYCHOLOGY
Edwin C. May 224

Day 2 OPENING REMARKS: Welcome to the 'Senaatzaal' room 243
Wim Kramer, Lisette Coly

DISCUSSING PARAPSYCHOLOGY AT UTRECHT: THE FIRST INTERNATIONAL CONFERENCE OF PARAPSYCHOLOGICAL STUDIES
Carlos S. Alvarado 245

PARAPSYCHOLOGY IN A UNIVERSITY SETTING
Deborah L. Delanoy 289

BEYOND PLATO? TOWARD A SCIENCE OF ALTERATIONS OF CONSCIOUSNESS
Etzel Cardeña 305

SELF-ORGANIZED REALITY
Brian Josephson 323

SESSION THREE: CLINICAL & MEDICAL CONNECTIONS — 'What do we know?' & 'Where are we going?'

CLINICAL PARAPSYCHOLOGY: TODAY'S IMPLICATIONS, TOMORROW'S APPLICATIONS
Martina Belz 326

SPIRITUALITY: THE LEGACY OF PARAPSYCHOLOGY
Harald Walach 363

DISTANT HEALING INTENTIONALITY AND BRAIN FUNCTION IN RECIPIENTS, USING FMRI ANALYSIS
Jeanne Achterberg 387

GENERAL DISCUSSION 413

SESSION FOUR: THEORIES, IMPLICATIONS, NEW DIRECTIONS — 'What do we know?' & 'Where are we going?'

THERE IS NO MIND-BODY PROBLEM IN PARAPSYCHOLOGY
Hoyt Edge 421

EMPIRICAL PICTURES OF TIME
Richard Shoup 463

PHYSICS WITH AN OPEN MIND
York H. Dobyns 490

GENERAL DISCUSSION 522

PERSONAL REFLECTIONS ON UTRECHT II
Chris A. Roe 533

ESCAPE FROM WONDERLAND
Gerd H. Hövelmann 559

FLOOR DISCUSSION: Charting the future of parapsychology 569

CLOSING REMARKS 584
Chris Roe, Lisette Coly

OPENING REMARKS

WIM KRAMER: Good Morning Ladies and Gentleman. My name is Wim Kramer and I serve as Secretary of Het Johan Borgmanfonds Foundation, who along with Parapsychology Foundation is proud to sponsor this three day conference *Utrecht II: Charting the Future of Parapsychology*. It gives me great pleasure to welcome you to Utrecht, the Netherlands. We have attempted to remain true to the original framework of the 1953 First International Conference of Parapsychological Studies whenever possible, which was structured with a Conference Board and Bureau along with various Committees. Based on the individual suggestions made by members of our present day Advisory Committee consisting of Eberhard Bauer, Etzel Cardeña, Hans Gerding, Gerd Hövelmann and Roger Nelson, and our Conference Bureau staffed by Nancy Zingrone and Carlos Alvarado along with the Conference Board, being Lisette Coly and myself, we are confident that we will remain true to the spirit of those attending the historically significant Utrecht 1953 Conference. Our conference — as you will note in your conference program booklet — consists of twelve presented papers and Invited Addresses all grouped within five sessions: Measures of Psi; Expressions of Psi in Life; Clinical and Medical Connections; Theories, Implications and New Directions; and Charting the Future of Parapsychology. All presenters were asked to reference two questions: 'What do we know?' and 'Where are we Going?' This conference was by invitation only, as was Utrecht 1953, and we are mindful of the expense and time commitments you have made to join us; we thank you for your generosity and commitment to the field for your participation in this conference. Each of you is able to make a significant contribution to proceedings and we welcome your expertise and commentary. You can appreciate we will have our work cut out for us in the coming days. Due to time constraints the conference framework was unable to encompass the totality of the richness of our field but certainly we feel our deliberations this time around will offer coming generations of scholars yet another blue print for advanced study and will hopefully serve to open new vistas for elaboration on current research lines and, if we are lucky, new avenues to pursue.

We are eager to make your conference experience both valuable and a pleasure. Annemarie Visser and Emel Sebüktekin, our two lovely conference hostesses, are on hand to offer any assistance. Later today

we will have the services of Lilith, antique Booksellers, who have brought with them a special selection of international historical and rare books on psychical research and parapsychology for your perusal. In addition, only a week or so ago a long lost film clip very extraordinarily came to light; it comes from the early 1950s and features Dutch psychic Gerard Croiset demonstrating his healing activities. Fortunately we have been able to track down an old 16mm projector so that we can schedule a viewing of this historic footage off-program on Friday afternoon. This is all the more significant for us as Croiset demonstrated his abilities during the first Utrecht meeting in 1953.

We hope that you will enjoy our historical city. The main building of the University is situated on the remains of a Roman army camp, underscoring our 2000 year history. Our conference room today is one of the 'hiding Churches' utilized during the Reformation for forbidden Catholic services. The surrounding area is known as 'Knockers', as people would knock on the doors as a warning when police entered the area. I can assure you, however, that this beautiful room and our deliberations will not be disturbed today!

Without further delay it is a pleasure for me to introduce Dr Hugo Verbrugh, President of our Dutch Het Johan Borgmanfonds Foundation.

HUGO VERBRUGH: Ladies and Gentlemen, it is both a great pleasure and a high honor for me to welcome you all on behalf of the Het Johan Borgmanfonds Foundation to this conference dedicated to Charting the Future of Parapsychology. To the best of my knowledge this will be the second impressive conference on parapsychology in Utrecht that carries the 'highly valued' Parapsychology Foundation trademark.

Hans Berends, our Treasurer, and I have witnessed and closely monitored over the past year how our Secretary, Wim Kramer has worked—and sometimes, I must add, has suffered—with all the preparatory conference details and I am pleased that, thanks to his painstaking efforts, we now have this conference materialize before us.

However, it must be stated that this conference would not have been possible at all without the experience and dedication of mother and daughter Eileen and Lisette Coly of the Parapsychology Foundation. They have internationally organized and hosted I believe over thirty conferences related to parapsychological topics over the past five decades and are greatly respected within the field. They have been

assisted by their staff members, scientists Drs Carlos Alvarado and Nancy Zingrone in their capacity as the Conference Bureau, who in addition to assisting in the crafting along with the Advisory Committee of this conference from its beginning were responsible for communicating with all presenters and observers. Last but not least, I would like to mention the efforts given by the five members of the international Conference Advisory who independently advised the Board and Bureau in early stages and continued to lend their support and expertise throughout the planning process. Utrecht's Board and Bureau along with the Advisory Committee and our very fine moderator, Dr Chris Roe all worked to bring the vision of a second international parapsychological conference in Utrecht to reality.

In the initial funding phase of this project Wim Kramer informed our Board in detail of the glorious history of the Parapsychology Foundation and all it had done in the past fifty years to promote parapsychology as a science by funding individual researchers, organizations and research projects. I am sure that many of the most well-respected researchers in the field today would not have been able to launch their careers in parapsychology at all without the initial financial help of the Parapsychology Foundation, nor without its support carry out so many research projects. We were also impressed to learn that the First International Conference of the Parapsychology Foundation held in Utrecht in the summer of 1953 was not only attended by many Utrecht University board members and high ranking officers representing the Dutch Government, but that also people like Aldous Huxley and Gabriel Marcel, whom I personally admire, came over to Utrecht and actively participated in those proceedings.

Well, I am not going to use any more of your valuable time here in Utrecht. I am sure all of you just want to start off with the actual program—after all, that is what has brought you all here in the first place. But before I pass the word to Mrs Coly, President of the Parapsychology Foundation, to formally open this conference I would like to make a final personal remark. Some fifty years ago I studied medicine here at Utrecht University and for some time I was a student-assistant to Professor Tenhaeff, doing research on the activities of a psychic healer. I still hold vivid memories of Professor Tenhaeff, and I trust that all your work in this conference in the next few days will be inspired and infused by the same spirit and dedication to science and scholarship that pervaded his work in Utrecht over half a century ago.

EILEEN COLY: Good morning Ladies and Gentlemen. To stand before you, with an opportunity to echo words spoken by my mother, Eileen J. Garrett, Parapsychology Foundation's founder, 55 years ago at the First International Congress in 1953 is a rare and unexpected privilege. I add my sentiments to Garrett's long ago opening remarks in which she said: "We welcome this opportunity to express our deep appreciation to our distinguished Dutch hosts, who have extended to us a cordial welcome, as well as practical cooperation in preparing our First International Conference in Parapsychology"; and now our Second Utrecht Meeting, *Charting the Future of Parapsychology*, with deep gratitude to the Het Johan Borgmanfonds Foundation.

It gave Garrett and now gives me infinite pleasure to see so many old and new friends gathered here in Utrecht once again. I agree with my mother's original statement that unhappily still remains true:

> ...that in thanking you distinguished scholars who have cared enough to join us here in Utrecht, each one present, no matter from which branch of science ... you have entered the field by way of sacrifice, for [both Garrett and] I know that this science does not enable young scholars to sustain themselves; each man or woman who makes their life study, namely the exploration of man himself, has done so knowing that they could possibly endanger their material success to keep the vision alive ...

It has been the continued goal of the Parapsychology Foundation emanating from that First Utrecht meeting to do all we can to be of assistance in support of your research and we pledge to continue our support in whatever way open to us in recognition of your valuable and courageous effort.

At this time let me introduce you to PF's Vice President and Executive Director who shares in Eileen Garrett's and my vision for the future of our science, Lisette Coly.

LISETTE COLY: Thank you Mrs Coly. As you can all imagine, professionally—but certainly also personally—I am elated and humbled at the extraordinary opportunity to stand before you in both my grandmother's and now my mother's footsteps in Utrecht. I literally grew up with constant references to the Utrecht meeting, both in the family and while working at the Foundation. It has been my long-held deeply cherished hope that PF would once again return to Utrecht for another groundbreaking meeting but it remained a somewhat distant

dream with lack of funds available until now when, thanks to the HJBF Foundation's appreciation of our efforts and generosity, Utrecht II can become a reality. Imagine my delight when Wim Kramer suggested in the spring of 2007 that we might combine our efforts to organize this conference, a collaboration that grew to be the very large undertaking we are experiencing today as *Utrecht II: Charting the future of parapsychology*—proof positive I would say that (as many of us may agree) at times miracles do happen.

Obviously we must express our gratitude to the Het Johan Borgmanfonds Foundation who have graciously made this meeting possible, with special thanks to my counterpart in Utrecht II's governing Board, Wim Kramer. Wim played 'matchmaker' and has worked tirelessly with me to mesh the resources of two very disparate organizations to make *Utrecht II: Charting the Future of Parapsychology* a reality. We all owe Wim and his organization a great vote of thanks in addition to UII's Conference Bureau consisting of Drs Carlos S. Alvarado and Nancy L. Zingrone who were charged not only with helping to conceptualize the conference but charged with all the copious conference communications while creating and maintaining UII's conference presence within PF's Lyceum website. Susan MacWilliam, PF's Film Archivist and Northern Ireland's artist of repute will record our efforts for posterity.

Every attempt has been made to remain true to the spirit of Utrecht I and hence much is owed to UII's Advisory committee who were instrumental in our meeting's conceptualization and formation, always available for consultation and input to our efforts. For this we thank Mr Eberhard Bauer, Dr Etzel Cardeña, Dr Hans Gerding, Mr Gerd Hövelmann and Dr Roger Nelson. In a few moments I will turn you over to our esteemed moderator, Dr Chris Roe, who has the unenviable task of keeping us all in line and on time, which may not be easy in the heat of discussion but will be invaluable to our efforts and for which we must be appreciative.

I would like to share with you some opening remarks of Utrecht I's Chairman, Dr Gardner Murphy who played such a large part in the formation, execution and results of that historic meeting.

> I suggest that the primary reason why parapsychology has failed to receive the intellectual and moral support which its importance requires is timidity, fear of ridicule, or a deeper level, fear of getting out of touch with our neighbors. … Frequently,

> our data call for boldness, both in announcing facts and especially in setting up hypotheses bold enough to do justice to the extraordinary nature of the facts. ... We frequently have need of 'outrageous hypotheses,' hypotheses which outrage the common sense of today. ...There is I think a very fundamental difference between a fanciful speculation and an outrageous hypothesis from which new directions in experimental research may take their start. How much shall we attempt to accomplish this week?

We unfortunately don't have a week this time around but we do have a very intense and stimulating three days planned ... so let us without further ado, "Bring on your bold hypotheses to do justice in the extraordinary nature of the facts!"

Ladies and Gentlemen, the Proceedings of *Utrecht II: Charting the Future of Parapsychology Foundation* are now open.

I give you our Moderator, Dr Chris Roe.

CHRIS ROE: Good morning everybody. I would also like to extend a warm welcome to you all to what promises to be a challenging and stimulating three days of presentations and discussion about the current state of parapsychology and its possible futures. In the tradition of Parapsychology Foundation events I want to encourage interaction between presenters and members of the audience, so as well as the limited period for questions at the end of each individual talk there will also be longer discussion periods at the end of many sessions to encourage more involved and free-flowing dialogue. Our first session is concerned with recent advances in the approaches taken to measure psi, and it is my pleasure to introduce the first speaker of this session, Dr Roger Nelson.

CONSCIOUSNESS AND PSI (CAN CONSCIOUSNESS BE REAL?)

ROGER NELSON

My inquisitive group of high school friends, all science nerds, discovered J.B. Rhine's *60 Years* book, and were impressed with the careful methods and the simplicity of presentation (Rhine, *et al.*, 1966). Taking nothing on faith, however, we conducted our own experiments, mini-replications. They gave just the sort of results needed to cause us deeper head scratching, and for me a lifelong respect for science at the edges. What we got in our experiments was nominal significance; the results did not blow our socks off, but instead tantalized our budding scientific minds in the way that only happens when there is complete cognizance and control over the science. That was 50 years ago—almost a match for the 55 years since Utrecht I. Yet, already then we could see the ingredients that must lead to confidence in the outcome of studies at the margins of what we know: care, cognizance, and control over the experiment from its very conception. For someone trained in the strict scientific mode, it is most persuasive to be there when the question is asked, to be engaged in the design, to oversee the data collection, to participate in the analysis, and finally to search for a useful interpretation. These are the features of my good fortune as a psi researcher.

As a young professor of experimental psychology, I found that parapsychology was the frame for the most frequent proposals by students for their research projects. That's probably still the case, at least in terms of student interest, though rats and memory probes and illusions and clever ways to look at motivation may be more acceptable to today's young professors. In any case, psi experiments are an excellent testing ground for experimental design and statistics, and there are any number of interesting paradigms that can be implemented with little more than pen and paper—or the ubiquitous laptops of the 21st century. We had fun, and learned something, indeed a great deal. We learned not only the methods, but also came to understand replication and variability, we discovered the value of patience and perseverance, and we learned humility before the data and pride in doing it right. This was experiential, and I hope it provided for my students the value that I found. It was excellent preparation for my full time career as a psi researcher beginning some 28 years ago when I

joined the Princeton Engineering Anomalies Research (PEAR) lab directed by Bob Jahn at Princeton University, studying the role of consciousness in the physical world. My work now is concerned with the Global Consciousness Project (GCP), about which I will say more later.

Definitions

Many correspondents ask, reasonably enough, how we define consciousness. Years ago, I came across a couple of insights concerning definitions that are worth recalling. One was a discussion by Ludwig Wittgenstein of the trouble we sometimes have understanding each other. I find it useful to remember his observations in a short paraphrase, "Language bewitches intelligence." Alfred Korzybski was very specific in his tome, *Science and Sanity*, which contains several widely used, pithy expressions about the difficulties encountered if we are careless in our attempts to describe our experiences. He famously said, "The map is not the territory" and "the pencil I name is not the one in my hand." Korzybski was the founder of an intellectual movement called General Semantics that has largely dissipated, to our considerable loss. Among many suggestions for achieving and maintaining clarity of communication was his colorful proposal to give all important words imaginal subscripts and superscripts to identify which of several meanings is intended, as well as contextual information like time and place. He thought it wise, if one says "love", to somehow specify whether it is brotherly, platonic, romantic, carnal love, or a mere political expression. Context does that for us much of the time, but it is never easy or certain that our listeners will get the correct message. This bit of folk wisdom applies: "I know you think you understand what I said, but I am not sure you realize that what you heard is not what I meant."

So, what do we mean by 'consciousness'? The usual answers are lists of examples, or synonyms and correlates, like awareness, attention, being awake and mentally active. In some contexts, consciousness is said to be just the activity of brain (or of mind—with a very different meaning). Consciousness can be defined to include more than the self-observed qualities, and touch on dreaming and other levels of brain activity termed unconscious or subconscious. A flip, but effective definition is, "Consciousness is that which allows you to ask the question." However, for the clarity required in scientific research, we

can use definitions in terms of the operations performed in the experiment. For example, in my current research "global consciousness" is defined by identifying and specifying precisely an event in the world that will cause very large numbers of people to feel a shared state of mind and emotion. We say that a major terrorist attack will produce a common focus, or that New Year celebrations will coordinate millions of minds during a moment of consciousness that is clearly defined by the operations of watching the clock and waiting for a midnight hug. We generate a testable hypothesis that this special state of 'consciousness' will have detectable effects in the corresponding data, as seen in precisely specified statistical operations.

Exactly in this way, what we mean by psi in general—if we want to do science, or even just to communicate clearly—is best defined operationally. Yes, we know what we mean, and we can make lists of examples and paradigms, but for clarity, the procedures and relationships we use to elicit anomalous outcomes in an experiment cannot be bettered. When we describe what we do in detail sufficient at least for conceptual replication, we define the object of study, the psi we wish to register and understand.

Experience

A suggested topic for this talk was, "Can consciousness be real?" That seems likely to be a reference to the aging conundrum asking whether consciousness is non-material. I don't propose to answer it, but would like to point to some thoughtful perspectives. Of course we already have looked at the vulnerabilities of language and the transmission of meaning, but prior to any attempt to describe or communicate it we have the personal experience of being conscious. That is hard to gainsay, but it is only a beginning point. Can we create a physical model, or any kind of objective modeling of consciousness? Over the last few decades, serious work has been undertaken, but I think it is correct to say there is no great success or widely accepted theory of mind that links that experiential consciousness to, say, physics. Susan Blackmore's recent book reporting conversations with 21 scientists and philosophers gives a good summary, which definitely does not answer the question, but does help to shape it in useful ways (Blackmore, 2007). She documents David Chalmer's 'hard problem', the matter of 'qualia', Hameroff and Penrose's gravitational collapse microtubule model, and thought experiments asking what would be the

difference between a conscious being and 'zombie' that was otherwise the same, but had no conscious experience. A survey of such attempts to deal with what consciousness is will leave most of us believing that we should make good use of operational definitions as described.

On the other hand, science progresses only if we keep working at the hard problems, attempting to form better and more precise questions. Ed Mitchell, the founder of the Institute for Noetic Sciences, writes about his 35 year quest to integrate science and spirituality into a model of reality that works for physics, but also can deal with the evidence from experience and research that establishes anomalous communication and nonlocal effects of consciousness (Mitchell, 2008). His "dyadic model" says that energy and information are both fundamental aspects of reality, with a common basis in the zero-point field (that is, outside our direct ken), and that they are dyadically coupled in the universe we experience. They are complementary, in the sense that while completely different, together they make a competent description—both are necessary elements in the equation. I think Mitchell is right, and if he is, we should be looking for the equivalent of an $E = MC^2$ defining this linkage with scientific precision. But there is no doubt that our world is made of energy and patterns of energy, which is to say information. Thus our awareness and intentionality can be seen as a part of nature with a role to play. Consciousness is real, and it does have direct effects in the world.

This is what psi research says, and it is a daunting statement that elicits serious skepticism, and unfortunately also outright dismissal, even including refusal to look at the evidence. What is missing? Why is a functional, effective consciousness that has reality and presence in the world so difficult to conceive? Most likely, it is simply a matter of observation, or rather the difficulty of subtle observation. While lifting a rock or talking to someone are acts that have obvious physical correlates (muscles, sound waves), psychokinesis or remote viewing do not. Without the immediate sensory feedback of causal relationships, we are not well equipped to recognize that mind has accomplished anything. Experience alone is not sufficient for any but the first, participatory observer and perhaps a few others who are able to vet the quality of the observation. For the rest of the world more is required, and for that we depend on scientific study, experiment, analysis, and modeling.

Personal experience is nevertheless the first, and in some respects the most persuasive evidence that something is going on out past the

boundaries of conventional psychology and physics. But … how can it be accepted by others? Here is a low-level example, in a brief correspondence with an EGG host in the Global Consciousness Project who had been having technical problems:

> HOST: *The 'kinky' cable in the neighborhood has been set on a straight path and Murphy has departed. The EGG is running. Using Debian and all is well:-).*
>
> RDN: *Marvelous. And I have a nice coincidence to report. While your (this) note was arriving, I was writing one to you —*
>
> *"Thanks for keeping me up to date. I hope you aren't completely bereft of connection. On the other hand, I sometimes think it would be good for me to "..."*
>
> *— and at that juncture my connection broke. The word I was about to write was "disconnect". How cool is that, even if a bit of a pain?*

That's personally impressive, but nothing to take to the bank. Here's another personal example, again nothing earth-shaking, but typically impressive (you had to be there): Peter Bancel, from Paris, and I were talking via Skype about presenting new findings. I said I'd like to include an analysis he had done two years ago in 2006 showing a daily variation in the GCP data that suggests just being awake is enough to make a faint global consciousness. We discussed that for a bit, and then Peter said he had "*this morning*" been thinking about the same analysis, and had decided it was time to update it. He's a cautious, even skeptical physicist, and I call myself 100% skeptical and 100% open minded, but we agreed we both were pondering this analysis independently at exactly the same time, an unlikely thing unless there is some kind of anomalous interconnection of minds.

Of course the usual criticism of such material as evidence applies—we do not know the base rate of coincidences of this nature, and they probably are much more frequent than we imagine.

However, we have more than a century of evidence, beginning with stories like this, and others much more impressive, including many so well documented that they are hard to ignore or explain without something like psi (Feather, 2006). Observations and collections of best case reports made up the early stimulus to studies in a scientific mode. In the early 20th century this development continued with experimental and analytical research (Rhine *et al.*, 1966). By the 1980s, a large and

complex array of experimental results had accumulated. Even in our relatively small field, the meaning and implications were difficult to see and understand in any comprehensive way. But, coincidentally, as it were, this could be alleviated by a newly developed approach to literature review, the quantitative meta-analysis (Utts, 1991). Over the last few decades, the accumulation of sound evidence for parapsychological phenomena has come to a level of richness and depth that I think begins to match the persuasiveness of personal experience—for those who will read and study the literature with care and an open mind.

Participation

Research with all the pieces in hand remains the best evidence because it combines as closely as possible the experiential and the scientific. I will give a few examples from the PEAR work, where for 22 years I participated in forming good research questions, designing the protocols and the statistical analyses, collecting the data (including data generated as a participant myself), analyzing and ultimately interpreting the results, all in cooperation with two, three or more others. We all knew the importance of getting it right. We had an extraordinary opportunity to do challenging research in a setting where we could invest the time necessary to do it well and to repeat experiments with variations that could inform and improve the work. We could and did look for what matters to consciousness when interacting with physical systems, and we were able to learn important constraints in our efforts to capture anomalous information transfer. At PEAR we had the luxury of expertise and resources, and we used them well because we all knew it was a precious opportunity to learn something. We did not want to waste our time or that of anybody who might look at our work. Here are some examples of what we learned.

Remote Perception

One focus at the PEAR lab was a long series of experiments looking at anomalous information transfer we called "remote perception" or "precognitive remote perception" (PRP). The paradigm is similar to the free response remote viewing work developed by Targ and Puthoff at SRI in the 1970s (Targ & Puthoff, 1977), and related as well to the

Ganzfeld work developed to a high point by Honorton (Bem & Honorton, 1994; Honorton, 1990). At PEAR we focused on quantitative assessment, using a set of 30 binary questions to represent the free response: is the scene hectic or calm; is it characterized more by straight lines or curves; are there people or not? Using the resulting performance measures, we sought to determine what the constraints and necessary conditions were for successful remote perception. We asked whether the effect was diminished by greater distances between the agent at the scene and the percipient, and whether the scores were different for perception attempted before the target was visited or after the visit, compared with on-time viewing. We studied whether people were more successful when the target was determined by volitional selection at the appointed time by the agent at the scene, or by random selection from a pool. We also explored variations of the quantification process, gradually increasing the number of descriptors in the questionnaire from two, to four, to a quasi-continuous scale with nine options. I can give here only a very brief overview of the results of this program over about two decades of work (see Dunne & Jahn, 2003, for a more detailed review).

The most important outcome was a confirmation of the primary hypothesis—percipients can acquire information about distant targets without normal sensory channels. The effect is subtle, but over hundreds of trials, the odds against chance explanations go to millions or hundreds of millions to one. The scores for precognitive and retrocognitive trials are similar to those for concurrent trials, with no evidence for regression over a range of several days. And distance also seems not to matter; the perception of targets at international distances is indistinguishable from relatively local targets. But some variations in the experimental conditions do have a clear effect. As we developed more refined scoring procedures, the ability to capture information about the distant target seemed to decrease. The effort to provide more nuance and flexibility to our participants turned out to be not a boon, but something of a boondoggle. Brenda Dunne, who led the PRP program, had misgivings about the quantification from the beginning because it shifted focus from experience to assessment. But it was a necessary experimental investigation, with an answer that is important. The ability to 'far see' is fragile, and its requirements must be respected. It cannot be forced into an arbitrary mold for the sake of the scientific question. Instead, we must shape our scientific approach to study anomalous perception without sacrificing the free movement of

the mind that enables it.

This is a critical point for research on psi and consciousness in general, and it is one we should understand well enough to make it clear to outside observers, both proponents and skeptics. The core understanding is that we must respect the unique character of what we observe. The answers we obtain are in part determined by the questions we ask (a photon will be seen as a particle or a wave depending on the way we observe it). We cannot squeeze or stretch a subtle talent or an ephemeral phenomenon into any arbitrary form, but must accommodate its native dimensions.

Mind-Machine Interaction

The second major experimental program at PEAR was mind machine interactions, or MMI. We began with random event generator (REG) experiments asking participants, whom we called operators, to change the random output to higher or lower numbers, compared with baselines (Jahn *et al.*, 1997). We had an engineering mission, which was to find out whether human consciousness in special states might affect sensitive electronic equipment. Given that context, it will be no surprise that we were dedicated to precision and accuracy, and to a thorough and wide-ranging assessment. Ultimately, we created several unique experiments addressing similar questions using electronic, mechanical, hydrodynamic, and thermodynamic systems. Some of these were so beautiful as to deserve a place in a fine gallery or museum, but this was to help create conditions conducive to the 'impossible' tasks we set our operators. Again, we were attempting to provide space and opportunity for creative consciousness, and support for the subtle requirements of interactions between intention and effects in the world.

All the experiments were technically sophisticated and aesthetically elegant in their design and implementation. We made a pendulum with a crystal bob on a rod of fused silica enclosed in clear acrylic. Measurements were taken with a razor edge cutting a light beam with timing by a 50-nanosecond clock. We made a delightful small fountain whose transition from laminar to turbulent flow we monitored with photodiode arrays to see whether intention could augment or hinder the descent from order into chaos. And we built a random mechanical cascade of 9000 plastic balls bouncing through an array of pegs into collecting bins, forming a distribution that we tried to shift to the left or

right by sheer will or intention. This was a complex mechanical instrument three meters tall, and it earned the ironic name "Murphy" after the famous law, but it served well to ask whether psi could change behavior on a macroscopic scale. And there were more such explorations: a dual thermistor experiment asking for focused temperature changes, an interferometer displaying a shifting pattern of concentric interference fringes, a Crookes tube with a series of evanescent spheres formed by luminescent gas discharge, fluctuating iridescent patterns in a birefringent plastic lever arm. Suffice it to say that we covered a lot of ground in nearly three decades of the PEAR lab.

MMI Findings

A short list of major findings in the PEAR mind-matter interaction program includes many confirmations or replications of others' work. Indeed, the PEAR REG experiments were an extension of the work of Helmut Schmidt in particular (Schmidt, 1973), to provide a completely independent assessment using the best available technology and designs. The research continued for more than two decades, so there is much informative detail. The following summary points give some notion of the span and depth of the research findings:

1. There is an effect of conscious intention on the output of random systems
2. The anomalous effect is very small, but statistically significant over many replications
3. Depending on conditions, effect size is approximately equivalent to parts per thousand
4. Both high and low intentions yield correlated departures from expectation
5. Baseline trials may show reduced variance, suggesting effects of a 'baseline' intention
6. Trials conducted with the operator in local and remote locations have similar effect sizes
7. Trials conducted with the intentional effort prior to the data collection are also successful
8. Experiments with two operators who are a bonded pair have significantly larger effects

9. Serial position analysis shows early trials have large effect which decreases, but recovers
10. Anomalous effects differ in magnitude and style for individual operators
11. About 15% of unselected operators achieve significant overall performance
12. Effect size and style (symmetry of intentions) transfer from REG to other experiments
13. Experiments with a wide variety of random sources show similar effect sizes
14. Effects appear to depend on time invested in intentional effort and may be teleological
15. Anomalous effects depend primarily on psychological factors, not physical parameters

Group Consciousness

In the early 1990s, as miniaturization of electronics allowed construction of small but competent physical random number sources, we developed protocols for collecting data in the field. The question was whether REGs might be affected by mere attention rather than intention, and more generally, whether special states of consciousness might have a kind of 'field' effect. A variant of the REG program was created to take data continuously, and allow marking of the beginning and end of time periods of interest. For example, we took the REG (connected to a laptop or palmtop computer) to concerts, rituals, religious ceremonies, sporting events, board meetings, and various other events that might create a state of 'group consciousness'. The protocol was simple: moments or periods that we judged likely to produce coherent or resonant thoughts and emotions among the people attending the event were marked, and the data were later extracted for analysis. The prediction was for a variance increase (since there was no directional intention, either high or low deviations from expectation would indicate an anomalous effect). We looked at many kinds of events that we expected would produce group coherence, and for a control condition, we collected data in mundane contexts such as shopping centers, busy street corners, academic meetings, etc.

These experiments were termed 'FieldREG' studies, and over several years we accumulated more than 100 datasets from 'resonant'

situations, and a smaller but substantial number of 'mundane' locations (Nelson *et al.*, 1996, 1998b). A number of special series were undertaken, including data collection at operas, cathedrals, and sacred sites such as temples and tombs in Egypt. In a nutshell, these experiments showed that the REG data tended to depart from expectation in those situations that were conducive to a melding of individuals into a group consciousness. We found a few categories that were especially powerful, or rather, reliable—in the FieldREG experiments, like the laboratory experiments, effect sizes tend to be small, so that repetitions of essentially similar conditions are necessary to accumulate statistical significance. On the other hand, using a time normalized yield measure (Nelson, 2006), these natural, real world situations have a somewhat larger effect size than that found in laboratory experiments. The largest or most reliable effects seem to involve ritual or some other influence that is designed to bring people to a shared state of mind. On consideration this seems reasonable, though we had to learn by trial and error what the most conducive situations might be. We also found that the combination of collective activity in a special place could be counted on to produce structure in the random data sequence. For example, the Egypt series comprised a traveling group of people interested in ancient Egyptian spiritual practices, who intended to chant or meditate in sacred sites. That is, there was a pre-planned set of resonance-producing activities in the appropriate contexts, intended as a respectful attempt to connect to the spirit of the sacred places we visited. This series is the most consistent—and hence statistically robust—subset of the entire FieldREG database (Nelson, 1997; Nelson, *et al.*, 1998b).

FieldREG Findings

What did we learn from several years and over 100 formal assessments of the FieldREG question? In the PEAR database, we can make a strict meta-analytic combination across data subsets, and from that draw robust conclusions. These are supported also by independent work (Bierman 1996; Radin *et al.*, 1996). In all such research, it is necessary to use operational definitions, namely, a description of what is done to create or identify the item of interest, the group consciousness. Given that background, a short list of findings includes:

1. Changes in REG behavior correlate with special states of group consciousness
2. Situations conducive to resonant interaction produce increased data variance
3. Practices designed to create group unity and coherence yield larger deviations
4. Some venues may reliably yield decreased variance, but more study is needed
5. Mundane or chaotic situations yield only normal random data sequences
6. We infer that group consciousness can exist and can have anomalous effects
7. The studies tentatively suggest information field or 'consciousness field' effects
8. The nature of the questions we ask partially determines the experimental result
9. The potential range of FieldREG applications is broad, and invites further study

Among the several replications of FieldREG work were some that looked at events in distant locations, and some that used multiple REGs. Notable among these were Dean Radin's examination of data from 5 devices in separated locations taken during the reading of the verdict in the O. J. Simpson trial (Radin, 1997), and Roger Nelson's collection of data from 12 REGs in Europe and the US during Princess Diana's funeral (Nelson *et al.*, 1998a). Both of these events engaged the attention of millions of people, and both showed statistically significant departures from expectation at the most critical or poignant times. These and similar probes suggested it would be valuable to have a continuous record of REG data that could monitor the world stage for indications that special states of 'global consciousness' might affect our instruments in a way similar to the effects of group consciousness.

Global Consciousness Project

We began planning and building a world-spanning network of physical REG/RNG devices in late 1997. The architecture of the network was designed to use the Internet (which was coming to maturity at that time as a world-wide web) to transmit data from remote

nodes to a central server for archiving. Here is a brief description of the technology: Custom software on continuously running computers at each node collects one trial (comprising the sum of 200 bits) each second, from an REG on a serial port, stores the trials on the local disk, and transmits the data to a server in Princeton in checksummed 5-minute packets. Custom software on the server stores the data in permanent archives with all data synchronized using network time protocols. The result is a continuously growing swath of parallel data sequences extending from August 1998 to the present time (Bancel & Nelson, 2008; Nelson, 2001). The database is publicly available for download by anyone with an interest in checking our analyses or conducting original research.

A large and comprehensive website at http://noosphere.princeton.edu provides details of the technology and methods, a complete record of the formal hypothesis testing we have done over the years, the primary results, a growing spectrum of deeper explorations of the data, and some interpretive efforts. To date, there are over 250 rigorously vetted, pre-specified events in the formal series, including tragedies and celebrations, natural and human caused disasters, planned and spontaneous gatherings of great numbers. The primary experiment consists of formal events that are specified in a prediction registry prior to any examination of the data. Relatively few events are selected, and the formal series comprises 1.5% of the full 10-year, 15-Gigabyte database. Since we are breaking new ground in psi research, there is little or no history of similar research to guide hypothesis specification. We therefore use a general hypothesis that allows the criteria for selecting events and analysis tools to be kept deliberately free:

> Periods of collective emotional or attentional behavior in widely distributed populations will correlate with deviations from expectation in a global network of physical random event generators.

A series of replications (analyses of data corresponding to the individual global events) using this general approach allows us to maintain formal rigor while exploring a variety of occasions that bring people to a common focus. By accumulating subsets of event categories, we gain insight into psychological (or sociological) parameters that help determine the nature and magnitude of anomalous effects in the data. The approach allows considerable latitude in identifying events and constructing test statistics, but with a number of

constraints. The events specified in our formal hypotheses all involve large numbers of people, geographical extension, an engaging emotive or attentional character, and they are expected to promote or entail mental coherence.

The GCP is an evolutionary development in psi research which differs qualitatively as well as quantitatively from prior research. The globally distributed network produces synchronized data in parallel sequences from dozens of physical random sources, allowing a class of investigations that includes inter-device correlations, measurement of momentary variance and covariance, assessment of distance and time as parameters, and quantitative research on the possibility that multiple random sources may augment or otherwise differentiate the response.

GCP Findings

We have found that the anomalous effects typically take a different form from that observed in laboratory REG research. During 10 years of operation we have specified and analyzed 250 global events constituting our operationally defined moments of "global consciousness". The nature and scale of the database provide a number of unique opportunities and findings:

1. Technology exists to gather evidence of global consciousness, suitably defined
2. When global events transpire, we find anomalous structure in the GCP data
3. The average effect size is small, about 0.3 to 0.5 sigma, but conceptually replicable
4. The odds against chance for the composite formal result are about 1 in 10 million
5. The anomalous effects are seen in the collective behavior of the global network
6. Deviation (or structure) is primarily seen as excess pairwise correlation between RNGs
7. Distribution statistics of RNGs are unperturbed, but they correlate during events
8. Two independent, orthogonal correlation statistics respond similarly to the formal events
9. The orthogonal measures of network correlation are also correlated with each other

10. There is differential response of correlation statistics to categorized subsets of events
11. Both correlation statistics exhibit a similar distance dependence with scale ~8000 km
12. Temporal behavior of correlations show the GCP effects have a time scale of 1 to 2 hours

Detailed discussion is beyond the present scope, but some comments are in order. It is essential to understand that we do not look for 'spikes' in the data and then try to identify what caused them. Instead, we identify the event first, and then analyze the corresponding data—we make a prediction before examining the data and then test it in the data. This process yields a replication series of proper hypothesis tests which in their aggregate constitute a test of the general hypothesis given earlier.

The significance of each of the enumerated results and of the composite bottom line has been confirmed by extensive simulation using pseudo-random data and direct re-sampling analyses from the network database. We find that while we can measure deviations in data corresponding to the identified events, the database as a whole exhibits parameters consistent with statistical expectation.

The discovery of two demonstrably independent statistics is important to the development of models, and helps to constrain the range of possible explanations. It also helps assure that the anomalous results cannot be ascribed to data selection. The discovery that the anomalies are not simple, direct effects on individual REGs but are driven primarily by inter-device correlations is an instructive surprise. It is yet another indication of the complexity faced by psi researchers, and an example of the importance of the questions asked. The range of distances over which the inter-node correlations are detectable is approximately 8,000 km, and weighted regressions show a significant decline in effect size over this range. This indicates that while the measured effect is indeed global, it is nevertheless sensitive to the geographical extent of the network and the distribution of the events. We can ask what the implications are for the widespread, albeit still tentative idea that psi effects are fundamentally nonlocal. Finally, temporal structure is also an important feature of the GCP data. Our operationally defined global consciousness would seem to have a 'moment' of an hour or two, perhaps corresponding to the much faster time-scale of human consciousness where a sensory or emotional

impression can form in a small part of a second, perhaps as little as 100 milliseconds.

Discussion

Over the history of parapsychology, many calls have been made for the 'critical' experiment that would at last allay all doubts about the reality of psi phenomena. Skeptics have persistently demanded ironclad research protocols (while unfortunately failing to learn just how good psi research is). Meta analysis has shown successful replication in several separate protocols with high confidence (Radin & Nelson, 1989, 2003), while 'counter' meta analyses using different data subsets and criteria have sought to disabuse us of any impression that the question is resolved (Boesch *et al.*, 2006). Such academic battles and their accompanying publicity have had some value. Parapsychology research has far better research protocols as a result, indeed, better than several mainstream sciences (Sheldrake, 1998), and those protocols should give anyone who actually knows the literature confidence that, in Gertrude Stein's pithy phrase, "there is some there there."

Of course skeptical vetting is critically important to good research. It is fair to say that we all have biases, and without help from our skeptical and critical friends, we make mistakes and overlook possible misperceptions and misinterpretations. My own work in the GCP provides useful examples. May and Spottiswoode (2002) attempted to confirm our analyses of the data on September 11, 2001, and found that the data were good. However, they criticized our analyses of the data on the basis that no clear interpretation can be made without a well-defined hypothesis. That is correct, of course, and (though in fact we had such a hypothesis in place) we were reminded that exploratory analyses we wished to do, while useful, must be presented as a preliminary to formal work, and clearly differentiated. Scargle (2002) made a strong point that one of the protective measures we take to ensure unbiased data (a logical XOR operation) must necessarily prevent any effect of the sort we report. While there is a certain futility in arguing that reported effects simply cannot be (I think Scargle could not escape his internalized physical models), this argument suggested a focused investigation of the possible ways for a psi effect to penetrate the barrier we erect to prevent bias. As a result, we are much closer to an understanding of mechanisms that might allow the effects we see (and are confident are quite real) to occur.

Finally, this legitimate and important aspect of experimental science, true skepticism, led to the collaborations with Peter Bancel that have deepened and solidified the GCP analyses. Peter originally undertook to discover whether some problems in definitions, in specification for analysis, in selection of events, etc., might negate the highly significant bottom line for the formal results. He did find some problems, such as partially redundant events and some that were too poorly defined to justify inclusion. He discovered what appeared to be inconsistency in the analytical recipes. But after excluding the errors and rectifying the analytical issues, we found no substantial change in the anomalous effect. In any case, Peter was sufficiently intrigued that he brought his expertise as an experimental physicist and mathematician to bear on the many years of accumulated data, and over the past few years this has allowed a progressive expansion in the range of our assessments. This collaborative work has provided independent perspectives that help assure valid assessments, and it has generated a body of convergent evidence that not only satisfies critical concerns, but greatly deepens and extends our insights into the data.

Ultimately, we must shift attention to modeling—creating the best approximations we can for mechanisms and explanations. These can be tested against the actual data, and to the extent a model fits the empirical findings, it yields insight, refines our understanding of the structure found in the data, and leads to predictions that can be tested in prospective designs. In the end, we want to find a reasonable theory that provides a bridge from the empirical work to an integrated description, an explanation for the remarkable capacities of human consciousness.

Convergent Evidence

There is a powerful general point to be made from the psi literature. Given that there are many experiments and observations of high quality showing anomalies in a wide range of disciplines, and independent findings pointing to effects of consciousness that are not accounted for in ordinary psychological or physical theories, we can say that there is excellent "convergent evidence" that consciousness interacts with physical reality. When there is just one opinion, or one experimental observation on a phenomenon, it is difficult to make a case. But with more than 100 years of research by highly qualified scientists looking from different perspectives at the extended capacities and limitations of

mind, we can consider whether their findings converge. I think they do, in no uncertain terms, despite and indeed with the help of criticisms that ultimately have strengthened the evidence. We have personal experience and observation of natural occurrences of psi. We have laboratory experiments on extra sensory perception, clairvoyance, psychometry, psychokinesis, and more. We have extensions of these efforts to learn something in the real world, some pragmatic and some purely experimental. Government and business have requested and gotten help from psi practitioners, sometimes with high profile public presence as in the Stargate program of remote viewing. Pertinent to our theme, such work may be regarded as applications of techniques and findings from controlled laboratory research (Dunne & Jahn, 2003; Targ & Puthoff, 1977). Similarly, studies of micro-psychokinesis in the laboratory have led to field research on group consciousness attempting to confirm that special states of resonance or coherence reportedly stimulated by ritual, music, collaboration, and cooperation may have a detectable presence beyond the experiential (Nelson *et al.*, 1997, 1998a, 1998b).

The natural extrapolation of field research with REGs into the Global Consciousness Project is a multi-level example of convergent evidence. Not only does the GCP present an independent and completely different perspective on the question of whether mind has real presence in the world, its application of powerful modeling and statistical techniques to search for structure in this large and complex database seek convergent evidence internally. The result is a collection of findings that are on the one hand demonstrably independent, and on the other hand complementary; they are interlocked pieces of a comprehensive picture. Again we find indicators of a real entity that is anomalous in the sense that ordinary physical models do not yet accommodate it. But this evidence converges with and extends the field studies of group consciousness and the laboratory research with individuals. The GCP results say essentially the same thing as do the results of decades of psi research in laboratories around the world, albeit in a different but very rich language. Consciousness is real. It has a role to play as a presence in the physical world. Our work as psi researchers is to go on with efforts to learn more about that presence, and to make clear that the role of consciousness in the world is both real and important. In this first decade of the 21st century, it is becoming apparent that that role is critical.

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DISCUSSION

DOBYNS: I noted on the earthquake graph that it said in the small print that it is a cumulative deviation of the statistic we are plotting, which would seem to imply that whatever statistic it is, is actually below expectation for an extended period, at least the 30 hours you plotted, and then abruptly goes through an inflection at the earthquake moment. In other words, this looks very much like a presentiment effect. Could you comment on that?

NELSON: I can comment on that by saying yes it does look like that with a time period that is much longer — presentiment is just a few seconds. Here if it is real it is something like 8 or 10 or more hours of some kind of change in the data that is substantial before the event to come.

WEST: I would like to ask, you have presented a great deal of evidence suggesting there are important effects here and you suggested things that we should obviously be doing to follow through. Can you make any comment on the report that work of this kind has ceased at Princeton?

NELSON: The Princeton laboratory closed in February 2007 after 29 years of well supported work. The senior people in the laboratory retired so there is nothing negative about it; there was no attack, we simply ran out of time.

PRESENTIMENT RESEARCH: PAST, PRESENT, AND FUTURE

EVA LOBACH

Past, Present, and Future of Presentiment Research

Common sense believes the past to be behind us. The past is about events that cannot be changed. The future, ahead of us, is about events that cannot be known. We appear to live our lives between the unchangeable and the unknowable, caught in the present.

This common sense notion about the nature of time is, however, fundamentally challenged. The most well-known challenge is posited by quantum physics. Although physicists agree that quantum mechanics accurately predicts the physical behavior of systems, a lot of imaginative theorizing is going on about what it really *means*. For instance, we understand from Einstein's general theory of relativity that time slows down the faster you go. Going faster than the speed of light might imply going backwards in time, and is not assumed to be possible in Einstein's theory. In experimental studies of quantum entanglement, however, entangled particles have been shown to behave as one, responding simultaneously, even when they are far apart. This suggests faster-than-light communication between the particles—it looks as if they 'know' what is going to happen to the other in a presumably unknowable future. How are we to understand this?

Another challenge to our common sense idea about time comes from parapsychology. There is already a wealth of evidence that indicates that the future is not always as unknown as we commonly believe, as information about future events appears to have traveled 'back in time', somehow affecting our knowledge about those future events. Unless we believe all is predetermined, and future events can, in principle, be predicted 100% from present conditions, our notion of the past as rigid and unchangeable appears to be incorrect as well. Despite the experimental evidence so far, parapsychology is far from being able to boast the same accuracy of their predictions as found in quantum mechanics. However, human behavior is arguably determined by many more factors than is the case with a pair of atomic particles, and psychology in general never claims to be able to make predictions with quantum-mechanical accuracy. Nevertheless, the evidence in

parapsychology in time-related experiments gives rise to the same puzzlement. What does it mean?

Interestingly, quantum physicists and parapsychologists are now looking in each other's direction for clues on how to seduce Mother Nature to disclose her hidden laws, and some have started to work together on parapsychological research projects. In parapsychology, two phenomena in particular—precognition and presentiment—indicate that time, or the relation between cause and effect, is probably quirkier than straightforward common sense has it. Both precognition and presentiment imply knowledge about the future. Precognition is about conscious fore-knowledge of future events, while presentiment indicates that future events are preceded by changes in physiological processes, which are generally not accessible to consciousness.

Occurrences of precognition—the conscious type of fore-knowledge—sometimes happen spontaneously, as in the case of John Dunne, who dreamed he was being chased by two men and a dog, an event that actually happened to him the very next day (Dunne, 1927/2001). An analysis of a large collection of spontaneous cases of precognition showed that about 75% of them happened in dreams (Rhine, 1981). Spontaneous examples inspired parapsychologists to conduct controlled experiments of precognition. These experiments generally involved predicting the future order of a stack of cards which was still to be shuffled. A meta-analysis of these studies concluded that the effect was small but highly significant statistically (Honorton & Ferrari, 1989). Thus, precognition appears to be real. We are not sure, however, if participants really have knowledge about the *future*. Perhaps they extract knowledge from concurrent perceptions ('clairvoyance') and infer from this information what is likely going to happen in the future. Studies set up to discriminate between precognition and clairvoyance compared the accuracy of guesses that were made either before or after a computer selected a future stimulus. If precognition is inferred from concurrent information, we would expect smaller effect sizes for precognition trials because of inference errors. A meta-analysis of studies comparing pre- and post-selection guesses found no differences in effect size (Steinkamp, Milton, & Morris, 1998). It remains unclear, however, to what extent precognition and clairvoyance are fundamentally different. To complicate things, it cannot be excluded that participants use psychokinesis (PK) to influence the choice of a target, which would arguably increase the accuracy of the guesses to above chance levels. Even retroactive PK is

a possibility, according to which the conscious or unconscious intention of some agent (participant, experimenter, or researcher analyzing the data) appears to create a bias in already registered but not yet observed results. And in some experimental designs, telepathy with the experimenter may explain the results as well. For this reason, anomalous results that look like precognition, clairvoyance, PK, and telepathy are often subsumed under the blanket term 'psi'. While precognition, clairvoyance, PK, and telepathy are often considered to involve consciousness, presentiment is about the relation between *unconscious* physiological changes and future events. Presentiment is also known as 'prestimulus response', 'presponse' or 'anomalous anticipatory response'. I choose to use the word presentiment throughout this paper because it sounds a little less awkward than the other ones.

As with precognition, spontaneous cases have probably inspired the experimental study of presentiment. Sometimes we may experience an unusual sense of dread concerning either ourselves or a loved one. We may be more or less aware of this sensation, but it is not associated with a clearly expressible conscious thought. In cases where even the sensation is not consciously experienced, we may later report that for some unknown reason we 'intuitively' acted in ways that depart from our ordinary routine, such as the woman who impulsively and uncharacteristically decided to take the day off from work at her job at the World Trade Center on September 11, 2001 (Broughton, 2006). It seems plausible that these decisions are preceded by specific changes in physiological processes, changes of the kind that are in turn hypothesized to affect our ordinary daily decision making (Damasio, 1996).

Although physiological processes such as EEG or electrical activity at the surface of the skin (electro-dermal activity or EDA) vary naturally all the time, we are not consciously aware of these changes. In the laboratory, these physiological changes can be measured by specifically designed instruments. In differential presentiment studies the participant is presented with different types of stimuli, and physiological changes preceding each stimulus type are compared to each other, or compared to physiological changes in the absence of stimuli.

In the following, I will first look at results of the earlier presentiment experiments. Next I will discuss a number of recent

studies, and the hypotheses that are currently being explored. Lastly I will share my thoughts about how we might proceed in this field.

The Past of Presentiment Research

The roots of presentiment research can probably be traced back to the middle of the last century, when instruments to measure physiological processes became more commonly available. A. J. Good reportedly suggested that measuring brain potentials on the surface of the skull (EEG) of someone who sits in a dark room as a light is flashed at random moments might allow us to discover whether "the EEG shows any tendency to forecast the flashes of light" (Good, 1961, cited in Radin, 2006, p. 163). In the seventies, there was in fact a study conducted to explore whether the EEG showed any tendency to forecast, not flashes of light, but the gender of faces in pictures (Hartwell, 1978). However, the results showed no significant differences in EEG for different genders, despite laborious (especially at the time) and extensive analyses. At about the same time, Vassy (1978) did report highly significant results in an experiment that was set up to measure telepathy. It is worth mentioning here because the study's design was rather similar to that of later presentiment studies. Vassy measured the participant's EDA preceding an electrical shock for which they either were or weren't warned telepathically by someone in another room. As with Hartwell's (1978) EEG study, in those days the judging and analyzing of physiological measures was cumbersome and prone to error. This is perhaps why it took a rather long time before more studies were undertaken in this direction. By the end of the last century, however, Radin picked up the trail using modern, automated equipment in the first of a series of presentiment studies (Radin, 1997). Radin got interesting, statistically significant results corroborating his hypothesis that physiological processes respond differentially to future stimuli with either high or low emotional content. These results were soon replicated by Bierman, and together they published a summary of five different presentiment studies in a 'mainstream' psychological journal (Bierman & Radin, 1997). Like in Vassy's study, these early experiments used mainly EDA as the dependent physiological measure of presentiment; this measure seemed to produce the most reliable results.

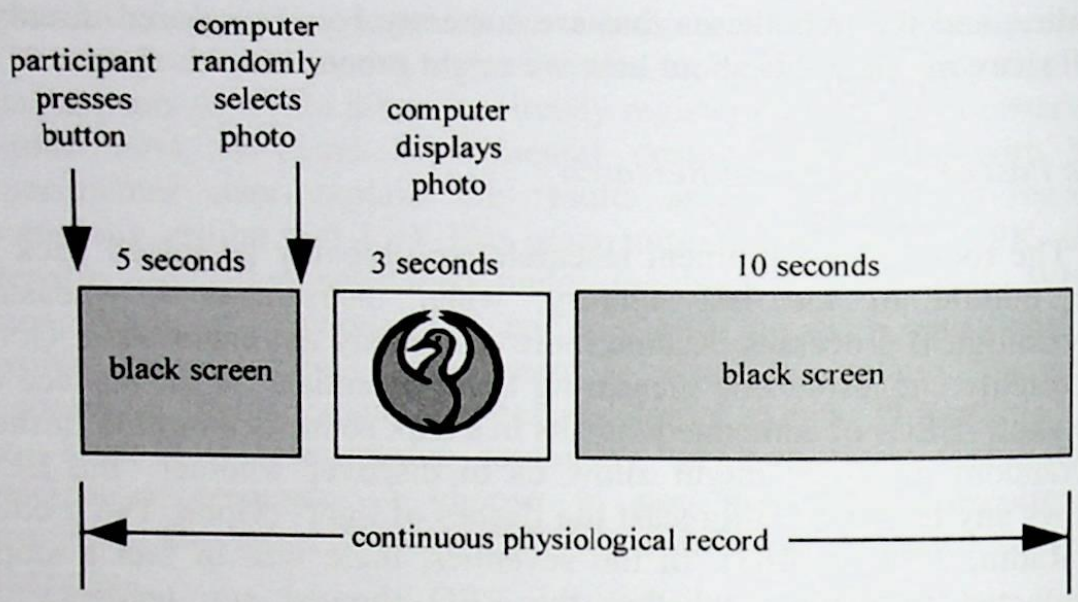

FIGURE 1.
Illustration of a trial in a presentiment experiment (from Radin, 2004)

In a typical presentiment study as reported by Bierman and Radin (1997), a participant is hooked up to an instrument to measure EDA. Most often this is done by attaching electrodes to two fingers of the non-dominant hand using a velcro band. Changes in skin conductance are measured continuously during the whole session. The participant is seated in a comfortable chair in front of a computer screen and remains passive, except when a signal indicates that he or she can start the next trial by pressing a button. The number of trials may vary, but is typically around 30. After the button press, there is a delay of a few seconds until a stimulus is presented, after which there is a cool-down period until the next signal (see Figure 1). Stimuli are drawn randomly from two picture pools. One pool contains calm pictures, such as photos of landscapes. The other pool contains arousing pictures with violent (e.g., a bloody car crash) or erotic content. These emotionally arousing stimuli typically produce strong *post*-stimulus EDA responses, as experiments in mainstream psychology have shown. Lie-detectors are based on this principle. In contrast, in presentiment studies the focus is on the time interval directly *preceding* the stimulus, from the button press until the picture is presented on the screen.

EDA measures can be analysed in a number of different ways. The studies conducted by Radin and Bierman looked at the average skin conductance level (SCL). The statistical analysis compares the SCL

averaged across all emotional trials with the SCL averaged across all neutral trials.

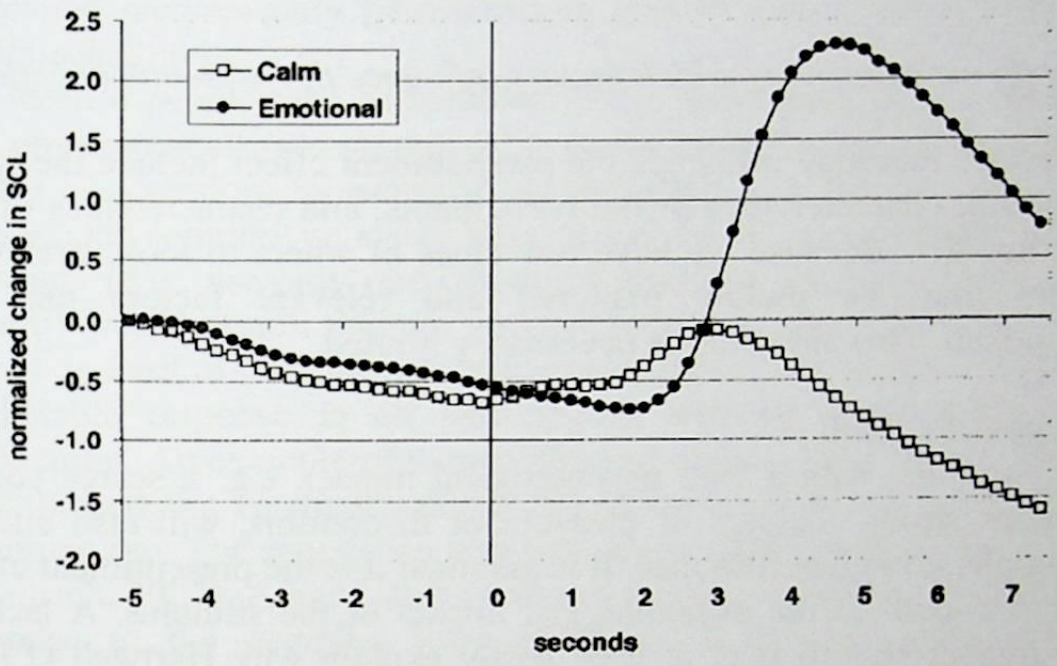

FIGURE 2.
Average normalized change in skin conductance level (SCL) for calm and emotional trials. The pre-stimulus period is indicated by negative seconds; stimulus onset was at time 0; stimulus offset at 3 seconds (from Radin, 2004)

Figure 2 clearly shows that the post-stimulus response in SCL is different for the emotional (black dots) and calm stimuli (white squares). The pre-stimulus difference between the two is much smaller, but because it is relatively consistent across trials and participants, it is still statistically significant.

These early, replicable successes soon gave pre-sentiment studies the aura of the 'Holy Grail' in parapsychology—experiments that can reliably produce psi-effects and so allow for the much wanted testing of specific hypotheses, so that progress can be made in understanding these strange phenomena. Indeed, a number of different hypotheses are presently being explored using presentiment studies.

The Present of Presentiment Research

In order to be able to 'chart the future', it is necessary to take stock of current evidence and theories. In the following, I will discuss experimental evidence about factors that influence the presentiment

effect, different types of dependent measures that have been used so far in presentiment studies, and a selection of current theoretical approaches that attempt to explain presentiment effects.

What do we know now: which factors influence the presentiment effect?

Factors that may influence the presentiment effect include the types of stimuli, characteristics of the participants, and characteristics of the experimenter. Because we have few clues of where to look, irrelevant factors may be unduly explored and relevant factors may be overlooked. This summary is necessarily limited.

a. Types of stimuli

A stimulus with a high psychological impact, e.g. it scares you or produces strong feelings of pleasure or discomfort, will also elicit a strong physiological response. It is assumed that the presentiment effect is proportional to the psychological impact of the stimulus. A lack of high-impact stimuli may at least partly explain why Hartwell (1978), who used pictures of male and female faces, was unsuccesful in producing a significant presentiment effect (Bierman, 1997).

Presentiment studies presently aim to use stimuli that have the highest impact without running into trouble with ethics committees. How do we assess the psychological impact of a stimulus? When using pictures as stimuli, researchers can make good use of the International Affective Picture System (IAPS), which is a stock of photos that have already been rated for emotionality by a large number of people (Lang, Bradley, & Cuthbert, 1999).

Although pictures are being selected to be either highly arousing or calm, even pictures in the same category are somewhat different in their emotionality rating. Thus, the correlation between pre-assessed emotionality and presentiment effect can be explored. This was done by Radin, who indeed found a very small (r = .04) but statistically significant correlation across a large number of trials (Radin, 2006). Of course, individuals often experience pictures differently from the average pre-assessed emotionality. Idiosyncratic responses to pictures may thus weaken a presentiment effect statistically, because it increases the variability.

Although stimuli that produce stronger post-stimulus responses may produce stronger presentiment effects, there is some indication that this is true only if stimuli have strong emotional content. A flashing

checkerboard stimulus creates a strong response in an EEG, but an EEG-study using a checkerboard stimulus and neutral and emotional pictures, demonstrated a significant presentiment effect for the emotional pictures only (Hinterberger, Studer, Jäger, Haverty-Stacke, & Walach, 2006).

Because people tend to habituate (stop responding psychologically and physiologically) to stimuli with strong emotional content over the course of a session, 'presensing' these stimuli may likewise diminish. A possible solution to avoid habituation is to use arousing stimuli sparingly (e.g., use only one emotional stimulus for every two neutral stimuli) or use stimuli that are unconditional, i.e. the response to these stimuli is hard to control. Unconditional stimuli will thus elicit a more predictable response in all participants, and are more resistent to habituation. An example of a unconditional visual stimulus is a flash of light. Indeed, we've already noted that this was Good's suggestion half a century ago. The suggestion was actually carried out by Radin (Radin & Lobach, 2007). A light flash produces a rather strong and predictable response in the occipital lobe at the back of the brain. Results comparing EEG in flash and no-flash trials showed a significant presentiment effect, but only for the 13 women participating in this study. The seven male participants showed on average a non-significant trend in the opposite direction (Radin & Lobach, 2007).

Inspired by Vassy's (1978) research using electric shocks, which is a strong unconditional stimulus, Spottiswoode and May (2003) used a loud burst of white noise as a somewhat milder stimulus (compared to electrical shock) to annoy their participants in a carefully designed study. Instead of trials of predictable length, they adapted the design in such a way that the participant was completely passive (no button press to start a new trial). By varying time intervals between stimuli, participants were also unable to predict—by any known means—when the next burst of noise would come. Like Vassy, they chose EDA as the dependent variable. Instead of using the SCL, as Radin and Bierman did, however, they looked at skin conductance responses (SCR), a characteristic bump after a slight depression in the skin conductance level. The proportion of pre-stimulus SCRs was significantly higher in the white noise trials than in the silent trials. A replication, using not just one type but a variety of startling sounds like sirens and gun fire, yielded essentially the same results (May, Pauliny, & Vassy, 2005).

A few years ago I supervised a student project study that served partly as a pilot study in a larger project undertaken by Dick Bierman at

the University of Amsterdam. Here we used two different sounds, an unpleasant one (a loud alarm) and a neutral one (gently plucking the string of a base guitar) (Lobach *et al.*, 2007). The two sounds were selected from a pool of sounds that were rated for their pleasantness in previous experiments. The study aimed to compare presentiment for long-term meditators (n = 30) and non-meditators (n = 17) in both an alert and a meditative state. The design was similar to Spottiswoode and May's in the sense that participants did not have to press any buttons, and the randomly selected sounds (alarm or base guitar) were presented at intervals with randomly varied length. We calculated the average amplitude of a SCR (bump) and compared these for unpleasant and neutral trials in both alert and meditative state. Contrary to our expectation, both groups showed no presentiment at all in the meditative state. More surprisingly, in the alert state, the amplitude of pre-stimulus SCRs in neutral-sound trials was larger than in the unpleasant-sound trials. The difference was statistically significant only for the long-term meditators (Lobach *et al.*, 2005)

b. Characteristics of participants

Few studies report gender differences in presentiment. Common sense and experimental evidence attribute more intuition and psi to women, and results from the few presentiment studies reporting results for the men and women separately appear to agree. The light flash study (Radin & Lobach, 2007) showed an EEG presentiment effect in women, but not men. A study (N = 26, 11 men) conducted by McCraty and colleagues, combining EDA, heartrate variability and EEG using 15 emotional and 30 calm pictures as stimuli, reported no EDA (using SCL) presentiment effect for either men or women (McCraty, Atkinson, & Bradley, 2004a, 2004b). However, both heartrate variability and EEG showed a significant effect, although the EEG results suggest that men and women process presentiment in different parts of the brain. Thus, reported gender differences in presentiment effect are so far limited to EEG measures of the brain.

Behavior traits like openness to new experiences, intuition, and extraversion have traditionally been reported as related to psi. As far as I know, only one presentiment study looked into this (Broughton, 2004). This study tested 64 participants in two separate sessions, using emotional and neutral pictures as stimuli and EDA as the physiological measure. No presentiment effect was detected, neither with Radin's (1997) or Spottiswoode and May's (2003) method of analysis.

Although the *post*-stimuli responses showed a significant test-re-test correlation, there was no such *pre*-stimulus correlation. A compiled individual presentiment score, averaged across the two sessions, did however correlate positively and significantly with the intuition scale of the MBTI (based on Jung's personality typology) and with the Openness scale of the NEO-FFI personality inventory. This confirms earlier findings in other studies of psi and personality characteristics.

In a series of three replication studies (Bierman & Radin, 1998), exploring a number of design features such as exposure time of the stimuli, the second and the third study showed a markedly reduced effect size. Because they could not plausibly link this to features of the study's design, the authors suggest that characteristics of the participants could have caused the discrepancy. Health care professionals following a course in Therapeutic Touch participated in the first study, while university students participated in the latter two studies. Almost 25% of the student participants indicated that they felt skeptical about paranormal phenomena.

Long-term meditators appear to differ from people who do not meditate, not just in their physiological responses but in particular characteristics of their brain as well (e.g., Davidson *et al.*, 2003). Although it is difficult to ascertain whether meditation is a cause of these differences, or whether pre-existing differences induce some people to become long-term meditators, there is some evidence that meditation may produce both mental and physical changes. One of these changes may involve an improved intuition and a clearer awareness of subtle emotional feelings (Nielsen & Kaszniak, 2006). As far as I know, three studies have examined effects of meditation on presentiment. The previously mentioned study by McCraty *et al.* (2004) investigated 26 practioners of the Heart Lock-In™ technique. This technique induces a state of "physiological coherence", which is "characterized by reduced nervous system chaos and increased synchronization and harmony in system-wide dynamics", according to developer Doc Childre of this trademarked technique (http://www.heartmath.com/health/professional/health_studies.html).
Although this technique is not described as meditation, there are a number of similarities, such as the attempt to focus or refocus the mind, and to generate and sustain positive emotions. Participants in the study were tested twice, once with and once without applying the Heart Lock-In technique. There was no control group. Skin activity preceding calm and emotional pictures showed no presentiment effect in either

condition. Heartrate changes showed a presentiment effect in the non-coherence condition for both men and women, and in the coherence condition for women only. EEG showed a significant presentiment effect in both conditions, but at different locations on the head in the coherence and non-coherence condition. Men and women also showed significant differences in different locations of the brain, both in the coherence and the non-coherence condition.

Lobach *et al.*'s (2005) study using two different sounds—already mentioned above—showed a *reversed* presentiment effect (pre-stimulus SCR higher for neutral than for emotional trials). This effect was only statistically significant in the non-meditative state for long-term meditators.

Long-term meditators ($n = 8$) and controls ($n = 8$) participated in an fMRI study by Bierman (2007), using 48 neutral, 16 erotic and 16 violent pictures as stimuli. Although data are presently still being analyzed, preliminary results of the BOLD signal replicate the significant fMRI presentiment effect reported in an earlier fMRI study (Bierman & Scholte, 2002). Meditators (both while meditating and while not meditating) and controls appear to show the effect equally strongly when comparing emotional (violent and erotic) with neutral trials. Looking at the two different types of emotional stimuli, however, a surprising result surfaced: Controls and meditators in an alert state are similar in showing a more pronounced presentiment effect for violent pictures than for erotic pictures. For long-term meditators in a meditative state, however, the presentiment seems to disappear for the violent pictures, but is pronounced for the erotic pictures instead (Bierman, 2007).

A few personal observations seem noteworthy here. One of the participants in the Radin and Lobach (2007) EEG light-flash study practiced Transcendental Meditation (TM) for many years. Her results were individually significant, but in the reversed direction from the large majority of the other women. In the student project with long-term meditators, during one of the first sessions involving a yoga teacher with 30 years of experience, the students watching the skin response in an adjoining room could hardly contain their excitement when they saw the EDA signal go almost completely flat when the yoga teacher went into meditation. And according to McCraty *et al.* (2004a), Dean Radin told them that he tried to avoid participants with long-term meditation experience because he believed their physiological responses were more subdued. From the scarce evidence

so far, the presentiment results with meditators are mixed but intriguing.

c. Characteristics of the experimenters and analysers.

Although characteristics of the experimenter, especially their belief in or openness to the existence of psi, are known to influence the results of psi experiments, I know of no presentiment study that has included this as a factor in the design. In an exploratory analysis of the Radin and Lobach (2007) study, results for the two experimenters did not differ.

What do we know now? Different physiological measures as the dependent variable

So far, physiological measures used in presentiment research include skin conductance (EDA, both SCL and SCR), EEG, heart rate and fMRI. All have shown a presentiment effect in one or more studies. The conclusion seems warranted that presentiment is distributed throughout the whole body (McCraty *et al.*, 2004b), as is the case with physiological changes involving emotions (cf. Broughton, 2006).

Analyzing physiological data is by no means straightforward. The problem starts with rather large individual differences. Some people produce no measurable skin signal; others have a very 'noisy' signal. EEGs are wasted by strong signals of muscles on the skull or uncontrollable electrical appliances nearby, and heartbeats are skipped at crucial moments. All measures contain artifacts. Criteria for eliminating them are relatively arbitrary. When you are finally left with a collection of squiggly graphs, which squiggles should you use exactly, and how do you translate them in a single measure that represents the signal you are interested in as accurately as possible? A number of different methods are presently being used to analyze these data, and some studies have tried to compare a number of different methods to test whether or not they make much of a difference, concluding in some cases that it didn't (e.g., McCraty *et al.*, 2004b) and in others that it did (Broughton, 2004; Hinterberger *et al.*, 2006). Most studies make a painstaking effort to describe as accurately as possible how the squiggles were processed, which is of course helpful to reach consensus about methods of analysis, and will eventually make it easier to compare results from different studies.

Because physiological processes can be quite different from person to person, individual presentiment effects may potentially cancel each other out. Maybe we each have our own presentiment effect, and it is only a matter of luck (or experimenter effect?) if these idiosyncratic effects do add up to produce significant effects. In the meditation project, Dick Bierman noticed that across two sessions, participants showed about 70% consistency, while only 50% would be expected if there were no effect at all. This may mean that about 20% of the people show a consistent presentiment effect, even though many may show individual characteristics in direction and timing of the effects. In the future we may need to tailor our analyses to accommodate these individual presentiment effects. Attempts to do this were already undertaken by the EDA presentiment study by Broughton (2004) and the EEG presentiment study by Hinterberger *et al.* (2006).

Using other types of physiological measures like the tension of facial muscles (EMG) may help further explore differences in presentiment for different types of stimuli. Looking at sad or funny scenes activates different muscles in the face, and there is already a whole body of psychological research available that may serve as a guide for analyzing the data. So far, there are some indications that at least violent and erotic pictures lead to somewhat different physiological presentiment patterns (e.g., Bierman & Radin, 1998).

What do we know now? A selection of current thoughts and theories

a. Evolutionary perspective

Instead of asking how psi occurs, some researchers have focused on the why (e.g. Broughton, 2006; Carpenter, 2008). If psi is part of our phenotype, it seems plausible that psi-genes have evolved at some point. Genes are supposed to be useful in a survival-related and offspring producing way. Building forth on the model of Stanford's psi-mediated instrumental response (PMIR) (Stanford, 1974, 1990; cited in Broughton, 2006), psi might somehow blend in with the unconscious part of the emotional system, which plays an important role in memory and decision making. Broughton believes conscious, controllable psi may lead to misuse and consequently affect survival in negative ways. Unconscious psi as part of a defense mechanism would make more sense. The unconscious aspect of it may partly explain its whimsical behavior both inside of the laboratory and outside. When psi unconsciously influences emotions, they may in turn elicit affect-

matching images from memory. According to this reasoning, psi is especially prone to produce images that are easily accessible to that individual. For the presentiment research, this theoretical approach would predict that situations and stimuli experienced as threatening are more likely to produce psi.

b. Decision Augmentation Theory (DAT)

In presentiment experiments, physiological processes preceding emotional stimuli are compared to those preceding neutral stimuli. If a strong, unconditional stimulus is used, such as a loud, alarming sound (May *et al.*, 2005), the 'neutral' trial is silence. From the perspective of the participant, there is only silence, punctuated now and then by a loud noise. So we would expect that it would make no difference which particular time intervals are used as silent trials. In practice, some random process assigns particular time intervals as silent trials. Nevertheless, we would expect that the skin response does not differ for the randomly determined neutral trials or any other time interval of silence, presuming they are not close to a noise stimulus. Oddly enough, this expectation turned out not to be true. Analysis showed that the statistical significance of the presentiment effect was not due to the predicted heightened activity preceding loud noise, but due to *reduced* activity preceding the randomly determined neutral trials of silence. There was no significant difference between time intervals preceding loud noise and silent time intervals that happened to be unrelated to the neutral trials (May *et al.*, 2005). The authors suggest that the presentiment result may be attributable to a psi-induced decision by the experimenter, who by some unknown means knew how to start the session at the right moment so the favored presentiment outcome would be found. The authors call this idea Decision Augmentation Theory: psi 'augments' the decision in such a way as to produce the desired result. Whether or not presentiment is indeed the result of a experimenter's prescient timing, we would assume that this interpretation of the results is most plausible in experiments where actions of the participant cannot disturb the experimenters decision making. Indeed, participants are completely passive in the experiments conducted by Spottiswoode and May (2003) and May *et al.* (2005). In studies where participants make their own decisions by pressing a button to start the next trial, as in the studies of Radin and Bierman, it is unclear how DAT can explain the presentiment results. Do the results here reflect a battle of wills between the experimenter's decisions and those of the participants?

Experiments that vary the participants' freedom to initiate the trials may throw some light on this issue. This would probably not be conclusive, however; theoretically, the experimenter may have psi-augmented knowledge about the decisions of the participant as well. Another approach would be to let a random number generator (instead of the experimenter) decide at what time the experiment should begin. This may not cut it either, because the experimenter may arrange it in such a way (unconsciously psi-augmented) that the random number generator will produce the desired session times. The future in DAT needs ingenious experiments.

c. Theoretical frameworks about time and causality.

Presentiment studies are sometimes said to be about retrocausality; a cause in the future retro-actively produces an effect in the past. Retrocausality may imply that there is something amiss with our idea of time, with our idea of causality, or perhaps both concepts are inaccurate—events may be related in some lawful way that does not involve time or causality.

Questions about time and causality do not only keep parapsychologists awake at night; some physicists appear to be similarly sleep deprived. Attempts to approach parapsychology from a physics point of view (or vice versa) seem promising. I cannot do justice to all theoretical developments, especially because physics is not my area of expertise. However, I would like to highlight three theoretical approaches.

The first approach is a theory proposed by Bierman (2008), who reasons from the notion that all physical formalisms are time-reversal invariant, i.e., whether you go forward in time or backward in time, you can use the same formula yielding a forward-in-time and a backward-in-time solution. The forward-in-time solution is the common one in the world as we know it. Trying to find the conditions under which time-reversed solutions may still occur, Bierman uses Price's interpretation of Wheeler and Feynman's (1945) discussion of time symmetry in electromagnetism (Price, 1996, cited in Bierman, 2008). In electromagnetism, the forward-in-time solution is to be expected in coherent electromagnetic transmitters producing so-called *retarded waves*, while the backward-in-time solution (coming from the future) would need a coherent electromagnetic absorber, absorbing so-called *advanced waves*. A coherent absorber would thus allow for events that go backwards in time.

Taking a leap from physics to psychology, Bierman assumes that the conscious brain can sometimes act as a coherent absorber and thus allow for time-reversed processes like precognition and presentiment. This idea gave the theory its name: Consciousness Induced Restoration of Time-Symmetry (CIRTS: Bierman, 2008). Looking for coherence in consciousness, clues may be found in brain activity. Normal EEG registration of brain potentials (reflecting neural activity of the brain) shows a disorderly and rapidly changing pattern. In some circumstances, e.g., during meditative states, the pattern of waves may become more rhythmic and orderly, and start to move synchronously over large areas in the frontal regions of the brain. This phenomenon is dubbed 'synchrony', and also, coincidentally or not, 'coherence'. Interestingly, the Heart Lock-In technique mentioned earlier brings about a kind of meditative state that is referred to as physiological coherence. Perhaps this is related or similar to the kind of coherence that Bierman believes can help restore time-symmetry. Bierman did suggest that meditation may be one of the conditions for the necessary coherence to come about. Although it may prove difficult to define the criteria for coherence, once this is established, the hypothesis that increased coherence is correlated with an increase in time-reversed events is definitely testable. The design of presentiment studies is suitable for this. In order to test the time-symmetry hypothesis, Bierman suggests to include trials where two stimuli are presented shortly after each other. If the presentiment effect depends on time-symmetry, Bierman hypothesizes that two presentiment 'bumps' will appear in the two-stimuli trials, versus only one bump in the single-stimulus trials.

A second source of ideas is a group of Italian researchers, who publish an online journal called *Syntropy* (www.syntropy.org). The group is inspired by the insights of Fantappiè (1941, cited by Vannini, 2007), an Italian mathematician. While working on quantum mechanics and special relativity equations, Fantappiè "noted that the retarded waves (retarded potentials) are governed by the law of entropy, while the advanced waves (advanced potentials) are governed by a symmetrical law that he named syntropy." (Vannini, 2007, p. 2).

> Suddenly I saw the possibility of interpreting a wide range of solutions (the anticipated potentials) of the wave equation which can be considered the fundamental law of the Universe. These solutions had been always rejected as 'impossible', but suddenly

> they appeared 'possible', and they explained a new category of phenomena which I later named 'syntropic', totally different from the entropic ones, of the mechanical, physical and chemical laws, which obey only the principle of classical causation and the law of entropy. Syntropic phenomena, which are instead represented by those strange solutions of the 'anticipated potentials', should obey two opposite principles of finality (moved by a final cause placed in the future, and not by a cause which is placed in the past): differentiation and non-causable in a laboratory. This last characteristic explained why this type of phenomena had never been reproduced in a laboratory, and its finalistic properties justified the refusal among scientists, who accepted without any doubt the assumption that finalism is a 'metaphysical' principle, outside Science and Nature.
>
> (Fantappiè, 1941, cited by Vannini, 2007, pp. 2-3)

According to Vannini (2007), Fantappiè saw living systems as a direct consequence of advanced waves; retrocausality might therefore be tested using living systems. As positive indications that this reasoning may be valid, Vannini cites the presentiment studies by Radin and Bierman, and research undertaken by Tressoldi and colleagues in Italy. In one of Tressoldi's experiments, participants looked at four pictures in a row, and had to guess which one of these four would later be selected by the computer (Tressoldi, Martinelli, Massaccesi, & Sartori, 2005). While the conscious choices were not correlated with the pictures selected by the computer, the heartrate decreased while looking at the picture that would later be selected as the target. This approach also reasons from the time-symmetry of physical formalisms and assumes that presentiments are instances of backward-in-time solutions to these formalisms. The Italian approach differs from Bierman's CIRTS theory (Bierman, 2008) in that it lacks the coherence condition. As soon as coherence can be operationalized, the two approaches will predict different outcomes, and can be pitted against each other in principle. The syntropy approach would predict that presentiment will occur in all living creatures (including, presumably, plants), while Bierman's CIRTS would predict that presentiment will be limited to consciousness in a state of coherence.

The third approach is from Shoup (2002, 2006), who builds forth on the work of Cerf and Adami (1995, 1996, 1998, cited by Shoup, 2006). In the traditional view of quantum uncertainty the collapse from superposed states into classical states, e.g., as in decaying radioactive

material, happens randomly. Because of its randomness, it is uncaused, and thus also bars a potential causal influence backward in time. Shoup argues, however, that a "superposed state is quite definite and not uncertain at all, it is just not in the laboratory basis." (Shoup, 2006, p. 172). Traditionally, the environment is not taken into account in a quantum measurement, where only two players are considered, the measuring apparatus, and the quantum system that is being measured. When the environment is included and seen as part of a unitary three-way interaction, Shoup argues, the outcome is dependent on future conditions in this three-way system. Usually these outcomes appear totally random to us, but under the right circumstances, given enough observations, they may present themselves to us as if they are influenced backwards in time. This (apparent) backward influence is only small, according to Shoup, because the universally increasing entropy biases the 'flow' of time in the forward direction, while the backward direction is 'uphill' and more difficult.

Shoup's suggestion is that in order for a macroscopic system—such as a human being—to be affected by future environmental events, it may need to behave in crucial ways like a Random Event Generator (REG). An REG uses quantum events (e.g., the decay of radioactive material) to produce (apparently) random pulses as output. In order to act like an REG, human beings would have "to reduce their determinism and increase the mutability of their mental choices to such a level that they are responsive to subtle backward influences" (Shoup, 2006, p. 177).

This is an interesting suggestion, although going from an unbiased RNG to an unbiased mind seems quite a leap. However, there is some evidence to support the idea that putting the mind on hold, as it were, is conducive to psi. Most spontaneous cases of precognition happen during dreams, when daytime consciousness is more or less left out of the loop. Also, one of the personality characteristics that has consistently been linked to increased psi performance is openness to new experiences (implying less bias). Shoup suggests that meditation may quiet the mind and thus similarly reduce the determinism of a busy mind. However, as we saw previously, the positive effects of meditation on psi seem somewhat mixed. Perhaps different types of meditation have different effects. Some types of meditation require a strong focusing of attention and may hamper the required state of indeterminism instead of fostering it. Still, trying to define mental states of unbiased indeterminacy and looking for relationships with psi

is certainly worth exploring experimentally. It may turn out that Shoup's mental states of unbiased indeterminacy are the same as the coherence posited by Bierman's CIRTS theory.

The above three attempts to understand presentiment and its consequences for our ideas of time and causality from a physics point of view all look promising and worthy of further development and experimental testing.

The Future of Presentiment Research

'Charting the future' of presentiment research first requires us to set a goal. What would we consider a success for parapsychology and presentiment research in particular? Ultimately, as with any science, the goal is to gain a better understanding of 'life, the universe and everything' by testing and revising hypotheses and theories. To increase the pace with which we go forward, we need more people and more money. There are already a number of encouraging developments resulting in more scientists listening to, and becoming involved in parapsychology.

Firstly, more physicists are becoming vocal about their interest in parapsychology. For instance, the 2006 San Diego conference on *Frontiers of Time, Retrocausation* organized by physicist Daniel Sheehan led to many thoughtful and engaging articles. Seeing more and more physicists taking this subject seriously may induce others to similarly consider the richness of parapsychological evidence, ready to help develop or support new interpretations of the fundamentals of physics.

Secondly, research in presentiment allows for testing hypotheses in designs that are very familiar to mainstream psychologists, making it easier to engage them, and attract their financial support. An example is the experimental design that helped discover the 'mere exposure' effect. The mere exposure effect describes the fact that we feel more attracted to initially neutral stimuli when we encounter them more often. These encounters don't need to be particularly positive in any way, 'mere exposure' is enough to make us like them more. If our initial feelings are negative, mere exposure decreases our dislike. If our initial feelings are positive, however, mere exposure tends to decrease our degree of liking. This phenomenon is therefore also described as a habituation effect; our affective responses to strong emotional stimuli will be less extreme the more often we see them. Experiments designed

to show this type of habituation can be adapted to look for time-reversed effects; instead of the usual order of being exposed to stimuli first and rating their attractiveness later, participants rate the stimuli first, and are being exposed to them later. Results show that we indeed tend to precognitively habituate to stimuli we will be exposed to in the future (Bem, 2003).

The same approach can be used for all kinds of experimental designs that are used to understand learning processes, and there are a lot of those in psychology. In the parapsychological version of these designs, we would expect better test results for elements about which participants will learn more later. Psychologists are familiar with these paradigms, which makes it easier to convince them that something strange and interesting may be going on. At the same time, the familiar design may encourage them to try it out for themselves.

Thirdly, the existence of psi is intimately linked with questions about consciousness. Consciousness may be on a par with quantum theory in terms of the number of questions it raises about the fundamental properties of reality. Consequently, researchers interested in consciousness are likely to look across traditional divisions in the sciences. Already parapsychologists present papers at consciousness meetings. Relatedly, studying the effects of different states of consciousness such as meditation or hypnosis on psi performance appears a promising approach so far.

In addition to meditation and hypnosis, another phenomenon in consciousness is the radical, sustained shift in perception that happens to some people, after which they experience reality from a 'nondual' perspective. The shift in perception is often described as 'awakening', and is accompanied by the realization that the previous perception of reality was illusory, like a dream. Leo Hartong (2001) describes how he experienced his shift in perception:

> Eternity, which I had understood as time without end, appeared as the absence of time. Everything was infused with life, including what, up until that moment, I had considered inanimate. All existence shared a common source and the first day of creation and the final day of destruction were seen as equally present. The Universe was neither big nor small. It revealed itself as simply One beyond all relative attributes, such as size, location, and time. While on the relative level, everything's purpose was shown to serve everything else in an intricate mosaic of perfect harmony, the totality of creation

> showed itself beyond purpose. I saw that it simply is as it is: Its own cause and fulfillment.
>
> (Hartong, 2001, pp. 109-110)

Nonduality teacher Nisargadatta Maharaj said, "No thing in existence has a particular cause; the entire universe contributes to the existence of even the smallest thing; nothing could be as it is without the universe being what it is." Interestingly, this last statement is reminiscent of Shoup's interpretation of quantum theory. Although both the nonduality view and Shoup's interpretation of quantum theory challenge our notions of time and causality, they are very different as well. Exploring the similarities and the differences may prove fruitful in formulating testable hypotheses that lead to a more fundamental understanding.

The scientific method relies heavily on our present interpretation of time and causality. If empirical data and theoretical developments in presentiment research lead us to conclude that time and causality differ fundamentally from what we hold them to be now, then the scientific method may find itself, paradoxically, in uncharted territory.

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DISCUSSION

NELSON: Thank you Eva, regarding the DAT explanation for results of this kind, as far as I am aware there is only the one experiment by May and Spottiswoode where that would seem to be a good explanation. Do you know if there are any other examples in the database?

LOBACH: There are at least 2 experiments, one by Spottiswoode and May, and one by May and Vassy. Zoltan Vassy is here so you may want to discuss these with him.

ROE: I have a question that I think relates to Dick Bierman's notion of time symmetry in psi effects. One of the impressive things for me about Daryl Bem's work is that the patterns he is looking for with his time-reversed effects — in terms of which kind of person is sensitive and what kind of material they are sensitive to — seem to emulate what we see with the regular psychological effect. I wonder if we would see something similar with presentiment as well; that is, if we have very strong responders (in the conventional sense) to violent or emotional stimuli might they also *pre*-respond more strongly than people who are not so reactive.

LOBACH: I do not know if there is any relationship between them. What I do know is that people who are more labile in their conventional responses seem to be more sensitive to these effects.

SEARCHING FOR NEURONAL MARKERS OF PSI: A SUMMARY OF THREE STUDIES MEASURING ELECTROPHYSIOLOGY IN DISTANT PARTICIPANTS

THILO HINTERBERGER

During the last decade a number of studies have been conducted to test the hypothesis of whether under conditions of strict sensory isolation the electro-encephalogram (EEG) of remote subjects can become correlated beyond chance levels (Fenwick *et al.*, 1998; Grinberg-Zylberbaum *et al.*, 1987, 1993; Radin, 2004; Standish *et al.*, 2001; Wackermann *et al.*, 2003, 2004). These correlations have mainly been measured in an experimental set up that is often sloppily termed a 'sender-receiver paradigm', a formulation we try to avoid for reasons mentioned below. In such studies one person, referred to as 'sender', is exposed to auditory or visual stimuli, while another person, the 'receiver', relaxes in a distant, mainly shielded room. The 'receiver' only or both participants are measured by EEG, skin conductance or even functional MRI — in the present study, we focused on experiments with EEG. Using this technology, clearly visible event-related potentials (ERPs) in visually or auditory stimulated brains can be evoked. The hypothesis of these studies was that stimulation of the 'sender' persons also elicits some kind of ERP in the EEG of the 'receiver' co-participants.

History of Studies

In the 1960s, two studies reported evidence for EEG correlations in pairs of subjects. In one of these, the pairs consisted of students and teachers (Tart, 1963), and in the other of identical twins (Duane & Behrendt, 1965). About 20 years later, Grinberg-Zylberbaum and colleagues reported the existence of what they called 'transferred potentials' (TPs) in a number of experiments (Grinberg-Zylberbaum *et al.*, 1987, 1993, 1994).

The terms 'sender' and 'receiver', as well as 'transferred potential' imply the model of a signal transfer between the two participants, in which one person is actually the cause and the other person is stimulated by its effect. Because such a mechanism is not warranted,

we avoided using this language in our own experiments preferring to use the term 'correlation' for the phenomenon, rather than referring to the participants as 'stimulated' and 'non-stimulated'. This approach renders opportunities for a wider range of explanations, e.g. assuming a generalized entanglement as an explanatory model for the findings.

However, the TP reported by Grinberg-Zylberbaum could not be replicated and doubts were raised concerning the experimental procedures used, for example as to whether the reported effects might be caused by electromagnetic effects of the stroboscopic lights used for triggering evoked potentials and the stimulation of the subjects. However, later experiments with a similar albeit refined design showed significant EEG correlations between pairs of isolated human subjects. (Fenwick *et al.*, 1998; Radin, 2004; Standish *et al.*, 2001; Wackermann *et al.*, 2003, 2004). In order to replicate the experiments of Grinberg-Zylberbaum, a number of important changes were introduced in most of these new experiments to overcome some of the shortcomings of the original design. Wackermann *et al.* (2003, 2004) used checkerboard reversals as computer animated stimuli, which provide a reliable visually evoked potential in an observer's brain. Other major changes included the treatment of subject groups and data screening. Experimental and control groups were treated the same way and were equal in number. Artifact screening by means of measuring the electrooccu1ogram (EOG) was conducted. Experimental rooms were electromagnetically and acoustically shielded. Appropriate statistical methods (non-parametric randomization statistics) were utilized that are sensitive enough and at the same time robust, because they do not depend on any distribution assumptions. Measurements to control for technical artifacts were taken (see Wackermann *et al.*, 2003, 2004). For instance, the control situation was identical to the experimental situation with the only exception that the computer screen providing the stimulus was covered with a black, opaque piece of cardboard. So far, the original experiment (Wackermann *et al.*, 2003) has been replicated twice with a larger number of participants, a larger number of electrodes and other statistical methods. Although not published yet, informal presentations of the results show that the effect is stable in the sense that statistically significant deviations from randomness occur. The effect also seems to be non-classical in nature, because there is no stable pattern to these deviations.

For this reason, we propose to follow this experimental design and probe it for its robustness. At the same time, it seems necessary to

elaborate on some critical points. For instance, in all the experiments conducted hitherto, the data-acquisition systems were internally connected to one system that provided the time stamp. Although electromagnetic crosstalk was not an issue, it would be more convincing if the effects could be seen in systems that were completely decoupled. Also, the question of whether a signal theoretical approach is valid needs elaboration. This could not completely be excluded in previous studies. The studies by Grinberg-Zylberbaum, Standish *et al.* (2001) and Fenwick *et al.* (1998) did not use electromagnetic shielding, whereas in the Wackermann *et al.* studies, electromagnetic shielding was present.

Approaches of our Three Replications

In order to independently replicate the effects found by Wackermann *et al.* (2003, 2004), the author conducted a replication of this approach in Tübingen in two non-neighboring laboratories (Hinterberger *et al.*, 2006, 2007, in press). However, as an improvement, we disconnected the computer systems after starting the recording to avoid stimulus crosstalk.

Another issue concerned which stimulus should be used. Radin (2004) observed that stronger potentials evoked in the sender were correlated with stronger effects in the receiver. We therefore addressed the question of different stimulus categories and used both standard reversal checkerboard stimuli that should replicate Wackermann's study, and naturalistic pictures, which are likely to produce stronger reactions. The study design and the results are reported below.

To address the debate about non-perfect shielding and conventional signal transfer, we set up two consecutive highly elaborate studies using a long distance as shielding that would make a signal theoretical approach implausible. On the other hand, long distances are not an issue, should the phenomenon fall in the class of generalized entanglement as predicted by Weak Quantum Theory (Atmanspacher *et al.*, 2002). Subjective case reports on telepathy would also suggest that telepathic connections are independent of distance. Therefore, in the last two of the three replication studies we used a distance between the participant pairs of 750 to 800 km, which would hardly allow for an electromagnetic signal transfer, but rather support the idea that the effect is driven by an entanglement process. These experiments were conducted in two laboratories, one being located either in Tübingen or

Freiburg (two cities in southern Germany) the other being located in Northampton (UK). The computers in the two labs were synchronized by the DCF radio clock signal that could be received in both locations.

Another point in question was whether closeness of relationship is important or whether it is only the systemic setup of the experiment that establishes the effect. We attempted to clarify this question by using pairs of subjects with different personal relationships and using pictorial material reinforcing the relationship.

Finally, the method of the non-parametrical statistical analysis was further refined as reported below.

Experimental Setup

In all three studies, we were using two identical EEG-recording systems (*EEG8*, Contact Precision Instruments, Inc., UK) which were placed in two different rooms. The first study was carried out in two non-neighboring laboratories at the University of Tübingen. At the beginning of each recording run, the two computer systems were synchronized with a cable connection, which was cut after the run had started. The second study was carried out between Tübingen (Institute of Medical Psychology and Behavioral Neurobiology, University of Tübingen) and Northampton (School of Social Sciences, University of Northampton, approx. 800 km distance) and the third study between Freiburg (Institute of Environmental Medicine and Clinical Hygiene, University Medical Center Freiburg) and Northampton (approx. 750 km distance). In both studies, the computer systems were started simultaneously by synchronization with the DCF radio clock signal, which is broadcast from Frankfurt am Main, Germany, reaching both laboratories. In all three experiments, the computers ran independently after recording started with a typical precision in synchronicity below 20 ms.

EEG was measured simultaneously in the stimulated and non-stimulated participants. Ag/AgCl electrodes were attached at the scalp locations Cz, C3, C4, Fz, Pz, Oz, A1, A2 (Int. 10/20 system). The vertical EOG was measured to allow for correction of eye blink and movement artifacts. Data were filtered by hardware to a range of 0.01 Hz to 40 Hz and sampled at 512 samples/sec.

In study one, 10 related and 10 unrelated pairs of subjects were measured. Since the effects we found were mainly seen in the related participants, we decided to use closely related pairs of participants in

study 2 (16 pairs) and study 3 (20 pairs), and assessed their relatedness with a questionnaire. Before each recording run, the related participants had to tune into each other for at least 5 minutes. As each one of the pairs could serve as stimulated and non-stimulated participant, study 2 and 3 provided data on 28 and 38 valid pairs.

TABLE 1.
Overview of the three studies carried out by the author with similar equipment

Parameter	Tü-Tü	Nh-Tü	Nh-Fr
Stimulus Design	80 IAPS (40Neu+40 Aff) 80 checkerboard 80 IAPS (40Neu+40 Aff)	cov. 80 checkerboard 80 partner 80 OEP 80 black	40 partner ca. 141 IAPS (47 aff+47 neu+47 pos)
	cov. 80 black screen cov. 80 IAPS (40Neu+40 Aff)	80 IAPS (40neu+40aff)	ca. 141 IAPS (47 aff+47 neu+47 pos)
Electrodes	Cz, C3, C4, Fz, Pz, Oz, A1,A2, vEOG,	Cz, C3, C4, Fz, Pz, Oz, A1,A2, vEOG, SCR	Cz, C3, C4, Fz, Pz, Oz, A1,A2, vEOG, SCR(non-stim)
IS Duration	3-6 sec	4-7 sec	2-10 sec (mean 6sec)
Stimulus Duration	1 sec	2 sec	0.5-5 sec variable (mean 2s)
Participants	10 related pairs 10 unrelated pairs	16 pairs, closely related	20 pairs, closely related
Age of participants	24f/16m 18-51 (mean 27)	16f/14m 21-59 (mean 36)	28f/12m 21-81(mean 42)
Valid session pairs	10+10	28	38
Analysis	1 EEG (MastRef), EOG, 6 bands 56 spatial band CAR	SCP, 6 ch EEG (MastRef), EOG, SCR, SCL 7 bands	SCP, 6 ch EEG (MastRef), EOG, SCR, SCL 7 bands
Processing rate	256 Samples/s	32 Samples/s	32 Samples/s
Random Comparisons	1 000	10 000	5 000

In all three studies, pictures from the International Affective Picture System (IAPS) were used for stimulation. The pictures were categorized into affective pictures with negative valence, neutral pictures and positively rated pictures. Additionally, study 1 contained the checkerboard stimulus for replicating the design of Wackermann *et al.* (2003, 2004). Study 2 additionally contained pictures of the partner and various self-selected photos (own emotional pictures, OEP). In study 3 we only used the three categories of the IAPS. No erotic stimuli were used. The pictures were presented in a pseudo-randomized order with random inter-stimulus intervals. The non-stimulated subject was seated in a comfortable reclining chair, and was instructed to relax while the EEG was taken. The parameters of the study design for each study are listed in Table 1.

Data Analysis

The entire data processing and statistical analysis was performed in *Matlab 7.2*. All the routines were developed and programmed by the author. In parts, analysis of the last two far-distant studies was similar to the analysis of the first study carried out in Tübingen (Hinterberger *et al.*, 2006, 2007, in press). A detailed description of the analysis of the last two studies is given below.

Pre-processing

The data were visually inspected and checked for periods of artifacts, which were excluded from the analysis. The EEG was corrected for eye-movements and re-referenced to both mastoids resulting in 6 channels of EEG (Fz, Cz, Pz, Oz, C3, and C4). For extraction of different features, a number of band-pass filters were applied to each EEG channel as follows. The frequency range between 1 and 45 Hz was chosen for the analysis of event-related potentials (ERP). Additionally, to look for effects in specific frequency bands, each channel was filtered into 7 bands, namely the slow cortical potentials (SCP, 0.01-2 Hz), Delta (1-4 Hz), Theta (4-8 Hz), Alpha (8-12 Hz), Beta1 (12-20 Hz), Beta2 (20-30 Hz, and Gamma (30-45 Hz). To reduce the number of variables, the filtered EEG was averaged for each band over all cortical electrodes. The band power of the 6 bands from Delta to Gamma was calculated by squaring and smoothing of the

signals. The SCP and the broadband EEG was analyzed in their original time course. In order to avoid an excessive influence of possible remaining artifacts, the band power was limited to 10 times its mean value. The EOG was also included in the further analysis after filtering to 0.01-2 Hz. This allowed for detection of possible startle reflexes of the non-stimulated participant contiguous with the stimulation times and resulted in 15 variables, namely 1 EOG, 6 broadband EEG for ERP analysis plus 8 channels with all electrodes merged. These signals were then down sampled to 32 samples/s for further analysis.

Statistical Analysis

All 15 channels were subject to the same statistical analysis. The analysis was applied to the data of the non-stimulated participants using the stimulus type and onset times from the stimulated participants. The stimulus epochs of the same conditions were averaged separately for each channel, sample and participant. For analysis of time series data such as SCP, EOG and the broadband EEG, a baseline value—taken from the 0.2 sec before stimulus onset—was subtracted. For the spectral data, a baseline consisting of the mean of the recording run was subtracted from the averaged spectra. Averaging was performed with respect to the pre-selected periods of artifacts.

Our non-parametric statistical approach requires a comparison with 5,000-10,000 virtual sessions. Therefore, a randomized selection of 5,000-10,000 possible stimulus sequences was calculated using the same rules in terms of inter-stimulus intervals than the actual stimulus sequence. Those random epochs were averaged in the same way as the real stimulus epochs. Because we were only interested in the size of the response and not in its direction, the absolute values of the averages were calculated and filtered with a Savicki-Golay Filter of second order using a window of 5 samples which produces a 7 Hz low-pass filter. From the average over trials, the highest 3 samples (100 ms) were taken as a reduction from the whole time series (0 to 3 sec after stimulus onset) into one signal value. This maximum value of the average of the real stimulus onsets was then compared with all maxima of the random averages and ranked according to its amplitude. This rank can directly be interpreted as a probability of achieving a significantly heightened signal. On a 5% level, the rank would have to be greater than 500. Using the inverse Gaussian cumulative distribution function Φ^{-1} the ranks can be transformed into z-scores according to

$$z^{rank} = \frac{1}{\sqrt{M}} \sum_{j=1}^{M} \Phi^{-1}(r_j) \quad (1)$$

where r_j denotes the rank of the j-th participant rank and M the number of participants (M = 28 or 38). The z-transformed values were averaged across all participants resulting in z^{rank}.

This leads to one z-score for each of the 5 conditions, each of 15 parameters, and each sample.

In a final step, we counted the significant time (z > 1.65) in all channels and condition. This should be about 5 % according to the 5 % significance criterion.

Results

The ERP-analysis in the time range during the stimulation and one second after has been analyzed for all non-stimulated participants. The global ERPs, including all electrodes and all picture categories, revealed no significant results in all three studies. The SCP and the EOG also remained non-significant. No significant variable could be found after analyzing each stimulus category separately. In contrast to the analysis of the evoked response time course, the responses in the spectral bands revealed some significant values, predominantly in the Delta, Theta, Alpha, and Gamma band. In the first study the unrelated participants remained non-significant for all bands and all stimulus categories. However, the related participants showed significant increase in the Theta ($z = 2.34$, $p = 0.01$) and Alpha band ($z = 2.14$, $p = 0.02$) for the affective pictures. The neutral pictures were significant for the Delta band ($z = 2.22$, $p < 0.02$). In the second study the 28 pairs could create a significant Alpha band increase for affective pictures ($z = 2.31$, $p = 0.01$) and their own emotional pictures ($z = 1.79$, $p = 0.04$). For the latter stimuli the Gamma band showed also a significant increase ($z = 2.08$, $p = 0.02$). In the third study the Theta band was increased when pooling all stimuli ($z = 2.48$, $p = 0.01$) and the increased Alpha band activity for negative affective pictures could be replicated the third time with $z = 2.52$ ($p = 0.006$) in a sample of 38 pair comparisons. A weak significant Delta band increase ($z = 1.89$, $p = 0.03$) and a Gamma band effect of $z = 2.50$ ($p < 0.01$) could be seen with positive emotional picture stimuli.

TABLE 2.
Overview of the results in all three studies. For the band power measures only significant bands are mentioned.

Analysis Parameter	Category	SepaCorr Unrelated	SepaCorr Related	DistCorr*	EntCorr*
Global ERP	All Pictures	n.s.	n.s.	n.s.	n.s.
	Affective	n.s.	n.s.	n.s.	n.s.
	Neutral	n.s.	n.s.	n.s.	n.s.
	Positive / Partner	—	—	n.s.	n.s.
	Checker board	n.s.	n.s.	—	—
	OEP	—	—	n.s.	—
Bandpower Analysis (significant results out of 7 bands)	All Pictures	n.s.	θ: 2.34 α: 2.14 γ: -2.95	n.s.	θ: 2.48
	Affective	n.s.	θ: 2.34 α: 2.14	α: 2.31	α: 2.52
	Neutral	n.s.	δ: 2.22	n.s.	θ: 2.26
	Positive / Partner	—	—	n.s.	δ: 1.89 γ: 2.50
	Checker board	n.s.	n.s.	—	—
	OEP	—	—	α: 1.79 γ: 2.08	—
SCP	All Pictures and all stimulus categories	—	—	n.s.	n.s.
EOG	All Pictures and all stimulus categories	n.s.	n.s.	n.s.	n.s.
Amount of significant values	All stimuli categories	6.25%		5.24% (Affective: 8.3% OEP: 8.3%)	6.05% (Affective: 5.96 % Neutral: 8.07%)

***Peak analysis in the time range; n.s. = nonsignificant; '—' means that the measure did not exist in this study.**

As the p-values have to be corrected for multiple testing with an unknown factor most of the significances might not survive such a correction. However, the repeated Alpha band effect accumulates to a z-score of 4.0 ($p = 0.00003$) which easily survives even a highly conservative correction. For a comparison of the results between the three studies Table 2 provides an overview.

The Alpha effect in all three studies asked for a closer look. In case of the result being an artifact this could show up as outliers in a few participants only. On the other hand the literature gives many hints that there might be only a few gifted people who show strong telepathic abilities. If our Alpha effect would arise from such gifted participants some of them should be highly significant while most subjects would remain around chance distribution. As Figure 1 illustrates more than two thirds of all participants contribute to the effect. Therefore, we have to reject the hypothesis of gifted participants showing a kind of Alpha increase upon their partners' affective experiences. It is rather a small effect that got significant in 9/76 participants instead of only 4/76 and many others showed a tendency that supported the overall significance.

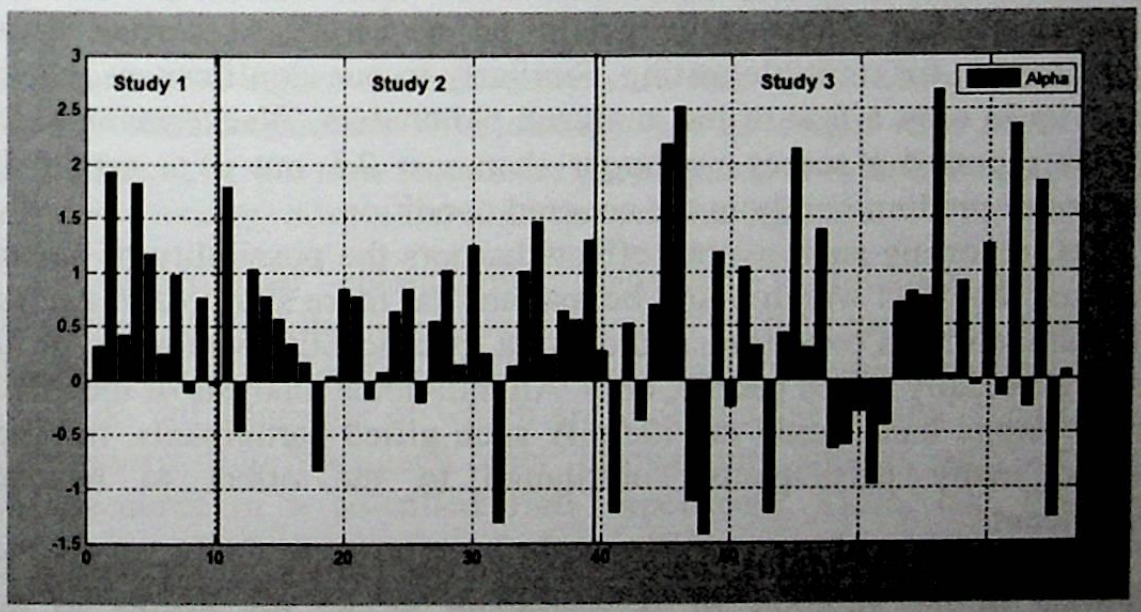

FIGURE 1.
Each bar indicates a non-stimulated participant's significance score in the Alpha band power deviation upon the stimulation of his/her partner with affective pictures. 76 participants of all three studies are shown not including the unrelated participants from study 1.

Discussion

Considering all three experiments, in each study we found slightly more significant values when counting the significant parameters of all stimulus categories and all participants (final row in Table 2). This hints at a positive result, in terms of the EEG response in the non-stimulated participants being slightly modified by stimulation of a related person. However, the effect is rather small. The measures revealing significances remain in the range between $z = 1.65$ and 2.5 or, in terms of p-values, between 0.05 and 0.005. However, we should also be aware that an unknown correction factor for multiple testing has to be applied to the p-values that has not been considered but which is also hard to predict. These results seem to be in line with other studies, such as those conducted by Wackermann *et al.* (2003, 2004). In contrast to Wackermann *et al.* (2004), who reported an increased amount of decreased activity in the non-stimulated participants, we found hardly any significant decreases in the activities in all stimulus categories and all 15 EEG measures. Despite having analyzed the signal decrease we focused predominantly on the increase, which is equivalent to the existence of an evoked response in the non-stimulated participant. However, if we decide to test the counterintuitive hypothesis of a decrease, it would be necessary to correct those significances for multiple testing. Similarly to our significances, which show up in only a few of the analyzed parameters, Wackermann *et al.* (2004) reported z-scores no larger than $z = 2.5$ out of a set of 19 electrodes predominantly in the covered condition.

Yet, reporting such overall effects harbors the possibility of having smeared an effect which might be real and far more significant, e.g. for a certain stimulus condition, and/or for a specific EEG parameter, or for some especially gifted people, only. An individual analysis of the effect in the Alpha band could not identify such gifted participants. In other words, many participants contributed to the effect to become significant.

The different stimulus categories have been analyzed separately. In contrast to the findings of Wackermann *et al.*, the results of the checkerboard stimulation remained non-significant. Thus, we can conclude that a replication of their approach failed.

At least two possible models could serve as explanation for the effects found in these and probably also in the earlier studies.

1) Artifact induced through analysis. The analysis of EEG signals usually leads to a multi-dimensional parameter space, i.e. several meaningful parameters can be derived from the EEG time signal, such as band power in various frequency bands. Furthermore, the way each parameter is determined offers a high degree of freedom. In our three studies, we attempted to focus on the most meaningful and most common methods. Still, there would be reasonable arguments for choosing different parameters. My experience with changing the analysis model slightly is that the results could change dramatically from significant to non-significant. As the human attention system is selective (and scientists are a human-like species) one might tend to focus more on positive outcomes and stop with analysis when an effect is found or even change the parameter to optimize the effect. In my analyses, I was aware of this psychological phenomenon of tending to support significant results. Avoiding such tendencies could be achieved by optimizing the method first on the data from the stimulated participant and then applying it to the EEG data of the non-stimulated participants without any change. If similar physiological reactions occur in the non-stimulated participants the effect should then show up. This was actually found to be the case in the third study in which we found a significant ERP. However, when taking a closer look the significance turned out to be due to a potential shift during the 200ms baseline period before the button press. In this case it cannot be decided whether such a baseline shift already has its origin in a preparatory presentiment of the non-stimulated participant, or whether it happened arbitrarily exactly during the time span of the baseline.

2) Generalized entanglement between stimulation and EEG parameters of the non-stimulated person. In that case our measured parameters are correlates to complementary variables, i.e. variables which do not commute or which are independent, so that they do not carry information from each other. In quantum physics, such variables are measures in a so-called Bell experiment. Here, e.g. the spin directions, which are measured alternatively, could be such variables. Transfer of the design to our EEG-correlation experiment to measure the Bell inequality is not trivial. Statistically, many of the EEG measures are not independent and therefore do not provide a set of complementary variables. However, different EEG measures could reflect correlations to different underlying variables which are complementary. If those complementary variables are correlated with the same complementary variables in the brain of the stimulated

participant, and in addition, if the stimulation influences those variables, then we could assume that the whole system behaves like an entangled system showing correlations between stimulation times and the non-stimulated EEG responses.

One property of the non-local generalized entanglement theory is the theorem of non-signal-transfer (NST). This means that it should not be possible in principle to reconstruct the state at the stimulated participant from the EEG response. One argument for the validity of the NST axiom in the present studies could be that the observed effects are not stable in the parameter in which they show up, but rather that they change their position from study to study (Walach, 2005). However, it is very important to note that fluctuating effects are a necessary condition to guarantee that a signal has not actually been transferred through such EEG correlations. However, it is not a condition that could serve as sufficient proof for the non-signal-transfer axiom that is valid in non-local correlations observed in entangled quantum states.

So far EEG correlation experiments have shown that unlike in stimulated persons there is no clearly visible and reproducible event-related potential in non-stimulated persons. The sensitivity for detection of such event-related signal changes in the EEG of the stimulated person in our final experiment was around 17 standard deviations away from noise, while the non-stimulated participant revealed z-scores smaller than 2.5 and often around zero. Therefore, we were able to conclude that if there is a correlation visible in the EEG activity, it must be very small. But this highly sensitive non-parametrical statistical approach probably was successful for detection of a very small increase in the Alpha band activity when exposing the related participant to affective pictures. However, because this has already been reproduced in three studies and the effect for affective picture material is also in line with our expectation, we found a pattern in the EEG that did not shift between the parameters as proposed by the theory of a generalized entanglement. The NST paradigm would be violated if we could predict such Alpha increase successfully for a subsequent experiment in that style.

Even if we claim that the significant findings in the experiments simply reflect the random expectation, the hypothesis of possible EEG correlations between two closely related people is not thus disproved. All the experiments analyzed the event-related response to a visual stimulation. But what about correlations in the spontaneous EEGs

between closely related pairs of subjects? We could expect that our EEGs might be to some extent correlated naturally (Standish *et al.* 2001). Therefore, it would be interesting to re-analyze our experiments and search for such correlations and test whether they are larger in the closer related pairs.

In conclusion, I would say that the paradigms we used in our experiments to test for so-called transferred EEG-potentials were very similar. Most other studies also aimed at the detection of an event-related response. Those similar approaches were important for replicating the reported effects. Now, after several replications, I would like to encourage researchers to invent a new hypothesis for searching for a possible physiological connection, even in the existing data, or to develop new paradigms more suitable for uncovering a possible connection between people.

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DISCUSSION

ROE: Thank you Thilo. I would not be so concerned about the issues with multiple comparisons given that your outcome measure is relatively crude; it seems to me you are dealing with likely interactions between various properties of the persons and of the individual stimuli that fluctuate from presentation to presentation and much of that fine detail is lost when in your final analysis you collapse both of those categories. The fact that you are getting significance in any of this array seems really quite impressive and its sporadic appearance may be necessary for a non-local interpretation. Let us now open it up to one or two brief questions before the formal discussion.

ROLL: You express some doubt as to whether this says anything about psi in the brain, but as I recall you got significance with respect to the alpha wave, and that could be important.

HINTERBERGER: I was doubtful that ESP phenomena could lead to a specific activation in the brain, because I think the brain signals reflect

the information processing of the brain but not where the signals come from. This is another level. It is very hard to find a center in the brain where ESP enters the processing system, an ESP receptor.

ROLL: Some regard the temporal lobe as an ESP receptor.

HINTERBERGER: It could be. But then we should do fMRI studies when we want to know these things and then do what I suggested here; look at real ESP events. We need to prove this in other ways by reports which fit together. So we have to create a real ESP phenomenon and then look at what is happening during that time. Then I guess we need lots of trials; a difficult job, I am not sure if anyone can afford that!

ROLL: I appreciate the exactness of your thinking and the exactness of your measurements.

VARVOGLIS: Two items, one is in terms of historical accuracy concerning physiological markers of psi; this goes back to Grinberg-Zylberbaum, and Douglas Dean did some work with the plethysmograph — of course it is not the central nervous system, but it is a physiological marker of transferred potentials. I agree with what you are saying about having a more selective way instead of just doing these experiments between two individuals and taking *all* the data there. Maybe we should focus on introducing more qualitative evaluations, or concentrating on optimal methods, such as taking a ganzfeld with creative populations and sender-receiver pairs who have a close bond, so we get more trials that are successful from a qualitative perspective that could be specifically studied to see if they give you any physiological markers.

HINTERBERGER: Yes, this is what I tried to argue for in my presentation.

BANCEL: Thank you very much; it was very enjoyable to hear such a precise and careful presentation. From what you have said it seems as though you have gone over your data very well, which is nice to hear. We were asked at the beginning to be bold so I was wondering if you could be a little bit bolder in your pronouncements. From what I take away from your talk, there is absolutely no evidence of any psi in EEG data. You seem to be walking around that a little bit and I wonder if you agree with my assessment of these experiments, which were very nicely done.

HINTERBERGER: The thing is that everybody finds something. But as more experiments are conducted even if we still have an overall effect, the effects do not get bigger as the number of trials increases, so I am not sure whether this is an effect or not. I wanted to solve the question, but at the end I came out with the same question as I had before; possibly a null result.

NELSON: I enjoyed you talk and have a very simple question, I think. You showed the evoked potential and showed the Cz comparison the mastoid, and so on, but your affective and neutral stimuli looked like they gave the same pattern; you look like you have an onset and an offset and an evoked potential for each of those, but it doesn't really have much to do with the stimulus or the nature of the stimulus. Is that so?

HINTERBERGER: It is natural that the onset ERP looks a little bit different to the offset ERP; the onset ERP is the strong response, which can also happen in the offset because it is new. As soon as something new appears in your visual system, the updating of the visual system creates an evoked response. The EEG responses that reflect how the information is subsequently processed do not show up in the offset ERP so they are slightly different but essentially they are as big.

GENERAL DISCUSSION

ROE: This next stage is a little bit experimental, what we are hoping for is something that is very informal; you are encouraged to ask questions and engage in debate. And we hope to draw out common themes or general issues as well as more particular questions that are prompted by the presentations that we have heard so far. I will take the Moderator's prerogative of asking the first question: in comparing the characterizations we have seen this morning of ESP and PK with what we may have heard in 1953 there seems to be a shift away from overt phenomenal experiences of ESP as found in spontaneous cases and Rhinean card guessing studies, towards a situation where we are dealing with a conception of psi that is much more tacit, implicit and unconscious. Do you think that is a fair reflection of the way the field is going? Do you think that is a fair reflection the way that psi operates? And do you think we should expect to find similar patterns here to the ones that have been identified with conscious psi responses?

NELSON: I think it is a fair characterization, but I believe there still is a good part of the spontaneous experience driving the design of recent experiments, and in some of the experiments psi is clearly operating, at least phenomenologically, at a conscious level—in the intention experiments one is 'trying to do' something. Whether it is really all happening at the level of real awareness or consciousness remains to be studied more, and there is considerable evidence that, for example, if you get out of the way and stop thinking about it they work better.

LOBACH: I am not as familiar with what was happening 50 years ago, but I would say that of course we now have more instruments to measure physiological changes so this inspires these types of experiments, but I would say they complement each other.

HINTERBERGER: When I started to get into parapsychology, especially in measuring telepathic phenomena, I thought measuring those phenomena, or proving their existence was not the main point I wanted to achieve; it is more important to apply this knowledge, to see what is really happening, how are they doing it, and probably learn something out of it about our own life; how it works, how it functions. And so in my life a central point that I want to focus on is what can we really do with these phenomena?

NELSON: Thilo's comment inspires me to add just one thing, and that is that I think one of the big changes is toward learning something. Earlier it was the task of most researchers to prove that psi exists. People were looking for critical experiments, looking for some way to make sure they were talking about real phenomena. I think in the meantime we really have come to the general conclusion that there is something there, and now we are more focused on trying to take it apart to find out what it is made of.

LOBACH: If it is allowed I would like to introduce another theme in this. It is related to what Thilo presented but it is broader, it's about what these events mean to us. You were showing there is an evoked potential when the picture is presented and then when it goes away again, but is there a way in which we can subtract the meaningfulness of the picture from the physical event of the presenting and the disappearing of the picture?

NELSON: You mean, does the content matter?

LOBACH: Yes, in the signal can you subtract the meaningfulness of the picture from just presenting it and letting it disappear again?

HINTERBERGER: I think the meaning of the picture is not reflected in the ERP. What is reflected is the information processing and possibly emotional arousal, but I think this is all we can see in ERP—more of the emotions which are associated to the meanings can be seen in other measures like skin conductance, and people often focus on skin conductance when measuring telepathic responses. But the question is still, what is important. I think the meaning is what we all assume is the most important issue. Roger's talk suggested to me that it is meaning that changes the random event generators, so all those big events *create* meaning, and that's why something happens. I could now be even bolder and say that all our transferred potentials experiments, all these correlation EEG studies, in which we find small effect sizes, maybe they are created just by the meaning. Not the meaning that the picture content gives to it, but the meaning which we create by our experimental set up. So, that would mean that our EEG studies are working like a random number generator. Then we can start again discussing whether the DAT Theory might be applicable to it or maybe its meaning to us is a magical thing which we create through our experiments. Is this the answer?

LOBACH: In fMRI studies, it has been shown that emotions are processed in particular areas of the brain. So if it is about the meaning of the experiment as a whole or the meaning of particular pictures, maybe fMRI or more fine-grained EEG may shed some light on this issue. If you can find that the activity of the brain is associated with emotional processing in the brain then it would suggest psi is more about transferring meaning between people in different places than sharing images.

ROE: Let's open the discussion to questions and comments from the floor.

CARDEÑA: Yes, I have a couple of things. Firstly about fMRI and the general issue of doing science with the brain. I think we are talking about a fiction. All fMRI give data that are essentially aggregate, they give averages. There is an enormous variation, not only in physiology but also in anatomy of the brain that has to be dealt with. I have done EEG studies for some years and what I see is such an extraordinary variation. When you look at the actual data of fMRI or whatever, you have an aggregate. Everyone is doing somewhat different things, so I believe the brains of different people who aren't even processing exactly the same information would react so differently that even if they were all producing psi they would not be very similar. The first general issue would be to try something like a biofeedback set-up with an EEG system for two people so they could learn initially to have the most similar kind of brain activity. When you were asking for ideas involving entanglement, I spoke to Harold Walach who agreed that in principle it might be possible to try and set up a feedback between two people that involved EEG elements. The feedback would be possibly auditory, based on the degree of coherence they showed in general EEG functioning. The psi step may be you take away the feedback and see if people could still converge even when separated by distance.

Secondly I think one thing we have not learned in more than 50 years is that despite having paid lip service to experimenter effects, there is no requirement from journal editors to say anything in our experimental reports about experimenters: gender, state of consciousness, nothing. We know that experimenters are supposed to have an effect—Dick Bierman says it all the time—and yet we act as if the experimenter does not exist, except when we are in conferences. I would challenge you to actually demand that, in the same way that

people must write about participants' information, they should have information about the experimenters.

ROE: Can I just qualify what kind of information you would include here? I'm thinking here that a paper in a qualitative journal would require a section on reflexivity that includes not just demographic information of the sort you mentioned, but also something about prior experiences and motivation for doing this particular study.

CARDEÑA: Yes, what is your motivation and when you were doing the study were you trying to get involved with the participants, were you trying to have an effective participation with them? Were you meditating, trying to blend with the participants, and so on? We mention it here but we do not see it in actual reports.

ROE: The SPR's *Paranormal Review* did include an interview by Caroline Watt with Richard Wiseman and Marilyn Schlitz that contrasted their different approaches to DMILS sessions — is that the kind of thing you have in mind?

CARDEÑA: Exactly, but I would like to have that with every article.

DELANOY: This is in part response to what Etzel just said and I think he has made an excellent point but I think in many ways you do find those data reported. There was a very interesting time in the PA several years ago that included a round table where very successful experimenters got together to discuss what they did during sessions, but also in preparation for sessions, what they were thinking for experiments, and so on. I suggest people might want to go back and look at that again to see what it was saying, as there were some important insights there into how successful experimenters did orient themselves overall to the studies. I know also in other studies this has been commented upon, for example in Edinburgh when Bob Morris, myself and Kathy Dalton were doing a ganzfeld-centered no-sender study, we stopped Bob doing it after 14 sessions because he just scored at chance. Kathy scored very well, I scored nothing and afterwards we were reflecting on the differences in our approach to the study and again there were very clear things that could explain the scoring differences between the experimenters. It is a good and valid point and one that has been addressed in the past that we should make sure to continue to include in our discussions.

GLICKSOHN: A question about the first two speakers today, Eva was talking about an impending sense of doom, which is a pretty neat idea but in Roger's data from what I remember, the data about 9/11 you see a massive effect from 9/11 and on but nothing prior to 9/11. So is this a case of absolutely no precognition?

NELSON: There is, in one of the measures where we can time things very precisely, a massive shift in the data for the first 4 and half hours before the first plane hit. We don't know how to explain that except as something like precognition, or presentiment on the part of a giant global consciousness, or something like a massive effect of the few people, the terrorists. I think we now have data in several cases that at least leads to the question, 'can we establish more certainly that this network of REGs around the world shows a reaction before the event that is believed to drive it?'

LOBACH: Weren't the earthquake data showing some sort of presentiment as well? At least it looks like it.

NELSON: With questions like this it is always a good idea to do some more work, to work harder to make sure that your first efforts in the analysis have not looked at things in the wrong way. In other words we need to do more work. But at the present time we have a pretty strong indication that at least 8 or 10 hours before the earthquakes, as shown on the slide with the sharp V-shape, the data starts changing in a way that clearly becomes significant well before the earthquakes happened and we do not know any way at all to think about that; if it is a real phenomenon we see, in other occasions we do not know how to explain that without invoking something like premonition or precognition, on the part of something like a great unconscious, global consciousness.

BANCEL: I just wanted to add one point concerning the data from the global consciousness project and to reduce a little bit the expectations one might have from the presentiment effects. I can speak for the global consciousness project at least when I say that we try to make statements when we have very significant statistics and when we have looked at the data thoroughly. In both of these cases, for 9/11 and the earthquake analysis, I myself would not be confident enough to say that there is evidence for something happening before the events occur. So I think it is very, very easy to take away a positive impression of an exciting potential effect by showing some data, without appreciating the uncertainties that are in the data. I have spent a long, long time looking

over both of those datasets and I am not ready at all to make any kind of statement to say there is some kind of precognition and I would encourage people not to go there because we do ourselves harm as a community if we overstate or are not very cautious. If we have to dial back later on it is not a good situation. I am sorry to be so negative all the time!

VARVOGLIS: I want to go back to this whole question of the sender-receiver pairing and the role of the experimenter, which Etzel brought up. We are developing a system at the Institut Métapsychique using an automated ganzfeld system, and the idea is to have similar induction procedures for the sender and the receiver immediately prior to the session: identical state induction procedures. So at least as far as the preparation of the 'wave function' is concerned it is similar on both sides. Then at the critical moment where the stimulus is presented, when the sender is exposed to the stimulus, that is where it cuts off and we see if there is some kind of continuity; if the shared state facilitates the 'transfer'. That would be one way of addressing this, making sure we take it as far as we can and maybe even alternating periods where we go back to having multiple drivers so that they are in a similar state. But I would like to go a little bit further, and this is not something we have planned so far, and to include the experimenter and say that maybe William Braud, Charles Tart and others who have spoken about this are right on target when they say that we have to evolve our thinking to think about the role of the experimenter and see the experimenter as part of the system. Therefore he/she has to be coherent with sender- and receiver-state as well. Today, given the fact that we can practically run our experiments automatically the experimenter actually has very little to do in some sense, in terms of tasks. We could imagine having the experimenter as well going through state induction procedures and being in the same state and operating from the same psycho-physiological perspective. This is conceivable and it might render things a little bit more fluid in terms of communication between everyone. I think in terms of meaning also William Braud and others spoke about the fact that there is a ritualistic aspect with psi experiments, and the more we get into the mechanics and technicalities of doing it, the less we are aware of the ritualistic aspect. It may not be politically correct to mention that, but a lot of good psi experiments were attached to that kind of ritual, which gave the experiment meaning for the experimenter, and they conveyed it to the subjects as well.

GERDING: Just to add something to this, at Utrecht we conducted ganzfeld experiments in which to encourage a sense of experiment togetherness the participants did some kind of magical introduction. They created their own magical procedures, lasting an hour or more before they started the ganzfeld session. Then we did one or two sessions in which the experimenter and subjects all participated and did very well.

DOBYNS: Since it seems as though we have got into an impromptu discussion about experimenter effects here I would like to point out that dealing with those competently is going to be a lot more challenging than any of the comments I have heard would indicate, since for example, on the simple matter of the same state of consciousness being involved, I know from direct first-hand experience I and Brenda Dunne were two experimenters at the PEAR lab and the exact same induction procedure that would put me in a blissed-out trance state would put Brenda into a screaming rage! So there are a lot of technical challenges before we can do anything intelligent with this concept. My actual question was for Thilo. Since you seemed to be finding pros and cons and to be unwilling to come to a direct and immediate conclusion about your results, have you considered simply applying standard meta-analytic techniques to your big array of independent scores, thus reducing them to a single bottom line where you can simply state that, with statistical confidence, my conclusion is 'whatever'.

WALACH: This is actually what I am trying at the moment, to summarize all those scores. But on the other hand one meta-analytical approach would be to just look at all the data to see whether there are more significant effects than we would expect by chance, throwing all the data together in one bowl. I am currently calculating a lot of things, but depending on how you do it you get different results, and this is a sign for me that some things are questionable; what is the correct thing? Usually if an effect is stable, if an effect exists, it does not matter if one changes slight parameters. What I wanted to add to the experimenter effect is that already this searching for significant results is perhaps the biggest experimenter effect in all our research. It is the analyzer effect, but we then ask is there a presentiment effect? This just adds another question which should be corrected for multiple analyses. All those searches we do for finding something and afterwards be presented as if it were the only search we did; but it wasn't, so we have to be very careful with this.

ROLL: Dr Hinterberger, I think the last question you asked from the list of questions in your presentation was: can we set up a real 'quantum consciousness' in the related equivalent to the Bell experiment to test the entanglement of consciousness. A paper from 1997 by Brian Josephson and Fotini Pallikari-Viras might be relevant; they claimed that whereas the experiments of Aspect and his colleagues in the 1980s were suggestive, the best basis for quantum entanglement came from parapsychological findings. What do you think about his answer?

HINTERBERGER: If that is the best basis then it would really be our challenge to define the real parapsychological experiment in a well-structured way, as precise as physics requires it to implement such a procedure.

NARANJO: I would like to comment on the correlation of REG outputs with global and often tragic events, particularly the fact that their significances appear and disappear in a very unstable fashion, and if we look at many cases from the past we see that this has been the case there too. This might mean that there is nothing to explain or that it is characteristic of the phenomena. It seems to me that it is inherent in the nature of the thing, and a fundamental issue is that the systems we study are not clearly defined or are defined in terms of what they are not so that we cannot be sure that we are bringing the system as a whole into the laboratory, especially where we think the system also includes the nature of the relationships and interconnections between the participants and experimenter. How can we ensure consistency of outcomes if we don't have consistency in what we bring into the lab? We do not seem to have any way to prevent this from happening in our experiments. We have to believe in the same system to in order to do it, and we have to positively define our phenomena and I hope there will be time for this in the next discussion.

NELSON: I think there is a single answer to your question, which is 'patience'. We really shouldn't be looking, given the kinds of signal-to-noise ratios we have, for the answer in any single experiment. Instead we really do need to continue for years. In the PEAR lab, for example, we found a well-defined REG experiment that with the first experiences of people, the effects were high, but they dropped off and they dropped off and it began to look as if they were going to go to nothing. But then they come back up again. So the patience there shows we actually have an interesting effect over time.

BANCEL: One last little point on ritual; Mario bought up this issue of ritual and I wanted to say something about secret ritual in physics, which physicists do during their experiments. This is actually a funny story because I was formulating my question in my head after your comment and then Professor Josephson turned to me and blurted out to me 'light a candle for the experiment!' So the story was that years ago I was doing low temperature physics experiments on superconducting tunnel junctions, and we used to make these tunnel junctions that are difficult make, and we would measure them electronically, and they would always blow out and we would have to go back and make another one. This was very tedious until we found out that the tunnel junctions wouldn't blow out on us if we did the experiments in the evening and if we lit a candle before we did it.

DREAMS AND ESP

ROBERT L. VAN DE CASTLE

Dreams and ESP in Former Times

Throughout recorded history, dreams have always been accorded a special place of acknowledgment whenever accounts of psychic events are reviewed. There are 217 references to dreams in the Bible, and many of these deal with future events that are predicted. In Numbers (xii, 6) is a passage that states, "If there is a prophet among you, I, the Lord will make myself known unto him in a vision, and will speak unto him in a dream." Probably the most famous Biblical account is where Joseph's insightful interpretations of the Pharaoh's dreams played a prominent role in enabling this ruler to avert the disastrous consequences of the forthcoming plague. An angel appeared to Joseph in a dream and told him about the importance of transporting the young Christ child to Egypt so that he could avoid Herod's edict that he be slaughtered. Later, another angel appeared to Joseph in a dream to indicate when it was safe to take him back to Israel. Dream books were available in all ancient civilizations in order to try and assist the dreamer to interpret whether his/her dream imagery foretold significant consequences for the dreamer. The most famous of ancient dream interpreters was the Greek, Artemidorus, who wrote a five volume work around 150 A.D. for his son that could be consulted for information about the dreamer's character and possible forthcoming events. He indicates in book five that "my main object was to collect information for you about dreams that have come true" (White, 1975, p. 229).

Dreams that seem to have a pre-cognitive element were definitely not viewed in a positive way by some Medieval Church Fathers. Father Pererius wrote quite extensively about the linkage between precognitive dreams and the Devil. In contrast to other dreams, "if dreams take place frequently which are significant of future events or occult matters... it will be considered, and not rashly, that the initiator of the dream is the Devil" (Kelsey, 1968, p. 291). Sir Edward Tylor, occupant of the first chair of anthropology at Oxford University, proposed that prehistoric peoples were greatly perplexed by two enigmas: trying to understand how a motionless, sleeping individual

upon waking is able to describe physical adventures and social interactions in distant places, and what the difference was between a living body and a dead one. The concept of a soul that could wander at night and survive death enabled these dilemmas to be resolved (Tylor, 1873). There has always been a strong linkage between dreams and beliefs in the presence of a non-material component in our existence.

Dreams and ESP in the 19th Century

In the 19th century, dreams and their possibilities for offering an expanded view of human potential were reflected in many publications. Under the pseudonym of Frank Seafield, Alexander Grant published an initial book in 1865 and a second edition in 1869 entitled *The Literature and Curiosities of Dreams*. This book, surprisingly not cited in Freud's bibliography, was 518 pages in length and contained 210 references. Part 3 of this book provided a collection of over 250 dreams arranged chronologically, some of which dealt with premonitions. For example, Lady Needham, the wife of Earl Harcourt, was visiting her father-in-law's home when she mentioned to her husband at the breakfast table that she had had a horrid dream and earnestly wished to narrate it. He laughed at her and declined to hear it. The family members gathered at dinner time and waited some time for Lord Harcourt, who was usually very punctual. When one of the servants was asked by the son if he knew where his father was, Lady Needham exclaimed, "Look in the well," and fainted. Lord Harcourt was found in the well, with his dog lying at his feet, just as she had seen him in her dream (p. 467).

Hendrika Vande Kemp (1977) provided 263 accounts of dreams that appeared in 84 periodicals or journals in the popular literature of the 19th century. In a 515 page dissertation entitled: *The Dream in Periodical Literature: 1860-1910*, she attempted to develop a taxonomy of dreams in which the major categories and dimensions of dreams could be classified. One involved the mechanisms of dreams, another a comparison between the dreaming and sleeping states, and a third grouping dealt with parapsychological topics such as telepathic and clairvoyant dreams, prophetic dreams and dreams of death.

Vande Kemp commented, "For the most part dream investigators were at a loss to explain the more interesting of these dreams—those involving telepathy or the presence of the dead and dying—with their new, scientific theories" (1977, p. 113). She noted that "dreams of the

dead were especially instrumental in propagating and reinforcing spiritualism and ideas of immortality. Especially interesting in this regard are dreams in which the dead return specifically to assure the living that there was *indeed* (sic) life after death" (pp. 115-116).

Vande Kemp provided many examples of precognitive dreams, such as this one which appeared in an 1866 issue of Harper's Magazine. A woman awoke her husband with a scream saying "the boat is sinking; oh, save them!" She eventually fell back asleep and an hour later began crying, "I see the boat is going down!" and again her husband soothed her. After a third occasion when she woke up screaming, "The boat is sunk, they are all lost!" The woman became so distressed that she went to her nephew who was staying with her and made him promise that he would not go on the planned fishing party. He didn't go, and his companions who did go, were all drowned (p. 290). A condensed version of this dissertation appears in a journal article by Vande Kemp (1981).

Large Scale Surveys of Spontaneous ESP Dreams

Whenever surveys have been conducted inquiring about whether the respondent has had any form of psychic experiences, dreams almost always are found to be the most frequent mental state associated with their occurrence. In Gurney, Myers and Podmore's *Phantasms of the Living*, published in 1886, over 5,000 individuals were asked about possible psychic experiences. When extensive criteria were employed to weed out cases that were not very impressive, a total of over 700 cases survived their scrutiny. One of the categories that was utilized by the researchers, although with some hesitation, was dreams. One of their objections raised in that pre-Freudian era was that dreams were "somewhat dim and shapeless things," but the investigators had a more sweeping objection. They were concerned "that among the countless multitudes of dreams, one here and there is likely to correspond in time with the actual occurrence resembling the one dreamed of... Can the chances be at all estimated? ... Before we can give weight to a dream-coincidence as pointing to anything beyond the operation of chance, we should inquire whether the event dreamed of is distinct, unexpected, and unusual" (pp. 298-302).

This concern by the investigators is certainly a reasonable and valid one. A modern day approach to trying to deal with this problem of estimating whether a given event or topic is 'unexpected or unusual' in

a dream was undertaken by Calvin Hall and me. In our book, *The Content Analysis of Dreams* (Hall & Van de Castle, 1966), we provide extensive statistical norms for the dreams of 500 male and 500 female American college students obtained in the 1940s, which contain frequency counts for a large number of scorable categories such as objects, characters, activities, etc. If our norms were utilized to evaluate whether the two spontaneous 19th century cases I cited earlier contained typical or atypical content this is what would be found. A "well" was mentioned in the dream in the Seafield book. Of the 1,170 objects mentioned in our 1,000 dreams a well was noted only once. In the other dream, a "boat" was mentioned ten times by male dreamers and twelve times by female dreamers.

Which gender is more likely to report a psychic experience? In analyzing the 702 cases reported in *Phantasms*, it was found that 58% of the percipients [person reporting the psychic experience] were women. A blood relative was the agent [person dreamed about] in 47% of the cases, a friend or acquaintance in 42%, a husband or wife in 6%, and a stranger in 4%. Dreams were involved in 149 cases (21%) and 79 of these dreams represented or suggested the theme of death.

The authors noted that "the passage from sleep to waking admits of many degrees; and a very interesting group of cases remain which cannot properly be classed as dreams, and yet which do not appertain to seasons of completely normal wakefulness" (p. 389). They called such experiences "borderland cases." When Dale (1946) examined the 110 borderland cases reported in *Phantasms*, she reported that in 25% of the cases, the percipient was just falling asleep, in 15% the percipient was just waking up, and in 60% the percipient was sound asleep and jolted awake by the impression that someone had entered the room or called their name. In all but six of the 50 "sound asleep" cases, the agent was dead or dying and dreamer felt that the agent was actually in the sleeper's bedroom.

Following in the same tradition of *Phantasms*, E. M. Sidgwick published a paper in 1923 in which she reviewed all the telepathic cases appearing in the *Journal of the Society for Psychical Research* (SPR) from volumes 3-19, which had not been republished elsewhere. She reported that there were 170 spontaneous cases involved and that dreaming was involved in about one third of these cases. In 1934, Saltmarsh collated all the cases dealing with precognition since Volume 1 of the *Journal* and *Proceedings* of the SPR. He concluded that a total of 138 cases of precognition seemed fairly firm and 68% of these had

occurred during dreaming and 3% during borderland sleep. Death was the theme in 34% of the 138 cases.

The American Society for Psychical Research (ASPR) sent a form letter to 203 subjects participating in a long-distance experiment and asked if the participant had ever had a dream or waking experience corresponding to a distant event. A total of 50 participants replied affirmatively and 52% acknowledged that the circumstances involved a dream. When Dale (1946) tried to obtain independent corroboration, she found that the task was a very difficult one. The ASPR also followed up with an analysis in response to another popular article (Dale, 1951). Of the 65 cases which were eventually considered as reliable, the agent-percipient relationship was either a close blood or marital tie in 28 cases. Nearly 50% of the themes involved death. Dreams were involved in 41% of the cases, and 11% took place during "borderland sleep".

In response to a popular article published in 1957, the British SPR received approximately 1,500 letters and after considerable follow-up work, 300 cases were considered to be worthy of analysis by the investigator (Green, 1960). The percipient was dreaming in 33% of the cases and in borderland sleep in 8% of the cases. Of the 103 cases involving precognition, 64 occurred during dreams and 10 during borderland sleep.

There have been several other large-scale attempts to evaluate the frequency with which psychic experiences occur. The Boston SPR mailed a questionnaire to 9,298 men and 718 women whose names appeared in *Who's Who in America* (Prince, 1931). A total of 430 persons from this distinguished group acknowledged a paranormal experience. Perhaps not surprisingly in terms of the recognition accorded to women in those times, only 7% of the persons polled were women, but they produced 12% of the cases. Dreams accounted for 25% of the psychic experiences that were reported, death was the theme in 45% of them and accidents in 20%.

When a public appeal was made for accounts of psychic experiences, the University of Freiburg received over 1,000 letters. Sannwald (1959) reported that 70% of the recipients were females and 61% of the agents were males. Death was the theme in 43%, but events associated with love, marriage, and births only totaled 3%. Approximately 50% of the cases were based on dreams, but if only precognition is considered, the figure rises to 60%.

Louisa Rhine, the wife of J. B. Rhine, was able to file over 15,000 reports about psychic experiences that were sent to the Parapsychology Laboratory at Duke University (Rhine, 1978, p. 21). She indicated that over 60% of these reports consisted of dreams and that approximately 2/3rds of these dreams could be classified as realistic. These realistic dreams contained 'complete information' (i.e., correct identification of the target person and the event) 91% of the time. The other approximately 1/3rd of the psychic dreams reported to Rhine were unrealistic and classified as 'symbolic' dreams. They therefore somewhat resemble the 'strange' dreams most people report, but psychic dreams often seem to have a certain degree of importance or tenacity that sets them apart for the dreamer. The errors in symbolic dreams typically involve the nature of the event rather than the identity of the target person. Her last book on the topic of reports of psychic experiences, *The Invisible Picture,* was published in 1981. In a survey of high school students, Haight, Kennedy and Kanthamani (1979) also found that 90% of the reported dream experiences contained complete information.

Palmer (1979) obtained completed questionnaires about their ESP experiences from 208 students at the University of Virginia and 354 residents of Charlottesville, Virginia. One of the questions was phrased: "Have you ever had a rather clear and specific dream that matched in detail an event during, or after your dream, and that you did not know about or did not expect at the time of the dream?" A "yes" answer was given by 36% of the townspeople and 38% of the students. The Association for Research and Enlightenment (ARE), an organization devoted to publicizing Edgar Cayce's readings, is located in Virginia Beach, VA. A survey of this membership by Kohr (1980) reported that 53% of the respondents reported a psychic dream.

In his book entitled *Dreams That Come True: Their Psychic and Transforming Powers,* David Ryback (1988) described how he had administered a questionnaire to 433 undergraduate students in Atlanta, Georgia. The students provided written accounts of personal dreams that they considered dealt that with the future. Some type of paranormal dream was reported by 67% of the students. Many were not very convincing, but several did contain a fair amount of detail about unlikely events that did eventually occur. Ryback attempted to corroborate these dreams by interviewing the subjects and contacting others who might be able to verify the dreamer's account. He estimated

that about 8% of the dreams he obtained could reasonably be considered paranormal.

Some interesting cultural variables were found when surveys were administered in other countries. Sannwald provided a report based upon 1,000 German cases obtained from letters sent to a newspaper; Hanefeld (1968) stated that 38% of the cases were based on dreams and that the dreamers felt their veridical dreams were more clear, more distinct, and easier to recall then ordinary dreams. A questionnaire about paranormal beliefs was administered to 182 university and 59 lycée students in the Ivory Coast region of West Africa (Brockhous, 1968). A belief in paranormal dreams was expressed by 57% of the students and 33% answered that they had experienced prophetic dreams. Students who had been raised in urban settings professed a lower incidence of belief in parapsychological phenomena than those raised in the countryside.

On a questionnaire administered to nearly 2,500 eighth-grade students in Northern India, 36% of the boys and 37% of the girls acknowledged some form of personal ESP experience. Dreams were involved in contemporary experiences for 54% of the boys and 53% of the girls. Precognitive experiences were reported by 51% of the boys and 57% of the girls. Male agents, particularly brothers, were more frequent for both sexes, and the most frequent theme for both sexes was death (46%) when contemporary ESP was considered (Prasad & Stevenson, 1968).

A total of 763 students at a Japanese university filled out a questionnaire about their spontaneous psi experiences in 1988-1989 (Hagio, 1994). Only 64 students were female. Precognition was involved in 50% of the cases and a dying or dead person was the most frequent agent in 29% of the cases. Dreams were the most frequent form of psi experience in 93 cases (46%). Another study of students from Tsukuba University in Japan was conducted by McClenon (1994). Other cross-cultural surveys of psychic experience reported by 385 students in an introductory psychology course in New Zealand can be found in Clarke (1995) and McClenon (1990) surveyed 314 students in the People's Republic of China.

Familiarity with the dream material associated with spontaneous cases is strongly encouraged for anyone seeking to appraise their evidential value or who desire to construct possible motivational models for their appearance. It would therefore be helpful to review the following books and bulletins which each contain over 100 dream

reports: *Phantasms of the Living* (Gurney, Myers & Podmore, 1886), *The Unknown* (Flammarion, 1900), *Human Experiences* (Prince, 1931), *The Mystery of Dreams* (Stevens, 1949), and *Hidden Channels of the Mind* (Rhine, 1961.)

If one were to attempt to gain an overview of *spontaneous* psychic cases acknowledged by the general public during the past 90 years, a fairly consistent finding is that dreams are involved in somewhere between 33-68% of the cases. If the focus is placed upon telepathic cases, approximately 25% involve dreams, but if the focus is put upon precognitive cases, approximately 60% involve dreams. Approximately 10% of cases occur while a percipient is in a borderland state of sleep. In terms of the relationship issues reviewed in these studies, psychic experiences occur to women approximately twice as often as they do to men, but males are involved with approximately 50% of the cases as the focus of who is dreamed about (agent). Close blood ties are involved in about 50% of the cases, and death is the most prominent theme in about 50% of the experiences, with accidents and injuries next in order of prominence. In the special emotional circumstances of a psychotherapeutic setting, dreams involving death, injury, or accidents are featured less frequently. The agents are most likely to be family members of the client or the therapist. These dreams about the therapist seem to occur more frequently whenever the patient feels neglected by the therapist (Devereux, 1953).

Dreams as a Ubiquitous Psychological Experience

There have been significant strides in understanding just how common the experience of dreaming is. On the basis of studies carried out in sleep and dream laboratories around the world, it is now possible to state with a considerable degree of certainty that everyone dreams, everyone dreams every night, everyone has approximately four periods of increasingly longer dreaming activity during each dreaming period, and the overall amount of time devoted to dreaming is about 100 minutes per night. There are a wide variety of physiological events that are associated with dreaming activity, including increases in heart rate and respiratory variability, decreased tension in selective muscular systems, and the presence of arousal in the genital organs. For an overall summary of how this increased state of knowledge has been obtained, the reader can consult the account I provided in *Our Dreaming Mind* (Van de Castle, 1994).

Knowledge of these dreaming parameters has brought the experimental study of psychic dreaming to an exciting new level of exploration. By monitoring the different types of EEG (brain wave) activity along with the other measures of change in psycho-physiological parameters, an investigator in a modern sleep laboratory can awaken a sleeping subject with a high degree of confidence that the subject will be able to provide a report of their immediately preceding nocturnal mentation. It then becomes possible to have another person concentrate on a stimulus configuration in a distant location and have this person attempt to mentally 'transmit' the visual stimuli and emotional states that they are experiencing. Since there are several periods of dreaming activity (REM sleep), it is possible to make several attempts during a single night to send these psychic communications to the sleeping individual who is awakened after each 'sending' period. It then becomes possible to compare how similar the stimulus (target picture) viewed by the sender and psychically communicated to the dreaming receiver match up in terms of imagery and emotional components reported by the dreamer.

The largest scale studies of this type were carried out by Montague Ullman and Stanley Krippner at Maimonides Hospital in Brooklyn from 1961 through 1978 (Ullman, Krippner & Vaughn, 1989). An overview of the procedures they employed and the results obtained was presented and statistically evaluated by Irvin Child (1985) who was at that time the Chairperson of Psychology at Yale University. He concluded, "The outcome is clear. Several segments of the data, considered separately, yield significant evidence that dreams (and associations to them) tended to resemble the picture chosen randomly as target more than they resemble other pictures in the pool." (Ullman, *et al.*, 1989, p. 199.) Child published his findings in *American Psychologist*, the official publication of the American Psychological Association.

Unfortunately, it took the Maimonides Lab seven years to complete 379 psi sessions. In order to 'streamline' the process, a new technique was devised. In this experiment, a computer is programmed to automatically select a picture from a random pool of pictures and display the target image repeatedly through the night on a computer monitor in an empty room. Each participant kept track of their dreams at home and the next morning all participants went to the lab and viewed four pictures, only one of which was the selected target. They then ranked their preference as to which most closely matched their

dream images. Their rankings were combined to create a single consensus vote on the most agreed upon match.

This new arrangement allowed the dreamers to pick up on the target stimulus, but did so in a way that didn't require a special laboratory, all night technicians, or even separate senders or judges. In 2003, British psychologists Simon Sherwood and Chris Roe from the University of Northampton, England, reviewed all of the dream psi studies from the original Maimonides series and these latest 'at-home' dream experiments. They evaluated 47 experiments involving a total of 1,270 trials and concluded that the successful 'hit rate' was so much greater than what would be found if only a random event were involved, that the odds against chance were 22 billion to one (Radin, 2006, p. 109).

Energy Fields That May Play a Role in Psychic Dreaming

Michael Persinger has been associated with studies of geomagnetic forces upon several different psychological states, including nights when psychic sensitivity seemed to be more prominent. Persinger and Krippner examined the first night that each of the 62 subjects participated in psychic dream experiences at the Maimonides laboratory. Geomagnetic measurements of the northern hemisphere were determined for each of these nights. They discovered that "the subjects' telepathy was higher during calm nights with little sunspot activity then during nights marked by electrical storms and higher sunspot activity... Analysis with the research subject who participated most frequently in the Maimonides studies yielded similar results; his telepathic abilities appeared to function best on calm nights with low geomagnetic activity" (Krippner *et al.*, 2002, pp. 101-102). Persinger (1987) was also able to demonstrate significant relationships between various indices of geomagnetic activity and some selected cases of impressive psychic experiences in the material surveyed by Gurney *et al.* (1886) in *Phantasms of the Living*.

A comprehensive review by Alvarado (2001) contains nearly 200 references pertaining to subtle energies and their possible relationships to psi functioning. He notes that the concept of subtle energy is central to the parapsychology of countries such as China (Leping & McConnell, 1991) and Japan (Kokubo & Kasahara, 2000). In the United States, the International Society for the Study of Subtle Energies and Energy Medicine (ISSSEEM) was founded in 1989 as a professional organization dedicated to exploring the potential effect of

subtle energies in health related areas. They publish proceedings of the papers presented at their annual conferences.

An exhaustive historical review of energy fields in medicine has been provided by James Oschman (2000) in *Energy Medicine: The Scientific Basis*. He discusses a wide range of research efforts that have been made on topics such as geomagnetic fields, acupuncture, ELF signals and the electromagnetic environment. Oschman has a provocative chapter on 'Energy Circles' that provides instructions and an illustrated diagram for demonstrating, "the flow of energy in groups" (pp. 245-248). Another excellent book on various related force fields is *Vibrational Medicine for the 21st Century: The Complete Guide to Energy Healing and Spiritual Transformation* (Gerber, 2000). Among Gerber's ten chapters is one entitled 'Magnetobiology and the Art and Science of Magnetic Healing', in which he refers to several different types of magnetism, such as geomagnetism, electromagnetism, biomagnetism, and solar magnetism (p. 275). In a provocative chapter entitled 'The Clash of the Paradigms: The Newtonian Mechanists vs. the Quantum Mechanics', Gerber states (p. 10): "From the perspective of vibrational medicine, our consciousness is not limited to the brain and central nervous system but is also seen as an integral aspect of the human heart."

Energy Connections of Heart Transplant Donors and Recipients

With modern advances in transplant technology, there have been some impressive accounts of remarkable psychological changes in percipients who have received a heart donation. In 1991, at a dreaming conference in Russia, I met Claire Sylvia who had been a heart-lung transplant recipient. She later shared her remarkable story in a book entitled *A Change of Heart* (Sylvia & Novak, 1997). She experienced surprisingly accurate dreams about her donor (an 18 year old man) who was killed in a motorcycle accident and described changes in her food tastes, style of dancing, etc., that were consistent with her donor's lifestyle. These wide ranging behavioral changes offer clues about the existence of the possible phenomena associated with cellular memories.

In his stimulating book, *The Heart's Code*, Paul Pearsall (1998) discusses heart energy and points out that the usual brain wave frequency is around 0-30 cps while for the heart it is around 250 cps. Pearsall is a psychoneuroimmunologist who became impressed with how under-recognized the heart was as a source of internal energy

communication within the body, and how it may also serve as a connecting mechanism with others' heart energy system. In his book, he discusses the reports of 73 heart transplant patients and their families and 18 donor family members.

Early on in his book, Pearsall describes a talk he gave to a group of mental health workers. A sobbing woman made her way to the microphone and described the following incident: "I have a patient, an eight-year-old little girl who received the heart of a murdered 10-year-old girl.... she started screaming at night about her dreams of the man who had murdered her donor. The mother and a friend finally decided to call the police and using the descriptions from the little girl, they found the murderer. He was easily convicted with the evidence my patient provided. The time, the weapon, the place, the clothes he wore, what the little girl he killed had said to him... everything the little heart transplant recipient reported in her dreams was completely accurate" (p. 7).

Here are a few examples concerning the link between the dreaming experiences of the recipient and the nature of the donor's personality:

- A 41-year-old male heart transplant patient received the heart of a 19-year-old girl after her car was struck by a train. The wife of the recipient said that with his new heart he was now like a teenager. "He keeps talking about power and energy all the time. He says he has had several dreams that he is driving a huge truck or is the engineer of a large steam engine. He is sure that his donor was driving a big truck that hit a bigger truck" (p. 88).

- A 36-year-old female heart-lung recipient received the heart and lungs of a 20 year-old girl who was killed while running across the street to show her fiancé a picture of her new wedding dress. "Almost every night, I had this dream about her. I know she was young and pretty and very happy. I've always been sort of a somewhat down type of person yet, somehow, I have this new happiness in me I never experienced before... I feel a strange kind of happiness, excitement, enjoyment that I cannot fully explain or put into words" (p. 88).

Pearsall talks about the mixed type of reactions that can be experienced with regard to learning details about who their donor was and what their lifestyle might have been. Some find themselves

very threatened by that possibility, while others wish to expand their knowledge. The latter fall into what Pearsall calls the 'heart path' and is taken by about 1 in 10 of the transplant patients, "these patients became intensely interested in the meaning of their heart transplants, were interested in the characteristics of their donor, and reported experiencing clear and intense dreams, not only about the transplant, but about their often very accurate image of their donor" (p. 92).

As a result of his interviews with the 73 heart transplant patients, Pearsall labeled those who go down the "path of the heart rather than the head" as 'cardio sensitives', and he felt they had a very perceptive "hidden observer." Cardio sensitives had 18 characteristics which were similar to the characteristics of easily hypnotized subjects. One of these 18 items was what he labeled as 'psychic-sensitive.' These patients were described by family members or friends as being 'psychic' or 'very sensitive' to things others are not sensitive to, and they showed this sensitivity long before their illness was diagnosed and their eventual transplant. Another one of the 18 items was the 'dreamer' category, who long before they had become ill or had a transplant, reported extensive dreaming, memory for dreams, and an interest in the significance of their dreams. Following their transplant, most of them reported dreaming about their donor.

Charles Siebert, a medical writer, wrote an article in the April 13, 1997 issue of the *New York Times Magazine* concerning one woman's heart transplant experience. In preparation for this, he attended a Valentine's Day party held for more than 100 heart transplant recipients. Almost every recipient reported 'spiritual memories' or feeling the energy of their donor. Siebert writes, "all the people I met at the party spoke in the same reverent tones about the angel in their chests, about this gift, this responsibility that they now bear, and a little prayer they say to the other person inside them. It was as if they were part of some strange cult, the tribe of the transplanted" (p. 81).

Pearsall makes a strong argument for the validity of considering that there can be a high order of interaction among cellular systems and that they can possibly become active in manifesting the eventual lifestyle of a person. Pearsall offers several possible sources for cellular memories including a neuropeptide theory, magnetic field theory, electrophysiological theory, etc., (pp. 118-121).

Psychic Connections between Identical Twins

Pearsall provides the following example of how strong cellular imprinting might manifest in people's lives. In a 1980 *Science* article Holden reported on *Identical Twins Reared Apart* (pp. 1323-1328): Jim Springer and Jim Lewis, identical twins who became known as the 'Jim Twins', were separated at birth and placed in different homes in Ohio. In 1979, they were reunited at age 39. When they first met, both were six feet tall and weighed 180 pounds. Here are some of the similar experiences they reported in their lives:

- They had each been married twice, the first time to a woman named Linda and the second time to a wife named Betty.
- They each had a son with the same name, but spelled differently: James Allan and James Alan.
- As children, each had a dog named Toy.
- Both had taken family vacations independently to the same 3 block strip of Florida beach called *Pas-Grille*. They both arrived in blue Chevrolets.
- They had each served part-time as sheriff's deputies in their respective communities.
- They each enjoyed the hobby of carpentry, which they practiced in similar basement workshops, making similar things, like a circular white bench around a tree in his back yard.
- They both disliked baseball and said that they were stock car racing buffs.
- They both chain smoked Salem cigarettes and savored an occasional Miller Lite beer.
- They each chewed their fingernails severely.
- Both had suffered from hemorrhoids and had undergone a vasectomy.
- Each was reported by their spouses to be very demonstrative in their affection and to constantly be leaving love notes around the house.

Surprisingly, very little systematic research has been done to investigate the psi ability of identical twins. A 1965 article in the journal *Science* entitled, "Extrasensory electroencephalographic induction between identical twins," described the efforts of Duane and Behrendt to study whether the EEG patterns stimulated in one identical

twin would be similar for the other twin separated at a distance. Encouraging results were found for two of the fifteen pairs tested, but the article was harshly criticized by scientists for lack of adequate description of their procedures. The authors subsequently acknowledged some of their shortcomings in descriptive details but encouraged others to try and follow up on researching this topic (Duane & Behrendt, 1966).

A somewhat similar psychophysiological study was carried out by Esser, Etter and Chamberlain (1967) at Rockland State Hospital, but they used a plethysmograph to measure blood volume rather than using an EEG. They claimed that "in a physically isolated subject, we have observed physiological reactions at the precise moment at which another person... was actively stimulated" (cited in Playfair, 2002, p. 41). The stimuli involved slides containing neutral or emotive words or phrases. Only one of the twelve pairs of subjects were identical twins. The authors printed out the complete chart record of these twins "to show how obvious the plethysmographic reactions are" (p. 42).

Using 26 pairs of identical twins, Barron and Mordkoff (1968) used a polygraph to measure if a monitored twin would respond to their twin viewing an emotional film. In one pair, one twin reacted to three out of eight 'arousal points' in the film, but the majority of the twins left the study because they objected to how they were being treated by the experimenters. The validity of their complaints was candidly acknowledged by the experimenters who admitted that the conditions were "unfavorable" for success.

The most extensive coverage of this overall topic is provided in Guy Playfair's (2002) excellent book, *Twin Telepathy: The Psychic Connection*. He laments the dearth of research that has been carried out in this area which would seem to have such obvious payoff benefits. He is also appropriately critical of the way that testing with identical twins has been carried out. Only a handful (eight) of identical twins had been used in studies reported in the *Journal of Parapsychology* and the design has involved testing whether an identical twin could successfully guess the identity of an ESP card when it was being 'sent' from the experimenter rather than from their identical twin. Nothing in the anecdotal literature suggests that an identical twin is successful at forming a psychic bond with anyone else besides their matching identical twin. Not surprisingly, the findings failed to support the existence of any telepathic linkage between the twin and the experimenter (Kubis & Rourke, 1937; Rogers, 1960).

If one were to consider identical twins as potential subjects, it would be important to also take into consideration whether an ego-syntonic or dystonic relationship exists between them. Some twins delight in having similar names, greatly enjoy wearing the same style of clothing, and in many ways living out their lives in sort of a mirror reflection of each other. They like each other, enjoy being in each other's company, are often able to complete each other's sentences, and so on. On the other hand, for some identical twins, it is important that they be perceived as completely separate and individual beings. To ensure this separateness they dress differently and display as many contrasting behaviors as possible to others to make sure that the message is sent that they are not one and the same person, but two entirely different and very distinct individuals. Presumably, the latter subjects would not make suitable twin pairs to demonstrate successful psi results in an experimental situation.

There has been a sparsity of work carried out with identical twins in the type of laboratory arrangement that was developed at Maimonides Hospital. On page 163 of the second edition of *Dream Telepathy* (Ullman, *et al.*, 1989), the authors comment: "how important is agent-subject rapport? We would say that it is a very important factor bearing on the success rate of our various subjects. In support of this we have limited, but provocative, results with identical twins." Two identical male twins from India who came to the United States for graduate work in electrical engineering, volunteered as subjects for a dream experiment. Their religious background was Zoroastrianism, a religion founded by the Persian prophet Zoroaster in the sixth century B.C.

Cyrus, the more dominant of the twins, was to act as telepathic agent, while Darius acted as sleeping subject. The randomly chosen target that night was an impressionistic painting of a New England church. That night Darius dreamed of a rectangular plaque of marble and a friend lighting candles. In his last dream, "there was some kind of scientific experiment going on, something like taking a head for someone else's body. It was a plaster cast with a head sticking out. I also got the definite impression of eating rock candy. Almost continuously" (p. 114). When Krippner queried Darius about his associations, he replied that his *only* association was with the Zoroastrian religious ceremony in which participants eat rock candy. Other associations to the ceremony included marble statues and lighting candles in the temple. An independent judge ranked the New England church as number 1 out of a pool of 6 pictures. One might speculate

that the one twin had to put his 'head in a different place' to relate to this different form of religious building.

I had the opportunity to work with two sets of identical twins in my dream laboratory at the University of Virginia. The women were approximately 40 years old. One was shown a randomly selected target picture involving a colored photograph of a crowded English pub with a waiter serving drinks at a table, a bar in the background, and an overall air of festivity and party energy. Her dreaming twin did a remarkably good job of describing a festive restaurant setting and other details that were extremely similar to the target picture sent by her twin. She gave the target picture a rank of 1 out of the 8 choices available.

One member of a pair of identical male twins in their 30s also achieved a number 1 rank out of 8 for matching his dreams to the target picture. The randomly selected target picture that one twin looked at involved a colored photo of a large waterfall in South America. His twin dreamed that he was in his room at our laboratory, but was initially angry in his dream because the water wasn't working and a plumber had to come in to fix the pipes. The twin's anger dissipated when shortly after the pipes began working, there was a conspicuous flow of water coming down in the shower stall and he was further pleased when he saw that there was a naked woman somewhat facing him in the shower.

Entangled Minds and Psychic Dreams

In his book *Entangled Minds: Extra Sensory Experiences in a Quantum Reality*, Dean Radin (2006, p. 6) writes: "quantum theory is a mathematically precise and exquisitely well-tested description of the observable world. Psychic phenomena are slippery, subjective events with a checkered past. But as it turns out, the fabric of reality suggested by quantum theory and the observations associated with psychic phenomena bear striking resemblances." According to Radin (p. 14), the following quote by Michael Brooks appeared in the March 27, 2004 *New Scientist* article, "The Weirdest Link", concerning entanglement research: "physicists now believe that entanglement between particles exists everywhere, all the time, and have recently found shocking evidence that it affects the wider, 'macroscopic' world that we inhabit." Radin goes on (p. 19): "I propose that the fabric of reality is comprised of 'entangled threads' that are consistent with the core of the psi experience."

As an example of some possible psi entangled threads, I would like to describe an experiment I created in which a glimpse of entanglement might be perceived. On pp. 433-436 of *Our Dreaming Mind* (1994), I describe my efforts to concentrate on a black-and-white photograph while sitting at my kitchen table in Charlottesville, Virginia. I had requested that a group of dreamers from the *Dream Network Bulletin* attempt to dream about what imagery might be contained in that photo. A total of 27 percipients mailed in letters about their dreaming experiences that night.

The photo showed an Indian woman from Panama standing in front of a large council house with slanted, wooden walls and a thatched palm roof. Standing on a large metal pot behind her was a young boy with his hands around a wooden pole and another child was visible in the house opening. I assumed that correspondences between the photo and dream elements of the percipients would be considered a manifestation of psi at a concrete level. I had written down quite a few associations about the photo which mentioned that the large pot was used to brew an alcoholic beverage that was connected with puberty ceremonies and that music and dancing accompanied other ceremonies held in this council house. I considered connections between my associations to the photo and dream elements of the percipients to be representations of a connection at an abstract level. I also woke up three times during the night and recorded several long dreams. I considered correspondences between my dreams and the dreams of the percipients to be taking place at a transpersonal level.

Claudia, a woman in Brooklyn, who was a complete stranger to me, managed to pick up on the target photo, my associations, and my dreams as well. With regard to the target picture, she mentioned her young daughter, hands on a structure of beams and poles, wooden sliding doors, and a building with an unusual looking roof. I had written that the thatched palm roof provides a good protection from the rain and Claudia wrote that she heard rain beginning and realized she didn't have an umbrella. In my written associations to the photo, I mentioned the puberty ceremonies for young girls that were held in the council house. Claudia described a reception room with low benches along the wall where many young women were excitedly getting ready for the first meeting of a church choir that required a special dress. The council house does have low benches along the wall.

It was our dream-to-dream correspondences, however, that totally amazed me. My first dream involved a fishing scene: I was sometimes

on a boat and sometimes on shore. The man I was with caught two large flounder and a woman kept insisting that I put them on top of a boat and gut them. I attempted to cut the fish open with a razor blade. Some blood came out; the fish's face turned into a man's face and he was bleeding. I told him to wash his face with water and said I would need his advice as to how to cut around his ears and nose.

Here is an excerpt from Claudia's first dream: "I am outdoors, perhaps on the deck of a ship... mounting the fresh, whole wet skin of a small whale or whale's head. After removing one eye (the only one, it's a side view) with a knife I'm using, I hear a conversation... (All of this could be influenced by a recent waking experience of washing flounder for cooking, but not removing their heads.) I feel a kinship, or sympathy with the whale, which at some point transforms into a person. The wet, stretched mounted skin is now a man's face, reddish-brown... I don't seem to notice the change of the whale." The odds against two strangers on the same night dreaming of being on a boat cutting open a fish and having the face of the fish turn into a bloody man's face while a conversation is going on are astronomical, and that both dreamers mentioned flounder delivers the final blow to any notion that the correspondences could have been mere coincidence.

My second dream involved providing drinks that cost 41 cents each for a group of students working on a project. There was also something about a mother dividing a cake or other dessert into two portions. In Claudia's second dream, she had stolen some money with several other people and offered to count another person's share. She wrote down figures which were an "odd amount of dollars and cents." Her daughter was with her and asked the others to count out a share with "equal denominations." Once again, my mention of an odd number of cents (41) and concern about a cake or dessert being divided into two portions is remarkably similar to Claudia's dream of an odd amount of "dollars and cents" with her daughter trying to count out a share with "equal denominations." In her next dream, Claudia described a film depicting college students working on an art project and I dreamed about a group of students working on a project.

In addition to these impressive correspondences between my dreams and those of Claudia, there were also some clear linkages between my dreams and those of other percipients. Rita Dwyer, a past president of IASD, dreamed she saw an animal face and an animal with an open wound that she wanted to sew up or heal before too much blood was lost. My first dream mentioned a fishing scene and other dreamers

mentioned an ocean, a sea, a river, pools of water, an ocean trip, a boat, a yacht, oars for a rowboat, fishhooks, and a hook.

In my last dream there was a crowd of people, a slanted auditorium, and some board members and I were sitting down to eat at a table on the main floor or lecturing area. A total of 11 dreamers made references to activities taking place at a different elevation: in a grandstand, high in a theater, on a stage, and a balcony, etc. I mentioned board members and one dreamer described a meeting in a civic auditorium, another described a chairman of a group trying to decide on a meeting time and another had a manager present and three dreamers had politicians present. Tables were mentioned by four dreamers. In another scene of my last dream, I was using a hose to water down a pile of leaves. This latter image is suggestive of urination and two of the nine male dreamers reported urinating in their dreams, and one woman dreamed that something like tea was being splashed or dropped on the pages of a magazine. (Urine was not mentioned even once in the 1,000 dreams that Hall and I used for our normative material on dreams.)

There were many overlapping items that were not represented in *my* dreams or associations, but could be considered as representing connections *between* the dreamers. It seemed that we all became midnight swimmers in a common cosmic sea. Projects in which individuals attempt to meet and interact with each other in their dreams have been described (Magallón, 1997; Magallón & Shor, 1990).

The similarities in dream content I've described above suggest that strong correspondences between imagery are shown when both individuals in a telepathic dyad are in altered states of consciousness. A confirmation for this hypothesis was reported by Rechtschaffen (1970), who noticed that there was often an impressive degree of correspondence between dreams occurring at about the same time of night when two subjects were sleeping in his laboratory at the University of Chicago. Here's one example he provided of this shared imagery: "In the first dream, one subject dreamt about students singing in Russia, and the other subject dreamed about students doing some kind of interpretive singing. In the second dream, the first subject was taking a violin lesson and the other subject was learning a guitar melody. In the third dream, the first subject was watching a James Cagney gangster movie, and the other subject reported a dream about a recent gangster movie, 'Bonnie and Clyde'" (p. 89). After observing a few other examples of simultaneous dreams, Rechtschaffen remarked, "We became very excited. We thought that maybe the question is not

so much what do dreams mean, but whose dream are you having?" (p. 89).

Rechtschaffen decided to use hypnosis to investigate the possibility of inducing simultaneous dreaming in an altered state of consciousness more carefully. Before the subject went to sleep for the night, he was hypnotized, and while he was in a trance, told that during the night he would have a certain dream…

> The very first night we tried it, we told the subject that he would dream of the death of Martin Luther King and of a fear of riots, and he dreamt that Martin Luther King had been shot, that somebody threw a rock and they were afraid a riot would start. The other subject, who had not received any suggestion, dreamt of a Negro policeman who was beating another man, and he was afraid that somebody would throw a brick and start a riot.
>
> On another night, we told the subject to dream that he was in an amusement park, having a very good time. He dreamt that, and specifically he dreamt about riding on a merry-go-round. The other subject had a dream of people laughing and running in circles and there were 'grinning, funny looking horses' in his dream. (p. 89)

In the next stage of research, Rechtschaffen explored simultaneous waking hypnotic dreams. His procedure was to hypnotize two subjects and tell each one, "When I give you this signal you will have a dream of 10 minutes duration. You will wake up after 10 minutes and tell us that dream" (p. 91). The subject who served as agent was given a specific suggestion as to what he should dream; the other subject, the percipient, who is at the opposite side of the building on a different floor, did not receive any suggestion. On one night the topic suggested to the agent was: "You fall in love with a beautiful girl in the spring… you walk in the wind and sun and are filled with bliss" (p. 92). The agent dreamed, "I was with a beautiful girl. It was a beautiful day… the sun was out; it was just the right temperature." The percipient dreamed: "I was in this swan-like boat in a lake, and I saw a really beautiful girl. The boat went over to where she was… there were angels singing… the girl was very beautiful. She had long blonde hair and it was very sunny and nice out" (p. 92).

Rechtschaffen was very impressed with these striking correspondences and observed that 'hits' when they occurred were

quite impressive and quite easy to detect. He indicated, "when you simply have judges matching the dream against a suggested topic, a hit does not reveal the degree of the hit. A simple matching procedure does not take into account the very unlikely probability of such a specific correspondence" (p. 92). Several other impressive examples of matching imagery are included his paper, *Sleep and Dream States*, which he presented at the Parapsychology Foundation conference held in Nice in 1968 and published in the Proceedings in 1970.

Porosity or the Multi-Faceted Nature of Psi

Twenty-five years ago, the Association for the Study of Dreams (ASD) was founded and continues to promote the understanding and appreciation of dreams; the organization is currently referred to as the International Association for the Study of Dreams (IASD). It publishes a newsletter called *Dreamtime* and its professional journal, called *Dreaming,* is currently published by the American Psychological Association. Membership is open to all dream appreciators. Further information about the organization and its activities is available at their website: http://www.asdreams.org.

Their annual conference has been held at many international venues such as Canada, England, Holland, and Denmark. Starting at the second conference held in Charlottesville, Virginia in 1985, which I hosted, I instituted a feature into the program, which has continued at all subsequent conferences, which we labeled as a 'telepathy' contest. A pool of four different colored photographs was selected to represent different target stimuli. The imagery differed in terms of color, shape, thematic content, degree of kinetic activity (e.g., a mountain vs. a person engaged in running activity), presumed emotionality displayed (happiness vs. fear), type of organism shown (a human being vs. an animal), the number of organisms shown (an individual vs. a group), and so on. Each photograph was sealed up in an opaque manila envelope and labeled either, 1, 2, 3, 4 or A, B, C, D. The agent would shuffle the envelopes and select a single one that they would take back to their room. After opening the envelope, they would concentrate upon the picture several times during the night and attempt to transmit the imagery and emotions it invoked to the participating dreamers.

The next morning, all four pictures were numerically or alphabetically identified and taped on a different cardboard box. The participating dreamers placed a copy of their written dreams in the box

that they felt contained the picture most closely corresponding to their own dream imagery the preceding night. A pool of three or four judges examined the copies of the dreams that had been deposited in the box containing the photograph that the agent had concentrated upon during the night. These dreams were then ranked for their degree of correspondence to the 'target' picture. A first and second place ribbon was then awarded at our Dream Ball to the individuals whose dreams most closely matched the target picture.

A 'telepathy' contest was held at each of the subsequent 24 ASD conferences. There were always impressive dream correspondences found for the 'target' picture. This testing paradigm seemed simple enough to demonstrate telepathy. The pictures had been pre-rated to assure that they would be quite dissimilar in content between each one and only one had been concentrated upon by the agent. That picture was therefore considered the only relevant target. What became troubling over the years of our contests was that there were often striking resemblances between the dreaming imagery of the contestants and one or more of the non-target pictures. We had been arbitrarily calling any dreams that did not seem to bear a resemblance to the 'target' picture as a 'miss'. However, in actuality, the close correspondences between some of the dreams and the alternative pictures that were part of the original pool were clearly 'hits' in terms of unmistakable resemblances to some of the 'non-target' pictures.

It is certainly possible to conclude that psi was present if a different modality besides telepathy was also considered to be operative. All the potential targets had been seen by the experimenter before they were placed in the four envelopes. It is also possible to consider that the dreamers were clairvoyantly tuning in to one of the more interesting concealed pictures throughout the night, if the artificial barrier of the envelope is overlooked or 'looked through'. Precognition could also have been at play because any dreamer could have gone forward in time several hours to see the pool of potential target pictures displayed on the four different boxes the next morning. They could then precognitively choose to dream about a particular picture because it fitted in more meaningfully with their own personal dynamics.

The finding that some type of fluid psi operation was demonstrated during each of the 24 'telepathy' contests provides strong support for the notion that the psi process is an extremely porous one; and any one of the psi modalities—telepathy, clairvoyance, or precognition—could have been involved separately, or in some combination. So it has been

a misnomer to label our procedure as a 'telepathy' contest rather than a psi or GESP (General Extrasensory Perception) contest. It is also possible to conceptualize that if one percipient strongly resonated to one of the four potential target pictures, other recipients could potentially tune into that target person and their subsequent dreams. Such a pattern of 'entanglement' is similar to that which I previously described in connection with the *Dream Network Bulletin* testing situation.

To get a flavor of the type of typical results obtained in such a contest, I'll provide a synopsis of the results from the Dream Telepathy Contest held at the Montreal Conference in July 2008. The following material was published in the Fall 2008 issue of *Dreamtime* by Rita Dwyer (pp. 20-21, 40), where the four pictures are shown. The contestants were both on-site at the conference and also participated internationally through a global internet connection. The picture concentrated upon by the agent was a color photograph of a man in a bathing suit who is holding onto a tall pole with a large sail containing many brightly colored patches, predominantly orange and yellow. He is standing on a windsurfing boat against a clear blue sky and appears to be sailing with a watery horizon at his waist with large splashes of water all around him.

The first-place winner reported the following dream: "Wading through the water about up to my chest or belly at the cottage and I decide to go fishing. I get a strong tug and realize I caught this very large, oddly colored fish. The fish's markings are in large color patches and I hold it now so it doesn't get away. I look into its eyes and it looks scared whether I will eat it or not. Additional note: this is also the exact location at our family lake where I would land our windsurfer." One contestant dreamed about a pirate ship, another about looking down from a balcony at a lake, and another dreamed about painting a surfboard. Another contestant dreamed about a triangle and some kind of child-like simple sailboat shape that was formed with material (cloth) of a dark orange color.

One of the 'non-target' pictures showed several presumably Jewish men with black hats and cloth shawls with prominent white stripes over their shoulders, standing in front of the Wailing Wall in Jerusalem. One of the contestants dreamed, "I am living with, and am a part of a middle-eastern looking family... (my father) says it is important to live when alive—not to live in fear. I still fear the pain of being shot or blown up in death." Another contestant dreamed, "A thin white thread

is running across the center of my field of vision. A sense maybe of stripes." The dream of another contestant was "open area … almost like a market … boss was a large, burly man in a hat."

Another non-target picture involved a close-up photograph of a young Russian girl standing on a snowy field. (This picture was the cover photo of a *National Geographic* issue and the word for "Welcome" appears on it in Russian.) One female contestant dreamed, "My sister Claire and I are nose-kissing (rubbing noses like an Eskimo kiss)". Another person dreamed, "As I am standing outside it begins to snow. I am now standing with crossed arms with a dusting of snow all over my body. The woman's daughter named Daisy comes out. They take a picture of me standing in the snow." Still another contestant dreamed, "Reclining chair on the bank of a lake but the bank was covered with snow."

The remaining non-target picture was of an elephant standing in a jungle with several tall leafy trees nearby. One contestant dreamed, "I am walking through a dense forest. Someone is chasing me. I materialize a skateboard and begin to fly through the forest. I notice a leaf in my left peripheral vision. It is a very deep green… I begin to become lucid and decide to stop. The soil is deep brown. The trees are very high and green… there before me stands an elephant—Debbie the elephant from the San Diego Zoo." Another contestant reported, "During my dream a grey elephant appeared. I was thinking, is it white or grey? But the picture remained as a grey elephant going to the right." Another person dreamed, "I saw an elephant in profile, vague image of a circus performer or half-dressed clown in the background."

It is clear that there were some striking correspondences, not only with the designated 'target picture', but also with each of the other three "non-target' pictures in the original pool of stimuli. What is of interest is that one of the 'non-target' pictures contained a somewhat unique theme—that of an elephant in the forest. A total of 45 dreams were submitted (36 on-site and 9 online). One might be able to dismiss the fact that there were three dreams involving elephants if elephants are a topic that is frequently dreamed about. However, if one considers there were only two references to elephants in the 1,000 dreams that Calvin Hall and I indexed in *The Content Analysis of Dreams* (1966) one should expect only one elephant reference to appear in every 500 dreams. However, when there were 3 elephants mentioned in 45 dreams, or one out of every 15 dreams it becomes possible to feel more

confidence in making an assumption that a non-chance finding was apparently involved.

The types of results just summarized for the IASD conference in 2008 are fairly typical for those that are obtained each year when such a contest has been conducted. The material presented above was selected, not because it was particularly impressive, but because it was the most recent and a published record of the results is available for confirmation and further evaluation. This steady pattern of successful results obtained during our annual conferences has played a role in shifting the attitudes of many of our members from being previously somewhat skeptical, to now being more open to the possibility that a psi process is involved in many aspects of dreaming.

In his recent book *Entangled Minds* (2006) Dean Radin stated "the modern ganzfeld experiment is as close to the perfect psi experiment as anyone knows how to conduct" (p. 117). I think, however, a strong argument could be made that the type of psi elicited in dream studies is more impressive than results found in the ganzfeld condition. Some experimental evidence for this claim is provided in a study by H. Kanthamani and R. S Broughton (1992). They described three studies in which a comparison was made between ganzfeld and dream conditions "to see whether the dream condition would continue to be better than the ganzfeld in eliciting psi information" (p. 28). Three experiments were carried out. The first one only involved a single subject, the second involved 10 subjects and the third involved 20 subjects. The target pool consisted of 50 sets of four pictures and a computer pseudo random number generator was used to select targets. Subjects were instructed to bring for evaluation a report of their night's dreams collected at home after giving themselves a suggestion that the target material would enter their dreams. The investigators found "the score difference was in favor of the dream condition, and was strongly significant ... The direct hit rate for the subjects in ganzfeld was 33% ... in the dreaming condition it was still higher at 38%." (p. 30). Interestingly, the authors speculated: "Perhaps the naturally occurring altered state functions more efficiently in gaining psi information than the laboratory-induced altered state condition" (p. 31).

Dream Helper Ceremony

As shown in the previous sections, psi is a multi-faceted communication system which enables people to connect at many levels.

This section will give an account of a procedure that Henry Reed and I co-developed which utilizes this process to connect with others at a deeper, more meaningful, personal level (Van de Castle, 1994). Its origins and some case histories are described by Henry in his book, *Dream Medicine: Learning to Get Help from Our Dreams* (Reed, 2005) in a chapter entitled 'The Dream Helper Ceremony: The 'Good Samaritan' Dreaming Circle.'

We decided to utilize psychic dreaming in a group context as a way to be of service to others. Rather than focusing upon a *target picture*, a group of dreamers focus upon a *target person*. This target person acknowledges that he or she has some troubling emotional problem, but does not discuss it or give any type of hint as to the nature of the problem. The group dreamers are called 'dream helpers'. Before retiring for the night, these dream helpers gather around the designated target person and engage in some activity of the target person's choice to create a feeling of group bonding. The group might meditate, pray, sing together, or sit silently, holding hands, etc. It is useful if the target person can loan some personal object, such as jewelry, a photograph, or an article of clothing to each dream helper. Wearing that object, or having it by the bedside, will enable each dream helper to feel a special energy connection with the target person when they go to sleep that evening.

All of the dream helpers renounce their right to experience any personal dreams that night and dedicate all their dreaming activity to being of service to the target person. They ask to be used as a psychic vehicle to achieve understanding and healing for the target person. Ordinarily, at home they might not make a special effort to record every one of their dreams for their own person benefit. However, since they have dedicated all of their dreams to the target person, they work diligently to maximize their dream recall and write down each dream they experience during the night. They don't want to cheat the target person out of the dreams to which they are entitled. Dream recall is generally considerably enhanced through this intentionality.

The next morning, the dream helpers gather again with the target person, and each one, in turn, describes in detail their dreams from the preceding night. Using a whiteboard or large easel and pad, the details of each dream are recorded so that everyone can review them for possible similarities in content and themes. A fascinating pattern gradually emerges as the warp of one dreamer's images is laid against the woof of another's, and dream strand after dream strand is woven

into a rich collective tapestry. The members attempt to speculate about what the possible emotional concern of the target person might be. After this process is completed, the target person typically reveals the nature of the problem for which they sought help.

Henry and I have participated in the dream helper ceremony on many occasions. Each time we have been impressed with the collective accuracy of the dream helpers in identifying the problem for which the target person sought help, and in sometimes coming up with a possible solution for it. In one session, the dream helpers reported a black limousine driving into the town of White Hall; someone being hesitant to accept an Oreo cookie; someone ordering an ice cream cone with one scoop of chocolate and one of vanilla; someone noticing the black and white keys on a piano keyboard; and Martin Luther King Jr. speaking in front of the White House. With each successive report, the theme of black and white became more obvious. On the basis of this repeated theme and some other material, the helpers speculated that the target person was involved in a dating relationship with a black man. In addition, several dreams also dealt with family dissension and parental lectures about obedience.

The target person, a white woman, was very surprised by these dreams. Most of the dream helpers were complete strangers to her and none were aware that she was dating a black man and struggling with the question as to how to deal with the negative reaction she anticipated from her family about her new secret relationship. One of the helpers dreamed that his watch was slow, and another saw a movie in slow motion. As the dream helpers discussed these dreams about slow motion, they suggested to the dreamer that she might move slowly in bringing up this relationship to her parents until she was sure she wanted to continue it.

Seldom does any single dream helper grasp the full significance of the target person's problem. Like the proverbial blind men and the elephant, one describes the tail, one the leg, one the trunk, one the tusks, one the ears, etc., and only when these discrete bits of information are assembled together is it possible to grasp that the common theme is that of the elephant.

The group was astonished and delighted at what they had accomplished. They felt they had been so successful because they were not attempting to gain anything for themselves; they were engaged in a healing service nourished solely from a sense of love. Everyone benefited and felt energized by their participation. The target person

was deeply touched by the obvious sense of caring and compassion that the group communicated to her. Although she had not verbalized the problem she sought help with beforehand, the dream helpers had been able to comprehend her problem, empathize with her feelings, and supportively reflect on how she might lessen the anguish her conflicts were currently generating.

An unexpected gift was also provided to the dream helpers. From my experience in conducting this event on several occasions, it seems that the target person serves as sort of a 'lightning rod' to consolidate whatever issue is the most significant unresolved problem for that particular group at that particular time. In this case, the issue of interracial relationships had been brought forward as being of major significance for the target person. Each dream helper could now reflect upon what possible relevance or significance that issue might have in his/her own life. Someone declining to accept an Oreo cookie is showing a quite different attitude than another individual who specifically orders and enjoys an ice cream cone with one scoop of chocolate and one scoop of vanilla. Various stereotypes of how blacks were viewed in this all-white group of dreamers could be very revealing if the person were willing to diminish his/her defensiveness.

Since the dream helper ceremony can be a very powerful technique for uncovering ordinarily hidden issues, it should not be attempted unless everyone is fully committed to openly dealing with whatever emotional issues are uncovered. The target person's problem should not be something trivial ('What sort of car should I buy?'); it should be some deep concern that one might discuss with a very trusted friend or counselor in the hope of reaching a better understanding of the emotionally challenging problem.

For one dream helper ceremony, the target woman's question, which she revealed after all the helpers' dreams had been reported, dealt with whether to enter some new, as yet undetermined, vocation. Almost everyone of the dream helpers reported dreams of extreme violence: wild animals were involved, someone was hit on the head with a hammer, etc. There were also several mother-daughter dreams with disturbing content; one had a mother duck and several drowned baby ducks. When I asked the target person why she thought there was so much violence in these dreams, and why the troubled mother-daughter relationships were portrayed, she broke down and confessed that her mother, who had been a psychiatric patient, had been quite violent and cruel to her when she was younger. Her mother had once

tried to drown her in a tub of boiling water, which helped explain the image of the drowned baby ducks. The subsequent group discussion suggested that maybe the target person needed to resolve this old issue with a therapist, before moving on to issues about a possible new vocation.

During one dream helper weekend, we had so many participants, that we divided up into two groups, with Henry leading one group and me leading the other. Each of the target persons was a female of about the same age, education, and socioeconomic status, however, the dreams of the two helper groups diverged widely. The dreams for target person A were highly appropriate for her, but did not apply to target person B, and the dreams for target person B were amazingly specific to her problem, but had minimal pertinence for target person A. It seemed as if each target person were a psychic magnet attracting only the dream filings of the helpers assigned to their magnetic field.

Some recent research involving a procedure adapted from the dream helper protocol was reported at the 2008 IASD conference in Montreal by researchers from Trent University in Ontario, Canada. Their paper was entitled: *Can Individuals Dream About the Personal Problems of Others*? (Smith, DeCicco & Moran, 2008). These researchers carried out two different experiments. In the first experiment, 12 students submitted two dreams apiece after looking at a target photo of an unknown woman. They tried to incubate information about the nature of any problems she might have. The target person was a female with breast cancer who was fearful that the cancer might be spreading to her leg. Among some of the dreams, was one in which a woman had a blank expression on her face and a knife in her right breast, and then the dreamer was in a hospital with the woman on a bed. Another dreamer reported a dream about a woman's arm that begins to smoke and turn black like a black plague eating her. A healer pulled something from the middle area around her breast.

In the second experiment, 66 students were involved and shown a photo of a woman and were told to have 2 dreams about her problem. The target woman had Multiple Sclerosis, her husband had one of his limbs severed in an accident and her son had been in a car accident. Among the dreams submitted, were several related to a woman with crippled hands, someone being hit by car, and some limbs being placed in a casket.

Special scales were constructed to detect salient features of each target woman's relevant problems. Four of these scales revealed results

that were statistically significant at a high level of confidence. A control condition was also attempted in which the students tried to dream about a photo of a woman that was computer generated but was not an actual living person. No significant patterns in dream content were found for this control condition.

I think the dream helper ceremony is a convenient way for an open-minded and interested person to directly observe psi in action and to experience the inner personal and collective power that results from such personal and shared dream awareness. "If the time ever comes when we all agree to use the formidable power of our dreaming mind as dream helpers for each other, we will witness a positive change in planetary consciousness greater than the negative change in planetary consciousness following the dropping of the first atomic bomb" (Van de Castle, 1994, p. 438).

Similar plaudits for the value of utilizing dreams to evolve our planetary consciousness were expressed by Jeremy Taylor, who has been working professionally with dreams for more than thirty years. He claims to have recorded nearly 12,000 of his own dreams in his personal dream journal and encountered an overall 'sample' of over 100,000 dreams in his work. These experiences have, "contributed to my conviction that all dreaming is deeply involved with the health and wholeness, not just of the individual dreamer, but the entire species" (Taylor, 2001, p. 196).

The Potential of Lucid Dreaming

In an article entitled, 'Proposals for future research' William C. Dement, remarked: "I would like to see future dream researchers attempt to develop trained introspectionists to give us somewhat more confident information about what goes on in the mind during sleep. There are some individuals who seem to be supremely good at recalling their dreams.... I think our major data about the dream world should come from those best able to describe it" (cited in Lairy & Salzarulo, 1975, p. 442).

An impressive introspectionist was the Marquis Hervey de Saint-Denys who, over a period of 20 years, recorded dreams from 1,946 nights in 22 notebooks and also drew many of his dreams. His dreaming exploits began at the age of 13 and within a year he found himself developing the ability to dream lucidly, i.e., being consciously aware that he was dreaming while he was in a dreaming state. His

book, published anonymously, under the title, *Dreams and the Means to Direct Them [How to Guide Them]* appeared in 1867, but a brief English version was not available until 1982 (Saint-Denys, 1982). His original book in French, contained 496 pages and six chapters on the opinions of others, proposed a number of concepts for understanding dreams that were in many regards similar to those later claimed by Freud. These concepts had been overlooked by Freud because he claimed that he had never been able to obtain a copy of the book. An historical perspective on this book is available in the *Lucidity 10th Anniversary Issue, 1981-1991* (pp. 311-317).

Saint-Denys became so fascinated by the possibilities of lucid dreams that he undertook a program of mental 'gymnastics' to develop his skills. After a period of about 15 months, he was able to experience a lucid dream on almost every night and he became a rather fearless individual in his dreams. When he decided to explore what would happen if he were to leap from the top story of a house in a dream, his immediate feeling was one of anxious curiosity before he leapt, but then he found himself in a crowd gathered around a dead man. When the body was being carried away on a stretcher, someone informed the dreamer that a man had thrown himself from the cathedral tower.

On another occasion, he dreamed that "my soul had left my body and then I was traveling through vast spaces with the rapidity of thought.... the idea came to me to visit the moon, and immediately I found myself there. I saw a volcanic terrain with extinct craters and other details, obviously reproduced from books and engravings I had read or seen, but singularly amplified and made more vivid by my imagination. I was well aware that I was dreaming, but I was by no means convinced that the dream was entirely false. The remarkable clarity of everything I saw gave rise to the thought that perhaps my soul had temporarily left its terrestrial prison, an occurrence there would be no more remarkable than so many other ministries of creation.... I immediately wished to return to Earth; I found myself back in my bedroom. For a moment I had this strange sensation of looking at my sleeping body, before taking possession of it again" (pp. 161-2).

Although it is true that Van Eden had published an article on lucid dreaming in 1913, it had been published in a source (*Proceedings of the Society for Psychical Research*) that was not widely recognized in scientific circles. The existence of such a possible phenomenon remained debated until it was confirmed by Keith Hearne using a combination of an EEG recording device and a method of ocular

signaling. That first laboratory lucid dream was recorded on April 12, 1975 in the Department of Psychology at Hull University. Alan Worsley was the individual who reported that first lucid dream in a REM monitored setting. Hearne later transferred his studies to Liverpool University and completed his PhD thesis in 1978, entitled *Lucid dreams: An electrophysiological and psychological study.*

In his autobiographical account, Worsley (1988, p. 44-48) claims, "I have had hundreds of lucid dreams in which I have done an experiment or made some observation. In sleep-laboratory work I have had 50 signal-verified lucid dreams... I achieved my first lucid dream by a deliberately developed technique, at about age 5. When I am dreaming lucidly, I sometimes believe that I am controlling one part of my brain with another part. The part I identify as my '*self*' seems to control to some extent the part which generates dream imagery. In my attempts while awake to understand what is happening, I have compared this process to using a computer with a display screen. In my television experiments I was able to watch a dream—so it seemed—on my dreamed television set. I could change the content using a knob to which I confidently attributed the magical property of 'channel-changing.... I have come to realize through consideration of my own dream observations that, like other skills such as driving a car or playing the piano which are initially practiced diligently with great effort and concentration, 'dreaming' is a learnable skill."

Hearne did not publish his results in any scientific publication at the time, as he had been hoping to develop some potential commercial applications for his findings. Stephen LaBerge, who was also a proficient lucid dreamer, claimed that he had recorded nearly nine hundred such experiences since 1977 (LaBerge, 1985, p. 9). He was unaware of Hearne's work and completed his doctoral dissertation on this topic at Stanford University in 1980. In 1985, he published his book entitled, *Lucid Dreaming,* describing several different areas of investigation that he had conducted. By performing a prearranged pattern of eye movements, the dreamer could indicate to the experimenter monitoring the EEG machine that he was in a lucid state. After a brief period of such signaling, LaBerge would wake the subject up and elicit his/her lucid report. LaBerge also demonstrated that it was possible to signal lucidity through Morse Code by sending a message using various bursts of short and long muscular contractions of his hand. LaBerge also invented a DreamLight: "It consists of a mask you wear over your eyes while you are sleeping, which picks up eye

movements with an infrared eye-movement detection device. When enough eye movements occur that the computer in the box 'believes' you are in REM sleep, the lights in the mask flash" (Laberge, 1988, p. 17). Use of this device enabled a higher percentage of lucid dreams to be detected and subsequently reported by the dreamer. A new era of scientific investigation was now possible in the area of lucid dreaming.

In 1981, Jayne Gackenbach completed her dissertation on lucid dreaming using subjects from the Association for Research and Enlightenment (Edgar Cayce) in Virginia Beach. She began the task of arranging a publishing venue for the numerous studies on lucidity that were springing up. In December 1981, Gackenbach published a *Lucidity Letter* that eventually became a quarterly newsletter. In 1985, the first journal issue of *Lucidity* was published. This journal provided a forum for individuals interested in various facets of lucid dreaming to exchange viewpoints about their experiences or theories. A collection of articles from previous issues was compiled in a commemorative book entitled, *Lucidity 10th Anniversary Issue, 1981-1991* (ISSN 0847-2688). A summary of the table of contents appears in Figure 1 to indicate the breadth of selected material that had previously appeared in the journal.

Part I: The Experience of Lucid Dreaming (14 articles)

Part II: Lucidity and Related States
- Section A-1: Altered States – Near Death Experiences (3 articles)
- Section A-2: Altered States – Out of Body Experiences (3 articles)
- Section B: Meditation (5 articles)
- Section C: Mystical and Divine Experiences (4 articles)

Part III: Empirical Work
- Section A: Induction (3 articles)
- Section B: The Sleep Laboratory (4 articles)
- Section C: Predisposing Factors and Individual Differences (5 articles)

Part IV: Theoretical Approaches (5 articles)

Part V: Historical Perspectives (10 articles)

Part VI: Applications
- Section A: Clinical Issues (10 articles)
- Section B: Other Applications (4 articles)

FIGURE 1.
A summary of the table of contents for the Lucidity 10th Anniversary Issue, 1981-1991

Articles pertaining to lucidity in recent years are available online at The Lucid Dreaming Exchange: www.dreaminglucid.com through the courtesy of Robert Waggoner. Another internet resource on various topics related to lucidity is a very active website, www.LD4all.com, maintained by PasQuale, a woman in Amsterdam. She has maintained this website for seven years and it has over 290,000 posts by more than 14,000 participants.

Several excellent books on lucid dreaming have been published. Among some of my favorites are: Ken Kelzer's (1987) *The Sun and the Shadow: My Experiments with Lucid Dreaming*; Gackenbach and LaBerge's (1988) *Conscious Mind, Sleeping Brain*; LaBerge and Rheingold's (1990) *Exploring the World of Lucid Dreaming*; and Robert Waggoner's recently published, *Lucid Dreaming: Gateway to the Inner Self.* Waggoner became aware of his ability to lucid dream when he was a young teenager and he has maintained an active dream journal for 30 years. He reports having experienced over 1,000 lucid dreams. In his book, he has chapters on such provocative topics as: *Healing yourself and others; Consciously connecting via telepathy; Forward-looking, precognitive lucid dreams; Mutual lucid dreaming* and *Interacting with the deceased.*

In *Entangled Minds*, Dean Radin (2006) comments, "One of the most surprising discoveries of modern physics is that objects aren't as separate as they may seem" (p. 1). He comments on how relationships extend through space and time, "these connections were predicted by quantum theory and were called, 'spooky action at a distance' by Albert Einstein... Scientists are now finding... the effects of microscopic entanglements, 'scale up' into our macroscopic world... Entangled connections... can persist over many miles... Some scientists suggest that the remarkable degree of coherence displayed in living systems might depend in some fundamental way on quantum effects like entanglement... Some even propose that the entire universe is a single, self-entangled object" (pp. 1-2).

I have been impressed with accounts that some lucid dreamers have provided which seem to reflect a view of the universe in which there is the overarching connection of linkage between time, space and matter implied in Radin's remarks. Here are a few examples. This dream was reported by Robert Waggoner in May 2003: "The dream begins in the empty dream space. As I stand in the glistening darkness, I notice something odd to my upper right. There hangs a shaft of glowing light—it seems a hexagonal (six-sided) shaft of intense, pale greenish,

glowing light about 6-8 inches across and four feet long. Semi-lucid now, I simply know that this shaft of light is 'precognitive information'. Suddenly the shaft of light comes slamming into my right temple. Like a painfully sweet energy being plunged into my mind, it comes pushing deeper and deeper. At this point—*bam, bam, bam, bam*—I experience four or five precognitive dreams seemingly in microseconds" (p. 202).

He wrote these dreams down as fast as possible and told his wife about this odd shaft of light containing precognitive information. He subsequently described the various precognized events, which occurred about three months later. This is how he described his subsequent reaction to these experiences: "When I realized the dream series had subsequently all come to pass, my mind reeled. How could three seemingly unrelated dream events come packaged in a shaft of light and then all come to pass months later in a twenty minute period?" (p. 202).

LaBerge and Rheingold (1990, p. 281) include this lucid dream report by 'P.K.' from San Francisco in *Exploring the World of Lucid Dreaming*:

> I decide at this point to visit the moon. I hold my hands out in front of me and fly upwards into the sky. Moving more and more rapidly, soon I sense a roundish shape behind my hands. I lower my hands, expecting to see the moon. The shock of what I see is very dramatic and startling: It is not the moon at all, but quite clearly it is the planet Earth! It is an exquisitely lovely vision, a gem glowing in soft greens and blues with swirling whites against a sable sky.
>
> ...I am floating in the midst of a vast, limitless darkness that is at the same time brilliant with countless stars, and very much alive. This aliveness is somehow almost audible: I feel I am 'hearing' with my entire being... This is an exquisitely wonderful place to be.
>
> Now I am beginning to move away from the stars and Earth, which become smaller and smaller until it disappears. Soon I am seeing entire solar systems and galaxies, moving and spinning harmoniously, growing smaller and smaller as they, too, gradually fade into the distance. I hover in space totally amazed. There is a profound sense of eternal energy everywhere.

> ...I ask, 'What's the meaning of the Universe?' ...The answer comes in a wholly unexpected form. Something is emerging from the darkness. It looks like some kind of living molecular model or mathematical equation—an extremely complex, three-dimensional network of fine lines glowing like neon lights. It's unfolding itself, multiplying, constantly changing, filling up the Universe with increasingly complex structures and interrelationships...
>
> It's as if I was seeing the invisible relationships connecting all things—the intimate molecular level superimposed over the vast and limitless Universe.

This is the dream of Fariba Bogzaran (1991, p. 169) who wanted to ask her higher self if she should carry out her graduate research about lucidity and the Divine. After some initial imagery with a hawk and a hummingbird, she reports:

> Suddenly, I see a dot of purple green color expanding in the sky. It keeps getting bigger, filling the landscape and moving towards me, changing into different rings of color. The space in which this is occurring is so vast that it is beyond my visual ability. As the rings come closer they change into particles of light moving extremely fast, creating lines that cover everything, everywhere. Strong energy starts to move inside me and my body is changing its form into these particles of light. I don't see my body any longer but I know I am still there.
>
> These particles slowly change into a night sky with stars. The sky moves like a movie screen from left to right with different planets on display. As the planets move, they change into different colored bubbles. There is no landscape anymore, just a vast space with giant air bubbles of different colors, with light shining through them. The bubbles transform into halos covering the infinite and vast space. I know that I am becoming a witness to different layers of the universe.

These awesome, mystical, lucid dreams strongly suggest that the dreamer is being treated to an illustrated slide show revealing the various layers of interconnection that is posited by theories arising from the latest conceptualizations of how the universe is being permeated by some form of consciousness.

After having been involved in many IASD psi dreaming contests over an extended period of time, I have noted that experienced lucid dreamers are much more likely to obtain surprisingly accurate dream matches with the pictures in the target pool. I can recall several contests with participants who are experienced lucid dreamers came up with surprisingly detailed 'hits'. Their psychic talents are not just limited to our conference contests. Recently Robert Waggonner sent me an urgent email requesting that I get a check up from my doctor because he felt that I was experiencing some health challenge. Since I had great respect for his psychic talents because of the numerous hits he had obtained in our contests, I followed up on his urgent message and made an appointment with my doctor. After conducting some tests, he concluded that my blood pressure measurements suggested that a new regime of medication needed to be implemented. I had been feeling some periods of light-headedness, but had not taken any initiative to contact my physician about these symptoms, until I received Robert's strongly worded email. I might mention that I rarely receive emails from Robert and had never received one before that was directed to my physical health.

How common are lucid dreams? After conducting several surveys on the topic of how common lucid dreaming might be, Gackenbach (1991) reported, "the best estimate of prevalence is that about 58% of the population have experienced a lucid dream at least once in their lifetime, while about 21% report it with some frequency (one or more per month). Additionally, 13% of dreams recalled in the morning after and recorded in dream diaries are likely to be lucid" (p. 233).

There could be many potential applications for the use of lucid dreams. Some have characterized 'remote viewing' as a type of waking lucid dream. A dreamer could attempt to go to some specific date in the future and look around to see what was going on there at that specific time and place. Since there have been reports of dreaming together in a mutual fashion (Magallón & Shor, 1990), this could provide an opportunity to compare the descriptions reported by each of the dreaming partners as to what they saw at the location they jointly visited. There have been several reports of rather remarkable healing taking place in lucid dreams; it would be interesting to see if a lucid dreamer could demonstrate some specifically designated physical changes, such as changes in body temperature or brain waves in their lucid dream.

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FIELD INVESTIGATIONS OF HAUNTINGS AND POLTERGEISTS

FÁTIMA REGINA MACHADO

Hauntings and poltergeist are very intricate anomalous phenomena reported in all cultures across the centuries (Goss, 1979). Experiencing, reading or listening to reports of these occurrences can lead us to two kinds of attitudes towards them: either we consider them a mystery or we consider them an enigma. Those who consider them a mystery will in general adopt a supernaturalist approach that seems to explain the phenomena but instead just describes the experiences and their underlying processes as if they were already completely known. Mostly religious, religious-like or pseudo-scientific doctrines are referenced. On the other hand, those who consider haunting and poltergeist reports an enigma are not satisfied with the ready and easily found supernaturalist explanations. Instead, such individuals are eager to solve the enigma. I believe scientific researchers are—or should be—members of the latter group, that is, scientific researchers should be the kind of person who is looking for patterns, clues and permanent answers to the enigma. And we cannot forget that being scientific means being skeptical enough to avoid accepting any 'ready' or 'easy' explanation and flexible enough to avoid imposing dogmatic beliefs about such amazing phenomena or experiences.

Different cultural or social groups often use distinct explanations for haunting and poltergeist occurrences. In Brazil, for instance, places considered haunted or the known sites of poltergeist-like events are popularly called 'mal-assombrados'. Especially in the countryside, hauntings and poltergeists may be attributed to the action of folkloric characters who act according to the beliefs of the region where they were originated. Several physical occurrences similar to those reported in poltergeist cases are attributed to Saci Pererê, a kind of Brazilian goblin who lives in the forests and loves bothering people and animals on the farms. Other spectral figures are thought to be people who have not followed the laws of the Catholic Church and were cursed, condemned to walk endlessly on nights when the moon is full (e.g., Lobisomem and Mula-Sem-Cabeça, a priest and his lover, respectively). Others (e.g., Mão de Cabelo, Alma de Gato) are entities whose forms are not exactly defined and whose purpose is to scare

people, especially children who don't behave well (Cascudo, 1947/2002). In Brazilian urban areas, interpretations of poltergeist and haunting occurrences are more often related to the belief in the existence of spirits of disembodied people who are acting out or who have just come back from 'the other world' to leave a message; Kardecian spiritism and African-Brazilian mediumistic religions promote this view. Another interpretation is offered by Pentecostal Churches who believe poltergeists and hauntings are related to the supposed action of the devil in our world. So, in each culture or group we can find specific explanations and very particular agents put forth as the cause of these anomalous occurrences (Machado, 2003).

Many representatives of mainstream science commonly consider reports of hauntings and poltergeists to be fictional accounts, psychopathological symptoms or the result of misinterpretation of reality due to ignorance about naturally occurring psychological, chemical or physical processes. But fortunately there are other scientists and researchers who pay attention to human experiences, and consider them crucial for the construction and revelation of reality. So there are researchers who have paid attention to what people have reported for centuries and instead of denying it or adopting a ready answer to it, they are working to try to solve the enigma of so-called anomalous experiences, of which hauntings and poltergeists form a part. Briefly, then, we can say that explanations for hauntings and poltergeists range from postulating that they are the result of:

(a) fraud (e.g., Kurtz, 1985; Randi, 1995);

(b) misinterpretation of reality (e.g., Gardner, 1985);

(c) psychopathology (e.g., Zusne & Jones, 1982);

(d) supernatural causes (e.g., Andrade, 1988; Kardec, 1857/1944a);

(e) anomalous environmental interactions such as possible psychokinetic and/or extrasensory experiences (e.g., Bender, 1976; Rogo, 1986; Roll, 1978; Houran & Lange, 2001; Mackenzie, 1982; among many others).

Poltergeist and haunting case collections show that these kinds of anomalous experiences have patterns that give us clues about their connection to the living human beings involved in the cases. Through observation of these patterns it has been possible to design

experimental studies in order to try to discover objectively what mechanism is involved in the occurrences. Among these studies are psychological and socio-cultural profiling of the experiencers, and other possible interdisciplinary routes into the interpretation of such personal experiences. Field investigation becomes very important for the development of experimental research and laboratory experimentation has begun to provide a controlled setting in which to check elements of those phenomena that occur spontaneously in daily life. So, we cannot close our eyes to what field investigations have shown us and have still to show to us. We cannot think that we already know all the features of the phenomena. We have to be conscious that we still have something to learn because we certainly do not have all the answers as yet.

The purpose of this paper is to point out what we already know about hauntings and poltergeists through field investigations, what are the main questions that remain without answers, and what may be done in the future to improve research procedures and to elevate the discussion of the data that has been collected so far. First of all, however, it is important to conceptualize the terms *haunting* and *poltergeist* more clearly and to review how such terms have been used by researchers.

Reviewing the Concepts[1]

In general terms, according to the patterns observed from field investigations, the fundamental difference between hauntings and poltergeists is that poltergeists are directly related to people and hauntings are directly related to places (Carrington & Fodor, 1953). However we could say that this is a didactical distinction because we cannot precisely classify all reported cases. To illustrate this I could say that, when I was selecting cases for my doctoral dissertation in which I analyzed the function and meaning of poltergeists (Machado, 2003), I faced the following problem: some reports that seemed to me to refer clearly to typical poltergeist cases were classified by the authors/researchers as haunting cases or as a mix of poltergeist and

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[1] This material is strongly based on the comments I have presented in my doctoral dissertation (Machado, 2003), in the third chapter of which I talk about the criteria to select the poltergeist cases that would compose the *corpus* to be analyzed.

haunting phenomena. Many libraries, such as those of the Society for Psychical Research and the College of Psychic Studies include poltergeists and hauntings in the same catalog entry. In fact, there is no consensus among researchers about how to differentiate these phenomena one from the other. The frontiers between hauntings and poltergeists seem to have the same distorted contour that extrasensory and psychokinetic experiences seem to have in general, which is not surprising considering that ESP and PK must form the base of haunting and poltergeist experiences.

Etymologically, *haunting* refers to the repeated apparition or sense of presence of something in a specific locale. Hauntings may be visual (in the majority of the reported cases), auditory or olfactory. In the visual cases, for example, people report having seen a ghost doing the same things in the same place but at different times. Haunting phenomena has been reported as persisting over years, decades, or even centuries.

Poltergeists, on the other hand, can last 'just' some hours, days, months or years, although reports of phenomena that persist for years are quite uncommon. Poltergeists are intimately related to specific individuals, a point that will be explored in more detail later when the review of literature is presented. The word *poltergeist* has a German origin: *polter* means playful or noisy, and *geist* means spirit or mind. Popularly, people seem to prefer adopting the meaning *spirit* instead of *mind* in this case, because it corroborates the belief that *poltergeist* phenomena are caused by an external agent, not by people who live or work together, for instance. Poltergeist phenomena consist of physical occurrences that seem not to obey established physical laws. Based on the reports of field investigations, we can list the following phenomena which are reported to occur spontaneously and recurrently: movement or breakage of objects; a fall of stones inside a house or building, damaging or not damaging the building itself; appearance of fire or water; problems with electrical appliances; lights turning on and off; sudden changes of temperature; currents of air where there is no open window or door, or any other device that could provoke such a current; appearance of excrement or soil in food; and/or other weird observable physical events related to personal belongings or specific objects. The term 'poltergeist' was popularized during the Protestant Reformation especially by Martin Luther who used the term consistently to refer to phenomena similar to those listed above. People believed—and many

still believe—that such occurrences were provoked by the devil or by disembodied spirits (Machado, 2003; Roll, 1977).

The term was re-introduced in England by the writer Catherine Crowe, with the publication of her classic text *The Nightside of Nature*, in 1848, and since then it was used by psychical researchers at the end of the 19th century, when the term was popularized by Frank Podmore (Goss, 1979). Ironically Podmore, who was a great writer, popularized the use of the term poltergeist through his works related to this topic in which he made it clear that he did not believe that such events were supernatural nor that they resulted from some extra-motor origin. Instead he believed that they were the fruit of frauds, misinterpretations or hallucinations (Podmore, 1896a, 1986b).

Among academic researchers, such phenomena have formally received another term: 'recurrent spontaneous psychokinesis' (RSPK). The term was coined based on evidence of the involvement of living agents in the process of the anomalous physical events reported. As I pointed out in another article:

> ...psychokinesis refers to a mental action on the environment (from Greek, psyche = mind; kinesis = movement); recurrent refers to the fact that the events are typically repeated for an undefined period of time; and spontaneous because, up to now, it has not been possible to discover any way either to control or predict the occurrences.
>
> (Machado, 2001, p. 228)

The term RSPK first appeared in the report of the Seaford Case published by J. Gaither Pratt and William G. Roll in the *Journal of Parapsychology* in 1958, but it was coined by Roll and elaborated on in his later work. Even with this more precise, less theory-laden term in the literature, the label poltergeist is still more frequently used by researchers, as can be seen in specialized literature on the topic.

It is common to consider poltergeists as a kind of haunting case due to the belief that poltergeist events are caused by disembodied spirits or entities. The term 'infestation' is also used to describe both phenomena, due to the interpretation that specific locations may be infested by spirits or entities responsible for poltergeist and/or haunting events.

In addition to differentiate between hauntings and poltergeists based on the connection of the reported events to places or people, there is another criterion by which to distinguish them, although, in my opinion, it is very weak: that is, the type of occurrences. For example,

anomalous physical events that do not involve apparitions of ghosts would be characterized as poltergeist phenomena and anomalous physical events that did include ghostly apparitions would then be considered a haunting case. According to this criterion some cases are designated as of the haunt-RSPK type, which means that such cases would be composed of ghostly apparitions as well as objective anomalous physical events of the poltergeist type. (e.g., Roll & Tringale, 1982)

Another approach that has been used to differentiate hauntings and poltergeists was suggested by Charles Tart:

> Haunting and poltergeist cases are of particular interest insofar as they seem to share these apparent (and spectacular) PK phenomena. But hauntings are traditionally associated with the belief that some aspect of a human personality which has survived death is responsible for them, while poltergeist cases seem to be generally associated to the living agent.
>
> (Tart, 1965: 190)

This perspective is very controversial because it touches on the delicate question of survival after death: hauntings would refer to that part of human beings that might survive physical death and might reveal or bring back information from the past. The ESP hypothesis, however, provides a possible naturalistic explanation for hauntings. That is, living human beings could have the capacity of capturing information from the past, translating it into materialized images, sounds or smells and thus bringing the content of the information to the consciousness in some hallucinatory way, as has been proposed by both G. N. M. Tyrrell (1942/1973) and Louisa Rhine (1965).

From my point of view, hauntings and poltergeists can present similar events. I agree that what differentiates them is the evidence for connection of the anomalous events to living people—which, if present, would fit the case into the poltergeist category—or the evidence for connection to specific places—which, if present, would fit the case into the haunting category. In any case, as I have said before, this is a 'didactical' choice that is important when it serves to orient research and organize reports.

Terminology and concepts adopted to refer to the experiences are not just minor details. But it cannot be forgotten that defining terminology does not mean having ready answers to cases or experiences before a detailed case investigation is done. In Brazil, it is

a serious problem, this notion that having a term means having an explanation. I bet that in other parts of the world this confusion also happens. It is important to consider the patterns, but it is also extremely important to be open to what spontaneous cases themselves have to show to us. If we do not do approach the features of the cases with an eye uncompromised by an easy term, we jeopardize the possibility of learning more from field investigations.

What Have We Learned About Haunting Features Up To Now?

Hauntings reported since Antiquity show that experiences involving ghosts and apparitions are a part of everyday human life (Finucane, 2001). Such bizarre experiences have supported—and continue to support—religious beliefs and doctrines. The presence of hauntings in everyday life is also reflected in cultural constructions in entertainment such as in film and television, for example (Edwards, 2001).

Only in the 19th century did scientific research of haunting phenomena start to be organized. The foundation of the Society for Psychical Research (SPR) in London in 1882 formally marked the beginning of organized investigations, but some independent researchers, such as William Crookes, William Barrett, Frederic Myers, among others, had already dedicated themselves to the study of haunting and apparitional cases and other kinds of phenomena such as those surrounding so-called mediums (Beloff, 1993; Fantoni, 1981) The SPR began to establish protocols for field investigation and experimental studies, the results of which were disseminated through specialized publications, research meetings and seminars (Broughton, 1991).

Two of the most important contributions of the SPR in that period were two surveys of spontaneous cases published in 1886 and 1894: the first resulted in the two-volume *Phantasms of the Living* by Gurney, Myers and Podmore (1896/1970), which focused on apparitions of the living; the second, called the *Census of Hallucinations* (Sidgwick, Johnson, Myers & Sidgwick, 1894), "focused on cases in which a dying or deceased person manifest[ed] to a percipient" (McClenon, 2001, p. 64). In the Census 9.9% of the respondents (1,684 out of 17,000 people) reported that type of hallucination.

Since the SPR was founded, other societies, foundations and research groups have been established so as to investigate formally psi phenomena in general. These newer institutions have paid at least some

attention to spontaneous cases and conducted or supported field investigations. As examples, we could mention the American Society for Psychical Research (ASPR) founded in 1885, the Institut Métapsychique International (IMI) founded in 1919, the Institut für Grenzgebiete der Psychologie und Psychohygiene (IGPP) founded in 1950, the Parapsychology Foundation (PF), founded in 1951, and the Foundation for Research on the Nature of Man (FRNM), today the Rhine Research Center, founded in 1965.

More recent surveys have shown that a good portion of the general population reports have had a haunting experience at least once (Fox, 1992; Greeley, 1975; Haraldsson, 1985; Palmer, 1979). What follows are some examples of the percentages of people who have reported having had contact with dead. In the USA 42% of the respondents to a 1984 national survey said 'yes' to a question about having had contact with dead; a national survey conducted in 1990 found that 9% reported they had seen or been in the presence of a ghost (Gallup & Newport, 1991), and 14% reported they had been in a haunted house (McClenon, 2001). Ten percent of the respondents of a survey conducted with students of Tsukuba University in Japan and 40% of the students surveyed at three colleges in the People's Republic of China have admitted they have had contact with dead (McClenon, 1994). In a survey conducted at a university in São Paulo in Brazil, 62% of a sample of 181 students reported they had seen an apparition; 14% of them reported they had been in a house they believed was haunted and 17% reported they had had a poltergeist experience (Zangari & Machado, 1996). The prevalence of such experiences in daily life makes field investigation worthwhile. However, even though haunting cases happen more commonly than many people imagine, it is not easy to conduct haunting field investigations especially because it is difficult to separate subjective impressions from objective phenomena. This kind of investigation depends strongly on human testimony and we all know how easily testimony is compromised by personal and cultural beliefs. However, the fallibility of testimony has not impeded important objective observations that have helped to develop and improve experimental studies on this topic (e.g., Radin & Rebman, 1996).

If we confine ourselves to what has been observed in field investigations of hauntings, and set aside what has been popularized by the movies, ghosts are not fluid floating creatures that terrify children and adults. Ghosts can seem very solid and real, have a three-dimensional form, can be seen in the mirror, not pass through walls,

and, in most cases, seem to ignore those who are observing them, just repeating the same acts over and over. If the haunting is confined to sounds and smells, they are repeatedly perceived by different people at specific moments during the day or at night.

There is a notable difference between what we call apparitions and what is classically called hauntings. Apparitions, unlike classical hauntings described above, are perceived by specific individuals and the sounds heard or, more commonly, the images seen are very familiar to the experiencer (Mackenzie, 1982). Apparitions of relatives or friends usually announce a death, or the arrival of the person depicted in the near future, or even help to get information about documents that were considered lost but were important for the person or persons who see the apparition. So, apparitions can be experienced collectively or can be a personal experience that only happens once (Machado & Zangari, 1996; Rhine, 1965).

The belief that apparitions and especially ghosts pass through walls is probably due to the fact that spectral figures can disappear by fading out, giving the impression that they have gone elsewhere without 'using the door'. Reports say that ghosts and apparitions seem to be very real especially because they behave as if they were a living person in the environment, looking through windows, using the doors to get in or to get out of rooms, climbing up or going down stairs etc., as reported, for example, in the Cheltenham case investigation (Morton, 1892).

Based on the reports sent to the Society for Psychical Research, Tyrrell (1942/1973) classified apparitions into four categories. His work was later reviewed by Andrew Mackenzie (1982) and corroborated by other researchers and their case collections:

(1) *experimental apparitions*, apparitions of persons who were purposely trying to appear in front of a distant person, a phenomenon that can be related to out-of-body experiences (Hart & Ella, 1953-56; Green, 1968; Irwin, 1994);

(2) *crisis-apparitions*, in which the person depicted by the apparition is someone who is in the midst of a crisis and may be asking for help (Rhine, 1948);

(3) *apparitions after death*, that is, an apparition of a person who has already died (Rhine, 1948);

(4) *ghost apparitions*, that is apparitions of a spectral figure seen at the same location on different occasions (Irwin, 1994).

Evans (2001) presented another term to classify the variety of ghosts that are reported: (a) *apparent*, referring to the individual whose ghost is ostensibly perceived by the witness; (b) *haunter*, referring to a ghost perceived as haunting a locality or, more rarely, a person; (c) *image*, referring to what the percipient perceives; (d) *projectee*, referring to the individual who is ostensibly projected in astral projection and is seen in another place by witnesses; and (e) *revenant*, referring to a ghost who is ostensibly returning from the dead. Such a classification is very controversial especially because it deals with the ontological problem of whether a ghost has an objective existence and also implies the survival question.

From the end of the 1940s to the beginning of the 1950s, Louisa Rhine selected, analyzed and classified 996 spontaneous case reports among those that had been sent to the Duke University Parapsychology Laboratory. She published her case collection in 1965, in a book titled *Hidden Channels of the Mind*, in which she compiled and analyzed spontaneous case reports. Louisa Rhine's work was instrumental especially for investigations on apparitions. Cases described served as a guide to the classification of different kinds of experiences in which extrasensory perception—and even psychokinetic—abilities seemed to be operating in daily life. So, Louisa Rhine did not intend to prove the existence of psi with her phenomenological study, but her objective was to detect features of spontaneous psi experiences. From this work we learned that: (a) emotional connection between people facilitates psi experiences, especially extrasensory ones; (b) relaxation, sleep or monotonous motor activities facilitate ESP experiences, which have high incidence in dreams; (c) crisis situations, especially tragic ones are the main context in which the reported cases take place; (d) personality influences the incidence of extrasensory experiences; (e) the four forms in which information gathered through extrasensory means are manifested are realistic dreams, non-realistic dreams, hallucinatory and intuitive experiences; and (f) apparently there is no significant distinction between the number of men and women who experience psi, even though it appears that more women submit reports.

For almost three decades (1920s-1940s) parapsychological studies were especially devoted to laboratory research. The case collection mentioned above was really important because it has reinforced the importance of field investigation. Field investigations of hauntings and apparitions continue to be conducted in the forms of observational research, surveys and case investigations *in locu*, case collections and

analysis of reported cases (e.g., Alvarado & Zingrone, 1995; Hufford, 2001; Haight, 1979; Haraldsson, 2006; Haraldsson & Houtkooper, 1991; Houran & Range, 2001; Maher, 2000; Maher & Hansen, 1995; McClenon, 1981; McHarg, 1973; Moss & Schmeidler, 1968; Rogo, 1978; Roll & Brittain, 1986; Roll, Maher & Brown, 1992; Roll & Nichols, 1999; Rosenberg, 1974). Field investigations *in locu* in particular have been improving with the utilization of newly developed technological devices that measure physical alterations at the places where haunting phenomena are said to occur in order to try to answer the question 'Do apparitions or ghosts have an objective nature?' New data has been collected and they have brought new information about the objective aspects of haunting phenomena. This latter point will be explored in more detail in another section, in which objective aspects of hauntings investigated in the field—including poltergeist-like occurrences—are presented.

What Have We Learned About Poltergeist Features Up To Now?

Poltergeists have also been reported since Antiquity. Carrington and Fodor (1951/1953) presented a case collection that included reports of poltergeist occurrences going back to 355 B.C. In the specialized and non-specialized literature it is possible to find several reports about mystics, saints and sorcery victims who were involved with poltergeist-like occurrences. Reports were published by such early members of the SPR as Barrett (1911), Lang (1903), and Podmore (1896a, 1896b). Reports were also written by ASPR members among them Hyslop (1913), and by other researchers such as Bayless (1967, 1984), Bender (1969, 1976), Betty (1984, 1985, 1986), Bononcini and Martelli (1983), Cox (1961), Eeman (1986), Eisler (1975), Flammarion (1924/1980); Fodor (1958, 1959), Gregory (1982), Hasting (1978), Lombroso (1906, 1909); Nichols (2000), Price (1926, 1933, 1945), Rogo (1974, 1979, 1986, 1979/1995) and Roll (1968, 1972, 1977, 1978), Pratt & Roll (1971), among others. In Brazil, we do not have a solid tradition of poltergeist phenomena research, but there are a few who are interested in the subject and have written about it and/or presented and analyzed some cases, among them Andrade (1988), Carvalho (1991, 1992), Friderichs (1980), Lima (1994), Machado (2001), Machado & Zangari (1995, 1998, 2000), and Tinoco (1989).

Until the 18th century, people believed that poltergeists were provoked by spiritual entities, fairies, witches and demons. From the

17th century on there were some reports of a few elementary field investigations (Alvarado, 1983). By that time, very little importance was given to the physical occurrences themselves. The cases reported were used to discuss religious questions, so what mattered was combating sorcery, diabolic possession and people's involvement with disembodied spirits.

In the 18th and 19th century, the decline of witchcraft, the development of mesmerism, and the expansion of the Spiritist doctrine proposed by Allan Kardec in France, among other factors, resulted in the organization of a more scientific approach to poltergeists—even though religious and superstitious explanations for the phenomena were always available (Machado, 1996; Martínez-Taboas, 1993). Various cases were described and published during the period. In the 19th century, the most famous case is the Fox Family case in 1848 (Lewis, 1848), although the classification of this as a poltergeist case is controversial. The Fox Family case was fundamental for the organized establishment of Spiritualist and Spiritist practices and doctrines. The case and others like it were also investigated by scientific committees who made systematic evaluations and conducted interviews about the weird physical phenomena that were reported. Researchers have spent days in the houses where the physical events were said to occur to try to observe them and to control the environment to the extent that fraud could be detected or ruled out. So, in the 19th century we had two kinds of interpretations of poltergeist phenomena: the naturalist and the supernaturalist. Interpretations that could be considered supernaturalist, especially those arising from Spiritism or Spiritualism, attributed poltergeist occurrences to the spirits of dead people or to non-human elemental beings (Flammarion, 1924/1980). On the other hand, naturalist interpretations were based on the scientific method and sought to find natural explanations for poltergeist phenomena (Fantoni, 1981).

For some 19th and early 20th century researchers, poltergeists were nothing more than fraud. Hyslop (1913), for example, proposed that poltergeists could result from unconscious human activity during a period of dissociation that could lead someone to commit fraud without their conscious awareness. In field investigations, however, some researchers observed that even though many cases were found to have fraudulent elements, there were at least some cases that seemed to be the result of anomalous environment interactions. Theories were proposed to try to explain such anomalous interactions and

experimental studies were planned to test whether mind could really influence matter.

Poltergeist field investigations have shown that poltergeist occurrences seem to be related especially to the presence of a specific person who lives, works or frequents the locale of the events. Such a person has been called the 'epicenter' of the case as if he/she was the center of the turmoil. It is really not good manners to refer to the focus person in this way. The term epicenter has a very negative meaning: people who are labeled in this way can feel socially stigmatized. David Hess (1988) prefers to call the central person of these cases the 'focus person', but the majority of researchers in this area have not adopted the term. Since the 1990s, the term most commonly used to denote the central person in a poltergeist case is 'agent'. This term expresses more clearly what seems to be the participation of an individual in the occurrences, instead of characterizing the agent as a victim as the term Hess suggested does. In my doctoral dissertation, I used the term 'protagonist' instead of agent, once I worked on the reports as narratives and considered the occurrences and their context as elements in a history. I was looking for coherence in this history through the connection of meaning that was brought to light by the signs inherent in the reports and the objective facts observed by their witnesses (Machado, 2003). Another reason I preferred the term protagonist instead of agent was because I considered poltergeists to be experiential facts, and the term protagonist is more neutral than agent, since I was not discussing the reality of the psychokinetic phenomena or the role of any kind of physical force that might be involved in the occurrences. This approach has helped me to demonstrate that fraudulent and genuine cases follow the same semiotic logic, a finding that emphasizes the importance of investigating and reporting cases even if they are fraudulent, because they can help us to understand the key that unlocks the utility of this kind of communication—that is, considering the poltergeist manifestation as a symbolic or metaphorical language allows us to view its features as an alternative for verbal expression (Machado, 2003).

Because of the observed correlation between the presence of living human beings and poltergeist occurrences, the 20th century has been marked by psycho-dynamic speculations and interpretations of poltergeist phenomena based on the tenets of psychology and psychoanalysis. This shift in emphasis has influenced field investigation approach and procedures in a remarkable way.

Carrington (1922) has proposed that poltergeists may be produced by a kind of energy that irradiated from the body of the agent during adolescence. He postulated that poltergeist occurrences may be connected to the expression or liberation of sexual or hormonal forces. This is an interesting hypothesis, but field investigation reports and case collections show that individuals at other stages of life than adolescence can be poltergeist agents, even though there does seem to be a tendency for a higher incidence of adolescents at the center of such disturbances. As McClennon (2001: 65) has summarized:

> William G. Roll (1977) analyzed texts describing 116 poltergeist cases.... Of the 116 cases, 92 seemed associated with a particular person (occasionally two persons).... Of the 92 'person-centered' cases, 56 involved females and 36 males. Ages of poltergeist 'agent' (the person around whom the activities seemed to occur) ranged from 8 to 78. There was no significant difference in average or median age of agents from era to era (female average = 15; male average = 17; median age = 13).

A tendency cannot be considered a rule, however. Unfortunately in Brazil, for instance, there are some so-called poltergeist investigators who arrive at the site where poltergeist phenomena are said to be occurring with ready answers. They look immediately for an adolescent in the group of experients and without careful investigation identify the poor guy as the one responsible for the occurrences—although generally the poor guy is a girl because it is believed that women are more likely to provoke this kind of manifestation. Such a quick judgement can be disastrous for the adolescent and for the whole family. And it can 'spoil' the case to the extent that the possibility of a serious and fruitful investigation is wasted (Machado & Zangari, 2000).

In the 1930s, poltergeist case investigations started to include testing supposed poltergeist agents for psychic ability. No significant results were found (Alvarado, 1983). Researchers also started to do physiological studies to verify whether the supposed agents had any kind of organic problems, but again nothing significant was found. By that time parapsychological research in general was focused on experimental studies and spontaneous case investigations were seen as being less valuable to the field as a whole than experiments.

In the report of a case investigation, Nandor Fodor (1948) made it clear that by that time, the person who would later be called an agent was then considered to be a victim of a kind of undesirable guest, not

exactly independent of the focus person, but rather something produced by a psychopathology. From a psychoanalytic point of view, and referring to a case he was investigating, Fodor (1959, p. 92) said:

> ... [that this was the] first poltergeist case in which, through the psychoanalytic approach, the poltergeist was banished and the victim's health and sanity were saved.

In the 1920s, psychoanalysis had already been used in a very rudimentary way by Countess Wassilko in her investigation of the Zugun Case (Mulacz, 1999; Rosenbusch, 1927). But in contrast to Fodor, Countess Wassilko was interested in discovering the causes of phenomena that occurred with or were provoked by Eleonore Zugun. She was not interested in extinguishing them, as Fodor was reported to have done.

In 1988, David Hess presented a paper at the 31st Parapsychological Association Convention in Montreal, Canada, in which he analyzed the efficiency of interventions used to stop poltergeist occurrences. Even though he considered poltergeist and haunting phenomena to be idioms of suffering, anxiety and affliction, Hess criticized the action of Fodor as a psychoanalyst in the case that occurred in 1938 because, as Hess (1988, p. 279) said:

> ...to some extent Fodor may have short-circuited the interpretative process by telling the patient what the disturbances meant rather than let her work this out by free association.... in this case he operated more as an oracle than an interpreter, and there is no evidence that he worked with her associations to the phenomena. Nevertheless, his canny interpretation of the meaning of the six-month period preceding the onset showed that he was able to help the woman interpret the meaning of at least one aspect of her affliction, and in relating the disturbances to her inner psychological conflicts he was able to bring about an end to the disturbances.

This bring us to an ethical question that field researchers have to face: if you have a poltergeist case to investigate, do you hope that the physical events last long enough to be analyzed or do you try to help the people involved calm down and—as we already know given the pattern of past cases—probably make the occurrences stop?

In the 1940s, John Layard (1944) proposed formally that poltergeists could provide "a covert form of release, and thus curative

function, for repressed conflicts" (cited in Rush, 1987, p. 63). Still in the 1940s, two events enhanced the interest of researchers in poltergeist field investigations: (1) the publication of significant results from psychokinetic dice experiments that suggested the possibility of mental influence over matter (Rhine & Rhine, 1943); and (2) the interest of the Duke University Parapsychology Laboratory in spontaneous case studies as sources of insight for the design of future experiments, as mentioned previously. The hypothesis of living human involvement in the production of poltergeist occurrences gained force. And it became so strong that it has become almost a ready answer to future cases.

Renewal of interest in poltergeist cases especially in the 1950s has contributed to the development of a protocol for field investigation. Diagrams of the buildings where the disturbances occurred were done, the trajectory of objects or the places affected were marked and measured, reports on the position of each person in the house at the time of occurrences happened were made, psychological tests were administered to the agents and eventually to those with whom they lived (Roll, 1969, 1970). These methodologies allowed researchers to uncover the focusing effect: that is, in poltergeist cases, occurrences can be focused on "particular objects, on types of objects, and on areas, such as a room or a shelf" (Roll, 1977, p. 390). This new era of poltergeist investigation was inaugurated by the famous Seaford case investigated in 1958 by J. Gaither Pratt and William Roll.

Roll was the first researcher who presented systematic reports of results of psychological tests—especially projective ones, such as the Rorschach and the Thematic Apperception Test (TAT)—given to supposed poltergeist agents and to other people involved in the cases he investigated (Alvarado, 1993). The results of the tests, confirmed by Hastings (1978) among other researchers, showed that the poltergeist or RSPK agents were unable to tolerate frustration, had almost no ability to face daily problems efficiently, repressed both hostility and aggressiveness, and had difficulty expressing these negative emotions verbally. The use of psychological tests was reinforced by the psychopathological approach to poltergeist cases and vice-versa. This approach to poltergeist cases was established, a position that arose out of the work of Fodor (1948, 1958), Layard (1944) and Owen (1964), among others. However, only rarely were poltergeists related to severe pathological syndromes such as schizophrenia, for example. The hypothesis that poltergeist events functioned as the exteriorization of inner problems with the relief and expression of tensions became even

stronger because field investigation reports presented evidence that the supposed agents were passing through difficult moments in their lives at the time of the poltergeist occurrences. In the case report on the Seaford poltergeist, Roll began systematically to use the term RSPK to denote poltergeist cases.

In the 1970s and 1980s, Rogo emphasized the possibility of adopting therapeutic approaches to stop poltergeist manifestations such as verbalization of tensions as in a group therapy (e.g., Rogo 1974). He proposed that the agent of a poltergeist case has a collective nature, by which he meant that the manifestations were the fruit of a context created by a group, not by a single person (Rogo, 1979, 1982). Rogo was also open to the possibility that spiritual entities could provoke poltergeists (Rogo, 1979/1995).

Before Rogo, Stevenson (1972) had already proposed the classification of poltergeist manifestations into two categories: those provoked by human agents and those provoked by disembodied agents. These ideas were reinforced by a few field investigations in which the researchers could not identify an agent or central person (Cornell & Gauld, 1960; Osis & McCormick, 1982; Pierce, 1973; Stevenson, 1972) or when the supposed agent or focus person was absent (Roll, 1970, 1976) during an occurrence. It is possible that these cases need more investigation before we can talk about the non-existence of a living agent. And even if there is some evidence of the effect of distance on the magnitude of anomalous physical events that occur with certain objects closer to or farther from the supposed agent, it is important to remember that psi does not seem to obey known physical laws. So, we cannot predict how psychokinesis would work in every poltergeist case.

The psychodynamic model of poltergeists has persisted until today even though it has been criticized (Alvarado, 1993; Martínez-Taboas, 1977; Martínez-Taboas & Alvarado, 1981). The criticisms are due to doubts about the fragility of projective test results (Gittelman-Klein, 1978; O'Leary & Johnson, 1979).

In addition to psychological tests, Roll also included physiological tests of the agents in his field investigations in order to determine whether physiological differences could explain the fact that some people become poltergeist agents and some do not. His studies in this sense are especially related to disturbances in the central nervous system, more specifically to epilepsy, something that has also been investigated by other researchers (McHarg, 1973; Pratt & Palmer,

1976; Roll, 1978, 2000; Solfvin & Roll, 1976; Thatcher, 1910). However, correlating poltergeist manifestations to problems in the central nervous system is still speculative (Alvarado, 1993; Martínez-Taboas, 1984).

As happened in haunting field investigation, the development of technology has improved poltergeist field research. In the next section I will discuss how technological advances have been incorporated into field investigation as well as review what else we have learned from haunting and poltergeist research.

Application of Instrumentation in Field Investigation of Hauntings and Poltergeists

As we have seen, up to the 1950s, field investigations of hauntings and poltergeists consisted mainly of qualitative studies accomplished through observational and survey methods (Harte, Hollinshead & Black, 1996). Schmeidler (1966) introduced quantitative statistical analyses of adjective checklists and floor plans to field investigations. This approach was further improved with the use of instrumentation in haunting and poltergeist research.

The development of technology has enhanced field investigation because it provides us with a potential means of understanding physical aspects involved in haunting and poltergeist phenomena (Houran, Lange & Black, 1996; Tart, 1965). Serious discussion of the use of instrumentation in field investigations began in the mid 1950s. Researchers such as Hans Bender and William Roll began to use devices to detect vibrations, electrostatic energy, sounds, temperature changes, and so on at poltergeist and/or haunting sites.

As pointed out by Harte, Hollinshead and Black (1996), several instruments have been used in field investigation: still and motion-picture photography with various types of film media, including infrared film (e.g., Bender, 1969; Karger & Zicha, 1968; Maher & Hansen, 1992; Maher & Schmeidler, 1975; Rosenberg, 1974; Taff & Gaynor, 1976; Uphoff & Uphoff, 1984); thermal sensors (e.g., Andrews, 1977); devices to measure geological and/or meteorological conditions (e.g., Dingwall & Hall, 1958; Osis, 1982; Persinger & Cameron, 1996; Randall & Randall, 1991); tape-recorders equipped or not with specialized microphones, parabolic dishes or diodes to capture subsonic and ultrasonic frequencies (e.g., Hövelmann, 1982; Pratt & Palmer, 1976); such devices for psycho-physiological measurements as

portable transistor electroencephalograms—telemetric units—(e.g., Solfvin & Roll, 1976), as well as those that can measure heart rate, respiration, or galvanic skin response (Hart, Black & Hollinshead, 1999); magnetometers (e.g., Radin & Roll, 1994, 1996; Roll, Sheehan, Persinger & Glass, 1996); radiation detectors such as the Geiger counter (e.g., Maher & Schmeidler, 1975); computers, such as a diode-based computerized random number generator (e.g., Maher & Hansen, 1992); and a multi-energy sensor array called MESA, that is, "a portable computerized multi-energy sensor array... [which] can facilitate the task by permitting one researcher instead of a group of investigators to operate the multiple sensors and collect EMF data" (Harte, Hollinshead & Black, 1996).

The use of instrumentation to collect and analyze physical data in field investigations has improved our objective knowledge about haunting and poltergeist episodes. As Harte, Hollinshead and Black (1996) have summarized so well:

> There is now an accumulating body of evidence alleging that known, conventional physical energies may be mediating or allegedly causing hauntings and poltergeist-like episodes. These energies include ionizing radiation (Devereux, 1990; Radin & Roll, 1994; Roll, 1994; Roney-Dougal, 1991), geomagnetic activity (Gearhart & Persinger, 1986; Persinger, 1981, 1985, 1988, 1993; Persinger & Cameron, 1986; Persinger & Lafreniere, 1977; Persinger & Richards, 1995; Randall & Randall, 1991) and localized electromagnetic and electrostatic fields (Cook & Persinger, 1997; Green, Parks, Guyer, Fahrion & Coyne, 1992; Persinger 1975; Radin & Roll, 1994, 1996; Roll, Sheehan, Persinger & Glass, 1996; Shalis, 1989). Persinger contends that extreme or unusual forms of electromagnetic fields (EMFs) can directly influence the physical environment (Persinger & Cameron, 1986; Persinger & Lafreniere, 1977) and the psychophysiological functioning of those who are exposed (Cook & Persinger, 1997; Persinger, 1988, 1993; Persinger & Cameron, 1986).

Surveys of and observations at haunting or poltergeist sites must be complemented by specialized devices as well as by other physical and psycho-physiological measurement instruments that have been developed and will be developed in the future. Keeping an eye on technology development is an important step in finding new ways of

collecting and correlating data in order to create new experimental designs.

Collaborating with psychics in field investigations

In addition to technological devices, some researchers such as Roll and Schmeidler have included collaboration with psychics, especially during on-site field investigations of hauntings. This methodological step is controversial. Although some interesting results regarding the subjective aspects of cases have been presented, collaborating with psychics has not contributed objective evidence by which we may understand the mechanisms involved in haunting and poltergeist phenomena. Psychics are supposed to capture information about the locale of anomalous occurrences no matter what their personal religious beliefs. Their impressions can then be tested against objective data obtained from instrumentation used on site (Moss & Schmeidler, 1968; Radin & Roll, 1994). What seems to be important in this situation is to try to use mediumship ability as a kind of bridge between the past and the present, and between the non-directly-observable data in the environment and the researchers.

Persistent questions and possible ways to answer them

Even though we have a good range of knowledge about poltergeists and hauntings, there are still important questions that remain unanswered. We already have a considerable number of clues and hypotheses about these phenomena that are really important because they offer some access to these anomalous occurrences and because they provide us with an incentive to continue looking for answers. Solving the questions raised by hauntings and poltergeists is important because psi experiences are part of everyday life as surveys and case collections have shown. So, it is worthwhile to invest time, money and efforts to improve our knowledge about these phenomena.

Among all the non-answered questions about hauntings and poltergeists the most intriguing ones may be: 'What do they mean?' and 'What is their function in our daily life?' As psi researchers we cannot forget these questions. Context is fundamental to our future research. Unfortunately, however, even though the psychodynamic view of poltergeists has been influential, I have noticed, for instance,

based on what I have read since I started to do research in this area, that many published reports on field investigations are still centered on the physical aspects of the disturbances. This same emphasis is even more visible in reports of field investigation of hauntings. It is true that evidence shows that hauntings are connected to places, but we cannot forget that people report these experiences. So, research cannot be limited to the history of the place or to physical measurements taken at the haunted site. Instead the individuals who experience hauntings should be considered even if a key element of each case is its fundamental relationship to the location. So, symbolic aspects involved in the cases must be considered seriously, and in many cases such aspects are ignored. When the context—including symbolic aspects—of poltergeist and haunting occurrences are not explored, we can lose very important data, especially considering that, even though such spontaneous experiences happen more frequently than is usually imagined, they do not happen every day.

As Alvarado (2002) argues, we need a balanced integration between research directed at proof and research directed at process. Emphasis on the extremes is not good, and anyone can see that while we have a considerable number of field investigations of poltergeists and hauntings, the emphasis of our field is on experimental research. Experimental research is extremely important, but being devoted only to experiments keeps us from understanding the magnitude of psi in daily life. On the other hand, we cannot restrict our research work to the compilation of cases.

Some of us can argue that there are not so many field investigations of hauntings and poltergeists because they are not easily available for study. This is true. But maybe we should pay more attention to less visible, less extreme psi experiences like apparitions, ESP dreams reports and isolated psychokinetic experiences. Such occurrences are not as pronounced or exciting as classical poltergeist and haunting occurrences, but they must have the same components and mechanisms of psi in them. By paying attention to these 'smaller' phenomena, we may be more able to assess the robust cases when these come along.

Another point to be considered is the importance of fraudulent cases. In terms of meaning—and again, symbolic aspects of these cases are important—fraudulent poltergeist and haunting cases seem to have the same function as seemingly genuine cases. As I have demonstrated (Machado, 2003), in terms of context the study of fraudulent cases can be very fruitful, in that they allow us to evaluate or reflect upon the

reasons that make someone 'choose' psi-like events to get what they want or to express their feelings or needs. In addition, it is important to remember that some cases may contain a mixture of consciously or unconsciously produced fraudulent events and apparently genuine psi events (e.g., Roll, 1969).

It is a fact that we still do not have a definitive answer to the questions about the reality of anomalous environmental interactions involved in hauntings and poltergeists. Despite all the evidence—and some of it is really strong—we still do not have the scientific proof we need. In any case, because hauntings and poltergeists are perceived or can be caused by people, another intriguing question to ask is why does not everybody have these kinds of experiences? Even though we have psychological and physiological measures of agents or percipients, it is true that we have not uncovered variables that differentiate those who have never had such experiences from those who have. So 'What makes the difference?' remains an open question. And the same thing is true when we think about the structure of specific situations: context can be very similar in ordinary conflict situations and in poltergeist cases, in the sense that context may be intimately connected to conflict or may even make conflicts meaningful. So, what is the specific role of context in poltergeist cases? And what is its importance in haunting cases?

Even the use of sophisticated technological devices in contemporary field research cannot provide answers to questions related to objective aspects of the phenomena. We already know that there is a correlation between environmental forces (electromagnetic fields, for example) and the incidence of haunting and poltergeist phenomena. But why do such poltergeists and hauntings occur in some places and not in other ones where environmental measurements are the same? We cannot answer that question yet. It is necessary to do more field investigations in which we consider both subjective and objective aspects of the phenomena. Real interdisciplinary work is needed. There seems to be an intersection among social, psychological, physiological and environmental aspects that we have not been able to detect yet, possibly because we have not integrated our differing research methodologies to the extent that we could have done. It is interesting to think about the possibility of special training for integrated field research, about establishing an interdisciplinary protocol that would guide all future scientific research on hauntings and poltergeists. Because we are talking about 'spontaneous' cases here and because nature is not

obliged to limit itself to our established protocols—even if we carefully develop a methodological protocol from our deepest understanding of what has been observed in field investigations conducted up to now—such protocol would still need to be evaluated periodically by peer-researchers for adjustments and improvements. And it would be important to tie experimental research to future investigation of spontaneous cases. Experimentation would complement field observations and measurements as well as provide us with more interesting data about the human beings involved in the cases. A network of field investigation could also be created to bring together researchers from different areas and perspectives that are dedicated to spontaneous case research, but also committed to the idea of using experimental procedures to test individual experiencers in the cases that are investigated. In addition to changing ideas about what constitutes research and how best to develop methodology further, the network could work as one possible avenue for the sharing of technology to investigate the physical aspects of the cases while maintaining an emphasis on subjective aspects of the phenomena. Such a network could promote increased levels of haunting and poltergeist field investigations while facilitating both the integration of data collected from experimental studies of the experiencers with that obtained from contextual studies of the haunting and poltergeist events as a whole.

Longitudinal studies of experiencers of haunting and poltergeist phenomena could also be done. Instead of abandoning the case when poltergeist occurrences stop or after physical measurements at haunted places have been taken, the network of field investigators could function as an observatory dedicated to following people and places involved in spontaneous cases over a long period of time in order to map subsequent effects of the original spontaneous experiences or even the consequences of fraudulent cases on the perpetrators and others who witnessed or were involved in the original outbreak.

The network of investigators could also help to break through the language barriers from which we all suffer. Reports written in languages other than English are barely read by those who are not native speakers (Alvarado, 1985). Translations and subsequent dissemination of reports originally written in different languages could be shared among and commented on by a wider group of researchers.

A practical question concerning haunting and poltergeist research is how to deal with the obstacles for conducting field investigations. As was pointed out by Machado and Zangari (1998, p. 76):

> The main difficulties facing investigators of poltergeist cases are: (1) the difficulties in accessing poltergeist events, because it is more common for a group living with a 'poltergeist situation' to ask for religious help or to call the police than to look for a researcher; (2) the rareness of poltergeist events; (3) the fact that a great part of the investigation depends on fallible human testimony; (4) the involvement of the press who are always looking for amazing headlines, whose actions help to 'contaminate' testimonies and 'spoil' the evidential value of reports of the occurrences; (5) the fact that many psi researchers and many scientists from other disciplines do not think that poltergeist cases are worth studying; (6) the importance of maintaining confidentiality of the names, addresses or events in a poltergeist case so as to protect the lives and privacy of the experiencers even if the evidential value of the case report suffers from the investigator's sense of integrity.

I would say that the investigation of hauntings pass through similar difficulties, and it should also be mentioned, both in poltergeist and haunting case investigations, that the lack of financial resources for the researchers' personal expenses (like transportation, food, accommodation) impacts on his or her ability to investigate the occurrences *in locu*. Similarly the lack of funds to buy and maintain instrumentation also limits the investigator's ability to take measurements on-site. The network of field investigators suggested above could help to solve this problem if it could find grants and aid from other organizations. Other types of difficulties can hardly be avoided. The future protocol of field research could include training for researchers in how to deal with the police, the press and with religious interferences. The rareness of cases is a problem that could also be minimized by the network of field researchers, because members would be spread across the globe and the appearance of any new exciting case could be disseminated quickly. The problems with human testimony and the need to maintain secrecy about the locations in haunting and poltergeist cases, and the anonymity of individual experiencers could be handled by the assignment of pseudonyms or other methods of obscuring details by the network.

The lack of value that field investigations in general and hauntings and poltergeists in particular suffer from is a problem connected to personal beliefs about what is worthwhile in science and also to the conditions under which we do this kind of research. Cases do not

follow a set schedule so we cannot plan a field investigation in advance so as to adhere to funding deadlines or arrange for a research leave from our place of employment. In spite of these difficulties, it is important not only to argue that spontaneous case research is important and must be done, but also to conduct field investigations more effectively in order to add to the body of data that makes up the findings of the field.

One important question persists: Are poltergeists and hauntings provoked exclusively by living human beings or are they provoked by disembodied spirits or entities? As I have already mentioned in this chapter, some researchers entertain the notion that disembodied spirits or entities may be the agents in some cases, especially when a focal person cannot be identified in poltergeist cases and even more so in haunting cases. It is possible however that such cases were interpreted in a supernatural way because of researchers' beliefs and biases. Most of the reports of field investigation of hauntings do not demonstrate—at least not explicitly—any effort to investigate the possible connection between people and their lives to the reported occurrences. Many times, context is neglected, while more importance is given to physical measurements.

This type of emphasis makes field research seem utilitarian: we want to get data in order to know about and get control over a kind of force or capacity we do not yet understand with the simple wish to use the knowledge we gain to develop more technology. Of course it is important to detect and understand this new force if it exists, and of course it is important to continue to improve our technical expertise. But what about the human beings involved in the haunting and poltergeist episodes? I feel sometimes we forget that field investigations of psi phenomena as they are experienced in everyday life can tell us about our nature as human beings, which is part—a big part, I would say—of the enigma called psi.

When I think about the possible involvement of supernatural forces in hauntings and poltergeists, I sincerely do not see how we can determine the plausibility of the spirit hypothesis one way or the other. To give a definitive answer to this question, we need to know the reach of the human mind, the limits of our psi ability if it really exists—and I believe it does. Survival is an interesting topic especially because, as human beings, we are fragile in the face of life and death, but as scientists, I think first of all we have to concentrate our efforts on discovering and understanding human abilities and limitations. Psi is a

promising naturalistic hypothesis that needs more investigation, more exhaustive testing. And we cannot forget that scientific method as it is known now is limited and serves to investigate empirically verifiable hypotheses. The reality or not of the spiritual world, of its influence in our world is not part of the scientific agenda. I am not saying that science denies the possibility of survival or of disembodied entities acting in our daily life. I am saying that science cannot confirm or deny the existence of spirits and/or their action in the world because the spirit hypothesis itself cannot be tested empirically. This fact does not impede researchers who collect reports and study the effects of anomalous experiences on the personal beliefs of the experiencers, but it does mean that the ontological reality of the spiritual world cannot be tested. The testability or not of the spirit hypothesis is a controversial question and it is possible to find different opinions about it in the field.

As I have said before, the fact is that we already know a great deal about hauntings and poltergeists, but there are still many open questions to be answered. Field investigations are worthwhile because they are essential to this task. As Alvarado (2002, p. 117) has said of spontaneous psi research in general:

> Our field needs to learn something about the phenomena, not to continue sharing marvelous stories for their own sake. My impression over the years is that a segment of those concerned with the study of spontaneous phenomena are not interested in explaining or understanding the phenomena. They seem to be happy to maintain the mystery for its own sake. In their view, the phenomena are something sacred that should not be probed too much. ... This could be because of religious or spiritual concerns that define the phenomenon as transcendental and beyond the reach of science, or it could be because of fear that if the phenomenon is explained, there will be no more mystery and thus no reason to focus on it. ... Whatever the reason, such attitudes hinder the development of spontaneous case research, leaving the field as little more than the glorification of the parapsychological.

We have a decision to make as a research community: Are we going to contemplate hauntings and poltergeists as astonishing cases or are we going to work together to decipher the enigma that these cases represent? I vote that we improve our collaborative research efforts in order to decipher the enigma.

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PERSONALITY VARIABLES IN SPONTANEOUS PSI RESEARCH: CONTEXTUALIZING THE BOUNDARY CONSTRUCT IN ITS RELATIONSHIP TO SPONTANEOUS PSI PHENOMENA

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Introduction

The idea of an *anomaly-prone* personality is an attractive one, and one which is supported in the mainstream (i.e., psychological and neuroscience) and parapsychology literatures. This is to say that by knowing how an individual scores on a given battery of personality questionnaires, one can ascertain the likelihood of that person experiencing and reporting spontaneous psi phenomena.[1]

This paper will present and evaluate how and why individual differences in 'boundary thinness' impacts upon the tendency to experience spontaneous psi phenomena. Boundary thinness will be introduced and described from a hierarchical perspective. This will begin at the physiological level, move up to the cognitive level (e.g., attention and representational boundaries), and include discussion of intrapersonal subjective experiences (including consciousness). Finally, it will explore *inter*personal experiences, and boundaries that are working at a societal/cultural level. In summary, boundary thinness can be understood to reflect a tendency toward greater interconnections or less inhibition within and between the neural structures of the brain (i.e., left and right hemispheres and cortical and sub-cortical structures), between subjective emotional states and states of consciousness (less distinctness between usually distinct states of consciousness such as being 'awake' or 'asleep', and more 'in-between' states), between

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[1] It is possible that we may also be able to ascertain the likelihood of their success on a genuine ESP task. However, the veridicality of ESP is somewhat outside of the scope of this paper, i.e., we cannot know for sure whether spontaneous psi phenomena are genuinely psychic or an example of a situation of 'pseudo psi', we can however, map what we know about the personality correlates of laboratory performance of ESP to subjective experiences and have a better idea about which experiences could *potentially* be genuine.

representational and other structures of the cognitive system and between other intrapersonal and interpersonal subjective experiences. This results in those with thinner boundaries exhibiting several characteristics:

a. being efficient at processing subliminal information and having more information associated with the functioning of evolutionarily older brain areas (namely the limbic system and reptilian brains) available to the cortex;

b. having more availability of information associated with the functioning of the right hemisphere (via reduced dominance in the left hemisphere and increased connectivity between the hemispheres);

c. having a tendency toward experiencing altered and 'in between' states of consciousness such as the hypnagogic state[2];

d. experiencing a fusion of emotions and thoughts;

e. being prone toward making cognitive associations;

f. experiencing extreme empathy and a proneness toward getting emotionally close to other individuals.

As spontaneous experiences reflect a mixture of pseudopsychic alongside potentially more genuine phenomena, this paper will explore how boundary thinness could shed insight on the etiology of both explanations for apparently psychic experiences.

This author considers that there are several psychometric variables reflective of boundary thinness. Here, Positive Schizotypy, Thalbourne's Transliminality, Hartmann's Boundary Thinness and Persinger's Temporal Lobe Lability will each be introduced as *anomaly-prone* personality types. These in turn relate to other psychometric variables associated with psi experiences, which include creativity, dissociation, absorption and extraversion. These (secondary) variables may also be associated with boundary thinness and as such, a *boundary* approach could allow for a deeper (and more parsimonious) understanding of spontaneous psi experiences. Much of the current paper will focus on the construct of Positive Schizotypy, although it

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[2] The liminal state of consciousness between wakefulness and sleep (e.g., see Mavromatis, 1987).

will be argued that all of these constructs are psychometrically and descriptively very similar.

This author also considers that a traditional 'personality' approach to understanding psi experiences may in fact be somewhat restrictive and should be elaborated in order to fully understand *how* individual differences may impact upon the tendency to experience anomalous phenomena. As such, there is room for deep elaboration on what we know and where we are going with a view to developing a greater understanding of anomalous experiences from this perspective.

This paper will review trait measures of boundary thinness, and move on to break boundaries down into their constituents in a hierarchical exploration of boundaries in the brain, mind and society. The argument that [thinner] boundaries are both a personality trait and state of consciousness, possible among everyone, will be presented. In addition, several suggestions for the manipulation of boundary thinness will be presented, which could have relevance to clinical psychology in addition to parapsychology.

Various *types* of thin boundary may exist within the overall personality construct. This is coupled with the observation that spontaneous psi phenomena are multifaceted. As such, there is also a need to delineate which types of boundaries are more relevant to understanding each of the various types of spontaneous psi phenomena.

Next, it will be argued that researchers considering using boundary relevant individual differences measures in spontaneous psi research should explore personality *profiles* in contrast to using individual personality scales. This will enable greater understanding of the mental health/wellbeing of the person reporting a spontaneous psychic experience.

This paper will then explore *why* an anomaly-prone personality might exist in the first place, and an argument for an evolutionary understanding of anomalous and paranormal experiences will be made. It will be argued that anomaly-prone personalities are associated with positive schizotypy, which is genetically related to schizophrenia. As such, anomalous experiences may be related to the genes associated with schizophrenia. An evolutionary perspective on ESP is not a new idea, although the idea that ESP might be related to schizophrenia is a twist on current thinking in clinical and personality psychology. An evolutionary perspective which maps parapsychology to the mainstream may also allow for greater acceptance of research into anomalous experiences.

Finally, it is considered that the perspectives of Situationism and Interactionism in mainstream personality research should be mapped to parapsychology. The basic premise behind this view is that personality is not a consistent/fixed internal code, but rather an emergent property of the mind/brain/body, that is both situational and interacts with other psychological (e.g., preferred state of consciousness, one's gender/gender role) social (e.g., the demands of the particular social situation) and environmental factors (e.g., of geomagnetism and solar influences). If boundary thinness is both a state of consciousness and a personality trait, it is important to ascertain which circumstances are conducive to spontaneous psychic phenomena (or accessing states of boundary thinness) in reference to one's own particular personality profile.

Boundaries and Anomalous Experiences

Spontaneous psi phenomena are more common among certain individuals, and are more likely in certain states of consciousness and situations. On the surface, these correlates may appear to be somewhat disparate. In terms of personality, psi experiences are more likely to be reported by those who score high on absorption (Irwin, 1985), dissociation (Pekala, Kumar & Marcano, 1995; Ross & Joshi, 1992), positive schizotypy (Wolfradt, Ouibaid, Starube, Bischoff & Mischo, 1999), temporal lobe lability (Persinger & Valliant, 1985), transliminality (Thalbourne & Delin, 1994), boundary thinness (Krippner, Wickramasekera & Tartz, 2001), extraversion (Eysenck, 1966) and creativity (Dalton, 1997).[3]

In terms of states of consciousness/situations, psi experiences have been associated with dreams (Rhine, 1953), the hypnagogic state of consciousness (Gurney, Myers & Podmore, 1886; Sherwood, 1998), meditation (Roney-Dougal & Solvin, 2006), being subjectively awake but doing repetitive tasks (Gurney, Myers & Podmore, 1886), the action of the right hemisphere (Broughton, 1983), 'consciousness

[3] For a comprehensive review of individual differences and psi experiences, the interested reader is referred to Irwin (2004) and Targ, Schlitz & Irwin (2000).

binding'[4] processes (Don, McDonough, & Warren, 1998), and subliminal perception (Beloff, 1973; Roney-Dougal, 1986).

In this paper, it will be argued that a parsimonious way to understand these variables and their relationship to psi experiences is by consideration of boundary thinness as both personality trait and a state. This way, spontaneous psi experiences are possible in anyone given the right state, but some individuals are more prone to experience such states in terms of their trait biological and psychological makeup. As noted in the introduction, there are a number of features associated with 'thin boundaries'. This basically reflects how connected or separate a neural structure or cognitive process is, in relation to other structures or processes in the brain or mind; 'weak' or 'thin' boundaries imply that there are many connections between structures and processes in the neurological and cognitive systems, whilst 'firm' or 'thick' boundaries implies fewer connections between such structures and processes. This reflects the extent to which a person is likely to be aware of information that is usually outside of the focus of attention or in the subliminal area of the mind/brain (including information processing from the right hemisphere). It also reflects the extent to which one is likely to experience certain altered states of consciousness. Boundary thinness versus thickness is fundamentally entwined with a tendency to experience spontaneous psi and other anomalous phenomena.

Boundaries are not a new idea in psychology or parapsychology. Indeed, the concept of thresholds and boundaries in the mind were explored over a century ago by Frederick Myers and William James (Thalbourne, 1999) and are also apparent in the thinking of Bergson in terms of *filter* theories of psi (e.g., 1913). In recent years, boundaries have been explored explicitly and psychometrically by two researchers in particular: Thalbourne in his construct of transliminality (e.g., Thalbourne & Houran, 2000) and Hartmann in his construct of boundary thinness (Hartmann, 1991; Hartmann, Harrison & Zborowski, 2001). A third route of understanding derives from the literature on positive schizotypy (e.g., see Claridge, 1997) and related variables (e.g., Persinger's Temporal lobe lability, absorption and dissociation).

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[4] Consciousness binding refers to the fusion of disparate elements of a perceptual object or scene into a unified conscious mental experience. It has recently been asserted that gamma wave synchrony may be implicated in binding. Interested readers are directed to, Engel & Singer, 2001 for more information.

These personality variables are associated with increased reporting of subjective paranormal experiences and may share neurological and cognitive attributes. The next section will present an overview of these variables, and their "anomaly-proneness".

Personality and Boundaries

Positive Schizotypy

Schizotypy is a term derived from 'schizophrenic genotype' and indicates a greater disposition toward schizophrenia (Claridge, 1997). This is currently understood to reflect a continuum (Bentall, 2000, 2003; Crow, 2008a), although psychologists differ in their understanding of the nature of the continuum. Some (e.g., the Chapman group, see Claridge & Beech, 1995) consider that *any* presence of 'schizotypal traits' are indicative of psychopathology, and take a quasi-dimensional view of schizotypy. Other authors (in particular, Claridge, e.g., 1997) consider schizotypy to be a fully dimensional personality construct. With this approach, the cognitive and perceptual anomalies associated with schizophrenia are considered to be normally distributed among the general population, with only extreme levels *potentially* resulting in psychotic breakdown. As such, few people would have extremely low and high expression of these traits, but the average person would exhibit a moderate level of the traits associated with schizotypy. This suggests a useful role for watered down schizotypal traits; indeed, there is some evidence for the idea of the high scoring 'happy' or 'benign' schizotype (Goulding, 2004, 2005; Holt, Simmonds-Moore & Moore, 2008; Jackson, 1997) who is psychologically healthy and exhibits adaptive traits such as creativity (Brod, 1997; Holt, Simmonds-Moore & Moore, 2008). The competing perspectives on schizotypy continue to co-exist despite considerable evidence for the better explanatory value of the personality explanation over that of the psychopathological 'taxon' (Rawlings, Williams, Haslam, & Claridge, 2008). The perspective of this author is that schizotypy reflects a personality continuum ranging from low scorers through normals to high scorers who may be more prone to schizophrenic breakdown.

Schizotypy is a multidimensional variable that is comprised of four factors (Claridge & Beech, 1995): positive schizotypy (magical thinking, hallucinations and altered perceptual experiences); cognitive disorganisation (attentional difficulties); negative schizotypy or

introvertive anhedonia (preference for solitude, lack of enjoyment from social sources); and impulsive nonconformity (impulse ridden, reckless behaviors).[5] The traits associated with positive schizotypy are those most associated with anomalous phenomena. This variable relates to subjective anomalous and paranormal experiences and beliefs (Simmonds & Roe, 2000; Wolfradt, Ouibaid, Starube, Bischoff & Mischo, 1999). Several authors have also found that Magical Ideation (as a measure of positive schizotypy) is a predictor of above-chance ESP scoring in the ganzfeld (Lawrence & Woodley, 1998; Parker, 2000a; Parker, Grams & Petterson, 1998; Parker & Westerlund, 1998). However, work using other measures of positive schizotypy (e.g., unusual experiences subscale of the OLIFE: Mason, Claridge, & Jackson, 1995) has found no significant relationship with ESP (Simmonds, 2003; Simmonds-Moore & Holt, 2007). A recent assessment of clusters or types of schizotypy scorers did indicate that those who scored high on positive schizotypy but low on the other 3 scales did better at an ESP test addressing psi as the unattended stimulus (Holt & Simmonds-Moore, 2008). Other work by Simmonds (Holt & Simmonds-Moore, 2008; Simmonds, 2003; Simmonds & Fox, 2004; Simmonds & Holt, 2007) has also addressed schizotypy and ESP from a multidimensional perspective, but the only significant relationship with ESP, when each scale was individually correlated with the psi measure was a negative relationship with impulsive nonconformity in Simmonds and Fox's (2004) research.

Hartmann's Boundary Questionnaire

Hartmann's construct of psychological boundaries refers to a continuum of boundary thinness in the mind and brain (Hartmann, 1991; Hartmann *et al.*, 2001). *Thin* boundaries refer to a relative connectedness of psychological processes, which is reflected in a thinking style of 'shades of grey'. *Thick* boundaries in the mind, on the other hand, refer to a relative separateness of psychological processes, which is reflected in a thinking style of 'black and white' (Hartmann, Rosen & Rand, 1998). These concepts reflect structural boundaries, (e.g., how connected neural structures are to one another), representational boundaries (e.g., how related representations and concepts are to one another), and boundaries in how one thinks or

[5] Impulsive nonconformity is however a controversial factor considered by some to reflect more manic tendencies (e.g., Loughland & Williams, 1997).

processes information (e.g., is thinking focused or unfocused/associative). Hartmann (1991) suggests that boundaries should be seen as one broad dimension of personality, although he considers that there are 12 types of boundary assessed by the Hartmann boundary questionnaire (BQ: Hartmann, 1991)—see Table 1.

TABLE 1.
Examples of boundary types from the Hartmann (1991) Boundary Questionnaire

Type of boundary	Example of a question addressing this form of boundary
Sleep/wake/dream	When I awake in the morning, I am not sure whether I am really awake for a few minutes.
Unusual experiences	I have had déjà vu experiences.
Thoughts, feelings, moods	Sometimes I don't know whether I am thinking or feeling.
Childhood, adolescent, adulthood	I am very close to my childhood feelings.
Interpersonal	When I get involved with someone, we sometimes get too close.
Sensitivity	I am very sensitive to other peoples' feelings.
Neat, exact, precise	I keep my desk or worktable neat and well organised.
Edges, lines, clothing	I like houses with flexible spaces, where you can shift things around and make different uses of the room.
Opinions about children and others	I think a good teacher must remain in part a child.
Opinions about organizations	In an organization, everyone should have a definite place and a specific role.
Opinions about people, nations, groups	There are no sharp dividing lines between normal people, people with problems and people who are considered psychotic or crazy.
Opinions about beauty and truth	Either you are telling the truth or you are lying; that's all there is to it.

Boundary thinness has been associated with subjective success at a psi task (Richards, 1996) and is higher among those who consider themselves to be psychic (Krippner, Wickramasekera & Tartz, 2001). A recent study found that overall thinner boundaries were related to biological PK (Palmer, Simmonds-Moore & Baumann, 2006).

Thalbourne's Transliminality

The Transliminality variable reflects "the hypothesized tendency for psychological material to cross thresholds into or out of consciousness" (Thalbourne & Houran, 2000, p. 861). It was originally derived from a factor analysis of several variables including paranormal belief, magical ideation, manic-like experience, depressive experience, creative personality and mystical experience. These clustered on a single factor (Thalbourne & Delin, 1994).

The transliminality construct, as it is currently defined, is comprised of absorption, fantasy proneness, magical ideation, paranormal belief, mystical experience, hyperesthesia, (a "hypersensitivity to environmental stimulation"—Thalbourne, 1998, p. 403), creative personality, manic experience and attitude to dream interpretation. It is currently measured employing a 17-item scale (Lange, Houran, & Storm, 2000). There is some evidence that transliminality is associated with increased psi performance in the laboratory (Sanders, Thalbourne & Delin, 2000; Storm & Thalbourne, 1998-1999, 2001) although results are mixed overall, and several studies addressing transliminality have found chance scoring (e.g., Simmonds, 2003).

Persinger's Temporal Lobe Lability

Temporal lobe lability may also be considered to reflect a personality continuum from normals through to epileptics in the general population (Persinger & Makarec, 1987). This reflects a continuum of electrical activity in the temporal lobes—effectively indicating how interconnected temporal lobe structures are with the rest of the brain. Lability can also change as a result of meditation, hypoglycemia (prolonged fasting), fatigue, hypoxia, alterations in vascular flow associated with drugs and the biochemical effects caused by personal crises (Persinger & Makarec, 1987; Persinger, 1989).[6]

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[6] The idea that boundary thinness is a state as well as a trait is revisited in a later section.

Normal people with heightened lability of their temporal lobes may experience a benign or sub-clinical variety of 'seizure', which may be associated with anomalous experiences (c.f. Persinger & Makarec, 1993). Those who score higher on this scale are indeed particularly likely to report paranormal beliefs (e.g., Persinger & Richards, 1991), subjective paranormal experiences (e.g., Persinger & Valliant, 1985) and anomalous experiences (e.g., Kennedy, Kanthamani & Palmer, 1994). High scorers are also prone to finding great meaning or significance in events, and are more likely to be creative and suggestible (Persinger & Makarec, 1993). Meaning and significance are associated with the functioning of the amygdala, (housed within the temporal lobes) which is effectively more available to the rest of the brain in those who score high on this scale.

How Do Personality Variables Inter-Relate?

It is clear that many of these variables share common variance, for example, Simmonds-Moore correlated and factor-analyzed scoring on schizotypy, transliminality, Hartmann's boundary questionnaire and temporal lobe lability and found one unified factor (Simmonds, 2005a; Simmonds-Moore, 2009). A similar study employing transliminality, a measure of positive schizotypy[7], temporal lobe lability and Hartmann's boundary questionnaire (Thalbourne & Maltby, 2008) also found one unified factor underpinning these variables.

Following a cluster analysis, Simmonds-Moore also found that there were two types of scorer in the data set, which supports the idea of a boundary basis for anomalies (Simmonds, 2005b; Simmonds-Moore, 2009) One cluster reflected high scoring on all boundary-relevant variables (temporal lobe lability, transliminality, and schizotypy, moderately high scoring on boundary thinness and high scoring on anomalous experiences). The second cluster had lower scoring on all boundary related questionnaires and anomalous experiences.

Temporal lobe lability has also been found to share considerable variance with schizotypy, (Brugger & Graves, 1997; Simmonds, 2003) and with the transliminality scale (Thalbourne, Crawley & Houran, 2003). Transliminality also correlates positively and significantly with schizotypy (Thalbourne, 1998; Thalbourne, Keogh & Witt, 2005). In

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[7] Thalbourne and Maltby (2008) used the Unusual Experiences subscale of the Oxford Liverpool Inventory of Feelings and Experiences as a measure of positive schizotypy.

fact, Thalbourne (1999) considers that "schizotypy represents what is probably the closest conceptually and empirically to transliminality" (p. 20). Transliminality also correlates positively with boundary thinness (Houran, Thalbourne, & Hartmann, 2003; Sherwood & Milner, 2004-2005). Until recently, boundary thinness had not been directly compared to schizotypy, but those with schizotypal personality do tend to score 'thinner' (Hartmann, 1991).

Relationship to Other Variables Associated With Psi Experiences

As noted earlier, spontaneous psi experiences are also associated with creativity, absorption, dissociation and extraversion. Most of these variables correlate with the psychometric measures for boundary thinness, and may themselves be associated with different forms of boundary thinness.

Creativity

It has already been noted that schizotypy is associated with creativity (Brod, 1997) and that creativity is a component of the Transliminality variable (Thalbourne & Delin, 1994). In addition, temporal lobe lability also correlates with creativity (Kennedy, Kanthamani & Palmer, 1994) and is particularly prevalent among drama students and poets (Persinger & Makarec, 1993). In fact, Gianotti and colleagues (e.g., 2001) consider that the tendencies underpinning creativity, schizotypy and paranormal ideation are all related to a proneness to make associations (see later discussion).

Absorption

The absorption variable (Tellegen & Atkinson, 1974) is associated with a proneness toward total internal attention and a tendency to experience altered states of consciousness (Targ, Schlitz & Irwin, 2000). It seems to be important in the psi process as both a state and a trait (Irwin, 1985). Absorption correlates with both schizotypy (Parker, 1999) and temporal lobe lability (Kennedy, Kanthamani & Palmer, 1994). Williams (1997) found a common factor between schizotypy variables of magical ideation, perceptual aberration and hypomania alongside absorption and paranormal experiences.

Dissociation

Dissociation is a clinical syndrome, personality type and state of being which refers to a lack of usual integration of thoughts, feelings

and experiences into the stream of consciousness and memory (Burnstein & Putnam, 1986). Dissociation is related to the increased reporting of paranormal beliefs (Rattet, & Bursik, 2001; Wolfradt, 1997) and subjective psi experiences (Pekala, Kumar, & Marcano, 1995; Ross & Joshi, 1992; Zingrone & Alvarado, 1994). Depersonalization and amnesia aspects of dissociation relate to the OBE (see Alvarado & Zingrone, 1997) while imaginative tendencies and detachment aspects of dissociation relate to anomalous sleep experiences (Watson, 2003).

Dissociation may relate indirectly to psi experiences due to a tendency to experience altered states of consciousness (Targ, Schlitz & Irwin, 2000). The dissociative OBE has also been associated with claims of subjective paranormal experiences (Alvarado, 2000) as well as elevated psi-performance (Dalton *et al.*, 1999). After Blackmore, Alvarado & Zingrone (1997) suggested that being out of the body may allow a shift in one's cognitive map of reality, which could potentially allow for psi experiences.

Interestingly, the limbic system is implicated in alterations in the state of consciousness (Roll & Montagno, 1985), in particular, dissociation. In support of this, temporal lobe lability relates strongly to dissociative experiences (Persinger & Makarec, 1993). Dissociation also correlates with positive schizotypy (Merckelbach, Rassin, & Muris, 2000) and transliminality (Thalbourne, 1998). Watson (2001) found that dissociation and schizotypy are strongly correlated but maintains that they are distinct constructs.

Extraversion

Extraversion is a complex variable relating to one's sociability, sensation seeking, impulsivity, and level of outgoingness (among other components). It is also conceptualized and measured in a number of ways (Friedman & Schustack, 2006). It appears to relate to some, but not all psi-related experiences (Targ, Schlitz & Irwin, 2000). For example, it relates to precognitive experience (Rattet, & Bursik, 2001) and ESP performance (Storm & Thalbourne, 1998-1999). Superficially, this variable seems rather different from the variables discussed thus far. However, Claridge (1967) has noted that different forms of schizotypy are associated with different 'arousal profiles'. This reflects tonic arousal, ranging from low to high anxiety and homeostatic arousal, which reflects the arousal profiles of extraverts and introverts. The arousal systems become partially dissociated in positive

schizotypy, which is overall more associated with high anxiety (physiological lability) and high extraversion. Although positive schizotypy does not relate to psychometric extraversion (Day & Peters, 1999) it does appear to relate to impulsivity (Dinn, Harris, Aycicegi, Greene, & Andover, 2002), perhaps in men only (Rim, 1994). This may reflect the impulsive component of the schizotypy construct, which correlates with positive schizotypy (e.g., Mason, Claridge & Williams, 1997). It may also imply that some experiences associated with the extraversion variable may also reflect the experiences of someone who is 'thinner boundaried'.

A Hierarchical Exploration of Boundary Thinness and Anomalous Experience[8]

To gain further insight into personality and spontaneous psi experiences, it is necessary to strip the boundary construct down to the components which may contribute to the psychological and parapsychological experiences associated with psychic (and pseudo psychic) experiences. There are several ways in which we can understand boundary thinness. Here, 'boundaries' in the perceptual-cognitive system will be considered hierarchically. This exploration will begin at the level of psychophysiology and move up to cognition, subjective conscious (intrapersonal) experiences and interpersonal phenomena.

Connections in the brain

Thin boundaries may reflect a neural system that has more connections; directly as more neural connections and indirectly in the form of a reduction in the level of neural inhibition, which results in more *functional* connectivity. Hartmann considers that a brain with thin boundaries has "more complex or multiple connections in the cortex", while those with thick boundaries have "relatively simple straightforward or specific connections" (Hartmann, 1991, pp. 241-242). To Hartmann, the normal nervous system has *fine-tuned* itself, whilst the thin-boundaried system, exhibits connections that have grown (or not died off/been neurally 'pruned'). Hartmann (1991) also

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[8] This idea was presented in an earlier form at a conference on *Perspectives on Anomalous Experiences*, and a paper on the same topic will appear as a book chapter within a volume of the same name.

suggests that the thick boundary brain may be considered to have completed its development, whilst the thin boundary brain may reflect a system that is still developing, and in some ways may be considered to be childlike.

Boundaries can also be considered in terms of the extent to which inhibitory processes[9] are active in the system. A thin boundary system would reflect a *reduction* in such processes. For example, McCreery and Claridge (1996) note that there is a dissociation between the tonic and homeostatic arousal systems in the positive schizotypal nervous system. This results in a weakening of inhibitory mechanisms in the central and autonomic nervous systems (whilst excitatory mechanisms are high), In turn, this results in a relaxed nervous system that is more open to incoming and lower levels of information. Less inhibition might also allow for the inclusion of more neurological structures and more cognitive processes which may have an impact on what is available to conscious awareness as well as the contents of consciousness.

The Role of Subcortical Processes and 'Vertical' Connectivity

Boundary thin personalities may be associated with increased connectivity (anatomical or functional) between subcortical and cortical brain structures. Thalbourne (1999), for instance, has suggested that the physiological nature of transliminality may be conceptualized as the interconnectedness of various distinct parts of the brain, namely in the form of a sensitivity of the neocortex to information which flows from subcortical parts of the brain (the R-Complex or reptilian brain and the limbic system—Houran in Thalbourne, 1999, p. 21). Thus, there would be a consequent increased level of communication between higher and lower parts of the brain. A similar theory has been suggested by Hartmann (1991) in terms of the boundary thinness variable. Interestingly, states of consciousness associated with a dominance of alpha and theta EEG rhythms (which reflect more influence of sub-cortical activity on the cortex, see Mavromatis, 1987) are related to extrasensory perception experiences (Alvarado, 1998).

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[9] Much of the neurological system functions as a result of a balance between excitatory and inhibitory connections between neurons and neuronal pathways. We are wired such that many of our nerve cells and neuronal pathways connect with one another, but are not all activated due to the execution of inhibitory controls. Less inhibition means that the pathways which do already exist are more 'used'.

The Right Hemisphere and 'Lateral' Connectivity

There is a greater contribution of information processing that is linked to the right hemisphere among those with thinner boundaries. For example, there is increased EEG coherence between the hemispheres among those who score high on positive schizotypy (McCreery & Claridge, 1996), which indicates greater communication between the hemispheres. [10]

There is also a shift in the usual lateralization patterns across the two hemispheres of the brain such that the right hemisphere has a greater role in high scorers on positive schizotypy (Gruzelier & Doig, 1996). For example, ambidextrous individuals tend to score higher on schizotypy scales (Somers, Sommer, Boks & Kahn, 2008), which reflects a more bilateral organization of language. Other authors have found that those who score high on positive schizotypy have a right hemispheric bias in their information processing (Brugger, Gamma, Muri & Shafer, 1993; Leonhard & Brugger, 1998).

In terms of psi experiences, Simmonds (2005a) found that ambidextrous individuals reported more subjective paranormal experiences. This may implicate the deployment of *both* left and right hemispheric processes in the etiology of these experiences.[11] This finding complements the finding that those possessing a complimentary thinking style, including both rational and intuitive[12] thinking styles, tend to be more likely to believe in paranormal phenomena than those with only intuitive *or* rational thinking styles (Genovese, 2005; Wolfradt, Oubaid, Straube, Bischoff & Mischo, 1999). The expression of *two* thinking styles simultaneously related to paranormal belief, experience and ability, and was associated with schizotypy. They

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[10] In addition, among those who exhibit positive schizophrenia, there is also a larger corpus callosum and inter hemispheric blood flow (see Luh & Gooding, 1999), which may manifest in a watered down form among those scoring high on positive schizotypy.

[11] This may reflect right and left hemispheric representations of language, alongside other cognitive processes.

[12] Irwin and Young (2001) did not find the combination pattern in their later study, and note that Wolfradt *et al.* failed to find it in a replication of their 1999 study. In fact, Irwin and Young found that paranormal beliefs were more related to an intuitive (and not rational) thinking style. However, there was a trend toward a relationship between a combination of rational and intuitive thinking and new age beliefs.

suggest the combination of the two modes results from the brain's attempted psychological regulation of the two very different types of thought. This may imply that processes associated with right and left hemispheres are both more involved in such experiences, which may lead to increased hemispheric connection (or communication) and ability to entertain two different ways of approaching the world simultaneously.

Persinger has suggested that some anomalous experiences may be explained by a relative discrepancy in the arousal levels across the two hemispheres of the brain (Persinger & Richards, 1991). This results in intrusions of right hemispheric processes, such as the homologue of the sense of self, into the left hemisphere (Munro & Persinger, 1992). This may result in experiences of a sense of presence (in addition to OBEs and feelings of detachment from the body—examples of *dissociation*). Munro and Persinger suggest that this type of mystical experience is due to a "hemispheric mismatch in temporal lobe theta activity" (p. 899). However, Houran, Ashe and Thalbourne, (2003) did not find any relationship between bi-laterality, preferred side of the body and entity encounter experiences.

The right hemisphere is also considered to be important in the psi process (Braud, 1975). There has been some empirical support for the role of the right hemisphere in psi processing (Broughton, 1983), but recent work has not supported a difference between those with a preference for the right or left hemisphere on a psi task (Alexander & Broughton, 1999). It may well be that a contribution of *both* hemispheres might be more informative when considering both subjective paranormal experiences and genuine psi performance.

Attentional Boundaries

Psi related experiences may also be considered from a cognitive perspective. For example, in terms of attention, a psi stimulus may be processed in a manner akin to an unattended stimulus that is usually filtered out of conscious awareness. The idea of a filter in the mind which sifts incoming information dates back to Bergson (e.g., 1913), Broadbent (cited in Roney-Dougal, 1986) and Myers (1902/2002). Roney-Dougal (1986) suggests that selection filters in the normal mind prevent our experiencing an overload of perceptual experience, which might otherwise lead to madness. If attentional mechanisms break down, then filtering mechanisms would allow too much information into the cognitive system, which may or may not include a psi signal

amid perceptual noise (from both outside and within the cognitive system). In those with marginally weaker filters, we may see evidence of more information entering conscious awareness. Indeed, Thouless and Wiesner (1946) considered psi to be an evolutionarily older method of communication that should therefore only manifest under conditions which are detrimental to perceptual attention.

Those who score high on positive schizotypy (and impulsive nonconformity) performed better on two cognitive paradigms addressing attentional filtering (Simmonds, 2003)[13]. These are the negative priming and latent inhibition paradigms, respectively (Claridge & Beech, 1995). Among those who do not score high on positive schizotypy, information that was previously ignored or irrelevant becomes less available when it is subsequently needed, as it is inhibited or filtered out of conscious awareness. Among high scorers on positive schizotypy, however information is readily available, even though it was previously ignored. As such, Claridge and Beech (1995) consider that these reflect 'superior performance paradigms' for positive schizotypes. Interestingly the dissociation variable is associated with enhanced attentional abilities and increased information capacity in working memory (de Ruiter, Phaf, Elzinga, & van Dyck, 2004; de Ruiter, Phaf, Veltman, Kok, & van Dyck, 2003).

Recent work (Holt, Simmonds-Moore & Moore, 2007) found support for the idea that ESP might work like the unattended stimulus, which is usually filtered out of conscious awareness.[14] In addition, the same study explored the idea that wider attentional boundaries in schizotypy and transliminality might relate to ESP. Creativity, belief in the paranormal and unusual experiences (which may function as the transliminality variable) correlated with the attention based psi effect, but schizotypy (as measured as a unidimensional scale) did not. Where schizotypy was measured in clusters, the cluster relating to healthy positive schizotypy demonstrated more efficient inhibition of the irrelevant psi stimulus (Holt & Simmonds-Moore, 2008). This work should be replicated to further elucidate the roles of attention and personality for psi experiences.

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[13] However, this is not always the case. Recent work did not find evidence of 'weaker' filtering of normal irrelevant information among positive schizotypes (Holt & Simmonds-Moore, 2008).

[14] This was undertaken by employing an adapted latent inhibition paradigm with the addition of two psi conditions.

Attentional widening may also relate to pseudo-psychic experiences. Bullen, Hemsley and Dixon (1987) suggest that anomalies of attention allow more automatic/preconscious processing to become conscious. This may be associated with experiential and perceptual anomalies, which may seem to be psychic, even if they are not, perhaps due to chance coincidences with events in the external world. It is important to undertake further work exploring the nature of the relationship between attentional boundaries and psi experiences.

Availability of Subliminal Information

'Subliminal' information (stimuli presented below the normal perceptual threshold for conscious awareness) may be more available to conscious awareness among those with thinner boundaries than those with thicker boundaries. For example, positive schizotypy is associated with an increased sensitivity to subliminal primes (Evans, 1997). In addition, those with thinner boundaries (as measured by the BQ), had greater access to imagistic stimuli (in 'image' or pictorial format) which were presented subliminally, than those with thicker boundaries (Levin *et al.*, 1998-1999). This tendency seems to translate to other mental imagery, as high scorers on positive schizotypy are also more likely to remember nightmares and enjoyable dreams (Claridge, Clark & Davis, 1997) and report hypnagogic imagery (Jakes & Hemsley, 1987).

This tendency may be mediated by lax attentional processing and a general reduction of inhibitory processes in the mind-brain which would allow greater access to lower levels of awareness among those scoring high on positive schizotypy and related variables. Subliminal information may or may not also include veridical psi information. Several authors have noted a similarity between subliminal perception and psi phenomena (Beloff, 1973; Carpenter, 2004). Roney-Dougal, (1986), for example, suggests that both psi and subliminal percepts are weak inputs to the cognitive system and are good substrates for the study of how information moves from the subconscious to the conscious mind. Roney-Dougal's review of the literature demonstrates considerable overlaps between the correlates of subliminal perception and ESP. For example, both processes influence conscious percepts and behavior, they are both associated with right hemisphere processes and occur more in altered states of consciousness. This is particularly the case with semantic and associational responses to stimuli. Finally, there is a similarity in physiological responses (e.g. by means of galvanic

skin responses) to both psi and subliminally presented targets, particularly when these stimuli are emotional in nature. Direct experimental comparisons have revealed that the two forms of perception are actually related to one another in many contexts (Roney-Dougal, 1986). Carpenter (2004) has noted that ESP may work at a stage prior to subliminal perception, working to *orient* the organism toward or away from a particular target or object. This would be seen in the laboratory as psi hitting or psi missing and fortuitous events in the real world. He notes that many experiments exploring subliminal areas of mind may be applied to our understanding of ESP.

Alternatively, increased awareness of subliminal information may be implicated in pseudopsychic experiences. In this explanation, distracting information from lower levels of consciousness and unfiltered information from the external world may not be ignored by those with thinner boundaries. This information may then become incorporated in a "chain of semantic activation which may lead to intrusions of irrelevant information into consciousness" (Williams & Beech 1997, p. 77). This information may then be experienced as 'psychic'. In support of this proposal, an experiment addressing the relationship between transliminality and performance at a subliminal card guessing task found that high transliminals outperformed low transliminals where they received subliminal primes as to the correct card choice (Crawley, French & Yesson, 2002). There was no difference where no primes were given which indicates that subliminal information was misattributed as psi among these individuals. Other work (Houran & Lange, 1998) has indicated a relationship between dream frequency and the reporting of precognition, which could indicate an increase in sense data which may be meaningfully related (coincidentally) to events in the real world.

Associational Thinking

The right hemisphere is linked with associational thinking. As there is a right hemispheric bias among positive schizotypes, they exhibit a particular tendency to form cognitive associations and connections (Brugger & Graves, 1996). For example, Evans (1997) found that there is an increase in the activity of automatic associational networks among

those scoring high on positive schizotypy (Evans, 1997)[15]. Brugger and Graves (1997) propose that a hyper-activation of the medial temporal lobes results in disinhibition of associative processing in positive schizotypes.[16]

This thinking style may underpin the propensity to see connections and patterns where none are really there (the Type I error or *apothenia*—see Brugger, 2001, Gianottti *et al.*, 2001). The tendency to report a signal in noise has been demonstrated empirically by several authors in both visual (Feelgood & Rantzen, 1994; Jolley, Jones & Hemsley, 1999) and auditory domains (e.g., Young, Bentall, Slade & Dewey, 1987). Perceived patterns are also experienced as more complex (Jakes & Hemsley 1986) and there is greater confidence in the decision that a signal is present, irrespective of the accuracy of signal detection (Nuchponsai, Arakaki, Langman & Ogura, 1999). Being prone to making associations is also related to making more unusual word associations, which supports the existence of a relationship between positive schizotypy and creativity (Rawlings & Locarnini, 2008).

The 'loose' cognitive style is associated with a tendency toward new age beliefs and practices (which includes anomalous and paranormal experiences—see Farias, Claridge & Lalljee, 2005). Gianotti and colleagues (2001) have proposed a continuum reflecting associative processing, which underlies the tendency to think creatively, experience paranormal cognition, and at higher levels, to have psychopathological delusions.

Brugger (2001) considers that the tendency to make a type one error *explains* the relationship between paranormal thinking and psychopathology. This group does not entertain the possibility that paranormal ideation might also include genuine psi phenomena. Simmonds and Fox (2004), however, explored this tendency (employing visual noise) and found significant psi missing in the overall study. They also found some interesting patterns between schizotypy variables and ESP. This suggests that finding a signal in noise may relate to psi, and that this is associated with schizotypy. In

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[15] This is likely to be underpinned by the biological features of boundary thinness—anatomical (more connections) and functional connectivity (decreased inhibition) as described earlier.

[16] In contrast, a hypo-activation of the frontal association cortex results in an inhibition of associative processing in negative schizotypes, presumably indicating a total lack of associational thinking.

addition, de Boer and Bierman (2006) also found some evidence for the ESP hypothesis in a study comparing believers and disbelievers on an image priming task, which included an embedded psi condition. Here there was no difference between believers and disbelievers but some evidence for veridical ESP with positive primes.

Roll (1966), Irwin (1979) and Broughton (2006) consider that the ESP response may be formed from the person's own memory traces, which presumably would be better accessed with more efficient access to associational networks. Interestingly, one of the limbic structures—the hippocampus—functions as a key to access cortical memories. Enhanced memory processes have been found to relate to temporal lobe lability (Persinger, 1996) and psi performance (Feather, 1967; Rhine, 1954; Roll & Montagno, 1985).

Perceived Meaningfulness

Those with thinner boundaries are also prone to find significance and meaningfulness in mundane perceptions (e.g., Brugger, Regard, Landis, Cook, Krebs & Niederberger, 1993). This is related to the tendency to form associations, and may be mediated by the activity of another limbic structure, the amygdala, which has a role in attaching meaning to both external and internal experiences which may often be rather mundane (Skirda & Persinger, 1993). As there is more vertical connectivity in boundary thinness, limbic processes may have more impact on conscious experience. Brugger *et al.* (1999) consider that thick boundaried individuals would be less likely to report seeing meaningfulness or perceiving a shape in randomness when one is actually there (the Type II error).

Positive schizotypy does relate to the tendency for people to endorse Barnum statements as personally significant (Claridge, Clark, Powney & Hassan, 2008). This tendency also has relevance to ESP scoring in the laboratory, as Parker (2000b) found that high scorers on the magical ideation scale were more likely to undertake a cognitive strategy which he called 'spread your bets' in attributing mental imagery content to telepathy. This was associated with seeing meaning (and with imagery being available). Those who were particularly high scorers on the scale (a New Age group) and those who reported more paranormal experiences scored higher in correctly identifying the psi target. Thus, this group had a greater likelihood of seeing correspondences which resulted in increased likelihood of attaining psi hits.

Synesthesia

Synesthesia is a blurring of sensory experiences where one modality is experienced when another modality is stimulated (Reber, 1985) and is considered by some authors (e.g., Marks, 2000) to be on a continuum in the general population. As such, a few individuals will be at an extreme and are classic 'strong' synesthetes. Weaker forms of synesthesia, which are possibly those associated with thinner boundaries, are often associated with the experience of colored alphabets (where certain letters or words are experienced as particular colors) and musical experiences of colors (where certain notes are experienced as particular colors), etc., which may be more of a cognitive form of boundary thinness. Different theories abound on the reasons for synesthesia, which include sequence learning, failure of neural pruning, sensory leakage of information in brain pathways, disinhibition of neural feedback, hybrid models and increased functioning of the limbic system (for good reviews, see Grossenbacher & Lovelace, 2001; Harrison, 2001; Marks, 2000). Synesthesia may be considered hierarchically in its own right, reflecting biological, perceptual, cognitive boundaries as well as possibly boundaries of consciousness.

In general, those prone to synesthesia, have less neural inhibitory processes than those who are not (Grossenbacher, 1997) and the majority of models imply that synesthesia relates to a thinning of boundaries in the mind/brain. Indeed, synesthesia relates to psychometric measures of boundary thinness (Hartmann, 1991) to schizotypy (Williams *et al.*, 1996) and to transliminality (Thalbourne, Houran, Alias & Brugger, 2001). Synesthesia has been suggested to be a building block of anomalous experiences (Irwin, 1999; Williams, 1997). Indeed there is some recent support for the role of synesthesia in the etiology of the OBE (see Terhune, 2009). In addition, a qualitative exploration of subjective paranormal experiences found that psychic experients often describe phenomena in a synesthetic manner (Simmonds, 2004).

Synesthesia may allow a person to see the world (or experience the world) in a different way, and as such have more access to psi phenomena, but this has yet to be explored directly in an empirical investigation.

Interpersonal Boundaries

One of the original boundaries on Hartmann's original BQ (1991) relates to interpersonal boundaries, such that an extremely thin boundaried person is likely to get 'too close' to another individual. In support of this, higher scorers on an empathy scale do tend to score higher on a measure of positive schizotypy (Rim, 1994). It has been suggested that empathy and telepathy may be very similar constructs (Donovan, 1998). Unfortunately, however, there is little empirical work exploring connections between empathy, boundary thinness and psi experiences.[17] However, there is some evidence that empathy and compassion between those who love each other may be visible as a physiologically based psi effect (see Radin *et al.*, 2008). In addition, recent work found that healing was more successful with pairings of people who were close compared to those who did not know one another (Achterberg, 2008).

Dalton (1997) found that close emotional and biological bonds are associated with increased performance at a psi task. In addition, Sheldrake (2003) also found that emotional closeness between individuals was an important variable in his assessment of telephone telepathy. Other research has found that those who are biologically identical (monozygotic twins) displayed EEG *coherence*; in other words they were acting as if there were *no boundaries* between them (Duane & Berendt, 1965). This finding has been extended more recently in the work of Grinberg-Zylberbaum and colleagues (e.g., 1994) who found that there was a physically observable apparently *transferred* evoked potential between the brains of pairs of participants who had got into a state of 'deep communication' through shared meditation. This work has recently been replicated using EEG and fMRI (Standish, Johnson, Kozak, & Richards, 2003).

A shared meditative state among several minds may also correlate with anomalous physical effects. For example, Radin and Atwater (2006) recently found a greater coherence of random number generators when a group of people were experiencing a similar meditative state induced by Hemi-sync tapes. This could imply that greater social 'connectivity' or boundary thinness could allow for more psi phenomena at a cultural level. Indeed, one might expect that

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[17] In collaboration with Dr Ginette Nachman, this author will be undertaking an empirical study exploring empathy and boundary thinness in a psi experiment.

cultures based more on the idea of *community* and *connectivity* might experience more paranormal phenomena.

Boundaries as a State of Consciousness

Thin boundaries might be better considered as both state and trait. Perhaps there are states in the brain/mind/body that are conducive to experiencing particular types of psi phenomena. Such a state would hypothetically be accessible to everyone, but more *readily accessed* by those who score psychometrically 'thinner'. It may also be that this state is closer to the waking state among those who have thinner boundaries. This section will explore boundaries as a state of consciousness.

Psychometric thinner boundaries may reflect greater access to the hypnagogic state of consciousness. Hypnagogia reflects the liminal state of consciousness between wakefulness and sleep (see Mavromatis, 1987). This is an interesting, and paradoxical state which seems to reflect many forms of boundary thinness as described in the preceding section. For example, thoughts include primary process thinking, incorporating dissociation of thought and image alongside magical thinking (Vogel, Trosman & Foulkes, 1966). Hypnagogic thinking may be described as *paralogical*, where everything may be related to everything else; as such, hypnagogic thinking may reflect associational boundary thinness (Vogel *et al.*, 1966). Hypnagogic thinking may also reflect attentional boundary thinness, as fatigue is associated with an impairment of normal filtering processes.[18]

Hartmann (1991) suggests that the sleep-related neurotransmitters norepinephrine and serotonin (Hobson, 1989) are important in terms of attentional boundaries. Norepinephrine effectively serves to increase the signal to noise ratio, affecting inhibitory processes and sharpening and increasing focused attention. He notes that increasing norepinephrine and serotonin in the cortex results in a thickening of boundaries, while reducing norepinephrine and serotonin in the cortex results in a thinning of boundaries. It is also the case that when one is wide awake, serotonin and norepinephrine are increased, whilst when

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[18] Impaired filtering is also the result of the consumption of the drug LSD and stress (Reed, 1988).

one is asleep they are decreased; particularly in dreaming sleep (Hobson, 1989).[19]

Interestingly, Mavromatis (1987) has suggested that hypnagogia reflects a reduction in cortical dominance with more availability of subcortical processes to conscious awareness. Hypnagogia is also characterized by the presence of alpha waves and theta waves[20] (Tanaka, Hayashi & Hori, 1996) which themselves relate to psi phenomena (Alvarado, 1998).

The hypnagogic state is not restricted to falling asleep (Hayashi, Katoh & Hori, 1999; Hori, Hayashi & Morikawa, 1994; McKellar & Simpson, 1954). In fact, hypnagogic thinking has also been observed in relaxed wakefulness during daytime hours without any expectation that it might occur and accompanying a waking EEG reading (Foulkes & Fleisher, 1975). It may be that hypnagogic experiences occur in wakefulness among all individuals (Foulkes & Fleisher, 1975), but are more likely among those who have psychometric thin boundaries (in particular, positive schizotypy e.g., McCreery, 1997). This is relevant to understanding psi experiences as hypnagogia is associated with the reporting of both genuine ESP and pseudo psi experiences (Sherwood, 1998).

Hartmann (1991) has suggested that those with thinner boundaries exhibit more 'halfway' states of consciousness between waking and sleeping and exhibit brain activity normally localized only in REM sleep at other times. This suggestion is supported by the presence of anomalies in the arousal systems of positive schizotypes. This renders them likely to experience changes in state at any time (even during daytime hours), which results in hypnagogic interruptions or 'microsleeps' into waking awareness (McCreery, 1997). Interestingly, McCreery also notes that microsleeps may result from both under *and* over arousal (i.e., sleep response results from being tired and incredibly over-stressed). If psi experiences are associated with hypnagogia, this contributes a parsimonious explanation for superficially disparate etiologies of psi experiences in the real world. Positive schizotypy is associated with hypnagogic experiences (Jakes & Hemsley, 1987;

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[19] As noted by David Luke at the Utrecht II conference, it may well be that boundary thinness could also be addressed in terms of chemical boundaries, which would include sleep related neurochemicals in addition to chemicals such as dopamine which are associated with increasing associational thinking (as noted in the dopamine model of schizophrenia).

[20] In addition, the state is often characterized by the presence of sleep spindles.

Pizzagali *et al.*, 2001), while negative schizotypy may reflect the opposite tendency, and is associated with a *lack* of lability in the nervous system (Mason, Claridge & Clark, 1997). The idea that schizotypy might function as a state is not a new one (see, e.g., Zanes, Ross, Hatfield, Houtler & Whitman,1998) but this may be useful in terms of further understanding why psi experiences are particularly likely under an array of circumstances in the real world.

Manipulating Boundary Thinness

If boundary thinness is a state, it may be possible to encourage anomalous experiences or prevent unwanted experiences associated with this state. In terms of manipulating boundaries, it is of interest that temporal lobe lability is encouraged by meditation, hypoglycemia (prolonged fasting), fatigue, hypoxia, alterations in vascular flow associated with drugs and the biochemical effects caused by personal crises (Persinger, 1989; Persinger & Makarec, 1987). Thalbourne, Crawley and Houran add that boundary thinness may be mediated by intense emotional states and driven by activation of the limbic system, which, via connectivity with the sensory cortex can produce anomalous phenomena (Thalbourne, Crawley & Houran, 2003).

Interestingly, the work of this author demonstrated that sleep quality was a direct predictor of anomalous experiences and boundary thinness (Simmonds, 2005a). As such, a reduction in sleep quality[21] may have an impact on boundary thinness and anomalous phenomena. It is of interest to note that disturbed dream patterns were recently found to relate to scoring on dissociation (Giesbrecht & Merckelbach, 2004). This may reflect an increased tendency to experience mental imagery or dream whilst awake, which could have an impact upon daytime anomalous experiences. Simmonds (2005b), however, found no direct support for the idea that reduced sleep length impacts on anomalous experiences, although longer and shorter sleep both related to scoring on a measure of positive schizotypy. This supports previous research finding a similar pattern (Reid & Zborowski, 2006). This may imply an indirect route toward anomalous experiences, but more work is needed to explore the complex relationships between sleep, boundaries and psi experiences.

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[21] Sleep quality may be disturbed for a number of reasons, both internal (e.g., more nightmares and dreams) and external (e.g., having a baby or noisy neighbors).

Meditation may be useful for manipulating boundaries. Hemispheric synchrony has been observed in transcendental meditation in terms of frontal coherence (a measure of functional connectivity) and lateralized asymmetry, a measure of inter-hemispheric differences in power (Travis & Arenander, 2006). Hemi-sync technology (to drive meditative states) has also been purported to increase synchronization between right and left hemispheres (Atwater, 2004). In addition, meditation (Woodfolk, 1975, cited in Yardi, 2001) and Hemi-synch induced meditation (Atwater, 2004) also result in less cortical control and greater influence of information associated with sub-cortical processes on conscious awareness. The effects of meditation seem to encourage more efficient processing outside of the normal range of attention (e.g., Sudarsham Kriya yoga meditation; see Srinivasan & Baijal, 2007), which may encourage greater access to usually subliminal components of the mind. Meditation may also encourage synesthesia [22] (Walsh, 2005), which was noted previously to be associated with psi experiences. Meditation and hemi sync have also both been related to anomalous experiences, including extrasensory perception (Atwater, 2004; Palmer, Khamashta & Israelson, 1979; Rao & Rao, 1982).

It may be that consciousness itself should be considered in terms of boundaries. Several authors (e.g., Engel & Singer, 2001; see Hebert, Lehmann, Tan, Travis & Arenander, 2005, for a review) have suggested that synchronization of 40 Hz (or gamma) activity in the visual system may serve to bind the features of an object into a unified conscious experience. Recently, alpha has also been considered to be important in binding (Hebert *et al.*, 2005). It may be that meditative and other states where boundaries are 'thinned' (vertically and laterally) may allow for a *more efficient* unity of consciousness. Hebert *et al.*, for example, found enhanced EEG alpha time-domain phase synchrony during transcendental meditation. Increased efficiency may permit mystical, spiritual and possibly paranormal experiences. This was supported by findings of Don, McDonough and Warren (1998), which indicated that psi information may have reached the neocortex but not to a large enough extent to be *bound* into consciousness.

In summary, a state and trait approach allows for a richer and more parsimonious understanding of the etiology of psi experiences. It

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[22] Synesthesia may also be induced by ingestion of drugs (Simpson & McKellar, 1955)

implies that psi experiences are more likely under certain spontaneous (e.g., stress, crisis) and deliberate (meditation, drug ingestion) circumstances in everyone, irrespective of personality, but are potentially more likely among those with thinner boundaries, as these individuals are more able to experience appropriate states. This approach also allows for the development of methods for controlling/encouraging psi experiences occurring in the real world. For example, if increasing sleep quality can thicken boundaries, methods to do this may allow for a reduction in unwanted psi experiences which have become overwhelming.

Types of 'Boundary' and Spontaneous Psi Experiences

There are different types of psychometric boundary thinness, which overlap with one another in potentially different ways. For instance, only *some* subscales of boundary thinness were found to be significant predictors of transliminality (Sherwood & Milner, 2004-2005). It may be that certain forms of boundary thinness are more relevant to spontaneous psi phenomena than others.

In support of this idea, research by this author (Simmonds, 2005a) found that some psychometric measures of boundary thinness (notably transliminality and temporal lobe lability) were statistically more directly related to the reporting of anomalous experiences than other measures (boundary thinness in particular). Boundary measures also had slightly different variables as their *own* predictors. As such, psychometric measures of boundary thinness may well all be measuring aspects of boundary thinness but some might be more directly important when considering the etiology of anomalous experiences than others. This makes sense in terms of their item content, as some seem to be more directly related to actual experience, such as altered states of consciousness, whilst others seem to function at a higher level and reflect more cognitive/descriptive experiences.

In addition, relationships between different measures of boundary thinness and anomalous experiences undoubtedly reflect the item contents of the measure used to address psi experiences. Some measures are 'wider' than others, for example, the Anomalous Experience Inventory (AEI: Kumar, Pekala & Gallagher, 1994) is a broad construct containing traditionally paranormal experiences in addition to anomalous experiences such as the out of body experience and dream-related phenomena. As such, boundaries related to the

action of the temporal lobes and the biological and cognitive biases which underpin the transliminality variable may relate more directly to experiences tapped by the AEI than a more narrowly defined measure of psi experiences (Simmonds, 2005a).

If one takes a hierarchical approach to boundaries, it may be that different *aspects* of boundary thinness could be associated with different *types* of anomalous and paranormal experience. In addition, some forms of boundary thinness may be more associated with veridical psi, whilst others are more associated with pseudopsi.

Hartmann's original BQ is actually broken down into 12 types of 'boundary' (see Table 1) and it may be the case that some, but not all of these aspects of boundary thinness are relevant for understanding psi phenomena. Richards (1991) found that only four related to perceived success at an interpersonal psi task. These reflected sleep, unusual experiences, thoughts and feelings and sensitivity boundaries. Interestingly, these reflect boundaries relating to subjective experiences in states of consciousness, cognition and emotion, but not those associated with (abstract) ways of thinking and interacting with the world. It may be that abstract boundaries are not relevant for understanding psi experiences, although more work is needed to delineate specifically which boundaries are relevant for different types of psi experience.

In addition to the association between hypnagogic thinking and paranormal experiences (Sherwood, 1998) there is some support for the contribution of other sleep variables to anomalous experiences. Reduced sleep quality functioned as a direct (albeit weak) predictor of anomalous experiences in addition to measures of psychometric boundary thinness (Simmonds, 2005a). The importance of sleep boundaries in the etiology of psi experiences clearly follows from the argument that boundaries can be considered as a state of consciousness as well as a trait variable.

Other work has indicated that the Sensitivity and Children subscales of a short form of the BQ (Rawlings, 2001-2002) were also found to correlate directly with PK performance in an experimental context (unpublished data from Palmer, Simmonds-Moore, & Baumann, 2006). It may well be that some boundaries have good explanatory value, whilst others are simply not relevant for the understanding of spontaneous psi phenomena.

In terms of psychometric measures of boundary thinness, there are currently three forms of Hartmann's BQ, which may allow for different

levels of understanding regarding the relationship between boundaries and psi experiences.[23] These are the original long questionnaire (Hartmann, 1991), a conceptually derived shorter version (Kundzendorf, Hartmann, Cohen & Cutler, 1997), and a statistically derived short version (Rawlings, 2001-2002). Despite the advantages of using short forms of the boundary questionnaire, it may well be that not all types of boundaries can be adequately assessed by these shorter versions. These measures may well allow for an excellent psychometric assessment of global 'thinness' or 'thickness', but different types of boundaries may function very differently with regard to different behaviors, experiences and beliefs. For example, recent work found that a mixture of thin and thick boundaries provided the best understanding of celebrity worship (Houran, Navik & Zerrusen, 2005). It follows that a fuller picture regarding boundaries and psi experiences may only be attained by employing the full BQ (reflecting 138 items), or a selection of subscales which seem to be more directly related to psi experiences.

Much more work on how boundaries relate to psi phenomena is needed. From the author's perspective, it is clear that boundary thinness may also be broken down differently to Hartmann's original understanding, in terms of a hierarchy reflecting various levels of understanding, from psycho-physiological, cognitive, consciousness, intrapersonal experiences and interpersonal experiences. Each type of boundary may be differentially relevant to spontaneous psi phenomena. Not enough research has explored how types or levels of boundaries contribute to psi experiences. It would be of great interest for researchers to attempt to distinguish which types of boundary are more related to the pseudo psi hypothesis and which could be more related to genuine psi phenomena.

There is little work that has empirically explored different aspects of boundary thinness and their relationships to psi and pseudo psi experiences. Indeed, it is particularly difficult to test the impact of lower levels of boundary thinness (e.g., neural and functional connectivity) on psi phenomena, as these are perhaps likely to impact on both psi and pseudo psi hypotheses (as they relate to neural and cognitive architecture upon which both may arise). There is some

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[23] A similar argument can be made for the schizotypy construct, whereby different questionnaire measures reflect different conceptual understandings regarding schizotypy.

empirical support for the relative contribution of processes associated with the right hemisphere for genuine psi experience (Braud, 1975, Broughton, 1983). However, at least for subjective psi experiences, the left hemisphere may also be involved, potentially to function as an interpreter (Wolfradt, *et al.*, 1999).

There is a lot of empirical support for the observation that the right hemispheric tendency to make associations/find a signal is associated with pseudo psychic experiences related to apothenia (Brugger, 2001). However, only a few researchers (de Boer & Bierman, 2006; Simmonds & Fox, 2004) have entertained a psi hypothesis. The findings of both studies indicate that the associational tendency may well contribute to genuine psi experiences (in addition to pseudo psi experiences), which suggests that more work should be undertaken on this form of boundary thinness.

In addition, although the associational tendency is considered as a continuum (with the tendency to make a type II error at the opposite end), little or no work has systematically explored the idea that disbelievers/those with thick boundaries might miss a signal. The current author plans to explore this idea in future research. There is much need for further empirical exploration of the relationship between associational boundaries with regards to the psi as well as the pseudo hypothesis.

There is also empirical support for attentional filters in the psi process. Holt *et al.*'s (e.g., 2007) recent work found evidence that psi may be processed in a similar manner to the unattended stimulus, and as such may usually be filtered out of conscious awareness. This work also found differences in psi scoring between healthy positive schizotypy and a less healthy form of positive schizotypy (Holt & Simmonds-Moore, 2008). As such, this indicates that boundaries are complex and that healthy boundaries may well be more related to better filtering of both psi and normally perceived irrelevant information.

Recent research also indicates that empathy and social boundary thinness could also be promising for understanding genuine psi related experiences (Achterberg, 2008; Radin *et al.*, 2008). Consciousness binding, which is considered to be one form of boundary in this paper, may be important for the psi process, as referenced in the findings of Don *et al.* (1998). Interestingly, schizophrenia is characterized by its lack of gamma binding across the brain (Strelets, Novototsky-Vlasov & Golikova, 2002) whilst meditation is characterized by its enhanced coherence across the brain. Meditation is associated with above chance

ESP (Palmer, Khamashta & Israelson, 1979; Rao & Rao, 1982), whilst schizophrenia is associated with feeling like one is being psychic, whilst actually scoring at chance (Greyson, 1977). Here, we might learn more about ESP by exploring boundaries of consciousness in these two contrasting situations. Hartmann (1991) noted that healthy individuals actually have a mixture of thin and thick boundaries, and that boundary thinness can be altered.

Only by careful and systematic further research will the true nature of the various relationships between different forms of 'boundaries' and spontaneous cases of psi be further understood. This is important, as manipulation of different forms of boundary thinness may then be possible. This could lead to greater control over psi experiences, which has relevance for clinical psychology as well as parapsychology. The implications of greater knowledge of the etiology of psi experiences from a boundary perspective could be that who are overwhelmed with subjective psi experiences may develop protective practices to 'thicken' relevant boundaries, whilst those who are interested in having more psi experiences may learn to 'dip' into psi states via thinning relevant boundaries. The next section discusses mental health and thin boundaries.

Profiles of Personality, Mental Health and Spontaneous Psi Experiences

As noted earlier, the traits associated with schizophrenia-proneness or schizotypy are distributed as a continuum (or continua) within the general population. It was also noted that there are two competing perspectives on how to understand such a continuum—the fully dimensional personality approach (Claridge, 1997) versus the quasi-dimensional taxon approach of the Chapmans and others (e.g., Claridge & Beech, 1995). Both models acknowledge the overlap between schizotypy and psychopathology. However, the personality approach considers that traits can also be associated with mental health/adaptiveness, whilst the taxon approach considers that traits reflect an association with psychopathology. For example, the taxon approach acknowledges the existence of watered down versions of schizophrenia (e.g., that the relatives of schizophrenics are characterized by other forms of psychopathology, in particular affective and bipolar disorders) but consider that this reflects diluted forms of psychopathology, rather than being a *personality* dimension. In support

of this, Nettle (2006) found that the mean score on schizotypy scales is indeed higher for those with non-psychotic affective disorders, schizophrenia and bipolar disorder in comparison to those who have no history of psychopathology. However, this in itself does not imply that schizotypal traits *per se* are equivalent to psychopathology.

In fact, there are adaptive and less adaptive components associated with positive schizotypy. For example, in the same study, Nettle (2006) found that highly creative individuals had scores on schizotypy which overlapped with the scores of those suffering from schizophrenia. Jackson (1997) found a similar pattern with regard to spiritual experiences, schizotypy and schizophrenia in his research. The benign or 'happy schizotype' was described earlier as someone who scores high on positive schizotypy and who is psychologically healthy in spite or because of the traits they possess, exhibiting spirituality and meaningfulness in life (Jackson, 1997) or extreme creativity (Brod, 1997; Holt, Simmonds-Moore & Moore, 2008; Nettle, 2006).

It is argued here that a more comprehensive understanding of boundaries, (in particular those associated with positive schizotypy) mental health and spontaneous psi experiences can be elucidated by addressing personality profiles/clusters. Although positive schizotypy is the factor that is most relevant to parapsychology, there are three other factors of schizotypy that are pertinent to understanding both psi experiences and mental health.

Recent work in personality research and parapsychology has focused on exploring clusters of schizotypy (Goulding, 2004, 2005; Holt, Simmonds-Moore & Moore, 2008; Loughland & Williams, 1997; Williams, 1994). Just as a factor analysis works by grouping variables that share common variance in a data set, a cluster analysis groups individuals or objects into 'clusters' according to their responses on a particular set of variables, such as the four subscales comprising schizotypy (see Hair, Anderson, Tatham & Black, 1998). Of interest for the current paper, cluster analysis of schizotypy dimensions has resulted in three or four clusters[24] (Goulding, 2004, 2005; Holt, Simmonds-Moore & Moore, 2008 Loughland & Williams, 1997, Simmonds, 2003; Williams, 1994). These reflect the following groups;

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[24] Clusters differ according to the variables put into the analysis, and differences in pre-specification of the cluster solution. Work by Simmonds and colleagues has included impulsive nonconformity in the analysis, whilst other research has not considered this variable to be relevant to the schizotypy construct.

happy schizotypy (score high on positive schizotypy but low on all other measures), low schizotypy (score low on all measures of schizotypy), high schizotypy (score high on all measures of schizotypy) and negative/disorganized schizotypy (score high on negative schizotypy and cognitive disorganization). Of interest to the parapsychologist are the two clusters with high scoring on positive schizotypy: happy schizotypy and high schizotypy. Both types of positive schizotypy have a strong relationship with paranormal belief and experience (Holt, Simmonds-Moore & Moore, 2008). However, the high scorer on positive schizotypy with the happy schizotypy response profile is more psychologically healthy than the high scorer on positive schizotypy who has the high schizotypy response profile (Goulding, 2004, 2005; Holt, Simmonds-Moore & Moore, 2008). As such, there are healthy and less healthy forms of boundary thinness, and the presence of negative (and other) traits seems to have an impact upon mental health. Other research indicates that possessing negative schizotypal symptoms alongside positive is associated with psychopathology. For example, Chapman, Chapman, Kwapil, Eckblad and Zinser (1994), found that although magical ideation was related to psychotic breakdown, this was more likely if one had negative in addition to positive traits of schizotypy. Nettle (2006) noticed that creative individuals who practiced poetry and art were characterized by scoring high on positive aspects of schizotypy but with a lack of negative symptoms (no anhedonia and avolition). In addition, Zanes *et al.* (1998) found a healthy and unhealthy form of positive schizotypy whereby inconsistent high scoring was related to creativity, while consistent high scoring was related more to psychopathology. Schofield and Claridge (2007) recently found that the relationship between paranormal experiences, schizotypy and mental health was underpinned by the cognitive disorganization factor; the relationship between positive schizotypy and pleasant paranormal experiences was associated with being cognitively organized, whilst the relationship between negative schizotypy and distressing paranormal experiences was associated with being cognitively disorganized. Interestingly, recent work has indicated that the healthy schizotypy cluster is also associated with better psi performance than other groups (Holt & Simmonds-Moore, 2008).

Recent research has employed several scales designed to measure positive schizotypy. This has often focused on use of the magical ideation scale (Eckblad & Chapman, 1983), although other work has

used other scales (e.g., the STA and OLIFE) and scales associated with the boundary construct. It is clear that each personality scale has different theoretical underpinnings and differs from one another despite all being concerned with 'schizotypy'. However, much research in parapsychology which explores the relationship between positive schizotypy/boundary thinness and psi experiences continues to employ unidimensional scales. This is problematic, as the happy schizotype can only be truly identified by filling out a multidimensional battery of schizotypy scales, such as the OLIFE (Mason, Claridge, & Jackson, 1995), or including a measure of mental health/wellbeing. If the bias toward unidimensional approaches continues, the two types of positive schizotype will not be distinguishable, when they reflect different personality types. As such, this author disagrees with Thalbourne (1999) that transliminality and schizotypy are one and the same. This is because both types of positive schizotype are characterized by thinner boundaries, which would be subsumed within any unidimensional measurement. Perhaps research should focus on the development of a scale which could quickly assess negative and positive boundaries. Unidimensional measures such as transliminality would then become useful in terms of distinguishing the types of scorer likely to be mentally healthy, creative, and psi conducive, versus those who are mentally less healthy, not creative, and not psi conducive. In summary, the relationship between thin boundaries and psi is more complex than it superficially appears to be, with two forms of boundary thinness. Multidimensional scales should be employed or shorter scales developed which allow for the assessment of psi conducive and non psi conducive boundary thinness (and mental health). Ways of encouraging healthy boundary thinness could also be explored, given that boundaries can be manipulated.

Evolution, the Schizophrenia Gene and Psi

This section will take an evolutionary perspective on boundary thinness and argue that, in a watered down form, traits associated with genetic schizophrenia (thinner psychological boundaries) may bestow biological, psychological and social advantages to their holder. Specifically, it will be suggested that the tendency to access boundary thin states (and therefore the anomalous experiences associated with those states) may be associated with the schizophrenia gene(s) (as positive schizotypy). As such, the tendency to experience psi

phenomena may be an indirect association with the schizophrenia gene(s). By arguing for an indirect selection of psi, the current argument extends previous arguments regarding the evolution of psi *per se*, as well as strengthening the argument for the healthy schizotype. It may also allow for greater acceptance of psi experiences in mainstream psychology and science.

It may well be that positive healthy schizotypy was selected by natural selection for its benefits to the individual and to society, which include creativity, religious experiences and paranormal (psi) experiences. It was noted earlier that positive healthy schizotypy (in the absence of negative traits) reflects a profile of mental health rather than ill health (Goulding, 2004, 2005; Holt, Simmonds-Moore & Moore, 2008), a tendency to be highly creative (Brod, 1997; Nettle, 2006), highly spiritual (e.g., Jackson, 1997), to report more psi experiences and a tendency to perform well on an ESP task (Holt & Simmonds-Moore, 2008). An evolutionary argument will be made for the selection and maintenance of psi experiences via an association with the schizophrenia gene (as positive schizotypy).

Why Does Schizophrenia Remain in the Gene Pool?

Several authors (Brüne, 2004; Burns, 2006) have noted the paradox associated with the idea that the schizophrenia gene(s) might be selected by evolution. Schizophrenics have an increased likelihood of suicide/early death, less likelihood of reproducing (i.e., reduced 'fitness'), and suffer greatly (as do their friends and families) at the hands of the disease. However, schizophrenia is common to all human cultures, and exhibits similar rates of incidence (1%). This implies that it may have occurred at a similar point in time, when it then spread around the globe. There is also evidence of a genetic influence on schizophrenia (e.g., in terms of twin and family studies) indicating that rather than being 'selected out' something is consistently inherited across generations (Burns, 2006). As such, the implication is that there might be something about the genetic makeup of schizophrenia which persists in the gene pool despite all the negative consequences for the schizophrenic individual (and family)[25]. It has recently been argued that the key to understanding why schizophrenia remains in the gene pool

[25] Brüne suggests that this could reflect the survival value of the genes themselves (genetic polymorphism) or via linkage to other advantageous genes (pleotropy).

lies in the relationship between schizotypy and creativity (e.g., O'Reilly, Dunbar, & Bentall, 2001). In this section, this idea will be extended to encompass mystical, spiritual and psi experiences (of which at least some may be genuine).

In order to be fit an evolutionary argument[26], there are a number theoretical considerations that boundary related traits should meet. 'Natural selection' refers to the tendency for those displaying traits that are associated with better survival benefits in a given environment (more food, better success at mating, longer lives) to be more likely to be picked as a mate. The offspring of those with the successful traits will also inherit them, and natural selection of that trait is then said to have taken place. As such, to be selected, traits should be genetically based (run in families); associated with an adaptation to the environment at some point in the history of humankind (otherwise known as an ultimate cause) and/or reflect a current level of adaptiveness (a proximate cause). According to Sanderson (2008), *adaptation* refers to the natural selection of a trait which promoted survival and reproduction at some point in our evolutionary history, while *adaptiveness* reflects the 'current usefulness' of any particular trait (p. 5). These are different concepts, as a psychological trait or physical organ could have evolved, but no longer be useful in the modern era[27]. A selected trait will be associated with increased fitness (which refers to the number of healthy offspring produced). Finally, behavior which is consistent with an evolutionary perspective will also favor those carrying similar genes (i.e., close family members) and will be biased toward survival of the individual and his or her genes.

Adaptation—Ultimate Causation

It has been argued that those in possession of the schizophrenia gene, who are unaffected by disease may have a reproductive or cultural advantage (see Burns, 2006 for a review and critique of theories of ultimate causation of schizophrenia[28]). There is some

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[26] Barrett, Dunbar and Lycett (2002) present an excellent discussion of theories and observations from the perspective of evolutionary psychology.

[27] Sanderson gives the example of the human appendix which must have been an evolutionary adaptation at some point in the history of the human being, but is no longer adaptive as it currently seems superfluous to the functioning of the human body.

[28] Burns notes a variety of theories, including physical protection of unaffected relative, social advantages, group selection models – which allow for the

support for the idea that schizophrenia may have been an adaptation; arising for some reason in the history of humankind which has relevance for psi experiences. For example, schizophrenia is associated with problems in consciousness, language and thought (Birchwood & Jackson, 2001). Davis and Lowell (2006) propose that solar activity may have instigated a genetic mutation which produced a brain capable of consciousness and thought and that the genetic instability associated with the adaptable, *creative* brain also produced disease. Others have argued that the evolution of schizophrenia is associated with cerebral asymmetry and language (Crow, 2008b). Burns, however, notes that schizophrenia may actually reflect variability in adaptiveness which allowed for the development of the social mind (he considers conscious experience to be fundamentally socially derived). As such, schizophrenia may reflect a kind of 'costly by-product' of the evolution of the social brain among humans (Burns, 2006). This is of interest for those interested in boundaries and psi experiences on two counts; firstly as psi experiences are often associated with social experience and secondly, that the neural circuits underpinning social cognition reflect an increased level of cortical (and subcortical) connectivity. That mental illness is a recent evolutionary occurrence is supported by findings in neuroscience reflecting the presence of susceptibility genes (DISC1) for schizophrenia in humans and monkeys but watered down or absent forms in animals purported to be less highly evolved (Bord, *et al.*, 2006).

Adaptiveness and Creativity

Several authors, (Nettle, 2006; Nettle & Clegg, 2006; O'Reilly *et al.*, 2001[29]) have explored the idea that creativity may serve as an adaptive advantage which is associated with schizophrenia in the modern era. For example, O'Reilly *et al.*'s (2001) review of the literature indicates both increased creativity and participation in creative professions among relatives of those suffering from mood disorders.

It has been suggested (see Nettle & Clegg, 2006) that artistic creativity may serve as what is known as a 'costly signal' in

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creation of new adaptive outgroups in line with beliefs of schizotypal charismatic leaders.

[29] O'Reilly et al (2001) present an excellent discussion of the argument for an adaptive component to the schizophrenia gene.

evolutionary terms[30]. This refers to the observation that female sexual selection may sometimes focus on male traits that are aesthetically pleasing, but potentially 'costly" in terms of their upkeep.[31] The trait and the preference are understood to be genetically determined in males and females, respectively, and there is an observable increase in the offspring produced by those with costly signals. If artistic creativity works as a costly signal, then we would expect to see more offspring among those who are highly artistic (in terms of their production). In the modern era evolutionary psychologists have looked to number of sexual partners as an index of fitness.

Interestingly, Nettle and Clegg (2006) found that not only is creativity related to evolutionary fitness, but that this is underpinned by positive schizotypy (and by implication, any underlying genes). They found that positive schizotypy predicted creativity[32], which in turn predicted the number of sexual partners. Impulsive nonconformity directly predicted the number of sexual partners (probably due to their reckless and carefree lifestyles, incorporating a liberal attitude toward sexuality). Interestingly, introvertive anhedonia negatively predicted both creativity as well as the number of sexual partners. This implies that healthy/benign positive schizotypy (particularly characterized by the absence of negative traits) may also relate to increased evolutionary fitness (more sexual partners) whilst scoring high on introvertive anhedonia (negative traits) is related to reduced fitness (fewer sexual partners).

However, natural selection does not work on the gene itself, but rather on the observable features coded by the genes and their interaction with environmental factors (the thin boundaried phenotype). Sanderson (2008) notes that adaptativeness may sometimes reflect a

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[30] See Barrett, Dunbar and Lycett (2002) for a good discussion of concepts and issues in modern evolutionary psychology, including the notion of the costly signal.

[31] One of the most common examples of this is in the peacock's tail, which is vastly heavier and more elaborate than its female counterpart, and may be quite a heavy burden to the bird. The tail is, however, a sought-after commodity among females seeking a mate, as it signals that the male is of good genes and can survive despite the potential disadvantages associated with having such a large and elaborate tail to drag around.

[32] Here I mean prediction in a *statistical* sense – i.e., as a result of a regression analysis. Regression addresses correlation rather than cause, i.e., we cannot infer that one definitely causes the other.

by-product of other (cognitive) developments. Creativity is associated with the perceptual-cognitive system associated with having thin boundaries/scoring high on positive schizotypy. It may well be that this pattern could translate to other experiences, associated with having thinner boundaries, either directly or indirectly (e.g., via creativity). Interestingly, religious experiences are also associated with positive schizotypy (Day & Peters, 1999; Maltby, Garner, Lewis & Day, 2000) and also seem to reflect an evolutionary adaptation (Sanderson, 2008[33]). Psi experiences may also reflect evidence of evolutionary selection.

Evolution of Psi?

Several researchers have considered whether ESP might reflect an evolutionary adaptation/function (Broughton, 1988; Taylor, 2003; Thouless & Wiesner, 1946). Indeed, this type of approach would certainly contribute toward answering the question regarding why psi phenomena might exist (Broughton, 1988). Broughton (2006), for example, considers that "evolution tends not to devise new systems where existing systems can be adapted and extended to serve new needs and confer new advantages" and that with ESP, this may reflect an adaptation of the emotion and memory systems. There is some evidence supporting an evolutionary explanation of psi phenomena. Indeed, in terms of evolutionary considerations, psi experiences may well have a genetic basis (Taylor, 2003), and are more likely among certain families (Cohn, 1994; Schilling & Mora, 2008). If Hamilton's idea of inclusive fitness is considered[34], we would be more likely to try to save family members (in particular, those who we share more of our genes with) when they are under threat. This may translate to psi experiences, where such experiences should occur more among kin, or in situations where survival of kin may be under threat. Stories of psi experiences relating to kin are common among those who are genetically related to one another (Rhine, 1961). In addition, biologically related pairs have also been found to perform better on a psi task than unrelated pairs (Alexander & Broughton, 1999).

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[33] Sanderson notes that in modern societies, religious belief is associated with better physical and mental health, and seems to be particularly associated with reducing stress. Religion is associated with promotion of family life and there are greater numbers of offspring than those who are less religious or atheistic.

[34] There is a lot of evidence in support of this in evolutionary psychology; see Barrett, Dunbar and Lycett (2002).

Other physiological work in parapsychology, e.g., the presentiment effect (Radin, 1996) indicates that psi may also be associated with an early warning system, with the autonomic nervous system responding precognitively to emotional targets that are associated with the fight-flight response. In support of an evolutionary explanation, violent stimuli resulted in stronger effects than those associated with sex (Bierman & Radin, 1997) which implies a greater early warning advantage in escaping a predator compared to having an opportunity to propagate ones genes. Psi could therefore reflect a perceptual function which is biased toward biological survival of the individual and his or her genes (as kin). Broughton (2006) notes that humans are by nature 'future-oriented', using information to plan future actions.

However, psi experiences do not appear to be increasing within the population at large (Taylor, 2003) and so do not appear to be directly selected. Taylor (2003) suggests that psi is biologically need-serving and proposes a theory titled "Evolution's Need Serving Psi" to describe how it might function within an evolutionary context. He notes many problems with other need-serving understandings of ESP, and argues that ENSP suggests psi is associated with an unconscious (but not constant) scanning of the local (and non-local) environment. This idea has also been postulated by other researchers (e.g., Carpenter, 2004).

Psi might usually be filtered out of attention by the highly developed action of the cerebral cortex. Information that is associated with survival may reach conscious awareness more easily than other information. Interestingly, scanning is also the job of the amygdala, which has been found to respond most efficiently toward events which are associated with survival (it reacts more to emotions of surprise and fear and the presence of blood than those of sadness and happiness; see Whalen, 1998). It has also been noted (Broughton, 2006) that the relationship between evolution and psi may be observed in its inherently emotional nature. Broughton (2006) suggests that ESP may have adapted existing brain systems and seems to be based on the emotional and memory systems (this reflects the limbic system and how memory records are accessed). This is of interest given that thinner boundaries are associated with a greater awareness of processes associated with sub-cortical structures and with a looser (associative) cognitive style. Might those with thinner boundaries be selected due to their greater ability to survive?

Indirect Evolution of Psi

Although psi may well occur in an evolutionary context, there are problems with the notion that psi was directly selected via evolution (Taylor, 2003). Psi ability may have been indirectly selected via evolution due to its relationship with the cognitive perceptual features associated with the schizophrenia gene (as thin boundaries). The idea of an indirect selection of anomalous abilities has previously been proposed by Winkelman (2000, cited in Sanderson, 2008) and McClenon (2004). Shamanistic practices emerged at the same point in human history, and may reflect the first religion; a "universal adaptation to biocognitive potential" (Sanderson, 2008, p. 8) reflecting an interaction between brain development and the demands of the social and physical environment. Winkelman observed that the rituals associated with shamanism, namely the altered state of consciousness is potentially adaptive as it allows for healing (and other) processes via the relaxation which results from being in the state. McClenon's (2004) Ritual Healing Theory suggests that the human capacity for dissociation, (as evidenced among those who score high on boundary related personality dimensions), developed as a coping mechanism and means for accessing relaxation-facilitating altered states of consciousness in certain rituals (particularly associated with Shamanism). Dissociative ability (which is associated with boundary thinness) may have been selected via evolution, in order to allow for altered states of consciousness and transpersonal experiences, including divine and paranormal experiences among them[35]. McClenon (2004) has suggested that there are biological and social benefits associated with having better dissociative ability, which led to an increase in the genes associated with dissociation and hypnotic ability. Dissociative ability would allow for altered states of consciousness, which enables healing. It also allows for the expression of a range of anomalous phenomena, including psychokinesis (PK) and other phenomena which became tied up with Shamanism. McClenon (2004) notes that altered states of consciousness may also drive genuine psi phenomena in a Batcheldorian manner (e.g., see Isaacs, 1984). The observation of apparent anomalies (it does not matter whether they are 'real' or not) would allow for belief (or suspension of disbelief) in anomalous experiences. This may, in turn, drive genuine physical phenomena, via

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[35] Daniels (2005) for example, has chronicled the types and the contexts of all possible transpersonal experiences, of which there are 25 in total.

access to a pro-PK state of mind, even when the original stimulus for that belief might have been faked (as in many rituals). Dissociation driven removal of disbelief might also drive genuine healing experiences via positive effects on the immune system and pain relief. There is some support for the Ritual Healing Theory, in particular, that hypnotizability may lead to transliminality which may then lead to anomalous experiences (Cooper & Thalbourne, 2005).

To date, there is no research which directly addresses the relationships between schizotypy, psi experiences and evolutionary fitness. Such evidence would contribute greater understanding to psi experiences, the existence of the thin boundaried personality, and the maintenance of the 'schizophrenia gene' in the gene pool.

Interactionism and Situationism: Their Impact on Spontaneous Psi Experiences

Interactionism refers to the observation that, despite some consistency in trait measurements of personality, people are not always consistent in their behavior across situations, or longitudinally across time. As such, in many ways, we might become different personalities according to different situations, and the motivations and appraisals we make of those situations at different times in our life. For example, Friedman and Schustack (2006) note "there is no reason to be disappointed or even surprised that a highly extraverted person may be sitting silently, avoiding a party; or that a highly neurotic person is sitting in a composed, poised manner, calmly comforting a child" (p. 383). In terms of personality and spontaneous psi experiences, we can apply this in terms of a complex combination of variables, which will serve to elicit boundary thinness or thickness. As we have observed earlier, boundaries are not fixed, with any given person being able to access a boundary thin state. Trait boundary thinness may reflect a natural tendency to enter such states. However, this tendency may be affected by a number of other social, psychological and environmental variables. Three such considerations for interactionism will be considered here, although this list is not exhaustive.

Interactions between State and Trait

Individuals in the real world may have different experiences according to a complex interaction between their pre-existing tendency to be thin or thicker boundaried and the conditions under which they

experience 'psi'. Given that there are individual differences in preference for, and likelihood of experiencing different states of consciousness according to personality, it may well be that psi experiences are possible or more likely in the waking state in some and in altered states in others (Tart, 1969). Spontaneous experiences should therefore be classified according to personality and state of consciousness to ascertain who is more likely to experience psi phenomena and under which circumstances. For example, it may be that some people (e.g., those with thicker boundaries) might only ever experience psi under extreme circumstances (e.g., stress), whilst others (with thinner boundaries) might regularly report a full range of psi experiences including some which might be more mundane. As those with thinner boundaries are prone to hypnagogic microsleeps (McCreery, 1997), they may be more likely to report anomalies where they *feel* subjectively wide awake. Further research could assess subjective states of consciousness and the context and content of experiences in relation to personality and psi experiences. It may be that those who are thicker boundaried might experience anomalous phenomena in situations they can readily attribute to other (more normal) explanations, e.g., 'I was lying in bed; therefore I must have been asleep or dreaming'. Thin boundaried individuals, on the other hand, might be walking or driving when such anomalies occur, given that hypnagogic experiences can interject into waking consciousness. As such, experiences may seem more real and less attributable to a normal explanation.

Psi experiences should also be monitored with regard to personality and the time of day when they occur. It would be interesting to note whether such experiences are more likely at certain times in the day (e.g., in the afternoon, when hypnagogic experiences might be more likely). Rossi (1986), for example, has noted that every 90 minutes, there is an increased tendency to experience dream-like mentation (as ultradian rhythms) in the daytime. Ultradian rhythms reflect 'trance like' experiences in a variety of modalities which may not be as pronounced as during nocturnal sleep. It may be that psi experiences may be more likely to occur during these cycles, particularly among thinner boundaried individuals. A diary study could illuminate our understanding of the relationships between personality, state of consciousness and psi.

It may also be possible to rate subjective experiences in terms of their level of apparent impressiveness and ascertain which may be more

likely to relate to genuine ESP in the real world. This way, an assessment could be made with regard to the relationships between spontaneous cases of psi, their 'impressiveness', the emotions/ subjective state of consciousness associated with them, and underlying personality. Innovative work by Holt (2007) has explored psi experiences occurring in the real world from an experience sampling approach. A diary study incorporating this approach might allow for a more in depth exploration of the relationships between personality, state of consciousness and subjective psi experiences.

In the laboratory, individual differences in state preference for psi may also have contributed to variability in whether psi is demonstrated in experiments such as the ganzfeld. The use of certain sample groups with individual differences in psi state preference may have resulted in the inconsistent outcomes in the ganzfeld literature since the time of the original Honorton PRL series. Some participants may score very well while others score below chance, thus canceling each other out and resulting in a null psi score overall. Considering individual differences alongside different states of awareness may help to explain the null outcome of Milton and Wiseman's (1999) meta-analysis. It may also help to explain the reporting of psi experiences among spontaneous cases in both waking and borderline states.

The interaction between the trait likelihood of entering boundary states (as schizotypy), the experimental manipulation (the ganzfeld, as a means to manipulate boundaries) and psi performance was explored by the author (Simmonds-Moore & Holt, 2007). It was expected that there would be an interaction between personality trait (schizotypy), state of consciousness (manipulated by experimental condition) and psi performance. Despite no significant psi effects overall, there was an inverse pattern for positive (and negative) schizotypes according to the state of consciousness the experiment was encouraging (a waking state ESP task and a ganzfeld). A cluster analysis of scoring on the personality scales and comparison of psi-scoring between groups indicated a weak and non-significant interaction pattern reflecting marginally better psi-performance among negative schizotypes in the ganzfeld compared to the waking state and marginally better psi-performance among positive and low scoring schizotypes in the waking state. Other work (Stanford *et al.*, 1988) has explored how absorption and extraversion variables may interact with components of the ganzfeld experiment, and found that absorption variable was unaffected

by white noise, while introverts were distracted by noise. This may also have an impact on the aetiology of experiences in the real world.

More work should explore how personality may interact with states of consciousness in terms of the types, content, and impressiveness of psi experiences reported.

Interactions between Gender, Personality and Psi Experiences

Psi experiences are commonly understood to be more associated with females than males. For example, in representative surveys of paranormal experiences, women often report more experiences than men (c.f. Blackmore, 1991), however, this difference is not always significant (Palmer, 1979). This tendency may reflect an interaction with psychometric boundary thinness, such that females are more likely to have thinner boundaries than males (Hartmann, 1991). A similar pattern is observed with the positive schizotypy variable, where females score higher than males (Lipp, Arnold & Siddle, 1994; Mason *et al.*, 1995; Mason, *et al.*, 1997; Miller & Burns, 1995).

Specifically, females exhibit thinner boundaries in terms of hemispheric processes where there is increased homogeneity and less lateralization of the temporoparietal regions and linguistic representations in the non-dominant (right) hemisphere (Persinger, 1994). As such, women have been proposed to be more prone to anomalous experiences associated with the intrusion of right hemispheric processes into the left hemisphere. Persinger (1991) considers that such experiences are fewer in normal men because the incidence of inter-hemispheric coherence is less frequent and the boundaries between the hemispheres less 'permeable'. It may be that these experiences are likely among men who score higher on one (or more) psychometric measures reflecting boundary thinness. Future research should focus on males who report more psi experiences, in order to explore these potential interaction effects.

It may also be that paranormal experiences are more associated with having a feminine psychology/thinking style, rather than being female *per se*. This tendency may relate to boundary thinness and impact upon likelihood to report spontaneous psi phenomena. However, anomalous experiences correlate with masculinity as well as femininity (Spinelli, Reid & Norvilitis, 2001-2002). As such, it may well be that gender role, which may actually reflect a fusion of both masculine and feminine forms of thinking (as androgynous or undifferentiated gender role—see Simmonds-Moore & Moore, in press) has an impact on both

boundary thinness and psi experiences.[36] The lack of relationship between gender role and boundary thinness in Simmonds-Moore and Moore's study, could reflect the use of a shorter psychometric measure of boundary thinness and the use of women only as participants. Future work might further explore these relationships, which could imply that society could function to thin or thicken some psychological boundaries.

Interactions between Trait and Environment

Traditional understandings of interactionism might need to be extended/expanded for parapsychology. For example, to date, research has not fully explored the interaction between personality and geomagnetism with regard to anomalous experiences. Many correlations in parapsychology (and mainsteam psychology) do not consistently hold up. This may be because many researchers take a unidimensional approach; by considering one variable and correlating it with a second variable (subjective paranormal experience or ESP performance), without considering how other variables might moderate the relationship. Geomagnetism, for example, seems to correlate positively with PK performance (Braud & Dennis, 1989; Chauvin & Varjean, 1990; Gissurarson, 1992:, Nelson & Dunne, 1986) and episodes of poltergeist activity (Gearhart & Persinger, 1986) and negatively with ESP experiences (Persinger & Schaut 1988) and psi performance (Persinger & Krippner, 1989).[37]

However, GMF may relate differently to psi experiences according to personality type of the participants in the study. For example, Radin, McAlpine & Cunningham (1994) found that among normal participants there was a negative correlation between geomagnetism and ESP hit rate. However, among creative participants, the direction of this correlation was reversed such that there was a higher hit rate when geomagnetism was higher. Palmer *et al.* (2006) also found a relationship between boundary thinness, GMF and psi (as PK), such

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[36] This study focused on women only, and found that androgynous gender role related to both paranormal belief and anomalous experiences. Psi performance was non significantly stronger among those with an undifferentiated gender role category.

[37] These effects may also be mediated by local sidereal time (e.g., Spottiswoode, 1997).

that the PK effect was greater among thin-boundaried participants when the GMF box was switched off.[38]

These patterns may be associated with the lability of the limbic system in thin boundaried individuals, which is sensitive to geomagnetic disturbances (Persinger, 1998). In addition, it seems that exposure to geomagnetism in excess of 30 nT immediately following birth relates to increased scoring on temporal lobe lability in human males (Hodge & Persinger, 1991). As such, boundary thinness itself may be influenced by environmental factors. Jawer's (2005) recent survey work supports the notion of an 'environmentally sensitive' individual, who has thinner boundaries and may be more affected by a range of environmental factors. However, recent work which experimentally manipulated environmental factors (including infrasound and geomagnetism) did not support a relationship between temporal lobe lability and increased environmental sensitivity in the aetiology of experiencing 'haunt' experiences (French, Haque, Bunton-Stashyshyn & Davis, in press).

A similar argument, which fits in with an evolutionary perspective on boundary thinness, has been postulated in terms of the relationship between solar activity, creativity and adaptability (Davis & Lowell, 2006). Solar cycles were found to correlate with the incidence of mental illness and creativity, which the authors consider reflects an interaction between solar activity and embryonic tissue. It could potentially be that these individuals are more sensitive to such activity.

It may be that melatonin in the pineal gland, may react to solar flares or the increased level of light on the earth (as one of the many correlates of increased geomagnetism). This pigment is particularly sensitive to light and has been found to correlate with subjective paranormal experiences (Persinger, 1988). In general, pineal gland activity seems to be affected by geomagnetism and has been suggested as important in the psi process (Roney-Dougal & Vogl, 1993). It is also related to the activity of the limbic system.

Solar activity, geomagnetism and melatonin should perhaps be considered when exploring the relationship between boundary thinness and psi. It seems that those with thinner boundaries could have a different relationship to ESP and other experiences relating to subjective psi phenomena compared to those who score thick or

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[38] This was the pattern across all participants, but greater among those with thinner boundaries.

average. More work is needed to further understand interactions and their relevance to genuine and pseudo psi experiences in the real world.

Conclusions

This paper has introduced and explored the notion of boundary thinness as both a personality type and a state of consciousness which seems to be associated with increased reporting of spontaneous psi phenomena (both in terms of ESP and PK phenomena). As these are taking place in the real world rather than the laboratory, these phenomena reflect a mixture of pseudo-psychic and potentially genuine psi phenomena. Boundaries reflect the functional/anatomical level of connectivity between neural, biological, and cognitive structures in the mind-brain and the extent to which conscious experiences are experienced as fused or separated. Boundary thinness may also be observed interpersonally at social and cultural levels. All levels of understanding allow for increased insight in the understanding of both spontaneous pseudo psi phenomena in addition to phenomena which may reflect more genuine psi processes. Boundaries are therefore an important construct for both psychologists and parapsychologists. Several superficially disparate personality types may actually map onto the thin boundary personality construct. As such, this allows for a parsimonious understanding with respect to why many variables relate to psi experiences. It is also the case that boundaries can be understood as a state (which itself can be accessed via under *or* over arousal), which may be more likely among those who score high on boundary-related personality variables. As such, boundary related approach also allows for greater parsimony in understanding why disparate states and situations all seem to relate to psi experiences.

In terms of personality, this paper focused particularly on the construct of positive schizotypy, which is understood to be related to the schizophrenia gene. Evidence was reviewed that indicates that there are two types of positive schizotype—the healthy positive schizotype (who scores highly on positive schizotypy in isolation) and the high scoring schizotype (who scores highly on positive, negative, disorganized and impulsive types of schizotypy). Both types of positive schizotype report increased levels of paranormal experience and belief, but only one of these is associated with genuine ESP and a profile of positive mental health, whilst the other scores at chance on ESP and demonstrates a profile of poorer mental health. As such, boundary

thinness cannot simply reflect the question of being simply 'thin' or 'thick'.

The relationship between psi experiences and the schizophrenia gene were explored an argument made that one of the reasons why schizophrenia may remain in the gene pool is due to the relationship it has to engendering psi conducive states of consciousness, in particular, among those who are healthy high scorers on the positive schizotypal personality dimension. Indeed, evidence exists in favor of the selection of healthy schizotypy in terms of increased fitness. However, positive schizotypy is complex, and profiles of scorers on this and the other factors of schizotypy are necessary in order to delineate the type of schizotype. It is also necessary to delineate whether psi is a direct or indirect consequence of boundary thinness. For example, it may function via dissociation or creativity (or both).

Boundary thinness must also be considered from the perspectives of interactionism and situationism. This is to say that each person has an individual personality profile in terms of boundaries, which will differentially relate to spontaneous psi phenomena according to situational factors, including how trait and state interact according to the demands of a particular situation (or experiment). This also includes factors associated with the physical environment, such as geomagnetism. Geomagnetism and solar activity may relate more to boundary thin personality types as these have been argued to drive areas of the brain which are more labile. It may also be that solar activity was associated with driving the genetic mutation which led to schizophrenia and consciousness in the first place. As such, personality types related to this gene could be particularly sensitive to these environmental fluctuations. There is room for much research in this area. In particular, the relationship between infrasound and personality has not yet been fully explored.

It is important that boundaries are contextualized in terms of the type of boundary thinness one has, and the situation within which one finds oneself. It must also be considered that there are various types of boundaries within the cognitive-perceptual hierarchy and that a completely open system (total boundary thinness) may be less prone to spontaneous psi phenomena than one which reflects a combination of thin and thick processes. Researchers should be seeking to identify which boundaries are more related to genuine psi phenomena whilst which might be more related to pseudo psi phenomena. Some boundaries may also be more related to certain forms of psi

phenomena. Equally, we should be addressing which types of boundary thinness might relate more to mental health and which to mental ill health. Some of this has been elucidated by recent research on attentional boundaries by the author.

Finally, following consideration of state boundary thinness and thickness, and the identification of a healthy and unhealthy form of boundary thinness, researchers should also be looking to understand and develop the means to elicit control over access to boundary related states of consciousness (and psi experiences). This may also enable a mapping of knowledge from the discipline of parapsychology into clinical psychology as well as into mainstream academic psychology.

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GENERAL DISCUSSION

ROE: We will open up the floor to more general questions or comments. I'd like to start things by noting that for me a common theme that ran across all three talks this afternoon was the emphasis on the importance of investing the experience with personal meaning; with schizotypy that may be a trait of personality in that you *over*-invest, and are more likely to attribute meaning where perhaps there isn't any, and personal meaningfulness seems to be a common feature that permeated the spontaneous experiences. Although meaning was mentioned this morning, I don't think that it had that same kind of power in comparison. I wonder if you had any advice based on your own experiences as to how we might invest experimental tests of psi with some of that relevance or personal meaning.

VAN DE CASTLE: Earlier I gave an illustration of entanglement and mentioned the highly unusual dream I had of a fish turning into a man's face and this other dreamer also had it, so it seems that the study is trying to show you that if you go with what people are sharing at the conscious level you get mediocre stuff, if you go towards the preconscious you get more, and if you can get both going conscious to unconscious, dreamer to dreamer you get even more. There was a PF conference in Nice, I believe in 1970, and one of the presenters was Al Rechtschaffen. He took his subject, hypnotized them and told them, "In a few moments I am going to suggest a dream to you". He then hypnotized them, and in one case the dream he gave them was about Martin Luther King; he had been shot and there was a great deal of concern about riots. Rechtschaffen takes another person and hypnotizes them and tells them, "You are now going to dream the dream that the other person had". It was almost exactly the same; King had been shot, they were picking up rocks and stones, policemen were coming out starting to patrol, and there was a great deal of concern over the riot. Rechtschaffen is not a parapsychologist, but he tried it and found incredible results and he was willing to report it, showing that the more you can get both people in at the same level, unconscious to unconscious, the better—Charley Tart had some great work on mutual hypnosis like that. So it's not trying to be all bright-eyed and bushy tailed when trying to do the test with someone, if you can both get into this deeper state in some way together, dream to dream, to me that would be one way of making it more personally relevant.

SIMMONDS-MOORE: I think we need to go back to the spontaneous experiences and basically really evaluate what people are *saying* and move that into the laboratory. For example, something Sally Feather and I are doing with PK experiences is to talk to people in depth about their experiences and what they mean to them, and this will inform future research in the laboratory as we will know more about the nature of the experiences. There are other ways we can approach qualitative research now, for example using transpersonal research methods to get more meaningful information about the actual experience. That's also going to be a spur for researchers, who I think can get bored just doing and hearing about laboratory experiments, and I think it may help increase the meaningfulness of the phenomena for the experimenter as well if they can hear some personal stories and take what you find in those stories back into the lab.

MACHADO: I would like to comment firstly on the importance of integrating field investigations with experimental studies, because if you do not do it we get separate data and do not give meaning to the experiences within the lab and we do not know the meaning of psi in our real life. So if you want to discover something else about this kind of experience or phenomena we have to integrate these kinds of phenomena to discover how they work. I think a good way of doing it is to bring into the lab those people who report cases and experiences in their daily lives, who perhaps pay more attention to little signs of psi in their lives that others don't pay attention to. It is a question of cognitive style or personality; I believe everyone can have some kind of psychic experience, but maybe not everybody can report it, so it is very important to pay attention to the little things. Integrating lab research with field investigation is most important. I cannot understand parapsychology or psi research that takes place in separate groups when they are complementary actions that build knowledge about what we live in our everyday lives.

SIMMONDS-MOORE: I think as well you could use new methods to fuse the laboratory research more with spontaneous cases. For example, Nicola Holt has been working with creatives using a PDA [Personal Digital Assistant] to have people record their experiences in a systematic fashion but while going about their daily lives. And you can use this to test hypotheses. If thin-boundaried personality types are having more interjections of experiences that are associated with sleep states then you might expect to see psi experiences happening

cyclically. You could automatically measure whether psi experiences occur when hypnogogic-type experiences are going to occur. Something like that may be new and exciting and meaningful in a way that you could fuse the two types of approaches into one.

MACHADO: I would like to point out something else in regards to longitudinal studies. It would be very important to study people who report spontaneous experiences or people who were considered to be the agents of a poltergeist case and tracking this person for a long period of time to see what are the consequences of the experiences in their lives. Even with possibly fraudulent cases, if you consider the case as a narrative you can see that the genuine case and the fraudulent case have the same structure, and the meaning can be the same so we could explore the consequences and functions of these things in their lives.

LUKE: I would like to go back to Christine's talk and add to what Etzel was saying about the neurological approach to looking at boundary thinness. You talk about it in terms of interconnectedness of neural structures, a more anatomical conceptualization of boundary thinness, but we may also have to think in terms of a neurochemical approach, particularly in terms of greater neural connectivity, disinhibition, and so on. Certainly recent research has found that psychedelic substances can lead to a reduction in the brain's inhibiting functions, as shown by an increased startle effect. This seems to map onto boundary thinness and to overlap with the conceptualizations you have here of its neurological basis, and may be another way of considering it.

SIMMONDS-MOORE: I was going to talk about neurochemistry but decided I already had too much in here. Hartmann talks about the chemicals of sleep and how serotonin and norepinephrine are associated with thinning and thickening of the boundaries associated with sleep.

LUKE: There seem to be associations between psychoactive substances and psi as well, of course.

WEST: A question to Christine. I know that you spoke mostly about the relation between personality types and spontaneous cases, but you did have some experience of translating it into experiments. I remember that in the past when people were doing experiments and correlating experimental results with personality types and attitudes they had a very pronounced view that sheep-goat was the most important factor. When doing that they found the sheep score positively and the goats

deviated from chance but in a negative direction. Do you find the same thing with your personality types, and if so does it produce a difference between positive and negative scoring, or between positive and null scoring?

SIMMONDS-MOORE: I don't think enough research has been done in this area to answer that question. I think this should be done. I don't think enough has been done in recent years to look at the performance of disbelievers or of people who score high on negative schizotypy traits. I think that definitely needs to be looked at.

VAN DE CASTLE: Well I think the reason the sheep-goat experiment got so much emphasis was because at that time it was their most robust finding—they did not get it every time, but if they had enough studies done they would find overall that the sheep did better. You could play with that. You could measure it for example by asking the question, "Could you accept a theoretical possibility that ESP exists?" Not that you have it, but do you accept its possibility. But who is that question important to? There is an Allport-Vernon scale of values and one of the scales is theoretical, so that people who scored above the 90th percentile on the theoretical scale, well they care about theoretical questions. So if you take a sheep that scores highly on such a theoretical scale and give them the Sheep-Goat question they will score even higher than a regular sheep. If you take the goats and they score highly on the theoretical scale they will score lower than a regular goat. So if you get something that is relevant to the sheep and goats it's there, and at the time that was our most robust measure. But I think our most robust factor is dreaming. If you look at Radin's book on entangled minds and you look at the meta-analyses, the Maimonides results are 55 million to one yet we usually think of the ganzfeld as being where the payoff is. There have been several studies done comparing individuals in ganzfeld and dream conditions and you get better scores with the dreams than the ganzfeld. So if you get something as outstanding as it is with the Maimonides results then that seems to be where the payoff is. And the beauty is that everyone could potentially dream. The work of Rechtschaffen is brilliant because you are having somebody dream someone else's dream with a method that is so strict, 'you're gonna dream about this and you see what the other persons dream was'. If I didn't know Rechtschaffen was such a strict tutor, I'd say this task is impossible because the dreams are so alike, just stunning. We all freak out that somebody got a big .05 finding in some

little area here, and then you see these correspondences that Rechtschaffen is getting and he is saying what started to occur to him was not 'what's the meaning of dreams?', but 'whose dream are you having?', because he said that when you get a hit with these kinds of things, they are so striking, you don't have to try and evaluate and go through this little nit-picking analysis process, they just kick you right in the teeth because they are so strong. I think a lot of psychic researchers themselves get very frightened of psychic stuff; as long as they get a dribbly .05 finding about twice a year, then you won't shake them up and they're okay, but if they get into these kicking-in-the-teeth, heavy psychic stuff, they don't know what the hell to do with it, and it really upsets them. So, I was playing around with the idea that we ought not to allow anybody to become a parapsychologist until you could give a bona fide of what was your powerful psychic experience that you've had, they now understand what the field is, rather than you have some abstract theoretical curiosity.

DOBYNS: I'd like to address Dr Machado's discussion of fraud or fraudulent poltergeist reports. Specifically, I frankly find it incomprehensible how the meaning of the experience can be the same for somebody who is out there rigging a fraudulent device to emulate the experience versus an actual agent (if there are such beings) who is having things flying around and doesn't know why or can't do anything about it. Moreover, the recent comment about addressing this as a 'narrative', I don't see it as really being helpful on this point because it seems fairly obvious that the reason fraudulent cases have the same narrative structure as the real ones is that narrative structure is now fairly widely known and a fraud is wanting people to think the experience is real.

MACHADO: Let me tell you about one fraudulent case we have investigated to try and address the question of why we might see features that can have parallels with genuine cases. Of course as far as the nature of the phenomena are concerned we have a big difference between genuine and fraudulent cases, since we cannot yet understand what kind of forces are at work in a genuine case. But in terms of meaning and purpose we might see similarities. We investigated a case in the countryside in Sao Paulo state in Brazil where fire was said to appear spontaneously in a house. We stayed for 2 or 3 days in the house and we saw many fires happening, it was very strange. We started to say, 'this phenomenon doesn't happen so easily so something wrong is

going on'. We left the camera turned on in the corner of a room in the house, and we could see the mother setting fire to some objects in a fraudulent way. It was very dangerous because she set fire to the crib. We asked why this woman would do these things. They were so poor and had few things yet she was destroying everything. The father was so worried; he was completely confused and wanted to stop everything because he was worried about the children, who were very young. Finally the mother admitted to the priest what she'd done, but not as a religious confession so we could know what he had been told. She said she was so tired of her life; she did not like her husband anymore, didn't want to be a mother, and wanted to work and do other things and wanted to finish everything. So she was distressed and was expressing her need to separate from her husband and leave, but as they didn't have money she needed the husband to be the one to leave so she could stay with her family. So she was creating a situation that would lead to a solution. We have other cases that seem to be genuine and where the objective or motivation seems to be the same. If you connect the meaning of the objects that are affected with the situation or context of the case you see that there is a symbolic or semiotic connection between these things. So when you are talking about the meaning of phenomena, the symbolism can be similar whether they are produced anomalously or fraudulently. In terms of meaning, if we detect a fraud, it's not worthwhile in terms of the ontology of PK, but in terms of the meaning in life, why people would choose this kind of expression over others as a solution for their problems can help us understand what motivates the apparently genuine PK occurrences. I hope that makes my meaning clearer now.

DELANOY: I just want to follow up and really re-emphasize the point that Christine made earlier at the beginning of this question period, when she mentioned Nicola Holt's work with PDAs. We have also had Bob talk about the very large effects that are commonly found with spontaneously occurring dreams and I agree with Fatima that we should be looking at ways to take field investigations into the lab. But technology has really evolved to the point where we can almost do away with the lab altogether, we can take very controlled experimental environments easily out into the field. People can generate controlled responses to experimental suggestions spontaneously when they feel like it. If we are looking forward to the future of parapsychology, where it is going in a way that would be much more ecologically valid and easier for people to relate to, I really think advances in technology,

such as PDAs and other devices, is giving us a way of moving into the future in a way that will really allow us to combine in a properly controlled way, the field, spontaneous cases and the lab. I would really like to get others' opinions on that.

MACHADO: Wellington Zangari and I have made with Dean Radin a study that was presented at the 1999 PA convention in which we took a computer to a place of ritual, so instead of taking the mediums to the lab we took the lab to the mediums, we went to their setting. It was an experiment about the influence in distant patients—they were in Las Vegas, we were in São Paulo, Brazil—and it was very interesting. We took the computer to their temple where there are saints, there is a mix of Catholic beliefs and spiritualism and African religion. We thought that perhaps it would be complicated for the mediums to accept our doing that—maybe it would spoil the research—but they loved to take part in it. So maybe the problem is in us, not in people in general. We had to change our attitude toward the research we were doing, not forgetting the protocols because scientific research has to follow rigorous protocols but thinking about this scenario I think it is really important.

VAN DE CASTLE: In terms of technology catching up, being able to identify different REM periods certainly enabled the Maimonides work to go forward because now you knew if you had a REM period and someone else is looking at a picture then you could wake them up to see if the dream is showing it. Similarly there is a lot of debate about lucid dreams; did such a thing exist, or did it only exist in California? What you are able to do in a lab is to show that the EEG indictates they are asleep and if the person makes a certain pattern, a left right left right movement, and you wake them up they will report a lucid dream. In my lab Joe Daniel was doing some research. He was able, with the help of some hypnosis, to take 15 women who had never had a lucid dream, and after one night in the lab 12 out of the 15 had had a lucid dream. He told them if you are in a lucid dream I have to wake you in 30 seconds if you don't re-signal me. One of the people in there did that for 11 minutes, so the EEG is showing she is asleep but every 30 seconds she was making the designated eye movements so she could stay in a lucid sleep for 11 minutes. Now the power of lucid dreams is another enormous topic of where we could go in terms of people hooking up with other people, going on trips together in lucid dreams, including a lot of fantastic stories about healing in lucid dreams. Some,

to me, are giving us some visions almost of how the physical model of the universe works. People starting in the lucid dream state have said they would like to know better the laws of the universe and how they work, and they get elaborate diagrams that seem to try and fit in with the latest theoretical models. It is so ripe out there and now technology has shown us that, yes, we can do this and lucid dreaming is fairly easy. You do not have to have an EEG for it; you can wear a mask with electrodes and when the eyes start moving during REM it activates a red light. If you notice the little red light you know you are lucid, you may interpret it in different ways in your dream—someone saw it as rear lights on a car being turned on, someone may see it as policeman but they somehow make an interpretation of the red light, but whatever way they see it they know the red light is there, and this reminds them that they are dreaming and they can learn much more quickly how to have a lucid dream.

FACING THE CHALLENGES OF PARAPSYCHOLOGY

EDWIN C. MAY[1]

Introduction

Before I can lend credibility to my view of the challenges facing parapsychology today, I believe I must give some of my personal background as to how I came to my current position and hold my views, which will be discussed in detail below. It is a sobering thought for me to realize that I helped conduct a psychokinesis experiment in 1971 when I was a post doctoral appointee in the cyclotron laboratory at the University of California at Davis. I joined that position in 1968 and at that time had never heard of any matters relating to parapsychology whatsoever. It is beyond the scope of this paper to relate the story how Professor Charles Tart and a conference at the University of California, Davis, turned my head, however slightly, toward the possibility of psi. But that experiment with Professor John A. Jungerman, my boss, was my entry into the field.

For later reference, I will briefly outline that experiment. We built a pendulum that swung back and forth with the period of a typical human heart—roughly one swing each second. The pendulum bob was a mirror so we could use a state-of-the-art laser interferometer to measure quite precisely where the bob was at each instant so we could monitor if and when a PK participant influenced the motion of the pendulum. There is good and bad news about such a set-up and they are both the same news; the device is extremely sensitive not only to putative PK but also to everything else. Sadly, after much expense and hard work we never were quite able to isolate the pendulum sufficiently to allow even a token participant to try to deflect it in some way. I like to joke that we had built an expensive and elaborate truck detector using seismic vibrations from Interstate highway 80 a few kilometers away. I

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[1] Due to a last minute emergency Dr May was unable to attend the conference. His esteemed colleague Dr Zoltán Vassy graciously agreed to present his address *in absentia*.

tell this story because it will have direct conceptual input to the challenges facing us.

Going even further back in time to 1962, I joined the physics department at Carnegie Tech (now called Carnegie Mellon University) as a new graduate student. Literally on my first day I was lost and walking down the hall saw hunched over some physics apparatus an Indian fellow I recognized. So I entered the lab and said, "Isn't that a gamma-gamma angular correlation setup?" Well, that shocked Professor S. Jha—a beginning graduate student should not know about such things. I told him that I had made angular correlation measurements as a physics major at the University of Rochester in New York during my senior year.

He asked me if I would like to work with him in the lab and naturally I agreed. At that time, I did not realize who I had just met—one of the most respected physicists in the world at that time, who specialized in nuclear structure. Over time, we became close working colleagues and personal friends. During my short-lived graduate career there, I was less than adequate in my studies primarily due to a lack of maturity, but at least I did well in Jha's laboratory. When Carnegie Mellon asked me to leave even without a consolation Master's degree, the US Army started knocking on my door to send me off to war in Vietnam. Fortunately, Jha hired me to work for him under a US Navy contract which afforded me an exemption from serving in the military.

After about six months or so, Jha escorted me down the road to the accelerator laboratory in the physics department at the University of Pittsburgh and introduced me to what turned out to be my next advisor—Professor Bernard L. Cohen, again one of the most respected researchers in his field. Cohen took me in with no questions asked based on Jha's suggestion. I was most fortunate, indeed. From then onward, I did well in my studies and met the requirements for my PhD in experimental nuclear physics. Professor Jha will come back into the story below.

This paper is rather difficult for me to write because it will express a degree of pessimism. Those that know me realize I don't even acknowledge the possibility of a half empty glass and my definition of an optimist is 'a pessimist with more information'.

Some of the Challenges

As I see it, the challenges facing parapsychology can be divided into three interdependent categories:

1. Those beyond our control.
2. Self-generated.
3. Technical.

Before I discuss each of these in detail, I wish to emphasize that some of my remarks may seem a bit harshly critical especially with regard to the *self-generated* category above, but I will also include myself among the targets for criticism. So what is the pessimism part? The problem is that I do not see any obvious near-term solution; thus, I think that our field will not prosper and contribute to the knowledge base any time soon.

Challenges Beyond Our Control

The first of a number of related challenges that are beyond our control is that there is almost no money in our field for research or full-time psi research positions. I personally may be an exception perhaps even unique as a counter example to this. For 20 years beginning in 1976, I enjoyed a full-time psi research position first at Stanford Research Institute (now known as SRI International) and later at Science Applications International Corporation (SAIC). That meant I was paid industrial scale wages with medical benefits and was given generous retirement packages. I do not revel in that, but rather, I am saddened by it.

Through the excellent work of the late Professor Robert Morris at Edinburgh University in Scotland, the world of academic parapsychology seems vibrant especially in the United Kingdom. However, even here much of the parapsychologists' time must be devoted to teaching traditional psychology and there is almost no time for research. At this time, there are precious few foundations offering grants for research and almost none of them will pay honest overhead funds that are critical for a successful research program. A rough rule of thumb is that for each unit of salary (e.g., Euro or US Dollar), one must raise an additional unit to cover the general indirect cost of doing business (e.g., paying the prorated heat, light and phone bills, etc). This has an obvious cooling effect on the administrators of most universities.

One foundation offers an upper limit of €50,000, which sounds like a lot of money, but in the scheme of good research projects even this sum is far below what is actually required. This kind of 1-year funding allows for only part time of a senior investigator and for financial reasons, the project must rely upon (perhaps unskilled) assistants to conduct the study and interact with experiment participants. And as we all realize, this may be counterproductive given the troublesome observation of experimenter expectancy effects and perhaps even experimenter psi effects.

Related to the lack of money is the problem of lack of 'respectability'. That is, even today after substantial technical progress, psi research and teaching has a certain 'giggle' factor. Of course, Morris's effort for nearly two decades led to positive results, but for most of the rest of academia outside of the United Kingdom, it is simply not safe for academics—even tenured ones—to admit of their interest in the field. I personally know of a few academics who are aware of the good data and results and are vitally interested in the field, yet they have sworn me to secrecy lest someone of note would discover their 'transgression'!

I cannot imagine any talented, young, freshly minted PhD that in their wildest dreams would choose a career in parapsychology. In effect, this obvious fact, acts as a kind of talent filter. That is, the best and brightest go off for 'real' careers and we get what is left over.[2] In short, we cannot offer careers to young people; rather, we can offer them a hobby. This, in turn, raises the question. Does our field have, or will have shortly, the necessary skill and training to address let alone answer, the difficult technical questions that must be answered to make progress? Sadly, I think not. As I said above, I include myself in this 'second tier' of researchers. While I was reasonably good at experimental design and the hardware/software problems, I was a mediocre physicist. A Nobel Prize was not ever in my future. For that matter, most everything but a teaching position in some small college was beyond my grasp. So the talent filter let me through.

Another problem that relates to respectability is publishing our good experimental results and speculative theories in mainstream journals. In some real sense this is a Catch-22 problem; respectability comes from

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[2] Beginning here and throughout this paper, I am speaking in broad generalities and averages, and as such there are some exceptions to all the points listed here.

publishing, but one cannot publish without respectability. While there are a few exceptions to this rule—mainly by brave well-known authors—most mainstream journals have a significant psi filter and generally will reject papers *a priori* by not sending them out for review. Probably this observation needs no examples, but I will provide one that may be illuminating. A few years ago, my colleague, James Spottiswoode, and I conducted a complex but highly successful psychophysiology experiment we call prestimulus response. We extended and improved the concept of presentiment in that by using acoustic startle stimuli as opposed to the cognitive affective stimuli, we removed a source of confound because of an obvious idiosyncratic response to various photographs. In addition, we substantially simplified the type of response we were looking for. As a result, we found nearly twice as many 3.5 second prestimulus regions that contained the defined skin conductance response prior to the acoustic stimuli than during the same length region prior to a silent control. Statistically this turned out to be a z-score of 5.08 with 100 participants.

We wrote a paper of 2,500 words aimed as a report for *Science*. We passed drafts of it to our colleagues and to a number of world-class professionals in the psychophysical research world. As a result, the final draft was as 'flawless' as was possible—a natural candidate for publication in a mainstream journal.

Knowing that if I sent in the manuscript cold, it would have zero chance of even being sent out for formal review, I asked a number of mainstream colleagues if they knew anyone on the editorial board at *Science* so that they could put in a strong word to let our paper go out for review. To an individual, they all complained that not only did they not know anyone there but they, too, had troubles getting their own work published in *Science*. So I had to go it alone.

I crafted a most tricky letter to Professor Donald Kennedy, the then Editor in Chief of *Science*, and former president of Stanford University—a few kilometers from our laboratory. My goal in a two page letter was first to establish my own bona fides, and then in a sense embarrass him with a Type II argument. That is, just because I do not have a recognized academic position (i.e., a technically unknown person); just because I do not work at a recognized institution; and, just because I work in a controversial field does not mean, therefore, that my research is wrong. In addition, I sided with him in that I understood his problem of little space and far too many worthy things to publish.

However, I offered a solution. He should invite me to give a talk at Stanford on this work and if the consensus was that my work was good science, only then would he offer to send the manuscript off for review and that I would happily abide by the reviewers' remarks. I sent the letter off via regular post with half an expectation that I would never hear back. Much to my pleasant surprise I received the following a week or so later:

> Thank you very much for your letter of June 5. Your background is obviously deserving of respect, and I'd like to be helpful. But the idea of marshalling a critical audience to hear you present your experiments seems a difficult and time-consuming way of dealing with what amounts to a pre-submission request. So I think that's asking too much, but I'll certainly look at something if you want to send it to me as an e-mail attachment or in some other way.
>
> Perhaps I should add that my personal history—dating back to the Rhine experiments in the 50s—I'm pretty skeptical in this area.

I sent him the manuscript, hard copies of the major references with a paragraph describing each of them, and a short list of mainstream scientists who were recognized authorities in psychophysiology all of whom had agreed to be listed as references. For the next nine months or so, I had a number of post mail exchanges with Kennedy with all his responses on Stanford University letterhead. Finally, I received the following on the American Association for the Advancement of Science (AAAS) letterhead:

> Dear Dr May,
>
> During your absence, I've had a chance to circulate your proposal around. I'm afraid that the view here is that we will not send it out for in-depth review. I was glad to be of some assistance to you in getting it evaluated, and grateful for your interest in *Science*.
>
> Sincerely yours,

It is difficult to understand his response. While it is very tempting to invoke some kind of fear of psi argument, I think the true answer is much more complex. Assuming Kennedy actually did pass the manuscript around, then the only comments he received back may have

been ridiculing and/or *ad hominem*. It is particularly frustrating in that our paper was rejected without any explanation whatsoever.

I am pessimistic that the best science we can offer, and this paper was certainly among the best, was rejected in a non-scientific way—too bad for science (with a lower case 's') and for *Science* the journal. It cheapens the processes. I have no argument for a solution to this challenge not of our making.

Challenges of our Own Making

This section contains many of the major points that I see as self-generated challenges. By no means is this list meant to be exhaustive.

Belief in our Own Phenomena?

In some of my presentations, I conduct an informal survey with a single question: "There is incontrovertible evidence for an information-transfer anomaly we currently do not understand. How many of you agree with this statement 100% with no reservations whatsoever?"

Herein lies a problem for us all. Sadly—and quite surprisingly—only about 10-20% of any given audience answers this question in the affirmative! What comes to mind is a significant paraphrase of a famous US cartoon called Pogo. One character whines, "Boo hoo! Why aren't we accepted?" The second character replies, "Yup, son, we have met the enemy and they are us!" It seems to me, if we cannot even agree within our own organization that there is something worthy of study, then there can be no hope of mainstreaming our discipline. I am shocked at the caveats, excuses, questions that my simple question invokes particularly in the light that even some of our harshest critics agree with it without reservation. It is very important to note that this question does not imply that one is agreeing with some form of attesting to the reality of ESP. Perhaps we are all so gun-shy in requiring that our scientific 'i's are dotted and 't's are crossed that we fear reprisals from some unspecified external enemy and that we quake at the thought of saying something—anything—definitive.

In a related matter, I have served on the PA Board of Directors a number of times and as the organization's President one year. There have been many attempts, all of which have failed so far, to arrive at a consensus about the phenomena in our field. What better place to post a priority list of phenomena than on the Parapsychological Association web site, a list that clearly delimits those phenomena for which the

organization believes that the evidence is in without question; it must also list other phenomena where the evidence is highly suggestive, but more work is needed; the list should include phenomena where the evidence suggests these are not likely to be real but further investigations are requested; and, finally, it should delimit which phenomena are clearly either not considered part of our discipline or are false. Failing to come up with such a consensus is yet another example of a challenge that is self-generated.

Replication Issues

Another area of self-generated challenges is replication. For the most part, we are not very good at it. Yes, the late Charles Honorton's Presidential Address at the 1975 Parapsychological Association convention clearly showed that the replication attempts published in the *Journal of Parapsychology* during a 5-year period were substantially more numerous and of better quality than those during the same period of time appearing in the *Psychological Bulletin,* a mainstream psychology journal. Our field attempted replications more often and realized a much higher successful replication rate than did the mainstream psychologists.

However nowadays the experiments are much more labor intensive and technically and mathematically sophisticated. How many of us can replicate the creative work of Dick Bierman's fMRI studies? First, most of us do not have access to such equipment; even if we did, we do not have the funds to carry out the studies; and finally most of us do not have sufficient mathematical sophistication to understand let alone conduct the required analyses.

From our own Laboratories for Fundamental Research, we have published studies involving entropic concepts and a model called Decision Augmentation Theory, both of which involve some sophisticated computation and analysis. These are also rarely replicated. Of course, this may be due, in part, to the talent filter described above. Not replicating these and other complex studies in the field can only detract from the scientific credibility we all desire. Clearly the examples above are not unique to the named researchers or facility.

Another area of replication where we engage in self-defeatism is conceptual replications that aren't. We have all experienced the frustration when we publish an idea and someone else throws out key elements of the protocol for seemingly arbitrary reasons, or makes

additions to the protocol without regard to its impact on the original concept. Sadly, there are far too many examples to list here; however, one was so egregious, that it requires special attention.

This year, Moulton and Kosslyn (2008) published an experiment in a respected mainstream journal. They tried to argue that if psi were real, they would definitely see it in their complex fMRI study. In their introduction, they correctly pointed out that one cannot prove a negative; yet, in their conclusions they said they could do just that and strongly stated that psi does not exist because if it did, they would have seen it in the central nervous system (i.e., the brain). Aside from that bit of questionable logic, the main problem was with the psi part of their protocol. I wrote a multi-page letter to the editor outlining the many protocol flaws that are far too numerous to list here, but I will mention a few. For example, they had no independent measure telling them that psi happened when the participant was in the fMRI scanner—an obvious and fatal mistake. But most terrible was that even if psi happened at the published effect sizes in the literature, they did not have enough statistical power to observe it in their study! Perhaps this example appears to be one beyond our control, but Sam Moulton, the lead author, was a former attendee of the Rhine Research Center's summer study program and, therefore, should have known better. In addition, Joseph McMoneagle and I spent a day with Moulton and Kosslyn at Harvard University having two lengthy but informal discussions about remote viewing, and I presented our prestimulus response work in an invited talk to a joint meeting of the Cognitive Neuroscience and Psychophysiology departments. The informal discussions and the invited talk were well received.

Their study was not a conceptual replication for psi. Yet this published study will garner significant negative attention.

Amateur Hour

Perhaps as a consequence of the talent filter and/or because of a lack of money and credibility, many of us appear as amateurs. This may also be an issue with regard to interdisciplinary research in general. As a physicist, I have often expounded upon psychological and personality theory and conducted measurements in these domains—what an amateur! I have heard some of my non-physics friends drone on about the virtues of quantum theory knowing next to nothing about it—more amateurs. How many of us who are not psychophysiology professionals make those kinds of measurements regardless? To professionals in

these disciplines we appear as amateurs playing with a hobby. I will illustrate with one example from our laboratory at SRI International.

In 1996, we published a paper about providing feedback subliminally in an anomalous cognition study (May, Lantz & Piantineda, 1996). One advantage that our program enjoyed was that we had sufficient resources to avoid the amateur hour; the third author on our paper, Tom Piantineda, was an SRI employee who had won prizes for his published research in subliminal perception in *Science.* We hired him for 2 days per week during the design and pilot phases of the study because of his obvious expertise in presenting subliminal stimuli. Because he did not know anything about psi research we gave him copies of all the published work we could find in the psi literature that involved subliminal perception and the use of tachistoscopes. He returned later with a stinging condemnation of the lack of quality and expertise displayed in this literature. He wondered whether the psi research community ever read even the most basic mainstream literature on the topic. With some obvious exceptions, my bet is that mainstream scientists reading our literature on other technical topics might come away with a similar response. Somehow, we must address this challenge.

Ideologically-driven Experimenter Expectancy Effects

Even psi researchers are human. We wander through the ill-defined boundary between conducting science to learn how the world works and doing science to prove our preconceived word view. Some might simply call that hypotheses testing, but I am speaking more toward a form of conceptual blindness. In this world, we too quickly accept the cherished hypothesis and do not explore or even consider other explanations that can equally account for the observables. Here is an example from our own laboratory.

The cherished hypothesis in our pre-startle stimuli, psycho-physiological research is that somehow the human nervous system reacts in advance to some future stimuli. Well into this work, we began, finally, to see another possibility—one now that has replaced the earlier cherished hypothesis—psi mediated experimenter expectancy effects, or better known as DAT. That model fits the observable data much better than did one in which the participant is said to react to a future stimulus. In our studies, the experimenter initiates a run and the participant does not physically interact with the hardware or computer.

In the presentiment research, the protocol differs in that the participant initiates each stimulus period, but is blind to the stimulus type, of course. This has been used as 'proof' that the effects are not DAT.[3] This view, of course, raises the question of how to identify the observable with regard to the spectrum of possibilities ranging from totally DAT on one end to precognitive responses to future cognitive stimuli on the other. I see these kinds of problem as further examples of self-generated challenges in that, when ideology begins to trump alternative hypotheses, we are in trouble.

Mixed Collection of Self-generated Challenges

Scientists are notoriously terrible presenters of their good work. I have attended talks, in which I am vitally interested, at conferences, and after a short period I am lulled into a glazed stupor. Part of doing good science is talking about good science. My graduate advisor had yet another important point: If you cannot tell your family what you do for a living in terms that they can understand, then you simply do not know what you do for a living. A major point here is that in presentations for sure and even in some papers, rigor and detail are not synonymous with clarity.

In a related topic, we are ineffective sales representatives of our work. The mainstream will continue to ignore us if we:

- Claim what other scientists know about their own disciplines is wrong at worst or incomplete at best; this is not a good initial strategy.
- Ignore vast literatures that argue against our cherished hypothesis. In some of our discussions of quantum theory, we tend to sweep under the carpet a huge amount of experimental and theoretical literature that has explored the thick boundary between things which are clearly classical and things which are clearly quantum. It could be that this vast literature will turn out to be incorrect, but failing to acknowledge that it exists and presents a contrary view makes us look like new-age ideologues rather than competent scientists.
- Quote scientists who are clearly in the tails of the opinion-space bell-shaped curve as representing the main view. I see this as like a lawyer finding an 'expert' witness to help make

[3] Private communication with a number of the principal investigators.

the case. It appears possible to do this regardless of the details of the case.

- Interpret criticism as a display of the critic's unconscious fear of some kind.

Finally, we all have a problem, self-generated and not, of collapsing disciplines and skill sets. Morris deserves unbounded credit for establishing parapsychology as a valid discipline within the university structure in the UK. However the resulting explosion of academic positions suffers from the fact that they are nearly all psychologists. The basic training of psychologists is not geared to addressing the interdisciplinary needs of parapsychology. I would have the same critical view if they were all physicists. We have an interdisciplinary problem that will not yield to the expertise within any given discipline.

Many of the self-generated challenges I have listed above have no obvious solution but on the other hand some clearly do (e.g., displaying our knowledge of the contrary mainstream literature).

Technical Challenges

I see the technical challenges facing us as the most serious issue. We can address the challenges beyond our control and most certainly remedy some of the self-generated ones, but the technical challenges might be fatal in the near term.

I can summarize them in one sentence: 'When, where, and how long does psi happen?' I will illustrate this with a simple example that is not only not out of the question but also, perhaps, even likely. Suppose we have a complex protocol looking for psi signatures in the central nervous system. We ask a participant to use psi to obtain information about a target system while we monitor the CNS either in an fMRI scanner or with a complex array of EEG leads. If the duration of a psi experience (whatever that turns out to be) lasts only 10 milliseconds or less for example, none of the above hardware will see it. A more likely situation that will yield no result in the study is that all the psi happened to the participant (unconsciously of course) while she/he was parking the car in the lot in front of the laboratory. In these examples, all of our process-oriented, highly competent, technically sophisticated protocols are a waste of time and resources because they will not and cannot yield the results for which we are looking. Related to this is the pesky

problem of experimenter mediated psi effects. Or more crassly, on whom should we paste the electrodes?

It seems obvious to me that we are facing what Einstein once said—Nature is simple, humans' interpretation of Nature is complex. I will overstate the problem for emphasis. If you think the last study I conducted was weird, look at this one! We need to discover how to collapse the problem space and look for more organizing principles. For example, if, as I believe, there is only one form of psi—precognition—then that putative fact reduces the number of things we must understand from many impossible things down to only one impossible thing. That sounds like progress to me.

Even if we do not know the when, where and the how long of psi, we must cease conducting evidentiary experiments, except as teaching aids, in anomalous cognition (i.e., ESP, ganzfeld, remote viewing) and random number generators. The data are in; the effects have been established. We have far too few and precious resources to squander in this way.

I mentioned in the introduction that I would return to the issue of the sensitivity of that pendulum PK experiment. This brings to mind an even deeper technical problem: we only have negative or operational definitions of the phenomena we study. For example, ESP is what happens when nothing else could or did happen; a similar definition holds for PK. These negative definitions place a huge responsibility on the experimenters to demonstrate that nothing else could have happened or, in fact, did happen. In the ESP world, this is relatively easy and inexpensive to guarantee. In the PK world, however, it is especially difficult and expensive because there are many 'natural' phenomena that will masquerade as PK. In the introduction example, a truck passing on a highway a few kilometers away will give an apparently anomalous signature. Controlling for such things by effort and control periods reduces the so-called effect into a statistical inference problem where DAT may be the primary mediating factor. The SRI team spent $500,000 on a single macro PK experiment involving piezoelectric strain gauges. Most of the money was spent on engineering details to assure nothing 'normal' could or did happen during experimental sessions. During a pilot phase, we were all excited with a rather large deviation in the data until close inspection proved that deviation resulted from very subtle vibrations set up by an elevator in the adjacent building. PK studies like this become so costly that we

quit asking for funds even from the relatively cash-rich US Department of Defense.

The other definitions of our phenomena are equally unsatisfactory. They are operational: 'do this specified ritual and what you get at the end of it we will simply label it ESP or PK depending upon the setup'.

As one consequence of these unsatisfactory definitions, in spite of the last 100 years of trying, we have yet to develop a comprehensive and encompassing model for the mechanism for psi; rather, our theoretical direction is too often premature in abandoning the established models and theories from other disciplines.

Finally, I wonder if we have the native skill and ability to face adequately the technical problems confronting us. Who among us can explore the subtle reactions of the brain or other parts of the nervous system, or push the boundaries of classical and, perhaps, quantum physics with regard to backward causation and psi detection. For that matter who can explore the leading edges of deep psychology to understand better that aspect of psi? While there may be some few exceptions in our community of researchers, for the most of us, what are we to do?

Conclusions

I have painted a pessimistic picture of our discipline. Along the way I may have insulted some or otherwise caused emotional consternation. Clearly that was not my intent and if I did so, I apologize and seek your forgiveness. Yet, I think these challenges are real and what is frustrating for me personally is that I do not see any near-term and obvious solutions. My colleague Professor Deborah Delanoy offered a glimmer of hope by pointing out that one positive aspect of Morris' legacy is that competent parapsychologists have 'infiltrated' the mainstream in the UK academic psychology system. Clearly this is very good, and I thank her for pointing this out to me.

I will close by returning to a quote from one of the founders of quantum theory in the fond hope that Max Planck was wrong in our case when he said: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

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DISCUSSION

BANCEL: I took the booklet last night and I looked at all the participants and observers and counted up how many had a physics background and how many had a psychology background, and it was something like 60 people with a psychology background, 7 or 8 with a physics background and maybe a couple of philosophers. After doing that I wondered what things would be like if those ratios were reversed.

DOBYNS: In response to one of the slides in which Ed asks rhetorically 'should we focus on applications?' I would say that is a good idea and good advice to the field as a whole. We have been beating our heads against a stone wall in academia for 5 or 6 decades, and getting something that works in the everyday real world that ordinary people can use and see in action would put the hostile critics in the position of trying to claim that the world is flat.

VASSY: There have been encouraging episodes regarding applications in parapsychology, I think. Hal Puthof, for example, won in the region of $100,000 using a simple forced choice method where precognitive calls were made by a majority vote after they had been coded so that participants didn't know when they were making the same call. Outcomes were used to inform speculations on the stock market, and the winnings were given to charity. So it is not impossible.

JOSEPHSON: There have been two practical applications in the past, although people may not believe you when you tell them about them. One has been described by Joe McMoneagle and involves finding a missing H-bomb somewhere in Africa, and the other is taking a peek inside the Iranian Embassy to get useful information during the hostage situation. This may come to the fore in the future with regards to cold fusion, and I wonder what would happen if you could buy something that would heat your house more efficiently—people might say you have done your sums wrong, it isn't more efficient, but if the public's heating bills are lower they might be more willing to accept it. Another

example of practical psi was in the newspapers a few months ago where there was an old folks' home with a psychic cat that seemed to know when someone was about to die. A few days in advance it would go over and comfort them and the staff would notice this and actually call relatives to come visit while they could—a real practical use of psi. The disbelief factor applies as it is not readily explicable, but there will always be that problem. Perhaps we need to give more thought to what practical applications there might be at this time.

ROLL: In regards to practical application, as far as I know there is only one type of psi, and this seems to be reserved for psi gifted individuals, which makes the prospects of an application for the general public more unlikely. Parapsychologists are mainly interested in people that do not have psi. But if you do work with psi sensitives then there is a method that is invariably useful. It is also the one investigated by Dr Tenhaeff, Eugene Osty and other people, and was used for example by Mrs Garrett. This uses object association to address real life questions. There seems to be no decline in this ability as there has been with forced choice methods, yet there has been no interest whatsoever as far as I am aware among parapsychologists. This to me is incomprehensible, firstly because object association can be repeated, and secondly you can have a real insight into psi from the qualitative experiments using this method. From a neuropsychological point of view, psi sensitives seem to produce considerably better, more consistent and more useful types of ESP. The question I would like to ask the group as a whole is, 'why are we not doing more work on psi gifted individuals?'

ROE: From my perspective, I think an important reason why some parapsychologists do not work with gifted individuals can be summed up in two words, *Project Alpha*. The stacked deck that we're playing with means that we are condemned if we do not discover fraud or denounce it quickly enough, as in Project Alpha, yet receive no recognition when we do discover it and publicise it, as in the Levy affair. There is also the case of *Project Beta*; during the Alpha affair Randi made clear reference to a second 'sociological experiment' that was ongoing at another center for parapsychology research, but when a fraudulent metal bending claimant who was being tested at the University of Edinburgh was caught in the act there was no press conference from Randi to commend the researchers for passing his 'test', and very little further mention of Project Beta. For me, as

someone who has read some of the magic literature but is in no sense an expert, there are too many dangers inherent in working with gifted individuals and investing a lot of time when at least on occasion it may turn out that they are fraudulent.

ROLL: Other applications involving gifted persons are illustrated by Noreen Renier, who in *Mind for Murder* describes some of her experiences as a psychic detective working with the police, often as a last resort. Using object association she gave clues with respect to the perpetrator of the crime, and at times these have been spectacularly successful — there are police records about this. This woman has given talks at the FBI about how she works. There has been a lacuna in parapsychology it seems to me since the work with mediums in the early SPR period which often involved object association experiments. The reason why we are all here is because of Eileen Garrett, yet we universally ignore people like Mrs Garrett, who was also very good at psychometry. It seems we have a compulsion to be insignificant and talk about borderline stuff, for which I have a deep regret.

VASSY: I tried this testing strategy in Hungary. We have some psychics who advertise their services, making it easy to approach them. There are some of them who really want to cooperate in experiments; they want to understand what they are doing. We have to ensure they do not deceive us, which is quite easy these days as we do not have to concentrate on their part when we can make the targets secure enough. They agreed to the protocol and liked this kind of experiment but still did not produce any extrachance results. After a while I gave up as I thought if we take them out of their own territory—in which they communicate face to face with their clients and then tell them things about their childhood and perhaps have access to subtle cues between them—then they are just ordinary people not psychics.

ROE: I was going to respond as well and say the method or approach you adopt also depends upon the research question that you are interested in. There are some people active in parapsychology today who are working with gifted people and psychics; people like Robertson and Roy working in Glasgow with high-profile mediums such as Gordon Smith, and in the US you have Gary Schwartz who has worked with a number of well-known psychics and mediums. However, other researchers in the UK, including myself, are interested in otherwise ordinary people that have spontaneous psychic experiences. Understanding their experiences provides an important

public service and gives a particular insight into psychic processes that we might not get from studying only the gifted. A useful analogy for me is to think of psi ability as something that is normally distributed in the everyday population, like the ability to walk; some people walk faster than others, some are more elegant than others. If you wanted to understand the mechanics of walking in any holistic sense, you don't focus all your attention on Olympic walkers; you speak to regular people because you want to see the various mechanisms people may use, you may study toddlers as well, who are not very good at walking but can give you an insight into stages of the process that might otherwise be concealed. Likewise I think that if we restrict ourselves only to study the virtuoso then we are going to miss out on a lot of information.

NELSON: Two remarks, one is an application we haven't mentioned, is healing. A large number of people are engaged in healing and some of that has been studied in a scientific way. Although there is still some controversy over the results, if you look at meta-analyses it looks like an application that is working to the benefit of those that get healed and working to some degree to improve our understanding of the capacities of human consciousness. On the subject of gifted performers, at the PEAR lab we had a policy to not use anyone except regular 'off-the-street' people, but perhaps one may be gifted. After we had worked for many years and gathered the kind of information to give you general distribution information we loosened up a little. Occasionally we would then accept the offer of someone that thought they were gifted to come in and try our wares. But it is rare for someone who does the kind of things that Bill was talking about to be interested in, or able to deal with, an REG or card guessing experiment, which is not connected to real life in the way that their normal experience of psi is. However, there was one particular person who, among other things, made occasional trips to Las Vegas and claimed that he had hit 19 times out of 20, which was enough to make him a good deal of money. He did 4 or 5 of our experiments—not huge databases but enough to get an idea—and I can say in a generalized way that his performance was more consistent than anyone else we saw.

DOBYNS: I too was prepared to allude to the experience of PEAR as support for the thesis that there is no such thing as a psi gifted subject, or if there is it is only in the sense that you may talk about a world class athlete as being 'running gifted'. I fairly recently completed an analysis of PEAR's REG database using statistical methods that had taken me

20 years to develop to do a hypothesis test on the distribution of effect sizes in the operator population. I found to my surprise, the best supported model was a pair of delta functions, with 15% of the operators collectively responsible for the effect and the other 85% doing nothing. So, for those PK experiments I would say the evidence supports the idea that there is a psi gifted population that consists of about 1/6th of everybody, which is not exactly a rare talent. Then again, there is the PEAR PRP (remote perception) database in which an overwhelming majority of the individual unselected percipients showed results better than chance—not individually significant because most do only a small number of trials, but there the effect appears to be very broadly distributed. PRP trials are closer than most laboratory experiments to the kind of task that gifted psychics are asked to perform in the real world, yet we see most of the population showing some effect in that realm.

VASSY: Did you write a paper about these findings, because to me they sound very important?

DOBYNS: You just reminded me that I haven't yet written a paper, just given a Society for Scientific Exploration (SSE) presentation about it!

MULACZ: As you said, Chris, if you are doing an investigation on walking you try to use ordinary people. On the other hand, my example is if you are investigating synesthesia you will not do it with ordinary people; the distribution of synesthesia is approximately .004%, so if you do it with ordinary people the results will be marginal. I think there are many more psychics than people with synesthesia, but that is why I think you should concentrate on the people who are gifted.

ROE: But surveys of the general population show that very few of them experience synesthesia, while surveys of parapsychological experience fairly consistently show that a majority have experienced ESP or PK in some form.

VASSY: It all depends on the phenomenon itself: is it distributed widely or distributed as a delta function? It is not a free choice.

MULACZ: Sure, but I think we should pitch our methodology on what we know about the distribution.

DAY 2 OPENING REMARKS: WELCOME TO THE 'SENAATZAAL' ROOM

WIM KRAMER: Good morning. We are gathered today for the remainder of our Invited Addresses in the 'Senaatzaal' Senate room in the University of Utrecht's main building. This building and the interior of the Senate Room are exactly as it was in the summer of 1953 as the First International Conference of Parapsychological Studies was convened. The paintings of many generations of Utrecht academicians that soberly oversaw those proceedings will bear witness to our efforts once again.

Both the City of Utrecht and the University are closely connected to the history of Dutch parapsychology. Not only via the 1953 conference but also the Parapsychologisch Instituut (Parapsychological Institute) at the Springweg that was headed for almost thirty years by Professor Tenhaeff and is headed today by Professor Gerding, as well as the famed Parapsychological Laboratory of the University headed by Professor Johnson and Dr Schouten that existed from 1971 until 1989. The history of parapsychology at Utrecht University dates back to the early 1920s when Dr Roels was promoted to be the first professor in The Netherlands with a Chair in experimental psychology. Professor Roels was interested in parapsychology and was one of the founding board members of the Dutch Society for Psychical Research. Although he wrote no formal articles, he allowed—indeed encouraged—his students to study parapsychological phenomena, including, happily, Tenhaeff. Many PhDs with a parapsychological component have been awarded in this room: Tenhaeff in 1933 on aspects of telepathy; Brink in 1958 on the value of information based on paranormal sources within Dutch criminal law; Schouten in 1973 on the 'standard method'; Houtkooper in 1983 on observational theories; Boerenkamp in 1988 on paranormal impressions of psychics; and Attevelt in 1988 on research into paranormal healing. The list continues, as will hopefully the dedication of those interested in parapsychology. It gives me great pleasure to share with you now some actual film footage that was taken at the 1953 meeting as provided by Parapsychology Foundation.

LISETTE COLY: I trust you have all enjoyed viewing the film clip and thank you Wim for putting this wonderful location into its proper historical setting, which is of great interest to those of us who are

attempting today to follow the path of parapsychological inquiry that has been so well trodden here in Utrecht.

This is a fitting moment to acknowledge two individuals who sat in these same halls and participated in the 1953 conference. We are very proud to recognize both Dr William G. Roll and Prof. Donald J. West, who are well known to students of parapsychology as veterans of the first Utrecht conference and who have continued their life-long pursuit attempting to find answers to the mysteries raised by psychic functioning. It is wonderful to have them both with us once again some 55 years later and we thank them for their dedication and contribution to the field.

DISCUSSING PARAPSYCHOLOGY AT UTRECHT: THE FIRST INTERNATIONAL CONFERENCE OF PARAPSYCHOLOGICAL STUDIES

CARLOS S. ALVARADO

In 1953 the journal *Science* published a report of an international parapsychology congress held at Utrecht entitled "The First International Conference of Parapsychological Studies" (Murphy, 1953a). One of the purposes of the meeting was "the effort to develop a scientific program to deal with all types of unknown relations between individual and environment, whether at present classifiable or not" (p. 402). Such an event has been described as a "turning point in the history of parapsychology as a science" (Garrett, 1953a, p. 1), and as "one of the most important events of the history of parapsychology" (Cassoli & Cassoli, 2000, p. 16, this, and other translations, are mine). Others have included the congress in lists of important events of the history of modern parapsychology (e.g., Rhine & Pratt, 1957, p. 207; White & Dale, 1973, p. 220).[1] Financed by the Parapsychology Foundation (PF), and sponsored jointly both by the PF and the University of Utrecht, the congress is the topic of my paper, and the inspiration for the present meeting, jointly organized by the PF and the HJBF and summarized in this proceedings.

The First International Conference of Parapsychological Studies has unfortunately been somewhat forgotten, as have many aspects of the social history of the field. This is the case in spite of the fact that there are some individuals alive today who participated in the congress. [2] It is my hope that this paper will rescue the first Utrecht congress from oblivion. I will focus on the antecedents, the organization, the content, the impact and importance of the conference.

Parapsychology before the Utrecht Congress

A variety of works have reviewed the development of twentieth-century parapsychology before 1953 (e.g., Amadou, 1954; Inglis, 1984; Mauskopf & McVaugh, 1980; Sudre, 1956/1962). While there are many aspects of this history that can be discussed, I will focus here only on previous meetings and selected aspects of ESP research.

Psychical Research Meetings

There have been many meetings held before 1953 in which parapsychology has been discussed. The topic was covered in the first international psychology congresses, particularly in the first four held in 1889 (Paris), 1892 (London), 1896 (Munich), and 1900 (Paris) (Alvarado, 2006; Noonan, 1977, pp. 137-140). Figures such as Théodore Flournoy, Frederic W. H. Myers, Charles Richet, Eleanor Sidgwick, and Henry Sidgwick participated in the discussions about psychic phenomena at these congresses, including such topics as veridical hallucinations, thought-transference, and mediumship. But the presence of psychical research in these congresses was a small part of the meetings and as time went on the topic disappeared from the programs (Parot, 1992).

A congress of psychical science was held in Chicago in 1893 which included papers by Myers, Henry and Eleanor Sidgwick, as well as by Elliot Coues, A. H. Dailey, Richard Hodgson, Minot J. Savage, and Alfred Russel Wallace (Myers, 1893). The first real attempts at meetings designed specifically for psychical researchers, and for the purpose of improving psychical research from the professional point of view, were those held in Copenhagen (1921), Warsaw (1923), Paris (1927), Athens (1930), and Oslo (1935). These congresses brought together many well-known investigators of the time, among them: Sidney Alrutz (Sweden), Ferdinando Cazzamalli (Italy), Hans Driesch (Germany), Gustave Geley (France), Pierre Lebiedzinski (Poland), Oliver J. Lodge (England), William Mackenzie (Italy), Konstantine Oesterreich (Germany), Walter Franklin Prince (United States), Albert von Schrenck-Notzing (Germany), Charles Richet (France), William H. Salter (England), René Sudre (France), and Angelos Tanagras (Greece). In addition to allowing for the presentation of research and theory, the meetings were attempts at professionalization, as seen in efforts to standardize the terminology in the field.[3]

These were not the only efforts to bring workers together in parapsychology. Smaller scale regional meetings were held years later (e.g., Rhine, 1949; La Vie Métapsychique, 1949). In addition, the workers at Duke University, headed by J. B. Rhine, attempted to chart the future of the field by sponsoring a discussion in the pages of the *Journal of Parapsychology* that was extended to two issues of the journal and to which, in addition to Rhine (1948) and his associates (e.g., Humphrey, 1948), others such as Hornell Hart (1948), Gardner

Murphy (1948), G.N.M. Tyrrell (1948), and René Warcollier (1948) contributed. The Utrecht congress followed and expanded on this previous tradition of attempts to organize and plan for the future of parapsychology.

Research and Institutions

By the time the Utrecht congress took place, the study of parapsychological phenomena had developed in different parts of the world to the extent that an immense literature on research and theories had been amassed, as well as a variety of groups and communities founded.[4] Among the developments were: physical and psychological theories, research areas such as the study of mental and physical mediums, and the development of both qualitative and quantitative experimentation.

Limiting the discussion to both qualitative and quantitative ESP experiments conducted during the 1940s and early 1950s, it is important to mention the work of such individuals such as Gaetano Blasi (1946) in Italy, Orlando Canavesio (1947) in Argentina, Whately Carington (1945) in England, Gertrude Schmeidler (1943) in the United States, W. H. C. Tenhaeff (1957) in Holland, and René Warcollier (1950) in France. A particularly important and influential research team was that centered around J. B. Rhine and his associates in the United States (e.g. Birge & Rhine, 1942; Cadoret & Pratt, 1950; Humphrey, 1945; Pratt *et al.*, 1940).[5]

The institutions that played an important role included Rhine's Parapsychology Laboratory based at Duke University, the Society for Psychical Research in London, the Institut Métapsychique International in Paris, the Institut für Grenzgebiete der Psychologie und Psychohygiene in Freiburg, and Tenhaeff's Parapsychological Institute at the University of Utrecht. In addition, there were several parapsychology journals available indicating the existence of some low-level organizational structure in the field, and presenting the work of a variety of researchers with different methodological orientations. Among the journals were the *Journal of Parapsychology*, the *Journal of the American Society for Psychical Research,* the *Journal of the Society for Psychical Research*, *Metapsichica,* the *Revue Métapsychique*, and the *Tijdschrift voor Parapsychologie*. All of this illustrates aspects of the work and the organization of parapsychology before the meeting at Utrecht.

By the end of this period an important event took place that was essential in the development of the Utrecht congress. This was the founding of the PF, the main organization behind the congress.[6] The PF was founded in 1951 in New York City. It was the product of the work of two women: Eileen J. Garrett and Frances P. Bolton. Garrett, a well-known medium, but also a publisher and businesswoman, was the PF's President and main driving force. She became known in parapsychology through her participation in studies of her mediumship (e.g., Rhine, 1934; Thomas, 1937). In addition, Garrett published autobiographies (e.g., Garrett, 1939, 1949), and other writings (e.g., Garrett, 1941, 1950).[7] Bolton, the PF's Vice-President, was a philanthropist who had a distinguished political career, serving in Congress representing Ohio for 28 years. She also served as a Congressional Delegate to the United Nations, and was active in African affairs (Eiben, 2000; Loth, 1957). Before her involvement with the PF, which she generously funded, Bolton financed parapsychology research at Duke University starting in the 1930s (Mauskopf & McVaugh, 1980, pp. 137-138).

FIGURE 1.
Frances P. Bolton and Eileen J. Garrett

The partnership of Garrett and Bolton produced a unique organization devoted to the support of parapsychology. The main aspect of this support was financial grants, the magnitude of which can only be fully appreciated by examining the Foundation's reports (e.g., Annual Report, 1955; *Ten Years of Activity*, 1965), and archives. As argued elsewhere (Alvarado, 2008; Alvarado *et al.*, 2001), the grants the PF gave were of key importance to the development of modern

parapsychology, supporting experimental and spontaneous cases research, theoretical, historical and philosophical work, as well as travel, publications, and general organizational aspects (see also Appendix A). The vastness of the funding program led J. B. Rhine (1971) to state the following about both Garrett and the PF: "There has been nothing else to compare with the generosity of this Lady Bountiful of Parapsychology as she willingly poured out the financial aid needed by isolated workers in many countries struggling to do something in or near the field of psi research" (p. 61).[8]

But funding was only one of the PF's contributions. "The most outstanding project initiated by the Foundation," wrote the anonymous author of a note in the *Journal of the Society for Psychical Research*, "is the International Conference of Parapsychological Studies" (The Parapsychology Foundation, 1953, p. 115). This was the beginning of a sophisticated conference program that has continued to the present (Alvarado *et al.*, 2001, pp. 8-14; International Conferences, 2008), a topic I will briefly discuss later in this paper.

Another event, and one that influenced the location of the congress, was the establishment of a chair of parapsychology at Utrecht ('Chair of Parapsychology in the University of Utrecht', 1953).[9]

While this is an incomplete survey of the pre-Utrecht congress scene I hope it shows that there was a great deal of international activity in parapsychology at the time the congress was organized.

The Organization of the Congress

Beginnings

On February 20, 1953, the *New York Times* published an article called "Notes on Science" in which parapsychology was discussed together with such varied topics as concrete, bats, lupus, and fertility. The author of the article stated that, thanks to Garrett's efforts, "the prospect of holding an international conference of parapsychologists this year in the Netherlands is bright" (Kaempffert, 1953). In fact, the First International Conference of Parapsychological Studies was the first major project of the PF.

The congress was part of Garrett's general interest in the dissemination of information through meetings. Such an interest was clear even before 1953. In the first report of grants and activities of the Foundation it was stated that: "Parapsychology Forums have been organized by the Foundation in the autumn of 1952 in Los Angeles and

San Francisco. The President of the Foundation delivered a number of public lectures and held meetings with small study groups" ('Scholarships and Research Projects', 1953, p. 1).

Garrett wrote after the congress that she felt that after the Second World War there was a new generation of scholars who needed a meeting ground to discuss parapsychology with older and more established workers. In her words:

> In any event, the Conference I envisaged could offer the personal contact between men of learning, the type of exchange which alone can open the way toward an evolution of progressive thought. In a subject whose nature had not yet stimulated all the spiritual curiosity of science, it did appear that only if scientists might meet in fruitful discussion of facts can a continuity of effort emerge, as each evolves his own method of research
>
> (Garrett, 1954, p. 7).

From the beginning, Garrett saw the congress as the "most important and ambitious project" of the PF ('International Conference of Parapsychological Studies', 1953a, p. 2). After Michel Pobers, the PF's Executive Director, made many international contacts in Europe, and after discussions with American workers in the United States such as J. B. Rhine, Jan Ehrenwald, and Gardner Murphy, there was a meeting held in Paris at the Institut Métapsychique International on December 20-22, 1952, the purpose of which was the organization of the conference. The meeting included Robert Amadou (France), Hans Bender (Germany), René Hardy (France), Raphael Khérumian (France), William Mackenzie (Italy), S. G. Soal (Great Britain), Jean Tenaille (France), Wilhelm H. C. Tenhaeff (Holland), Robert Tocquet (France), René Warcollier (France), and George Zorab (Holland).[10]

They met with a memo from Garrett that offered some guidelines. Garrett (1952), who wrote to J.B. Rhine that she had been asked to prepare a memorandum for the Paris meeting, proposed the following three general topics:

> (1) Experimental parapsychology (recent experiments, methods of investigation and of scoring, instruments and machinery, etc.). Following Prof. J. B. Rhine's proposal, it was suggested that only scholars actively engaged in experimental work during the past five years should be associated with the work of this first commission.

(2) General parapsychology (discussion of reports on parapsychology in psychoanalytical treatment, methods of investigation of spontaneous cases, new fields of research, etc.).

(3) Organizational problems. Parapsychological bibliography and an Encyclopedia of psychical research (suggested by Dr Murphy); and International Association for experimental Parapsychology (suggested by Prof. Rhine); future conferences, etc.

('International Conference', 1953a, p. 2).

Furthermore, Garrett also suggested that there should be no more than 40 participants, that English and French should be the languages of the conference, and that the event should be held in the summer of 1954. In addition, she proposed:

> Committees for each of the three divisions of the conference should have two chairmen (whenever possible one American and one European), appointed in advance, who would be responsible for drafting the detailed agenda for their divisions, for soliciting and examining reports, etc.
>
> There should be an executive committee, composed of the six divisional chairmen and two representatives of Parapsychology Foundation, Inc.
>
> ('International Conference', 1953a, p. 2).

The committee accepted most of the suggestions, with the following modifications:

> a) Instead of the three divisions (experimental parapsychology, general parapsychology, organizational problems), the Paris conference suggests four divisions: (1) experimental parapsychology (quantitative studies and statistical investigations); (2) experimental parapsychology of non quantitative and non-statistical nature; (3) general parapsychology, with special reference to various parapsychological theories; (4) organizational problems. Detailed programs would be worked out in advance, and 'rapporteurs' would be selected for each division.
>
> b) The Paris group unanimously and emphatically recommended not to postpone the conference to 1954, but to convene it in Utrecht, Holland in 1953, from July 25-to August

> 2. Postponement of the meeting till the following year would be regretted by all those interested.
>
> c) For membership on the Executive Committee, the Paris group made the following proposals: 1st Division: Prof. J. B. Rhine, U.S.A., and Prof. S. G. Soal, Great Britain. 2nd Division: Prof. Hans Bender, Germany, and another member to be suggested by the Foundation. 3rd Division: Mr. René Warcollier, France, and another member to be suggested by the Foundation. 4th Division: Prof. Gardner Murphy, U.S.A., and Mr. René Sudre, France.
>
> ('International Conference', 1953a, pp. 2-3).

After two days the Paris group sent recommendations to the PF, among which was that Gardner Murphy be appointed Chair of the convention[11] and that Pobers be the Secretary General.[12] They also suggested the following:

> *The minimum purpose* of the Utrecht Conference should be: to assemble prominent scholars from different countries, representing a great variety of approaches to parapsychology, to facilitate personal contacts, to enable the first exchanges of views and information in this field to take place in a broadly comprehensive and nonpartisan atmosphere.
>
> *The maximum purpose* of the Conference could be to take stock of the present position of parapsychological research in the various fields and in the different countries, to determine and agree upon what type of research is most urgently needed at this time, and to have a number of specific projects, national and international in scope, discussed and recommended by the experts for future consideration and adoption by the Parapsychology Foundation.
>
> (Pobers, 1954c, p. 10)

Committees

The PF decided to go forward with the recommendation and they constituted on February of 1953 the following executive committee under Murphy's chairmanship: Hans Bender (from Germany), John Björkhem (Sweden), F. J. J. Buytendijk (Holland), Gabriel Marcel (France), H. H. Price (Great Britain), J. B. Rhine (USA), Emilio Servadio (Italy), S. G. Soal (Great Britain), René Sudre (France), Wilhelm H. C. Tenhaeff (Holland), Robert H. Thouless (Great Britain),

Hubert J. Urban (Austria), and René Warcollier (France). Later Buytendijk was replaced by Th. Fischer (Holland), George Zorab (Holland) joined, and Rhine dropped out from the conference entirely, to the point of not attending at all. [13]

The committee, according to Pobers (1954c), continued to be active throughout the conference, at the end of which it delegated its work to a follow-up committee constituted by Murphy (Chair), Fischer, Garrett, and Pobers (Secretary General). Both Robert Amadou and George Zorab were invited to serve as assistant secretaries of the congress. In addition, Kathleen Goldney took care of organizational work regarding England (The First International Conference of Parapsychological Studies, 1953b, p. 3).

A Dutch committee was also formed that included M. A. Reinalda (Governor of the Province of Utrecht), C. J .A. de Ranitz (Mayor of the Town of Utrecht), G. A. W. ter Pelwijk (President of the Curatorium of the University of Utrecht), V. J. Koningsbergen (Rector Manificus of the University of Utrecht), and A. Plesman (President of KLM Airlines) (Dutch Honorary Committee, 1953). The practical aspects were in the hands of Fischer, Tenhaeff, and Zorab.

Several cities such as Paris, Rome, and Zurich had been mentioned as the location of the event, but Utrecht was selected due to the newly created parapsychology chair held by Tenhaeff. "The decision to hold the Conference in Utrecht," wrote Pobers (1954c, p. 11), "was not only a well deserved tribute to the scholarship and the outstanding courage of Professor Tenhaeff—it reflected the determination of our Foundation to work *with* and *within* the Universities, striving to obtain the recognition of Parapsychology as a science having its rightful place in the academic curricula."

The Executive Committee chaired by Murphy was faced with a difficult task, the arrangement of the program of the conference through postal correspondence between eight countries. As summarized by Pobers (1954c, p. 13):

> The general trend and the central idea of the program were (1) to offer every opportunity to the members to exchange and discuss their research findings during the official meetings, (2) to allow ample time for private talks and informal meetings and discussions.... [At Utrecht] members were invited and encouraged to submit written reports, as substantial as possible, but oral presentations had to be strictly limited, both in number and in time.

> Some twenty reports were mailed to the members before their arrival in Utrecht; many more were distributed during the Conference. It was obviously impossible for anyone ... to read all the material... but oral presentation of all this material would have required not a six-day study conference, but a sixty-day assembly.

An anecdote told by George Zorab (1987) illustrates some of the forgotten organizational aspects of the program. Zorab said that in April 1953, while he was in bed sick, Robert Amadou and Michel Pobers were not successful in obtaining hotel rooms at Utrecht for the conference attendants, and "Mrs. Garrett had called the whole thing off" (p. 141). When he was well, Zorab went to speak with the owner of the best hotel in town. The hotel owner did not want to rent the rooms because he had suffered too many last minute cancellations with conferences. In Zorab's words: "As I knew that Mrs. Garrett and Mrs. Bolton had lots of money I told him that I was prepared to deposit with him a thousand guilders. If the conference was not held at the date agreed upon the money would be his. He consented, and so the conference was held" (p. 141). Later on, Zorab continued, he had other tasks: "I had to do everything myself such as hiring direct translators from Geneva, renting rooms in the University building where the conference would be held, and so on" (p. 141).

Expenses

At one point the expenses for the meeting were estimated to be around $25,000. The first issue of the PF newsletter reported: "This amount is small, considering the tremendous new vistas such an international gathering will open to the new science of psychical research, but it is large in terms of the very limited number of people who realize the importance of parapsychology in the atomic era" ('International Conference of Parapsychological Studies', 1953a, p. 3). An estimated budget prepared by Pobers, and dated May 11, 1953, suggested the event would cost $29,525 (Pobers, 1953a).[14] This estimate included the expenses incurred in: travel fellowships for 40 participants ($19,400), the work of the General Secretary ($4,320), typists and office tasks ($1,500), translations and preparation of documents ($1,000), rental of tape recorders, translation system and personnel ($875), transportation in buses ($400), and Sunday excursion and luncheon ($600), among other expenses.

A memo showed that, by June 9, 1953, $25,000 had been contributed by Bolton. Furthermore, the memo listed contributions by the PF ($3,925) and by the Dutch Committee, the latter apparently to cover the expenses of an excursion (Owen, 1953a). By September 18, $33,000 had been raised for the conference (Owen, 1953b).

Furthermore, the organization of the event took a great of deal of time. Garrett (1953a, p. 1) estimated that the PF worked intensely for around ten months, an effort that involved a variety of tasks such as extensive correspondence, editing and translation, and meetings and discussions. Although it was not mentioned by Garrett, this must have included multiple salaries of the individuals involved in the tasks.

Parapsychology at the Congress

The Program and the Participants

The congress took place at the University of Utrecht between July 30 to August 5 of 1953 as a joint effort of the PF and the University. The purpose of the meeting was to present the state of the art of parapsychological research, recent research, and theory, and to chart future developments, topics I will briefly discuss in the next sections.

The daily schedule was divided between meetings of working groups (from 9:30 a.m. to 12:30 p.m.) and plenary meetings when everyone came together (from 2:30-4:30 p.m., and 5:30-6:30 p.m.). The proceedings were both filmed and taped, and translation services were offered to the attendants. As described by an attendant: "The desks were fitted with headphones through which by the turn of a knob one could hear the miracle of English, French and German simultaneous translation. A tape recording was made of every spoken word, and each day we were supplied with a transcript of the previous day's verbatim reports .... (My own collection of records had to be sent home in three separate parcels) ...." (Hankey, 1953, p. 242).

The social aspect was not neglected. In addition to meals and informal meetings, there were excursions arranged by the Dutch ('The Excursions', 1953; Hankey, 1953, pp. 242-243).

Furthermore, the congress was not open to everyone, it was by invitation only. No specific organizations were asked to send representatives, and no one was expected to represent anyone but themselves. While originally the number of participants was put at 45, this had to be doubled. Furthermore, there were individuals who

wanted to attend but could not be invited, a situation that created some resentment (see 'Apologies and Thanks', 1953; Pobers, 1954c).

TABLE 1
Members of the Utrecht Congress

Lydia W. Allison (USA)	C. A. Meier (Switzerland)
Robert Amadou (France)	Heinrich Meng (Switzerland)
Hans Bender (Germany)	H.M.J. Mitchell (England)
John Bjorkhem (Sweden)	C.W.K. Mundle (England)
Gothard Booth (USA)	Paul Vasse (France)
G. Spencer Brown (England)	René Warcollier (France)
Hilde Bruch (USA)	G.D. Wassermann (England)
Orlando Canavesio (Argentina)	Thorstein Wereide (Norway)
Wilfred Daim (Austria)	Joseph Wüst (Germany)
Laura A. Dale (USA)	George Zorab (Holland)
Eric Dingwall (England)	Gardner Murphy (USA)
Jacques Donnars (France)	H. Musaph (Holland)
Jan Ehrenwald (living in the USA)	J. Fraser Nicol (England)
Jule Eisenbud (USA)	Edward Osborn (England)
Th. Fischer (Holland)	F. Claude Palmer (England)
Anthony G. Flew (England)	Denys Parsons (England)
Haakon Forwald (Sweden)	C.A. van Peursen (Holland)
Gebhard Frei (Switzerland)	J.J. Poortman (Holland)
Jean Gebser (Switzerland)	Dorothy H. Pope (USA)
William H. Gilliespie (England)	H. H. Price (England)
Kathleen M. Goldney (England)	Wolf D. Rasch (Germany)
Hornell Hart (USA)	William G. Roll (then from England)
Gustav Hartlaub (Germany)	Louis Rose (England)
Peter Hohenwarter (Austria)	William H. Salter (England)
Winfried Hümpfe (Austria)	Hans Schaeffer (Germany)
Betty Humphrey (USA)	Gertrude Schmeidler (USA)
David Kahn (USA)	Michael Scriven (USA)
Jan Kappers (Holland)	Emilio Servadio (Italy)
W. Kat (Holland)	Aage Slomann (Denmark)
Raphael Khérumian (France)	S. G. Soal (England)
J.M.J. Kooy (Holland)	Stefano Somogyi (Italy)
Sven Krohn (Finland)	F.J. Stratton (England)
D.J. van Lennep (Holland)	René Sudre (France)
William MacKenzie (Italy)	W. H. C. Tenhaeff (Holland)
A. Mak (Holland)	R. H. Thouless (England)
Gabriel Marcel (France)	S. W. Tromp (Holland)
Marcel Martiny (France)	Montague Ullman (USA)
Robert McConnell (USA)	Hubert J. Urban (Austria)
Joost A. M. Meerloo (Holland)	Christine Vasse (France)

Table 1 presents 78 members of the congress listed in the conference proceedings ('List of Conference Members', 1955), among whom were presenters (59%) and attendees. As is still the case in recent parapsychology meetings, most of the members were men (90%). While the list is not exhaustive—many Dutch local and other individuals were not included—it shows many figures well-known in parapsychological circles. Among those on the list were Hans Bender, Eric J. Dingwall, Kathleen M. Goldney, Betty Humphrey, William H. Salter, Gertrude Schmeidler, Emilio Servadio, S.G. Soal, René Sudre, W.H.C. Tenhaeff, and René Warcollier. Some members of the conference were eminent figures in their fields, such as philosophers Gabriel Marcel and H. H. Price. Gardner Murphy, Robert H. Thouless, and D. J. van Lennep were well-known psychologists.

As seen in Table 2, members came from 13 countries, the most frequent being Great Britain and the United States. However, and as pointed out by Murphy (1955, pp. xvii-xviii), countries such as China, India, and Japan were not represented. He looked forward to the time when "another international meeting will include more of humanity than is comprised in the Western European culture" (p. xviii).

Interestingly, and providing a link with the past, three participants had attended the old congresses held in the 1920s and 1930s: William Mackenzie, William Salter, and Thorstein Wereide. They presented a joint statement acknowledging the earlier meetings, and referring to the 1953 meeting as a "successful start ... in reviving international co-operation in this important field of study, after the interruption caused by the Second World War...." (Mackenzie, Salter & Wereide, 1955, p. 134).[15]

Some of the organizers also made opening remarks. One of the most important comments was made by the Chair of the proceedings, Gardner Murphy:

> How much shall we attempt to accomplish this week?
>
> Breadth we shall certainly seek, but if this is to be the historic occasion which I hope it may become it certainly cannot attempt to survey all the facts, or even all the classes of facts, known to our subject. Rather, for the sake of perspective, it will, I think, have to attempt clarity, order, good sense, and a respect for sound method, and beyond it all, a sense of understanding one another, a spirit of intellectual comradenship in which true communication is possible....

> We shall attempt first of all to set up the organizational structure of the week's work: then establish the four working groups announced on the program which you have received, using the working group on the 'Personality of the Sensitive' as an occasion for plenary meetings in which every approach, biographical, cultural, clinical, experimental, quantitative, philosophical, integrative, can be attempted. Towards the end of our meeting we shall have several plenary sessions, devoted to the tasks of surveying and understanding what we have done, determining the form of our contact with the public, defining our relations to other sciences and to the universities, finding and cultivating research workers, and looking forward to questions regarding the future of parapsychology and its contribution to civilization (Murphy, 1955a, p. xix).

TABLE 2.
Geographic Locations of Congress Members (N = 78: taken from 'List of Conference members', 1955)

Country	Percent
Great Britain	24%
USA	19%
Holland	17%
France	12%
Germany	6%
Austria	5%
Switzerland	5%
Italy	4%
Sweden	3%
Argentina	1%
Denmark	1%
Norway	1%
Finland	1%

Working Groups

In addition to general topics discussed in plenary sessions and in simultaneous sessions, the latter were mainly for the presentation of the work of working groups. According to an attendant: "It was difficult to make the choice between three programs offered at the same time—something like having a smug seat in the Center Court at Wimbledon but knowing that the most exciting matches were proceeding on Courts 1 and 2 (Hankey, 1953, p. 242)."

These working groups dealt with quantitative studies, the psychotherapeutic and psychoanalytic approach, spontaneous phenomena and qualitative research, and the personality of the sensitive. In what follows I will summarize aspects of the original four groups, some of which papers are listed in Table 3 (N.B. this is but a selection of some of the papers presented—see also Amadou, 1955, and 'Proceedings of the First', 1955).

Quantitative Studies. This working group consisted of statistical experimental studies and discussions of methodology. A highly controversial paper was 'Psychical Research as a Test of Probability Theory', by G. Spencer Brown (1955) in which he questioned assumptions of randomness in the statistical evaluation of card-guessing tests to the point of saying that parapsychologists could not claim they were dealing with forms of communication. His ideas were critically discussed by several participants, among them S. G. Soal (1955). According to a summary of the reception of Brown's paper: "It was generally felt that, while this paper led to realization of the necessity for vigilance in examining presuppositions underlying applications of statistical methods, it did not give ground for doubting the general validity of the results of quantitative researches evaluated by current methods" ('Summary No: 1', 1953).[16]

An example of an experiment was 'Researches in Psychokinesis by Means of Placement Technique' by Haakon Forwald (1955) from Sweden. The experimental task was to attempt to influence dice to fall in a particular area of a table. Forwald reported statistically significant effects consistent with intention obtained in 71,500 die throws. Another experimental report came from France. Authored by Paul and Christine Vasse (1955), it described statistically significant dice and card tests with their daughter when she was one year old and later aged 3½ years.

There were also conceptual papers, such as that presented by English psychologist Robert H. Thouless (1955). He discussed common methodological problems in experiments. These included the lack of randomization of experimental conditions, inappropriate methods to assess significance of difference between two groups of participants, and the use of simple experimental designs instead of more appropriate complex ones. In the discussion of this paper "Mr. Kahn doubted whether any of these defects would be found in recent experimental work. Dr Thouless replied that he could give examples of all three. Criticism (e.g. by Dr Schmeidler) centered on the second of the above alleged errors of method" ('Summary No: 2', 1953).

TABLE 3.
Examples of Papers Presented at the Congress

Group & Authors	Title
Quantitative Studies	
G.S. Brown	*Psychical Research as a Test of Probability Theory*
H. Forwald	*Researches in PK by Means of Placement Technique*
R.A. McConnell	*Possibilities & Pitfalls of Quantitative Methods in Psi Research*
S.G. Soal	*Is ESP a Statistical Artifact?*
R.H. Thouless	*Experimental Design in Parapsychology*
S. Tromp	*Recent Experiments on Physical Aspects of the Muscle-Tonus-Reflex (Dowsing)*
P. Vasse & C. Vasse	*PK and ESP Experiments with Martie Vasse*
J. Wüst	*The Influence of Some Physical and Chemical Factors on Parapsychological Phenomena*
Psychotherapeutic and Psychoanalytic Approach	
W. Daim	*Parapsychological Observations During Psycho-analytical Treatment*
J. Ehrenwald	*Psi, Science, and Psychoanalysis*
J. Eisenbud	*Psi and the Problems of Disconnection in Science*
H. Meng	*Parapsychology, Psycho-Hygiene and Medical Psychology*
J.A.M. Meerloo	*Precognition in the Therapeutic Setting*
E. Servadio	*Depth Psychology and Parapsychology*
M. Ullman	*The Dream, Schizophrenia, and Psi Phenomena*
H.J. Urban	*Parapsychological Research at a Psychiatric Clinic*
Spontaneous Phenomena and Qualitative Research	
H. Bender	*Precognition in the Qualitative Experiment*
H. Hart	*Traveling ESP*
W. Kat	*The Need for "A Medical Verification Center"*
W. Mackenzie	*Biological Roots of Parapsychology*
R. Sudre	*Parapsychology in Nature*
W.H.C. Tenhaeff	*Psychoscopic Experiments on Behalf of the Police*
G. Walther	*A Plea for the Introduction of Edmund Husserl's Phenomenological Methods into Parapsychology*
R. Warcollier	*Antagonisms in Mental Images and the Problem of Psi-Missing*
Personality of the Sensitive	
S. Krohn	*Methodological Sensualism and Parapsychology*
J. F. Nicol & B. Humphrey	*The Exploration of ESP and Human Personality*
G.R. Schmeidler	*Personality Tests and ESP Scores with College Students*
W.D. Rasch	*Psychodiagnostic Examinations of Sensitives*
O. Canavesio	*Electro-Encephalography of Metapsychical States*

A different approach was taken by Solco W. Tromp (1955) from Holland. In his 'Recent Experiments on Physical Aspects of the Muscle-Tonus-Reflex (Dowsing)', Tromp discussed a variety of dowsing experiments that included the detection of magnetic fields, measurement of electrical skin potential variations in dowsing zones, and measurement of properties of the soil. He argued that his findings suggested that there was a "real physiological process resulting from stimulation by external geophysical fields" (p. 25).

Psychotherapeutic and Psychoanalytic Approach. This emphasis was clinical, including psychotherapy and psychoanalysis. Jan Ehrenwald (1955) wrote about 'Psi, Science, and Psychoanalysis'. He argued for the existence of elements of ESP in the psychoanalytic situation that provided unique identifiable aspects of the experience allowing both for verification and an examination of psychological significance. Emilio Servadio (1955) believed that telepathy was an efficient tool in analytic work. Furthermore, he suggested that ESP is less practical than usual forms of communication, but that its usefulness was to allow us to understand the mind. The process, he thought, belonged to a "pre-individualized unconscious and primitive psycho-biological world" (p. 70). Other psychoanalytic ideas were presented by Wilfried Daim (1955) who discussed cases of ESP during psychoanalytic treatment. ESP was seen to exert a significant influence on the analytic process. In his discussion of 'Parapsychological Research at a Psychiatric Clinic', H. J. Urban (1955) defended the possibility that in some pathological conditions sensory deficits were compensated for by ESP. He reported that some patients showed changes in ESP capacity after narco-analysis or electro-shock, and that psychic phenomena may be a function of the superconscious.

According to a report of the discussions after some the presentation of some of these topics: "Dr Rose and Prof. Servadio and Dr Daim spoke about the necessity of gathering more data and getting more psychiatrists interested in the field. Dr Ehrenwald stressed the need to evaluate situational and dynamic factors involved in Psi occurrences in the psycho-therapeutic situation .... Dr Rose recommended the introduction of the issue of parapsychology in the psychiatric and psychologic curriculum" ('Final Summary', 1953).

Spontaneous Phenomena and Qualitative Research. This working group included a variety of spontaneous psychic phenomena as well as qualitative approaches that included conceptual discussions and

qualitative experiments. Hans Bender (1955) discussed 'Precognition in the Qualitative Experiment', which consisted of a report of the empty chair precognitive experiments with Dutch psychic Gerard Croiset. Tenhaeff (1955) presented other observations of Croiset's attempts to help the police such as finding missing objects and persons that were alive, and obtaining information about dead missing persons, and about crimes.

American sociologist Hornell Hart (1955) discussed out-of-body experiences. His talk emphasized those experiences with veridical perceptions. "The cases presented," he wrote, "(which range in time between from 1850 up through 1938), plus 29 other cases summarized in a longer paper, would appear to establish a *prima facie* case for the reality of ESP travel. But the hypothesis cannot be regarded as conclusively proved" (p. 92).

The section also included a conceptual talk by William Mackenzie (1955) about 'Biological Roots of Parapsychology'. He argued that psychic phenomena provided a link between all living things and that ESP was a principle in nature. In his words: "Without constant and active extra-sensory perception, construction and maintenance of huge termite colonies would be inconceivable" (p. 100). Furthermore he believed that animals could show paranormal knowledge. Through ESP, Mackenzie said, "the individual maintains constant relations with other organisms, with the Bios and the Cosmos, in order to maintain conservation of the species" (p. 102).

Personality of the Sensitive. This section was devoted to the psychology of individuals who provide evidence for ESP. In work that had been funded by the PF, J. Fraser Nicol and Betty Humphrey (1955) used personality questionnaires in conjunction with card-guessing. They found that the best results were related to self-confidence and to emotional stability. Another card-guessing study was reported by Gertrude R. Schmeidler (1955), who reported that poor adjustment produced chance ESP results, that aggressive participants had below chance scores, and that there were statistically significant high scores with participants that said that they were relaxed during the tests.

W. D. Rasch (1955) discussed 'Psychodiagnostic Examinations of Sensitives'. The handwriting of 39 persons with spontaneous psychic experiences and eight psychics was analyzed. They tended to show unstable dissociative personality structures (with tendency towards extraversion). Furthermore, they were described as emotionally impressionable, and as having lively, sensitive, and tense personality

structures. Furthermore they had a high susceptibility to outside stimuli and disturbing influences. In an interesting, and then very novel study, Orlando Canavesio (1955) discussed the electroencephalographic activity of psychics from Agentina. The EEG patterns were described as showing normal activity, and as being similar, but not identical, to Loomis' A and B stages of normal sleep.

Other Presentations and Recommendations for Future Work

Miscellaneous Topics. There were some discussions of other topics. Roundtables were held about parapsychology and the natural sciences, healing, and philosophical issues ('The Utrecht Conference', 1953). In the latter, William G. Roll "discussed terminology and attempted to provide a set of definitions of the terms and concepts commonly used in psychical research" (Pobers, 1954c, p. 21). Later the same day there was a meeting devoted to mediumship and the issue of finding gifted subjects.

Additional papers focused on other topics. These included a glossary of terms in several languages (Amadou, 1953), and the compilation of a bibliography of parapsychology (Frei, 1953). In a theoretical paper presented in a plenary session, G.D. Wassermann (1953) developed ideas that brought together parapsychology and fields such as psychology, neurophysiology, and morphology. This included, among other topics, discussions about psychokinesis and precognition. In comments about the paper both Jan Ehrenwald and Gardner Murphy focused on precognition ('Summary No: 5', 1953).

Furthermore, the group on spontaneous and qualitative phenomena was invited by Tenhaeff and Fischer to witness a demonstration of ESP by Croiset (Pobers, 1954c, p. 19). There was a committee of future projects presided over by Th. Fischer that held several meetings through the congress. Among other topics the committee focused on discussions of funding.

Publications. In addition to the presentation of papers from the working groups there were meetings organized by a committee for future developments. Some of the plans for the future included publications:

> It is recommended that Utrecht should remain a center for the publication of the reports of the Conference. It is further recommended that, if possible, Utrecht become the world headquarters for the publication of works in the field of parapsychology.

> Finally it is suggested that the question of publication of the reports and proceedings of the Conference should be referred to a three-man committee consisting of Dr Osborn, Mr. Zorab and Dr Pobers. If possible, such Committee should meet in London in a later part of September 1953 and work out a complete plan for submission to the Chairman of the Conference, Dr Gardner Murphy.
>
> ('Resolutions', 1955, pp. 130-131)

According to Pobers (1954c, p. 25) this publication of parapsychological work was not to de-emphasize the importance of established journals, but to complement them. However, this never materialized due to a variety of problems.[17]

The actual proceedings of the congress were never published in full. An unpublished report at the PF archives dated September 24, 1953, records the decision not to publish the full proceedings ('Conference Report', 1953), but to publish instead some papers as special monographs (which did not take place), and to include some in *Tomorrow* magazine, a publication started by Garrett. However, a few years later the PF published abstracts (*Proceedings of the First International Conference of Parapsychological Studies*, 1955), and some papers appeared in French under the title of *La science et le paranormale*, as a special issue of *Revue métapsychique* and as a book (Amadou, 1955).[18]

Organizations. An outgrowth of the conference was the creation of two organizations at Utrecht, a project that did not go forward. Garrett wrote:

> In its resolutions, the Utrecht Conference made three specific recommendations with respect to our work in Europe:
>
> a) That we establish at Utrecht a European Headquarters of the Foundation;
>
> b) That all the Conference papers and other parapsychological works be published at Utrecht;
>
> c) That our Executive Director, Dr Michel Pobers, be authorized to spend each year several months in Europe, coordinating the work in the different countries and acting as a "liason officer" of the Foundation.
>
> (Garrett, 1953a, p. 2)

The PF archives have some information about the creation of two organizations that did not develop as planned. One was the

International Center of Parapsychological Studies (ICPS), and the other the Utrecht Workshop of Psychological Research (UWPS) (Pobers, 1954a; 'Statement about Utrecht-Based Organizations', n.d.). The ICPS was to "assist Prof. Tenhaeff in a program of experimental work at [Tenhaeff's] Parapsychology Institute, in cooperation with other European centers such as the SPR. Dr Soal, Dr Thouless and others have agreed to take part in investigations of sensitives, new types of card experiments, etc." (Statement about Utrecht-Based Organizations, n.d., p. 1).

The UWPS was to "deal with so-called 'border-line problems,' such as unorthodox healings, psycho-physiological studies of mediumship, psi factors in psycho-analytical and psychiatric practice, psychical phenomena in coincidence and accidents, etc." ('Statement about Utrecht-Based Organizations', n.d., p. 1).

Conferences. There were also follow-up committees for the holding of future conferences:

> It is recommended that a Second International Conference of Parapsychological Studies should take place four years from now, also under the auspices of a university, and that in between a few specialized Conferences or symposia should be arranged, notably the following:
>
> a) A Conference on Relations between Parapsychology and other Scientific Disciplines (perhaps to be held at the University of Heidelberg). This should be attended by a small number of leading scientists of different countries in the different branches of the so-called natural and exact sciences, in order to discuss the most promising methods and types of research in future parapsychological studies, with the view of obtaining support for further research ...
>
> b) A Conference on Philosophical Interpretations (perhaps to be held in Switzerland under the leadership of Dr Marcel, Dr Price and Mr. Gebser).
>
> c) An international Conference on the Investigation and Reporting of Spontaneous Cases, perhaps to be held in London under the leadership of Mr. Salter and his associates of the British Society for Psychical Research, which, it is hoped, will prepare an outline of the subject matter to be considered at the Conference ('Resolutions', 1955, pp. 131-132).

The last two were held after Utrecht and were organized by the PF, together with other individuals. They were called International Philosophic Symposium (in 1954) and Conference on Spontaneous Phenomena (in 1955). In addition, the PF held other meetings during the 1950s. These included the International Study Group on Unorthodox Healing (1954, a topic discussed at Utrecht), the International Symposium on Psychology and Parapsychology (1956), the International Research Coordination Meeting (1957), the Conference on Parapsychology and Psychedelics (1958), the Conference on Parapsychology and Pharmacology (1959), and The Study of Precognition: Evidence and Methods (1959).[19] All of these meetings were inspired by the discussions at Utrecht (for information about these meetings see International Conferences, 2008; *Proceedings of Four Conferences of Parapsychological Studies*, 1957; *Proceedings of Two Conferences on Parapsychology and Pharmacology*, 1961). All of these later conferences, and particularly the ones on healing, philosophy, and spontaneous phenomena, are part of the legacy of the Utrecht congress, as channeled through the PF.

Bibliography. A bibliographical committee was appointed that eventually ended in George Zorab's *Bibliography of Parapsychology* (1957). As stated in the introduction to the bibliography, Zorab was a member of this committee together with Michael Scriven, Eric J. Dingwall, and Gebhard Frei. Scriven left the committee and Zorab was at the end responsible for the final bibliography. This work, he states, was a selected, and by no means a complete, bibliography. [20]

Contact Between Researchers. Another recommendation was about future contact between researchers:

> An important purpose of this Conference is to promote international cooperation by correspondence between those interested in specific problems of psychical research. It is the sense of the Conference [that] the Follow Up Committee should encourage, facilitate and support individuals and groups of members, who wish to engage in cooperative projects of international research insofar as such projects commend themselves to the judgement of the Follow Up Committee.
>
> ('Resolutions', 1955, pp. 132-133)

It is not clear how much of this actually came to pass after the congress. However, the work on out-of-body experiences by Hornell Hart was an outgrowth of his presentation in the meeting (Hart &

Collaborators, 1956). Hart (1957) wrote later that this project was a development from the 1953 Utrecht conference and the 1955 conference of spontaneous phenomena: "Between these conferences, this investigation was promoted by forty-eight collaborators from twelve countries .... They carried on creative discussion by correspondence, by means of which they clarified the basic definitions, helped to develop an Evidentiality Rating Scale, criticized each other's ideas about apparitions, and added new cases to the collection on which the study was based" (p. 94).

Following the discussions on psychotherapy and ESP at Utrecht, a group of psychiatrists associated within the American Society for Psychical Research developed a questionnaire to collect information about ESP during psychotherapy (Ehrenwald, 1955a; Murphy, 1954b, p. 76). It is not clear whether this project developed further.

Five Year Plan. As stated in the published proceedings about meetings held by the International Advisory Committee on Future Projects the "most inspiring meeting of the group" was that in which Garrett "developed the prospects of a five year plan of continuous support and interest in parapsychological research by the Parapsychology Foundation" ('Resolutions', 1955, p. 130). Pobers (1954c) discussed Garrett's statement at the conference as follows:

> She assured the Committee that, after having spent so many years in practical research, she knew intimately the conditions of life and work in the different countries and could appreciate 'the courage required from a professor or doctor who, in entering this field of parapsychology, is doing so at the risk of his academic career' .... According to Mrs. Garrett's statement, through conferences, publications, scholarships and grants secured by the Foundation during the next few years, parapsychologists in the different countries are to be encouraged and helped to find new resources, to prepare a brighter future (p. 27).

Other Topics. Other suggestions did not develop into further action. These included the future work of committees to further ethnographic (specific groups) and ethnological (folklore and beliefs) studies, to continue studies related to psychoanalysis and psychotherapy, and to develop a center for the study and verification of healings. However, there was a follow-up on the topic of healing by way of the above-

mentioned conference on the topic the PF held in 1954 (*Proceedings of Four Conferences of Parapsychological Studies*, 1957).

Thanks to organizers. Many members of the parapsychological community expressed their gratitude to the PF and the Dutch authorities involved in the organization of the conference. Certificates signed by some of the attendants were given to Garrett, Bolton, and Pobers in recognition of their work (Pobers, 1954c, p. 28). The one given to Garrett is displayed at the Eileen J. Garrett Library of the Parapsychology Foundation. This document includes many signatures such as those of: Lydia Allison, Jule Eisenbud, Anthony Flew, Robert McConnell, H. H. Price, William Salter, Emilio Servadio, W. H. C. Tenhaeff, and Robert Thouless. Murphy (1953b) later referred to the "generosity of the Parapsychology Foundation [which] made it possible to organize the Conference on a scale that could not previously have been attempted" (p. 179, footnote). The Dutch contribution was also acknowledged. As Garrett (1953b) wrote before the event: "The hospitality offered to the Conference by the University of Utrecht is a gesture of great generosity and remarkable foresight" (p. 1).

Certainly the work involved in the organization of the Utrecht congress was prodigious. In addition to all the logistics at the location, the crafting of the program, and the selection and invitation to participants, there was much work after the event, as seen in the above-mentioned projects. But in addition to this there was much administrative work inside the Foundation. An example was a report of activities Pobers (1953b) sent to the lawyers of the PF. He wrote:

> The first two days after my return from Europe were spent in inter-office discussions, meetings with the accountants and auditors, regarding the Utrecht Conference reports, Conference expenses, a visit to the attorneys, etc. During the remaining fifteen days, I have written 1) a report to the Follow-up Committee, 2) a report on the Special Committees, 3) a report on my proposed activities in Europe. I have completed, for the auditors, 4) the first part of the financial report of the Utrecht Conference, involving classification and analysis of some 700 vouchers and receipts, and 5) the second part concerning scholarships and grants (p. 1).

The Importance and Impact of the Congress

While parapsychology has a long history of attempts to organize its subject matter through a variety of conferences and meetings, there has been little historical work about the topic. Such social events—important to all sciences—serve to transcend local developments and, in turn, foster collaborative research. They also help to assess the state of the art and suggest possible future directions.

The congress was, to a great extent, a state of the art summary of aspects of the parapsychology of the era. The emphasis was on ESP, something that continues to be the case today. Interestingly, the issue of research on survival of bodily death was not covered. This may have been part of the design of the conference planners, since Murphy (1955, p. xix) had already commented on the practical impossibility of including all possible areas and topics.[21]

It may be speculated that the magnitude and range of the event, and the attempts to organize the field by charting future research, were a manifestation of the growing professionalization of parapsychology. We may speculate further and suggest that the meeting probably gave workers in the field a more cohesive image of the profession, presenting different specialties and research communities. After all, this event was the largest of its kind ever, representing a variety of current problems of methodology and theory, as well as showcasing the existence of a small but significant international community willing to come together for a common cause. We believe this is, at least in part, what Garrett (1953b) had in mind when she wrote that "one of the most important things the Utrecht meeting will achieve will be to provide an opportunity for so many scholars to get together, to get to know each other, to renew old friendships and build new ones" (p. 1).

In addition, Utrecht was, and remains today, an example of the virtues of international collaboration. Murphy (1953a) stated that the congress "marked an important forward step in understanding surmounting the barriers of nationality, language, and professional specialization" (p. 402). It was the first real international meeting since the old psychical research congresses held between 1921 and 1935, and probably the largest event of its kind in the history of the field. In an email to me Gertrude Schmeidler recollected: "It was a real international meeting. Many besides myself were intensely interested in meeting the persons whose work we had been reading so avidly for

years, and were also curious, mildly interested, in hearing those whose approach was different from our own" (Schmeidler, 2007).

While one hopes that events like this will improve international communication, and Pobers (1954c) believed this did happen, Murphy (1954a) argued that the conference had some communication problems. He discussed the topic as follows:

> Now the first thing to notice was that there was some stratification by language group and by nationality. That is to say, people from certain countries tended to go to certain kinds of meetings. You could be perfectly sure that the French and the Dutch would turn up in large numbers at discussions of spontaneous and qualitative phenomena; you could be perfectly sure that a large proportion of the English-speaking group would turn out for the quantitative sessions.
>
> Now what will this mean as far as international understanding is concerned? Of course you can have head-phones, à la United Nations; you can have marvelous three-language communication system; then you look around and find at times that almost nobody is using head-phones, and you begin to wonder. It isn't that the English-speaking people are so perfectly versed in French and German that they don't have to have any head-phones. You begin to wonder if there are other things involved. And before long, you begin to realize that you have mostly English-speaking people talking to English-speaking people at this particular meeting. This is again a little exaggeration, and please forgive it. But what you discover is that there is a polarization of each group around the national interests that characterize that group (p. 91).

Murphy believed that there were national and cultural differences that made real communication difficult. A problem he mentioned was that some leaders in the field were important—or even known—to some, but not to others. In his words: "There isn't any one person in psychical research that all psychical researchers have read—not Richet, not Geley, not William James, not Driesch. There isn't any one name that brings up to all psychical researchers a systematic pattern of findings or ideas" (p. 92).

While Murphy admitted that there was some communication between different nationalities, he believed the cultural blinders were too strong. In his view, even in the case of dealing with the same

phenomena there were different conceptual and methodological assumptions: "We have the specially colored glasses which are issued in childhood to the children of each cultural group" (p. 93).

Even considering these problems, which are not unique to the meeting in question, the congress has been considered generally an important historical event in modern parapsychology. It has in fact become a 'myth of origins' in the sense that a number of people consider it to have been an historical marker that justified beginnings and transitions in the field (on this concept see Vandermeersch, 1994). This is clear from the fact that the first Utrecht has been mentioned repeatedly in the literature as key event in the history of parapsychology (e.g., Cassoli & Cassoli, 2000, p. 16; Rhine & Pratt, 1957, p. 207; White & Dale, 1973, p. 220).

It has been claimed that parapsychology achieved scientific recognition because of the congress (Avant-Propos, 1954, p. 6). I have already mentioned Garrett's statement that the congress was a "turning point in the history of parapsychology as a science" (Garrett, 1953a, p. 1). Others have made similar statements. In Brazil some have claimed that "'official' recognition [of parapsychology] as a science comes from 1953" (Quevedo, 1964/1972, p. 25).[22] But I believe that, as important as Utrecht may have been for the identity of parapsychologists and for international relations at the time, it was neither a turning point nor the origin of overall scientific recognition for the field. The congress brought visibility and publicity to the field outside parapsychology (e.g., Murphy, 1953a; 'Psychical Research', 1953).[23] Representatives of Utrecht university may have had an unusually favorable attitude towards the field, but their opinions did not represent science as a whole.[24] The first Utrecht congress was organized for insiders and did not seek or include any legitimizing action by mainstream science. In a contemporary report of the event the meeting was characterized as "a gathering of specialists, experts in various sections of the field" (The First International Conference of Parapsychological Studies, 1953a, p. 1).

So what was the congress' impact on parapsychology? Some projects inspired or presented at Utrecht were developed later (Hart & Collaborators, 1956; Murphy, 1954b, p. 76). But many topics were not followed up and there was little change in the structure of parapsychology. Later developments in the 1950s (for overviews see Amadou, 1954; Rhine & Pratt, 1957; and West, 1954a) have shown

that the same topics of interest and the same methods in use before and at the conference were of interest and in use after the conference.

Nonetheless, the congress, seen from a present perspective, is more than a 'myth of origins' consisting solely of fictitious elements. An event such as the first Utrecht congress may also be conceptualized as having contributed in an important way to a sense of common identity by bringing many researchers together in a common cause. Perhaps no other event in the modern history of the field has allowed parapsychologists to get such a general view of their field, a view that included research, theory, and organizational aspects. But the impact of this event is difficult to measure today and perhaps we are minimizing its importance unjustly.

It is possible that the founding of the Parapsychological Association (PA) illustrates this impact of the congress on the sense of identity of some modern parapsychologists. The PA, established in 1957, may have been related indirectly to the Utrecht congress.[25] As stated in the PA website: "Long before the PA became a reality, many of those active in the field had felt a need for better communication with their colleagues. The international convention held by the Parapsychology Foundation at the University of Utrecht ... in 1953, had demonstrated to those present that meetings of professional workers could help to overcome their isolation and act as encouragement to research" ('History of the Parapsychological Association', 2008). Of course, other factors were also influential, among them the efforts of J. B. Rhine and associates to organize the field in the United States.

A more important contribution to the future may have been Garrett's Five Year Plan as it came into being at Utrecht. Garrett's plan gave direction both to the PF's own activities and to the support of the wider field through grants and the organization of subsequent conferences. While the ideas for some of the following conferences came from the attendees at Utrecht, it was the organizational work and financial resources of the PF that made those events a reality. This, more than anything coming from Utrecht, provided the field with forward movement. In other words, the energy and ideas developed at Utrecht, channeled through the work and resources of the PF in the form of later conferences and grants, was a key element in the development of parapsychology in modern times.

The conference program developed by the PF after Utrecht (Alvarado *et al.*, 2001, pp. 8-14; 'International Conferences', 2008),

which brought together parapsychologists and scientists and scholars from other disciplines, is still remembered by some workers in the field. The meetings close in time to Utrecht were those held during the 1950s. Topics included the relationship of parapsychology to philosophy and psychology, and specific phenomena and areas such as healing and spontaneous phenomena (*Proceedings*, 1957). Later conferences also centered on psychedelics (*Proceedings*, 1961), anthropology (Angoff & Barth, 1974), quantum physics (Oteri, 1975), the repeatability problem (Shapin & Coly, 1985), methodology (Coly & McMahon, 1993), and mediumship (Alvarado, 2005), among many other topics.

The argument for the importance of the PF grant program, particularly as it played out in the 1950s and 1960s, is difficult to gauge without a detailed analysis of grant archives at the Foundation (see Alvarado, 2008; Alvarado *et al.* 2001, pp. 4-6), a project that is well beyond the scope of this paper (for an idea of the range of the funding in selected years see Appendix A). Suffice it to say that the PF funding activities following the Utrecht conference, which were initially based on Garrett's Five Year Plan, represent one of the most important long-term philanthropic contributions in the history of parapsychology.

Concluding Remarks

My examination of the Utrecht congress has been brief. Much more could have been written about each of the topics here discussed, as well as about the behind-the-scenes controversies and organizational efforts. In addition, more research could be conducted on the impact of the congress by an examination of the personal correspondence of many parapsychologists. In the meantime, my study of the congress' history is presented here as the inspiration for the current meeting which also met in Utrecht, in October of 2008, and as part of the history of the continuing efforts of workers in parapsychology to improve the field. Such events as these, part of the shared social history of the field, need to be studied in more detail.

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Notes

1. The congress has been briefly discussed by Alvarado *et al.* (2001, p. 7), Murphy (1953a, 1953b), Pobers (1954c), and Sudre (1956/1962, pp. 45-46), among other discussions (The First International Conference of Parapsychological Studies, 1953a, 1953b; Frei, 1953/1954; Hankey, 1953; Psychical Research, 1953; The Utrecht Conference, 1953; Utrecht Session Test New Realms, 1953). Some of the papers of the congress were published in French (Amadou, 1955) and abstracted in English (*Proceedings of the First International Conference of Parapsychological Studies*, 1955). At the time of the congress the attendants received a privately bound book including the papers presented.
2. I am referring to Wilfred Daim, Jacques Donnars, S. David Kahn, Bonaventura Kloppenburg, William Roll, Michael Scriven, and D.J. West.
3. These congresses are discussed by Sudre (1956/1962, pp. 44-45). Lachapelle (2005) has presented a study of the politics behind these congresses. Examples of conference proceedings include those issued for the first two (*L'etat actuel des recherches psychiques,* 1924; Vett, 1922). On terminology in the congresses see Zingrone and Alvarado (1987).
4. On these developments see overviews of the history of the field such as Beloff (1993), Dèttore (1976), Gutierez and Maillard (2004), Inglis (1984), and Tischner (1960). Many parapsychologists have written texbooks covering the period, among them Amadou (1954), Servadio (1946), Sudre (1956/1962), West (1954b), and Zorab (1949). Zorab's (1957) bibliography illustrates the range of topics in the parapsychological literature just before and around the time of the conference.
5. On the work of Rhine and collaborators and its impact see Mauskopf and McVaugh (1980), and Rao (1982). Many other workers conducted research, among them: Cassoli and Marabini (1954), Dufour (1952), Fernández (1953), Fisk (1951), Murphy (1949), Soal and Bateman (1954), and Urban (1949). Much useful international bibliography of the immediate period before the congress appears in Zorab (1957).
6. For additional information about the PF see Alvarado (2008), Alvarado *et al.* (2001), Angoff (1974, pp. 201-205), The Parapsychology Foundation (1953), and *Ten Years of Activities* (1965).
7. For additional information about Garrett see Alvarado (2002b), Angoff (1974), Hastings (2001), McMahon (1994), and Rhine (1971). For a bibliography of works by and about Garrett see Alvarado (2002a).
8. In addition to Appendix A it is of interest to go over the first public report of grants which included, among other projects, funding provided to: Betty Humphrey and J. Fraser Nicol to study personality correlates of ESP; H.J. Urban to study telepathy in paranoia; the Institut Métapsychique International to publish the *Revue métapsychique*; and to Hans Bender to collect materials relevant to phenomena, beliefs, and practices in Germany during the war (Scholarships and Research Projects, 1953). Some organizations, such as the research groups of Tenhaeff and Rhine, received a great deal of funding. For example, Rhine acknowledged that a $10,000 grant given to him for his Parapsychology

Laboratory in 1955 was "very substantial and even an essential part of the fiscal year budget of the Laboratory" (Annual Report, 1955, p. 1).

9. The establishment of Tenhaeff's chair was noticed out of Holland (Chair of Parapsychology, 1953).
10. This, and what follows, is taken from International Conference of Parapsychological Studies (1953a), and Pobers (1954c). On the Paris meeting see La Vie Métapsychique (1953).
11. Murphy was a good choice. In addition to a warm and pleasant personality he was well known and respected in American psychology, as well as in parapsychology. Before the Utrecht meeting Murphy had been involved in mediumship and ESP studies, had written for the general public, and had worked with Rhine disseminating and coordinating ESP research (Murphy, 1923, 1927, 1941, 1949; for overviews see Berger, 1988, pp. 148-186; Murphy, 1957).
12. The selection of Pobers was a natural one. In addition to representing the PF, he had made many of the initial international contacts leading to the organization of the congress. By 1953, as seen in a biographical sketch in the PF archives (Michel Pobers, n.d.), probably written by Pobers himself, Pobers had considerable experience as a writer, journalist, newspaper owner and editor, and as a student of literature and diplomacy. He held two doctorates in letters and jurisprudence, and could speak and write in six languages. He seems to have been held in great esteem by the conference attendees. The conference Resolutions (1955) said that Pobers' "unlimited energy and diplomacy have been our universal envy" (p. 129).
13. Rhine and Garrett had many disagreements over the years. For example, Rhine (1953) wrote that he had been invited by Garrett to cooperate with the PF, but stated "there is, I am afraid, too much difference in standards." Rhine wrote to Garrett somewhat ambiguously about the congress: "With your great understanding of human situations and relations, I know you can see our difficulty in trying to associate ourselves with anything as far in the background now as the International Congresses of Psychical Research. It is a little like the question of taking over an old house. I imagine you have to decide, too, how much you can spend on renovation" (Rhine, 1952a). But he expressed agreement with Garrett's initial ideas about the organization of the event (Rhine, 1952b). In his opening address to the congress Murphy (1955) expressed "deep regret that Dr Rhine could not be with us, though I rejoice that Dr Hart is here to represent the Parapsychology Laboratory, and that Mrs. Pope, Managing Editor of the *Journal of Parapsychology*, is with us" (p. xvii).
14. According to *EH Net* (http://eh.net/hmit/) by the standards of 2006 this amount would have a buying power of $228, 851.31. This is using a Consumer Price Index that compares the cost of goods and services of the past to those for 2006.
15. René Sudre also participated in the Utrecht meeting (Sudre, 1955) and in the old congresses, but he was not included in the statement. For background information about opposition to Sudre's participation in the conference see Evrard (2007).
16. Brown's critique was presented before the Utrecht meeting, as seen in the July 25 issue of the English journal *Nature* (Brown, 1953a). It was in fact an important and widely discussed criticism of the validity of results of statistically evaluated parapsychological experiments during the 1950s, and one that generated much controversy in 1953 (e.g., Brown, 1953b; Stratton & Thouless, 1953). Later critiques of Brown's ideas include those of Wasserman (1955) and West (1954a, pp. 343-345).

17. Part of this were the problems between Pobers and the PF. After the Utrecht congress Pobers' relationship with Garrett and the PF deteriorated and he stopped working for the Foundation. The Dutch, represented by psychologist J. van Lennep from the University of Utrecht, wanted to continue having Pobers working with them in projects funded by the PF (this is clear in a letter at the PF archives, Van Lennep & Pobers, 1954).
18. Examples of published papers, or parts of them, were: Dingwall (1954), Eisenbud (1956), Nicol and Humphrey (1953), Vasse and Vasse (1954), and Warcollier (1954).
19. A folder at the PF archives with the correspondence of George Zorab—who was appointed Director of the PF's European Research Center in 1957 (Foundation Appoints European Director, 1957)—shows the time, effort, and logistic problems involved in organizing PF conferences. A report by Zorab (n.d.) about his efforts to organize a precognition conference shows the process of selecting participants, the difficulties in dealing with them, the compilation of their ideas, as well as the projected cost, and possible location of the event.
20. Work on the bibliography was made possible thanks to a PF grant to Zorab for $1,000.00 (Garrett, n.d.). Zorab (1955) stated that he went to England to discuss the bibliography with Eric J. Dingwall. Examples of reviews of the work include the comments of Cadoret (1957), and of Salter (1958).
21. In his opening address to the conference Murphy (1955, p. xx) argued that while the survival prpoblem could not be solved with current methodology, he believed it should not be forgotten and placed his hopes in future developments. Historically the issue of survival has been of great importance for the development of parapsychology (Alvarado, 2003), but there was little empirical research on the topic during the 1940s and the 1950s. Nonetheless, discussions of different sorts continued during the period, as seen in the writings of Hart (1959), Murphy (1945), and Price (1953).
22. Wellington Zangari (2006) wrote to me that, in Brazil, many affirm in their writings that in 1953, and because of the Utrecht congress, parapsychology obtained recognition as a science. Some examples of this that Zangari provided include Aresi (1980, p. 62), Friederichs (1982, p. 38), Marinuzzi (1977, p. 45), and Valente (1997, p. 29). According to Zangari: "The Utrecht Congress has become sort of a watershed: before it parapsychology was only a disorganized discipline and was not accepted as a science, afterwards, it was a recognized and systematic science" (my translation).
23. In a private report to the PF Pobers (1954b) mentioned interest in the congress in Germany and Switzerland after the event was held. He wrote he met with Carl Gustav Jung: "I was very fortunate to receive an invitation from Dr Jung and to spend with him many hours, going through some of the Utrecht reports" (p. 2).
24. The same may be said of the laboratories and chairs established in different universities. While important, these events have never represented the recognition of science at large, nor of specific disciplines such as psychology, beyond acceptance based on specific local conditions.

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Appendix A: PF Funding, 1954-1955

To justify my opinion that the most important consequence of the Utrecht congress was Garrett's continuing support of parapsychology, as stated in her Five Year Program, it is necessary to present at least a summary of the vast funding contributions of the Foundation. While a detailed study of this is beyond the scope of this paper, one can get a partial view of the subject through the PF's grant reports (see also Alvarado, 2008; Alvarado *et al.*, 2001, pp. 4-6). The following overview of grants for the years 1954 and 1955 will give an idea of the wide range of the grant program for two years after the Utrecht meeting, a range that shows their importance for parapsychology around the world (I am not including some grants for anthropological work that do not seem to be directly related to parapsychology nor conference support) (Annual Report Emphasizes U.S. Research, 1955; Trustees to Hear Annual Report, 1955).

Many examples of experimental work were funded in 1954. Among these were ESP studies in relation to paranoia (Hubert Urban, Austria), personality variables (J.F. Nicol and B. Humphrey, USA), and Australian aborigenes (R. Rose, Australia). The experimental work increased in 1955, when the following workers from Rhine's laboratory at Duke university were funded to study precognition (K. Osis, R.A. White, W. Nielsen), and to study differences in target materials (R. Van de Castle, R.A. White, W. Nielsen). Funding was also provided at the Duke laboratory for the reanalysis of aspects of Soal's work (E. Foster), for a review of the literature of ESP and personality variables (Van de Castle and G. Mangan), for the development of an instrument to measure attitudes to ESP (White and Van de Castle), and for PK dice experiments (Mangan). ESP experiments from other places were also

funded in 1955, as seen in work about personality variables and beliefs (Nicol and Humphrey, USA; J. Crumbaugh, USA), auditory subliminal perception (A.J. Bachrach and others, USA), differences in scoring in telepathy and clairvoyance conditions (G.R. Schmeidler), comparison of GESP and precognition testing conditions (S.D. Kahn), and studies with young school children (P. Vasse and C. Vasse). There was also a study of physical aspects of dowsing and of ESP and climate (S.W. Tromp, Holland).

Non-experimental projects were also funded. They included, for 1954, psychological studies of psychics (W.H.C. Tenhaeff, Holland), studies of phenomena in Germany during and after the Second Great War (H. Bender, Germany), studies of healing cases at Lourdes (also in 1955, D.J. West, England), recording of physical phenomena (A.D. Cornell, England), studies of psychics and psychic experiences in the British Isles (also in 1955, E.J. Dingwall, England), studies of out-of-body experiences and apparitions (also in 1955, H. Hart, USA). In 1955 studies of hauntings and physical and mental mediumship, some involving recording instruments, were also funded (A.D. Cornell, England), as was a study of spontaneous precognition (L.E. Rhine, USA).

There were also grants for other projects. In 1954 funding was provided for administration and research expenses (Society for Psychical Research (England), the establishment of a laboratory (W.G. Roll, England), and to travel to Duke University's Parapsychology Laboratory (J. Crumbaugh, G. Mangan). In 1955 the following projects were supported: study of the relationship of parapsychological studies and South American petroglyphs (Amazonia Foundation, USA), a seminar on spiritual healing force (Wainwright House, USA), and training of mediums (College of Psychic Science, England).

Support for parapsychology continued, and was augmented, during the following years. As before the grant programs supported a variety of topics, methodologies and approaches (*Ten Years of Activities*, 1965).

PARAPSYCHOLOGY IN A UNIVERSITY SETTING

DEBORAH L. DELANOY

Introduction

This paper will consider the role of universities in charting the future of parapsychology. In doing this it will generally consider the history of parapsychology's association with universities and provide a more detailed coverage of the pros and cons of working in universities for researchers. Most unfortunately, I have never been able to pride myself on my precognitive abilities, and thus will not devote much time to trying to gaze into the future to see what it may hold. Instead conclusions will be drawn about the present and, at least, the most likely short- and medium-term utility of embedding parapsychologists within university environments.

At this early stage, it would be sensible to make a disclaimer. This paper will take an approach that is more similar to presenting an impressionist landscape than establishing carefully worked out facts and figures. Its goal is to look for general patterns and trends in complex settings that may shine some light on the best ways forward. There will be no attempt to establish hard and fast truths. We all will be able to think of specific exceptions to most, if not all, of the more general points that will be made. Thus this paper starts by extending apologies to everyone who in reading it, thinks of the (many) exceptions and becomes concerned about significant researchers, research centers and events that have seemingly been overlooked. The objective herein is not to chart the nuances of the relationship of universities to parapsychology, of which there are admittedly many, but rather to see the larger picture—get the lay of the land, so to speak, without getting bogged down in detail. Also, as my personal experience is in British universities, the UK will provide the basis for much of what follows.

Firstly, I will consider a very brief and generalized history of parapsychology in university environments. In doing this, I will limit myself to relatively recent times in the 20th century when the university setting was perhaps more similar to its current configuration than was the case in the latter part of the 1800s, when the first learned research

societies investigating psychical topics were founded. This is not to say there was no relationship between academia and the early societies and researchers interested in exploring psychical research. In Europe in the 1880s, Charles Richet worked at the University of Paris (and was a Nobel Laureate) and among the founders of the Society for Psychical Research (1882) were notable academics at the University of Cambridge, Royal College of Science (Dublin) and eight Fellows of the Royal Society, including two Nobel Laureates. But as universities have changed considerably over the last century, it seems most useful to focus on more recent times.

A landmark event in the association between 'modern' universities and parapsychology was when Duke University hired William McDougal to head up their Psychology Department (1927). McDougal brought Joseph Banks (J. B.) Rhine to work with him at Duke, and Rhine remained on the staff until his retirement in the mid-1960s. At Duke, Rhine quickly rose to a position of both national and international prominence, with his innovative emphasis on laboratory-based research and his subsequent papers and books on parapsychology attracting interest not only from the public, but also from other academics. A charismatic figure, Rhine proved to be a very successful fund raiser. By the time he retired from Duke, sufficient funds had been raised for J. B. and his wife, Louisa, to set up an independent research center, the Foundation for Research on the Nature of Man (FRNM) in 1965. Although not part of Duke University *per se*, FRNM was located next to the university and maintained ties with it via interested staff and students (FRNM is now known as the Rhine Research Center or RRC). Rhine's work attracted students from a variety of backgrounds to work with him in his Duke Parapsychology Laboratory (founded 1935) and latter at FRNM. Indeed, many of the key parapsychological researchers of the latter 20th and early 21st century worked with and trained under Rhine including Gaither Pratt (who became the lead researcher at Rhine's lab and later worked at University of Virginia or UVA), William Roll (who founded the Psychical Research Foundation or PRF and worked at West Georgia College), Rhea White (founder and Director of the Exceptional Human Experience Network), Robert Morris (worked at the University of California at Santa Barbara and Irvine and Syracuse University before taking up the Koestler Chair at Edinburgh University), Rex Stanford (a psychologist at St. John's University), Charles Honorton (worked at Maimonides Medical Center before founding the Psychophysical Research Laboratory or PRL),

John Palmer (involved with the JFK University Graduate Program, and FRNM/RRC) and Richard Broughton (FRNM/RRC and latterly, the University of Northampton). Due in no small part to the influential role played by the Rhines, both at Duke and later at FRNM, the vast majority of parapsychological research activity during the last century was conducted within the United States.

During the latter part of the 1900s, following the lead of FRNM and aided by funding from a few wealthy benefactors and other sources, the majority of parapsychological research was conducted outwith universities, usually in privately funded research institutes—such as the Psychophysical Research Laboratory (PRL), FRNM, Mind Science Foundation (MSF), PRF, Science Unlimited Research Foundation (SURF), Institute of Noetic Sciences (IONS), etc.—or by government-funded research programs, like the remote viewing work at 'think tanks' such as Stanford Research International (SRI) and Science Applications International Corporation (SAIC). Of course, there were important exceptions, such as the Princeton Engineering Anomalies Research (PEAR) group based at Princeton University, Gertrude Schmeidler (City University of NY), Charles Tart at UC Davis, Stephen Braude at the University of Maryland, and Hoyt Edge at Rollins College, to name but a few. Nonetheless, the great majority of research conducted in the United States during the latter part of the 20th century was done independently of established universities.

During this same period (latter 1900s), in Europe most parapsychological researchers adopted another approach and, by and large, embedded themselves in university environments. Some examples include, Hans Bender who had a chair at the University of Freiburg (as well as founding a private research Center, the IGPP), W. H. C. Tenhaeff and later Martin Johnson and Sybo Schouten, all at the University of Utrecht and Erlendur Haraldsson at the University of Iceland. In Great Britain, a very influential individual who adopted this approach was John Beloff who worked at Queen's University in Belfast, before coming to Edinburgh University in 1963. Beloff conducted some noteworthy experimental work and used his university position to supervise the doctoral studies of a number of individuals who are still active in the parapsychology today, such as Adrian Parker, Richard Broughton and me. However, arguably Beloff's most significant contributions to parapsychology were his roles in creating the Koestler bequest, in paving the way for it to be accepted at Edinburgh University and in being a respected and influential voice on

the selection panel that hired Robert Morris as the first holder of the Koestler Chair in 1985.

Morris had a strong vision of how the Koestler Chair could best be used to advance parapsychology and, during the nearly 20 years that he held the Koestler professorship, he substantively implemented his objectives. Like Beloff, a key component of his approach was to advance the field by supervising PhD students who would have a good training in parapsychology, as well as other key areas of psychology that would enable them to find jobs at other British universities. Thus Morris used his position at Edinburgh, as well as the Koestler funds, to supervise talented, well-qualified individuals who, after obtaining their PhDs, would gain their own positions in other universities and then 'be fruitful and multiply' by subsequently training more research degree students to follow in their footsteps.

In the UK there is currently a trend to refer to parapsychologists in terms of their Morris/Koestler Chair (Koestler Parapsychology Unit or KPU) lineage. Thus, a first generation 'child' of the KPU received their PhD from Morris at Edinburgh, a second generation 'grandchild' was supervised to completion of their PhD by one of Morris's previous PhD students, a third generation or 'great grandchild', was supervised to completion by a 'grandchild' of Morris's and so on. Currently, there are 27 individuals who were supervised to completion by Morris of which 18 are currently working as full-time academics or associated with university-based research groups, 12 of these at UK universities. In the UK, there are currently nine second generation KPU 'grandchildren' who have completed their PhD studies, of which seven are now employed at UK universities, as well as another 12 'grandchildren' whose studies are on-going. No third generation 'great grandchildren' have yet completed their PhDs but four are underway at this time, with many more anticipated in future years as the second generation become more senior and active in supervising PhD students at their respective universities. In short, in 1985, when the Koestler Chair was founded, parapsychological research was being conducted at only two or three UK universities; today full-time staff at 16 UK universities have parapsychological training at the doctoral level, with some of these universities building sizable research groups, e.g., at the University of Northampton, the Center for Study of Anomalous Psychological Processes (CSAPP) has five full-time academic staff engaged with parapsychology in varying ways (including three of the speakers at this conference).

Pros and cons of independent research centers

While the majority of this presentation will examine the pros and cons of embedding parapsychology in universities, brief mention should be made of these factors when applied to the most common alternative to academic settings, namely the independent research center. As the vast majority of these centers have been in the US (again, with notable exceptions, such as the IGPP in Freiburg, Germany), this will provide the context for the following consideration.

On the positive side, in the United States, the non-university based research centers/programs have been tremendously productive. Being able to focus almost exclusively on their research activity, these centers provided many of the key parapsychological advances of recent decades, for example, methodological and theoretical work associated with or stemming from the Maimonides dream research, the ganzfeld developments, remote viewing and its theoretical spin-offs, meta-analyses, DMILS (Direct Mental Interactions with Living Systems) and presentiment research to name but a few areas. Also, once the centers obtained financial support for their work, they had a considerable degree of freedom in what they studied and how they approached their research, which arguably extended to a greater degree of risk-taking and innovation.

Yet despite this great output of important scientific work, today the number of new researchers in the USA is very low, in comparison with the UK, for example. Similarly, the number of independent research centers is a small fraction of the number that existed only 25 years ago. When discussing the state of parapsychology with US-based colleagues, I frequently hear them bemoaning the negative attitude held towards parapsychology by universities, mainstream colleagues and funding sources. This current situation points to apparent negative aspects associated with the freedom enjoyed by and productivity of the independent research centers.

The negative factors can be broadly grouped into three inter-related areas: producing the next generation of researchers; career structure/security; and relationships with mainstream colleagues. With many of the key researchers in the US working outside of universities, the training of the next generation of researchers becomes an increasingly difficult task, left to the relatively few academics that are within university settings. Today in the US, most academics with parapsychological research interests are at or near retirement, with no

apparent generation of academic parapsychology successors. Indeed, in recent years surprisingly few PhDs have focused upon parapsychological topics in their doctoral research in the US. While there are many and varied reasons for this, one key factor could be the lack of employment opportunities and a career structure for those who have a degree involving parapsychological research.

The lack of a career pathway is inherent in the funding structure of independent research centers, whose existence has in many instances been based on the support of wealthy benefactors. While wealthy donors have been an important resource for independent research centers, they have not generally proved to be long-term or especially reliable funding sources. Nor can their support be readily replaced, if changing interests and/or circumstances bring a stop to funding. With a few notable exceptions (e.g., Rhine, Beloff, Bender and Stevenson), parapsychologists have not been especially effective in attracting the substantial donations/ bequests that are required to maintain a thriving research group/center for more than a relatively short period. Additionally, the complex nature and small effect sizes that characterize our research can make it difficult to sustain the funding support of successful businessmen and women who are used to having tangible and noteworthy results shortly following a substantial investment. Even when exceptionally large donations have been obtained, there are too many examples of the funds being siphoned off into other areas. These factors, combined with the difficulty of donations or bequests maintaining their initial value and thus their level of support over time (due to fluctuating financial markets, inflation and associated ever-increasing salary and overhead costs, etc.), all contribute to the uncertain funding future of most independent research centers. Thus unfortunately, it is not uncommon for leading, non-academic researchers in our field to find it difficult to support themselves solely as parapsychologists.

Arguably, another very significant effect of this removal of parapsychology from the normal activity of universities in the US has been an increasingly negative perception of parapsychology within that country by mainstream science, which is largely taught, advanced and promulgated by universities. By largely remaining aloof from universities, parapsychologists may have unwittingly contributed to their increasing isolation from their mainstream colleagues, who have very little exposure to work that is not on-going within academia. Another result of being isolated from the 'corridors of science' is that

there is no common ground and/or venue for the normalization of parapsychology. These are but some of the subtle, if not insidious, aspects related to the lack of everyday relationships with mainstream academia.

Of course, much of the above is speculative in nature, but there are some undeniably problematic aspects of opting to work with independent research centers. These difficulties may well have contributed to the negative situation I frequently hear my US colleagues decrying. Thus the potential negative aspects associated with independent research centers deserve careful consideration when attempting to chart the future of the area.

Positive factors associated with university settings

Turning now to positive aspects of pursuing parapsychology in an academic environment, I will focus on four related areas. In the following sections I will be basing my discussion on UK university environments, as this is what I am most familiar with. The positive factors that I will consider are:

1) provision of a relatively stable income/career structure;
2) parapsychologically-related skills are valued, respected and required;
3) access to, training and development of the next generation of researchers; and,
4) parapsychologists are a part of mainstream science activities, leading to the 'normalization' of topic.

While academia is not exactly renowned for high levels of staff remuneration, it nonetheless provides reasonably good job security (although tenure is no longer offered in the UK), a stable income, a generally adequate life style and relatively good pension arrangements. While long hours may be the norm, the working environment typically offers good flexibility, with relatively good holiday allowances. On balance, for current and prospective parapsychologists, there is much to recommend an academic career when compared with other types of employment. Of course, the most important 'pro point' of academia is that it provides the opportunity to pursue one's own parapsychological research and to educate/supervise others who are interested in pursuing

parapsychological topics, at both the undergraduate and postgraduate levels.

If I'm having this discussion with US colleagues, it is at this point that someone usually notes that there are no employment opportunities in universities for those who have pursued a parapsychological topic for their PhD. However, this has not been my experience. Parapsychology is a broad multi-disciplinary topic and areas of mainstream interest are easily combined with parapsychological questions. For example, recent PhDs from Edinburgh and Northampton have obtained employment at other universities as they can offer specialist expertise in areas as diverse as neuro-cognitive psychology, individual differences and qualitative/quantitative methodology. These are all traditional areas that have allowed the students to incorporate parapsychological research into their degree work while simultaneously honing a 'sellable' mainstream expertise.

Another important, positive factor is that parapsychologists usually bring valued and respected areas of expertise to the typical undergraduate curriculum. As parapsychologists are (helpfully) required by our critics to be masters of experimental design, we have much to contribute to the methodological and analytical training that typically makes up a significant proportion of undergraduate education. While these areas are often thought of as rather dry and boring by many students, parapsychology enables a more interesting slant/subject matter to be brought to these topics. Also, one of the most difficult skills to develop in undergraduates is the ability to apply critical thinking. Here parapsychology has a great deal to offer, not only by getting students involved in thinking through some of the critical debates surrounding various parapsychological work, but also in stretching their understanding of the nature of science—how sub-disciplines grow, how mainstream science reacts to new findings that are at odds with some accepted tenets, the basis on which findings are accepted/rejected, and so on.

Of course, it is at university that most students will encounter their first 'serious' understanding of their chosen area of study. As a lecturer, one has many opportunities to include parapsychological information in one's lectures, both as exemplars to help illustrate issues in subjects not directly associated with parapsychology, as well as in more specialist parapsychology modules or courses. For example, while I haven't taught a course that focused on parapsychology since leaving Edinburgh University nine years ago (although such courses are taught

by my colleagues at Northampton), I would usually include at least one parapsychological example in any one or two hour lecture to undergraduates, regardless of whether I was lecturing on Health Psychology, methodology, developmental psychology or individual differences. It is in these lectures that we have the opportunity to first capture the interest of the brightest of our students and to support this interest as far as the student's wish to carry them, from their undergraduate essays and research projects to doctoral theses.

But universities provide far more than simple education; they are where mainstream science is defined, advanced, challenged and redefined and disseminated. I often think of universities as 'clubs'—and while it's not that difficult to become a member of these clubs (depending upon the specific club, of course), it is very difficult for outsiders to gain access to the 'inner corridors' of academia where science develops and evolves. Universities are where parapsychologists need to be if they are to be seen as a part of the business of science, as opposed to being 'apart' from it.

The impact of having parapsychologists on staff goes far beyond the actual research they conducted, the lectures delivered and students supervised. I would argue the most important aspect is the invisible 'normalization' of the topic that occurs when your colleagues see you performing the same demanding tasks and duties as themselves—and doing it well. When one is an academic staff member, you become one of the 'us', and your area of research, which may or may not be of any specific interest to other staff, will nonetheless be accorded the same status and stature that is accorded to other staff members. If the work you do is respected by your colleagues, without even realizing it, this respect will be extended to your area of research more generally. Recently, I had to keep from laughing when chatting with the Head of Psychology at the University of Northampton, she exclaimed in surprise 'you mean some people still doubt the validity of parapsychological research?' Of course, 12 years ago, before the first Edinburgh postdoctoral student (Chris Roe) started working in the psychology division, this statement would not have been made. But now at Northampton, and at an ever-growing number of other UK universities, parapsychology is 'business as usual' and you would no more heap ridicule on it than you would on your respected and trusted colleagues, who may happen to be involved with it.

Negative factors associated with university settings

While the above has illustrated some of the very positive factors associated with embedding parapsychologists in university settings, there are negatives as well. Some of the main problems with pursuing parapsychology in universities fall within six overlapping issues. These are:

a) there are many competing tasks and duties;
b) the orientation / priorities of higher education in the UK change frequently;
c) increasing pressure on staff to generate income via research and knowledge transfer activities;
d) a shortening of study period for research degrees (MPhils / PhDs);
e) the multi-disciplinary nature of parapsychology; and,
f) the impact upon career progression

While it may seem too obvious to require stating, academics have many competing responsibilities that have little to do with pursuing their own research agenda, teaching parapsychology and/or supervising student parapsychological research projects. The amount of teaching that staff actually do varies greatly across universities, but at the teaching 'front line' found in some new universities, staff can spend up to 18 hours a week lecturing in front of a class. Assuming that each hour of lecturing requires, at the least, another hour for preparation period as well as equivalent period for marking of essays and exams, those duties alone can provide an academic with a very full working week. Add to this the supervision of undergraduate and postgraduate research projects (which will often have nothing to do with parapsychology), the support of up to 30 personal tutees, a myriad of administrative duties, not to mention the endless committees and working groups associated with academic life, and it is clear that it can be difficult to find time for research during the academic teaching terms. Previously the teaching year would still leave two to three months over the summer for research purposes, but increasingly teaching is becoming a year-round activity to accommodate increasingly non-traditional student populations (e.g., part-time mature students in employment, and so on). In short, it is not necessarily a straightforward matter to find the time to pursue research, especially

not at universities where economics dictate a relatively heavy teaching load.

Universities in the UK must also respond to an ever-changing set of governmental priorities, which can make long-term research strategic planning difficult. I will use the University of Northampton as an example to illustrate how these changes can impact upon universities and the working lives of their staff. Less than 20 years ago, the University of Northampton was called Nene College and was a respected teaching education institution. In response to government objectives in the early to mid-1990s to get 50% of the population into Higher Education (HE, meaning university-level education), many colleges that previously were specialized in specific employment-oriented training (so called polytechnical colleges) sought to broaden their remit to attain university status. Nene College responded to this opportunity and in the mid-90s achieved their first taught degree awarding powers, and in 1999 became the University College of Northampton, one of a number of 'new' universities. When I first came to Northampton in 1999 my role was to increase the research base of psychology (external grant income, publications and conference presentations), and expand the research degree student population. This role was a lead-up to the application for full university status that would include research degree (i.e., MPhil & PhD) awarding powers. The institution's application was successful and in December 2005 its name was changed to the University of Northampton. More recent government agendas, combined with changing student demographics, have seen a new shift of emphasis towards being a regional-facing university, with widening participation goals (making university accessible to non-traditional university populations), an increasing orientation towards employer engagement (offering teaching that will meet employer needs), teaching tailored for specific professional groups that is given year round without the traditional summer breaks (e.g., training for the police and probation services) and a strong focus on knowledge transfer activities, alongside more traditional research.

Furthermore, over the same general period, research funding has changed dramatically from a situation where external funders were expected to support the additional needs of a successful research proposal (e.g., research assistants, materials, etc.) but the university would meet the academic's salary, pay for facilities and other infrastructure-related costs, to the present environment where research and knowledge transfer activities are expected to fully cover the time

academics spend on the research and, on top of the direct costs of the research project, external research funding should also cover the indirect costs, e.g., associated overheads, which tend to be approximately as expensive as the direct costs (thereby doubling the cost of the work). Of course it is rare for grants to cover all the associated overhead costs, but pressures are mounting for funding bodies to increasingly meet more of these expenses, even though charitable foundations rarely cover any overheads. These factors have the general impact of making research more expensive to fund, resulting in there being less funding to spread around (in lieu of increasing the amount of funding generally available). Also, to be seen as 'high value' work, research and knowledge transfer activities increasingly need to incorporate some overhead contribution as well as ideally helping to meet regional and/or employer/business/social enterprise needs. None of this bodes well for the small, traditional funders of parapsychological research, and demonstrates the need for parapsychology to break into more mainstream funding sources.

During this same period, due in part to a more constrained financial environment, the length of time in which research degree students should finish their PhD has shrunk from a time span where five to eight years was fairly commonplace for a full-time student, to one where full-time students are expected to finish within four years and part-time students within six years. Any student failing to complete their studies within a seven year period will now make a notable 'black mark' against the institution. While larger universities with several thousand research degree students can absorb the odd 'late completion', smaller / newer universities cannot afford to fall foul of these regulations (e.g., Northampton only has about 200 PhD students, although these numbers are steadily increasing). This tighter time period is especially constraining for areas such as parapsychology where students have rarely had much undergraduate exposure to the area. In other words, research degree students have to attain a reasonable familiarity with parapsychology in general, as well as sophisticated expertise in their specific research area within a significantly shorter time frame than was previously the case. This could bring a number of problems to the field, including students who are less well-rounded in their general knowledge of parapsychology, unless their undergraduate degree was obtained at one of the increasing number of universities offering parapsychological modules or they have taken one of the on-line distance learning courses.

Parapsychology, despite its name, is a multi-disciplinary area. But increasingly researchers are coming from a psychology background, as opposed to previous generations where parapsychologists had undergraduate and perhaps an MSc postgraduate degree in a wide range of topics including history/sociology of science, anthropology, physics and philosophy. In the UK the reason for the growing emphasis on psychology is due to the Koestler Chair being located within a psychology department, while other subject areas have failed to obtain a level of funding that would assist in supporting the staff needed to foster a large number of PhD students. Nonetheless, at least in the UK, PhD students are often self-supporting, but even so the lack of research degree students undertaking parapsychological research in areas other than psychology is quite limited, e.g., Robin Wooffitt at York University can supervise students in sociological areas and Pete Lamont at Edinburgh can take on those interested in historical perspectives. But these are isolated examples, and there is a clear need to seed universities with staff having parapsychological expertise beyond that commonly associated with psychology. In some cases academics have kept their association with parapsychology quite separate from their daily academic life. It may be helpful if established staff 'put their heads above the parapet' once they have reached a level of eminence where they are no longer concerned with adding another hierarchical title onto their name. Once they have obtained a good standing within their field, they are well-placed to make it known that they have a balanced, indeed, perhaps skeptical (in the true sense of the word) interest in parapsychology. This may be the most direct route to winning a wider-spread of acceptance of the validity of pursuing parapsychological research in areas other than psychology.

Parapsychology has long had a reputation as having a deadly impact upon one's career progression. An example of this could be John Beloff, who despite considerable accomplishments, never progressed beyond the rank of Senior Lecturer. Not that this is any indication of a lack of accomplishment, but one could argue that his eminence in parapsychology and his many contributions to it, as well as to the philosophy of psychology, were not adequately reflected by his academic promotions. On the other hand, one could, and I would, argue that if this was once a factor, 'the times they are a' changin'.

A lot of this change is down to Bob Morris' approach to the field. He accepted speaking invitations to every UK university (often including repeat invitations) and typically started off his presentations

with 'disarming' information about the psychology of deception to ensure that his audience realized he was well informed about how one could be misled into thinking something psychic had occurred, when in fact there was a much more mundane explanation. He never took a defensive and/or aggressive position, but rather sought to be the voice of reason. He had a wonderfully engaging and winning ability to find shared problem areas with whomever he was speaking. He refused to be pushed into 'us and them' corners, masterfully disarming critics by occupying the center ground and thus making any extremism on their part all the more apparent. Happily, Morris passed these winning approaches onto his students and those who were privileged to work with him. Today in the UK it is increasingly difficult to talk of career progression being damaged by association with parapsychology, at least within psychology. A key sign of this is that students with parapsychology-related PhDs are finding permanent jobs at good universities, despite a very competitive academic job market. With regard to higher levels of career progression, using my own university as an example, at Northampton the only two professors in the Psychology Division have ties of varying degrees with parapsychology (myself and Harald Walach). Also, I am currently the Associate Dean for the School of Social Sciences, with wide-ranging responsibility including research and knowledge transfer, internationalization and all aspects of postgraduate education within the 12 subjects that comprise the School. Yet I have never published or presented a paper that was not unabashedly parapsychological or joined any standard psychology associations, choosing instead to be firmly affiliated with the Parapsychological Association and Society for Psychical Research. These factors have not been disadvantageous to me (to the best of my knowledge), nor to others who have successfully obtained good academic posts and those who have reached the top academic positions while having close and evident ties to parapsychology. While parapsychology may have had a negative impact in previous decades, this appears to be a diminishing phenomenon, and increasingly, at least in the UK, skilled academics no longer need to fear that their career prospects may be limited by having a research base in parapsychology.

Charting the future of parapsychology

In the introduction to this talk I promised not to engage in lengthy speculation about what the future may hold. However, in the course of writing this presentation several factors have emerged as salient features which need consideration when looking to the future. The first of these is that despite the difficulties associated with university employment, undoubtedly the best place to advance parapsychology is from within academia. While private research centers offer many tempting advantages, ultimately their disadvantages can lead to the field shrinking, instead of expanding, and to an isolated position, separate from and lacking access to the mainstream, making the normalization and acceptance of parapsychology very difficult, if not impossible.

We must seek to encourage younger colleagues to gain the education needed to obtain university posts so that they can fully exploit the many advantages that come with entry into the 'university club' to the greater good of parapsychology. Simultaneously, those who are able to accept and supervise research degree students must realize the serious obligation they have to parapsychology—their students represent the future of the field. The most able and best qualified students must be sought as the standing of parapsychology within universities is indelibly related to the quality of the staff doing the work and research. Supervisors also have an obligation to ensure their students are well trained to present the field in an open, 'occupying middle-ground', cooperative manner that will win gradual acceptance, as opposed to taking strident or defensive positions that can fan opposition. Our students must have 'sellable' expertise in areas other than parapsychology to ensure they can gain academic employment and they should be very skilled methodologists, whose research will lead and embody good practice at their universities.

The good news is that advances into academia are being made. Today there are more university academics, versed in and pursuing parapsychological research, than at any other time in the history of psychical research/ parapsychology. Unfortunately, these gains are not evenly distributed at universities around the world. While there are still academics at a number of US universities (e.g., Tart, Braud & Hastings at the Institute for Transpersonal Psychology; Utts at the University of California, Irvine; Bem at Cornell, etc.), there is not a notable 'next generation' presence in US universities. Currently, it is in the UK and

in Sweden where real advances are occurring (Sweden: Parker & Goulding at the University of Gothenburg; Cardeña at Lund University and Dalkvist at the University of Stockholm). In the UK, the number of universities with parapsychologically-trained academic staff appears to be approximately doubling every decade, e.g., parapsychologists were found at circa three universities in the late 1980s, at seven universities in 1999 (according to Matt Smith's PA Invited Address in 1999), and my most recent count shows parapsychology staff working at 16 UK universities in 2009. Of course, we are not a universally established and respected area of study yet. With approximately 140 universities in the UK, there is still a long way to go and much room for improvement. But if we continue to double our numbers every decade, within the next 40 years parapsychology will be represented at most universities. Whether we will get there or not remains to be seen, but we are currently moving in the right direction. If the current increases continue in the UK, the example of how best to advance parapsychology in other countries will be evident.

BEYOND PLATO? TOWARD A SCIENCE OF ALTERATIONS OF CONSCIOUSNESS

ETZEL CARDEÑA*

> The safest general characterization of the European philosophical tradition is that it consists of a series of footnotes to Plato (Whitehead, 1939, p. 39).

It is no longer a death-knell to a person's academic career to confess to an interest in consciousness. In the context of the enormous number of recent books on consciousness (especially neuropsychological and philosophical treatises), specialized journals (e.g., *Journal of Consciousness Studies*, *Consciousness and Cognition)*, and associations (e.g., Association for the Scientific Study of Consciousness) with periodic meetings, it seems almost uncouth to wonder whether we have a science of *states of consciousness.* Yet, however welcome the 'new' science of consciousness may be, there have been some striking gaps in its development and it can be argued that at least in some respects we have not improved on the contributions made by Socrates and Plato more than two thousand years ago.

Among the most important hurdles in the study of consciousness are conceptual vagueness and obfuscation and the facile assumption that a materialist account of brain/mind relations is unassailable (e.g., Crick, 1994) when in fact there are good reasons to at least question such a model (Kelly, Kelly, Crabtree, Gauld, Grosso & Greyson, 2007). In a related vein, the parapsychology community is well aware of the bizarre situation in which discussions about the ontology of consciousness occur amidst a complete ignorance of or disregard for the considerable amount of data produced by parapsychological research. Although less fatal, the failure to even consider relevant evidence is reminiscent of the 19th century disregard of Semmelweis's theory that asepsis might have something to do with puerperal fever and the enormous number of childbirth deaths because his ideas ran counter to the established medicine of his time (Nulland, 2004). Much

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more could be said on this issue, but that is not the goal of this paper.

Another limitation in the discussion about consciousness has been the false assumption that serious, important work on consciousness started with recent cognitive and neuroscientific research, when in fact there have been fundamental and still neglected theoretical and empirical contributions to a science of consciousness by William James, F. W. H., Myers and others at the inception of psychological science (e.g., Kelly *et al.*, 2007). After them, even during the hegemony of behaviorism, other authors (e.g., Martindale, Natsoulas, and Tart) made notable contributions to this topic even before it could be mentioned in polite company among psychologists.

Also, neglected in most of the discussions on consciousness has been the fact that we transit among different states of consciousness even while awake, that such states organize experience, cognition, physiology, and behavior, and that what is postulated about one state may not apply to another. Some decades ago, Ludwig (1966) reintroduced a serious discussion of the topic and Tart (1975) provided the bases to conceptualize states of consciousness, including the transitions between them. Yet, this work has had a modest impact in current discussions of consciousness—with a few exceptions (e.g., Baruss, 2003)—and there have been few theoretical developments recently. Putnam (2005) cogently argued that knowledge about states of consciousness ("discontinuous ensembles of self-organizing variables", cf. Wolff, 1987) is necessary to understand childhood development and variations in psychopathology. I will argue that a development of a science of states of consciousness is also foundational to further the progress of research in parapsychology, but that much work needs to be done before such a science amounts to more than a collection of interesting facts. Because some of the developers of a science of consciousness have also worked in parapsychology, this discipline is well positioned to make important contributions as long as it avoids some common mistakes.

With that goal in mind, I will point out various common conceptual misunderstandings or limitations in the discussion of states of consciousness and will use my and others' research to illustrate how they may be resolved. At the end, I will propose necessary steps to build a discipline of states of consciousness. A recent review by Roe (in press) on the link between different states of consciousness and psi phenomena is very much in harmony with some of the points made in this paper. In this paper, I will follow Tart's (1975, p. 5) definition of a

distinct (or discrete) altered state of consciousness (ASC) as "unique, dynamic pattern or configuration of psychological structures, an active system of psychological systems," that is qualitatively different from the individual's ordinary state of consciousness. Because consciousness is an ever changing process, small, non-qualitative changes (e.g., being a bit more or less attentive) should be considered as just variations within a state, not as distinct states (Tart, 1975).

1. States of consciousness are not the same as induction procedures

Even sophisticated discussions of states of consciousness typically make the mistake of conflating qualitatively distinguishable states of consciousness with procedures or techniques that might or might not bring about such experiences. For instance, Vaitl and collaborators (2005) failed to distinguish between alterations of consciousness (e.g., near-death experiences) and psychological procedures that may or may not have an effect on the consciousness of those exposed to them (e.g., meditation, biofeedback). In contrast, an authoritative definition of hypnosis describes it as a *procedure* that may or may not produce ASCs; this definition is moot as to what those alterations may be (Kirsch, 1994; my emphasis). Furthermore, many of the alterations in consciousness sometimes elicited by a hypnotic procedure may also be triggered by very different contexts, such as a traumatic event (Cardeña, Maldonado, van der Hart & Spiegel, 2009). Nonetheless, it is more fruitful to subsume these alterations under one of the descriptive senses of 'dissociation' (Cardeña, 1994) than to talk about the 'trauma state of consciousness'. Similar alterations of consciousness should be discussed under the same rubric (say, state of consciousness or anomalous experience X), regardless of their antecedent procedures or events.

Stanford (1993) earlier on drew the attention of parapsychologists to the fallacy of equating a procedure with a state. His conclusion is supported by the results of a recent study in which a simple hypnotic induction with the only suggestion to go into a 'deep' state of hypnosis produced noticeable effects in the phenomenology and neurophysiology of individuals previously identified as high hypnotizables, but had little or the opposite effect on low hypnotizables (Cardeña, Lehmann, Jönsson, Terhune & Faber, 2007; see also Pekala & Kumar, 2007). Furthermore, it is not justified to even assume that there is only one distinct altered state for those individuals who are responsive to a particular procedure, a point that I will address further.

2. The terms used need to be defined and described

If, as Socrates maintained, the beginning of wisdom depends on defining one's terms, consciousness remains a fairly unwise area of study. Natsoulas (1981, 1983) is the *rara avis* that struggled with the complexities of defining what the various concepts and problems of consciousness are, while most other authors just bandy commonly-used terms that are never defined or explained. The assumption is that the reader will know what the writer is talking about, but very often it becomes apparent that the writer is not even sure him/herself or conflates different senses of the same term. Let me discuss two examples commonly found in the literature.

The first one is *trance*. Not uncommonly authors in psychology and anthropology describe an individual as being in a trance or a trance-like state. What they mean is rarely made explicit. Some years ago (see Cardeña & Krippner, in press), being utterly confused as to what authors meant by this term I decided to consult the venerable Oxford English Dictionary (OED). And yes, my incomprehension was understandable. The OED included six different senses of *trance*, which differ in important ways, yet most authors using the term tiptoed around this issue. Furthermore, much more information about conscious experience is provided when the components of a complex variable are analyzed. For instance, a correlational analysis of 2,000 responses to a questionnaire on absorption (the ability to become more or less fully focused in an activity or attentional object) suggests that this process can be further deconstructed as to whether it refers to a narrow or expanded consciousness, and whether the focus is 'internal' or 'external' (Tellegen, 1992).

Another example, found in both the psychological and parapsychological literatures, concerns the word 'unconscious'. There are so many far from interchangeable senses of the term, including the Freudian, the Jungian, the cognitive, and even the political unconscious that unless authors explicitly describe how they are using the term, it is nearly impossible to have an idea of what they mean. Naturally, the same criticism applies to discussions of 'making the unconscious conscious', 'accessing the unconscious', and so on. This search for terminological lucidity does not in any way deny the fact that most of the terms we use are 'fuzzy' (Lakoff, 1973), but even fuzzy terms need to be clearly articulated. Any complexity should lie in the nature of the phenomenon rather than in the lack of conceptual clarity.

3. States are not fixed, unchangeable entities

For good or for bad, it seems that at least for the time being we are stuck with the term *altered states of consciousness*, although a slight variation, substituting *alternate* for *altered* gained a bit of traction some years ago (Zinberg, 1977). A related term, anomalous experience, has been proposed, but it is not interchangeable because anomalous (i. e., unusual or 'out of the ordinary') experiences may be an ordinary rather than an altered state of consciousness among some people (Cardeña, Lynn & Krippner, 2000). Synesthetes, for instance, regularly experience that stimuli elicit an additional perceptual experience to the expected one, such as a particular letter also inducing the experience of a color, a phenomenon that most of us will experience, if at all, during an altered state (Marks, 2000).

The problem with the term altered (or alternate) state of consciousness is that it denotes a homogeneous and unchanging state of affairs. For instance when talking about the ordinary or waking state, the variety of changes occurring from one moment to the next are somewhat ignored, despite the fact that a defining characteristic of consciousness is its constant change (James, 1890). Although rarely used, to speak of different *modalities of experiencing* is a more accurate term in the sense that the gerund denotes an active process rather than an unchanging event.

Let me illustrate how considering a state of consciousness as basically immutable veils important changes. While doing research on individuals scoring in the highest 2-3% of hypnotic responsiveness (i.e., hypnotic virtuosos; Cardeña, 2005), I found that although there was reason to conclude that a hypnotic procedure induced qualitatively distinct alterations of consciousness in them, there were at least three discernible and distinct patterns that emerged spontaneously, without any specific suggestions: during self-evaluated *light* hypnosis, respondents mostly mentioned subtle alterations in their physical body, in *medium* hypnosis they described being in a different phenomenal place than their physical body, and during *deep* hypnosis they reported being in a dreamlike reality and/or transcendental experiences such as merging with a light or complete emptiness (see Table 1). Thus, within the alterations in consciousness produced by hypnosis among high hypnotizables there are discernibly different forms of experiencing, and their characteristics depend on when the experience is collected. More recently some colleagues and I have replicated and extended these

results with a between-subjects design by studying the whole spectrum of hypnotic ability, evaluating not only highly hypnotizable individuals, but also those scoring in the medium and low ranges of a hypnotizability test (Cardeña *et al.*, 2007).

TABLE 1.
Reports of characteristic phenomena according to hypnotic depth

	No	Light /Medium	Deep/very deep
Body sensation	Same	Relaxation, spinning	Disembodiedness
Emotion	Same	Mildly positive	None or more intense (e.g., 'awe, wonder')
Attention	Same	Focused on body changes	Free-floating
Memory	Same	Same	Infrequent retrieval of forgotten material
Thought	Same	Decrease of 'mental chatter'	Totally absorbed in event, or absent
Imagery	Poor	Simple (e.g. geometric forms)	Complex imagery, light, blackness
Time sense	Normal	Slow	Timelessness
State of Consciousness	Same	'Trance'	Akin to lucid dream; or transcendent SOC
Transpersonal experiences	None	Well-being	Merging, becoming one with all; 'void'

The dynamic properties of consciousness can also be evaluated according to both small and pervasive changes within a state (i.e., neurophysiological microstates; Vaitl *et al.*, 2005) and longer and more impactful transitions between states (e.g., from being awake to going under anesthesia, or going to sleep). The latter seem to involve cognitive disorganization and may have properties that differ from more stable states (Tart, 1975). Other than the study of hynagogic and hypnopompic transitions (e.g., Foulkes & Vogel, 1965), these phenomena have not received much scrutiny in other states of

consciousness, with few exceptions (e.g., Putnam, 1988). The proposed relationship between PK abilities and release of cognitive effort during testing (Stanford, 1977) may involve a transition between states that creates greater neural lability, a distinction that may have implications for performance in PK tasks (Holt & Roe, 2006).

4. Even within the 'same' state there are vast individual differences

Already in the fourth century B.C., Plato had noted in the *Phaedo* that not all initiates into mysteric ritual experienced consciousness alterations (Plato, 1961). However, it is not only the case that only some people will respond to an induction, but also that within the same general type of state there can be important differences. I have already mentioned the example of the ordinary state of synesthetes, but even unremarkable individuals can have important differences in their ordinary state. For instance, Hurlburt (in Hurlburt & Schwitzgebel, 2007) has reported how imagery may be a typical content in some people's experience, but a very rare event in others.

Furthermore, even the same phenomenal event may be apprehended and interpreted in different ways. In the study with hypnotic virtuosos (Cardeña, 2005), every participant reported spontaneously that they had suddenly seen a very bright light (the experiment was conducted in a dark, isolated room, so these reports did not have an external, physical referent). For some, this light was experienced as a source of transcendent bliss ('all the good things'), whereas for others it was just a sudden brightening of their imaginal experience and nothing else. As Bakan (1973) remarked, a common source of confusion in psychology (and parapsychology) is to assume that research findings indicate a *general* function, which asserts something presumably true of each and every member of the class, instead of an *aggregate* function, which asserts something presumably true only of the class as an aggregate. Thus, it is necessary to carry out research at the group and individual levels. An excellent recent example of the need to analyze not only group data but also data from exceptional individuals is provided by Morgan and Stegner (2008). They point out that although hypnosis in general provides a boost to sports performance that is no greater than other strategies to enhance motivation, there have been individuals in their and others' research that have attained what they call 'superhuman' abilities after a hypnotic procedure, after additional research controlling for alternative hypotheses. Exclusive consideration

of mean (aggregate) results, or discarding data outliers would hide this important finding. Whether a similar pattern occurs in controlled psi experiments will depend on researchers integrating idiographic and nomothetic research approaches.

5. Traits and states are interactive processes

In an influential chapter, Honorton (1977) reviewed the literature on a probable connection between some ASCs and putative psi phenomena. The important set of studies using homogenous sensory stimulation, or *ganzfeld,* was to a large degree based on the conclusions of this chapter about the importance of lack of variability of sensory stimulation. However, a review of a possible connection between psi and personality traits (i.e., predispositions to think, behave and experience in a particular way) published in the same tome as Honorton's chapter, arrived at the rather unpromising conclusion that the only clear result about traits is that psychologically balanced individuals might do better in psi research than those with psychological problems (Palmer, 1977).

Since that time, two findings have altered the study of personality considerably. The first has been the discussion that the traditional 'big five' personality traits taxonomy that had been dominant for many years has serious limitations. The second is that we gain in understanding, and are able to explain greater variance, when both personality traits and a particular context are considered simultaneously. Besides other problems with the big five model adumbrated by McAdams (1992), an alternative taxonomy to that system includes a (temperamental) trait not considered by the earlier classification, namely *self-transcendence*, or the propensity to experience oneself as part of a larger unity (Cloninger, Przybeck & Svrakic, 1993). This trait has high unique genotypic variance (Gillespie, Cloninger, Heath & Martin, 2003), and moderate positive correlations with hypnotizability, absorption, and related constructs (Cardeña & Terhune, 2008) that have been often associated with various anomalous experiences (Cardeña *et al.*, 2000). It is of interest that a content analysis showed that self-transcendent experiences during a ganzfeld psi task were positively correlated with successful selection of targets (Carpenter, 2004).

As for the second issue, personality psychology has shown that the best predictor of behavior and experience involves consideration of

both personality traits and situations (e.g., Bowers, 1973). It is thus difficult to explain how parapsychology has mostly focused on situations that might alter an individual's experience such as hypnosis or the ganzfeld without researching the effect of that procedure on different individuals. Depending on the individual's trait, an induction may bring about an ASC or nothing at all. The interaction (or joint effect of two or more variables) between a trait and a context may help explain, for instance, the very successful outcome in a psi task with a group of music students (Schlitz & Honorton, 1992) many of whom presumably share one or more similar traits. Most other ganzfeld studies using the same procedure but with different, more heterogeneous groups have not produced such a successful outcome. A good example of how analyzing interactions may reveal psi effects is a PK study by Holt and Roe (2006). Whereas there was no significant main effect for high or low machine or human lability, the hypothesized significant interaction between individuals with high trait lability and low lability systems, and conversely, was supported by the data.

6. We must develop a good taxonomic system

It is not much of an exaggeration to assert that without Linnaeus's classificatory system, biology would have taken much longer to become a full-fledged science. Regrettably no such development has occurred with respect to alterations of consciousness. That science is a vast simplification of the richness of life must be kept in mind (Feyerabend, 2001), but it is very difficult to discern meaningful patterns when there is not even an agreed upon set of definitions, descriptions, and a basic taxonomic system (O'Connor *et al.*, 1997). The study of states of consciousness has suffered more than others not only because of the complexity of the subject matter, but also because of the lack of a classificatory system. It could be argued that we have not gone much further than Plato's classification of the *manias* in the *Phaedrus* dialogue. In it, Socrates states that when individuals are not in their usual senses (which we could interpret as being in an ASC), they may have important and useful insights into reality. These manias or inspirations occur in four different realms, the prophetic (as in the case of sibyls), poetic (artistic creation), telestic (self-development and overcoming of disease and sin), and erotic (all-encompassing love) (Plato, 1961). This is not at all a bad classification of self-transcendent

experiences, and we can wonder whether we have gone beyond this Western model, not to mention the more specific classifications of meditative experiences developed in the East (Davidson & Goleman, 1977).

Modern classificatory attempts of states of consciousness have had various limitations and are at times overly simplistic. For instance, the theoretician Fischer (1986) proposed that states of consciousness could be arranged in a circular continuum of arousal; a "cartography of non-ordinary states." His model, though, has various problems, including the equivocation of techniques with states (e.g., zazen), arguable descriptions (e.g., equating creativity with the REM state), and so on. As useful as his attempt to bring attention to this problem was at one point, it is clear now that the proposal of a linear hypo- vs. hyper-arousal continuum does violence to the intricacies of the various relationships between the sympathetic and parasympathetic nervous systems (Berntson, Cacioppo & Quigley, 1993).

A far more elaborate map was proposed by Clark (1983) as a general tool to plot mental states. However, his model is anchored on a number of competing theoretical models rather than on descriptive categories, and perhaps because of its complexity seems not to have had an impact on the field. A more recent model (Kokoszka, 2007) suffers from some of the same problems. Nonetheless, the general idea of some type of multidimensional scaling to classify alterations of consciousness has already been fruitful in the study of different states of mind during childhood (Putnam, 2005) and deserves further development.

The lack of any general taxonomy of states of consciousness is illustrated by a comparison of two recent classifications. Berenbaum, Kerns and Raghavan (2000) suggested that anomalous experiences can be classified according to their onset/course and various phenomenological dimensions such as how pleasant the experience was. In contrast, Vaitl and co-authors (2005) proposed a taxonomy based on level of activation, awareness span, self-awareness, and sensory dynamics. Perhaps the most striking aspect when comparing these two systems is that other than a commonality in proposing level of awareness as essential, they differ in virtually everything else. Some of us took a step in that direction by asking the contributors to an anthology to discuss the same categories so that features of various anomalous experiences could be identified (Cardeña *et al.*, 2000), but this area needs further development.

7. Where do we go from here?

The parapsychological community has had to face a number of hurdles throughout its history, many of them unfair, but some of its own making. As an example of the latter category, mention should be made of the practice of publishing papers only in conference proceedings instead of peer-reviewed journals, whether in parapsychology or in more general disciplines (Alvarado, 2003). This would be scandalous in other disciplines. More relevant to this paper, a number of publications in parapsychology (and other fields) have not demanded that the authors provide clear definitions or descriptions of what they mean when they refer to specific ASCs. There are valid and reliable introspective methods (e.g., Pekala & Cardeña, 2000), so there is no justification for not evaluating the effect of a reputed consciousness alteration technique. Editors should demand from authors what Socrates asked of his interlocutors so many centuries ago, to define their terms and to evaluate the effect of their manipulations on the participant's state of consciousness.

There is also a need for greater knowledge and sophistication by remaining engaged with larger disciplines such as psychology, biology, physics and others. For instance, I mentioned earlier how the discussion of a trait/situation interaction was accepted a long time ago in psychology, yet has been mostly absent in the parapsychological terrain. We need considerably more and better research specifying what traits produce what ASCs under what situations, and we need many more studies evaluating specific experiences and performance in psi tasks, such as that of Carpenter (2004). At a more impressionistic level, there has been discussion in the remote viewing literature of attitudes that may be conducive to psi abilities (e.g., Targ, 2004), but these proposals need to be researched more systematically.

We know little about what specific experiences and states are associated with performing successfully in parapsychological tasks, and this will not change unless we have a much clearer conceptualization of states of consciousness in general, and of the specific experiences of individuals both within and across sessions. Parapsychology should integrate idiographic and nomothetic perspectives, and systematically research both general patterns and idiosyncratic responses. It is generally agreed that some individuals perform noticeably better in parapsychology tasks than the rest of us, yet careful and controlled case studies have been mostly absent in contemporary research, despite some thorough examples of this method in the older literature.

Parapsychology may learn a lot by studying further the individual characteristics, processes, and strategies of researchers and of those who are most successful during a psi test (cf. Schlitz, 1992).

Finally, we need a taxonomic system based on the actual experience and behavior of the individual, disregarding theoretical or even religious proposals as to the ultimate nature of these experiences. Specific descriptors of the main aspects of the experience, as well as their changes across time should be used both within and across individuals. To reiterate an earlier point, any term used should be clearly defined and described. However, simple operational definitions such as 'x is what such questionnaire measures' should also be avoided, considering that different processes and strategies ensue even among high hypnotizables responding to the same suggestion (e.g., McConkey, Glisky, & Kihlstrom, 1989). Also, using vague terms (e.g., 'nirvana states') or confusing inductions and procedures with states of consciousness will only perpetuate our current level of confusion. When relevant, significant differences within procedures (e.g., types of meditation) and states (e.g., types of mystical experiences; Wulff, 2000) should be thoroughly studied and spelled out. After the initial phenomenological work is done, there is a good chance of building a neurophenomenological taxonomy in which biological processes are related to identifiable experiences (cf. Lutz & Thompson, 2003). Baars (1997) asserted that subjective experience and brain functioning have a 'close mapping', and specific events such as transcendent experiences have different brain dynamics than other processes (Cardeña *et al.*, 2007; Newberg & d'Aquili, 2000). Yet, a systematic comparison between specific alterations of consciousness and physiological functioning is in its infancy. This, and other developments in the study of consciousness, requires engagement with broader and multidisciplinary research because specialized knowledge in many areas such as brain mapping cannot be obtained by cursory reading.

Researchers in anomalous psychology and parapsychology can contribute to the development of a sophisticated science of states of consciousness and go beyond the extraordinarily promising contributions of Plato, James, Myers, Tart, and others, but only if they avoid the mistakes that have plagued the field for too long. We still have to fully accept the invitation by William James to develop a dynamic, cross-cultural phenomenology of consciousness, related to the psychophysiology of the organism but with relevance to the humanities and the sciences (Taylor, 1998).

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DISCUSSION

HÖVELMANN: Etzel, about 20 years ago during the very first Euro PA conference I expressed very similar concerns about the term 'psi conducive states', for many of the reasons that you mentioned. Unfortunately it didn't have any major effect on the field. Do you have any recommendations for journal editors of any preferred or standardised terminology that could be used?

CARDEÑA: I am not surprised you would have raised this earlier, and I'm sure you expressed it better than I just did. I do not have recommendations right now except for the general recommendation I just gave that we demand more precise descriptions. Perhaps this would make a good future project. In our book *Varieties of Anomalous Experiences* we asked all of the contributors to address the same issues from their different perspectives; this was so that people could more directly compare out of body experiences with near death experiences with mystical experiences, and so on. I am planning an article in which I and others will try to synthesize some of that and try to start developing more of a taxonomy built from what people are actually reporting. At this point I will not be getting into physiology—I think neurophysiology will be extremely important, but will come afterwards. Developing a neuro-phenomenology can be extremely important and can then tie in with changes in brain activity, hormonal activity, and so on, but the basis is what people are reporting.

VON LUCADOU: Whenever you have something to do with qualia then we run into a problem; we can have no clear taxonomy because we have no definite states. The problem is first you have a different step, you have to find the dynamic of the system and then you can describe the dynamics with a certain taxonomy. This is a step further so I do not think you will be successful in describing qualia states at the first level because it is so different—everyone has a different frame of reference. So if you look for certain things to describe and you are outside of the Cartesian Cut, then it is simple; but if you go to qualia inside, then it becomes very individual, and the only thing that you can describe are the dynamics of the system, which show certain behaviors that can be used to describe the taxonomy of the system, using the theory of dynamic systems. But this description cannot be at the level of states. That is a very general remark.

CARDEÑA: I will respectfully disagree. I suppose I am much more phenomenologically oriented than you are because I think that even though in theory one may object and ask how can you compare qualia, where people are telling you about very different things, I would be confident that you can find some consistency or points of agreement. Let me take the example of mysticism, which is one of the typical experiences about which people say is so subjective an experience that we cannot even talk about it. Yet, when they look at what people are describing they are strikingly similar, they are comparable. Of course, I am sure there is going to be error variance, but when you look at the 'surface manifestation' I find there are enough common links for me to see similarities. And this can be improved if we use participants who have been trained to observe and describe their subjective experiences. Now, this is not at all to go against your notion about dynamic systems and multi-dimensional scaling for example when talking about variables. For me that would be the second step, for you the first, but perhaps they could be done simultaneously to come up with something better than we have.

SELF-ORGANIZED REALITY[1]

BRIAN JOSEPHSON

I shall describe a new approach to modelling reality, synthesising ideas of Steven Rosen, Ilexa Yardley and Stuart Kauffman. Conventionally, physics presumes a specific fundamental mathematical equation, the solutions to which represent all possible realities. The alternative that we discuss is that domains of order progressively self-organize into more comprehensive domains of order, with the longevity of complexes at the various levels being a decisive factor in determining what manifests, as is the case in biology.

This is emergent law rather than pre-existent law, and demands very different kind of thinking to the usual kind. For example, there is no universal description of what is the case but instead many descriptions, corresponding to the variety of effective divisions of the totality into figure and ground. Such descriptions are not merely 'in the mind of the scientist' but (again, as is the case in biology) an integral part of nature's processes, while the determination of the nature of space is also an aspect of these processes. These ideas have clear implications for parapsychology.

DISCUSSION

ROE: Brian, one theory that does suggest an organizing principle (that can support or supplement epigenesis) is Sheldrake's notion of morphic resonance. I wondered what your views were on his theory.

JOSEPHSON: You probably could include it, and generation of form is a part of this whole picture. Is there much evidence in favor of it now?

ROE: I think theoretically it is rather vague—which, given the account you just gave, might make it more appealing—but empirically those tests that have been published have generally supported his predictions. The problem is that his theory does not uniquely make those

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[1] Due to the complexity and novelty of Professor Josephson's presentation he preferred to give a verbal overview rather than publish his ideas prematurely. These will be developed elsewhere in due course.

predictions; parapsychology in particular offers us other accounts that might expect to see the same differences.

VARVOGLIS: Brian I was fascinated by some of the things that you said about this interplay between chance, necessity and intelligence, and the third factor is one that people do not normally acknowledge. If you follow the intelligent design idea, is the intelligence something that is already there ('dark intelligence' or whatever), or does it emerge out of the interplay between structures and chance, as something that emerges and is then reabsorbed in an entropic fashion, much as biological systems drop back into the disorder after a short time? Is there something necessary about the emergence of intelligence?

JOSEPHSON: It may be everything. People talk about the possibility of beings in another reality able to interact with us. However, with mathematical creativity there may some interplay between a mathematician's mind and background structures. There may be a complex of answers but there is something pre-existing that is an essential part of the process, and has to be taken into account when explaining any given outcome, but cannot be taken into account in advance because of the infinite number of possibilities.

ROLL: The whole logic of this was unknown to me and is very exciting. Professor Josephson has hinted in an earlier paper that there are similarities made between quantum physics and quantum mechanics and a biological corollary of that. To see step by step the logic of how we can arrive at quantum mechanics from a biological perspective that is combined with psi is quite amazing.

JOSEPHSON: Our papers on this are available on my web pages, at http://www.tcm.phy.cam.ac.uk/~bdj10/mm/articles/PM.html.

VON LUCADOU: I am interested in your notion of a third observer. It has been shown mathematically that if you have two entangled systems that are separate, under certain conditions these systems are entangled too. So you create automatically a third entangled system which is the entanglement between those two systems. Does this fit with your notion of the third observer?

JOSEPHSON: Yes, it fits very well and I think entanglement is one of the things where there is a link with quantum mechanics. And there may be some special kind of entanglement of systems that know each other well. I think the idea that a third system can affect the way that other

systems interact is important, and could have implications for ideas about psychological space and inter-relationships and so on. There may be all sorts of hidden concepts in complexity that are only now just coming out, just as information theory produced something new. One hopes that the string theorists will make similar discoveries—the way that strings turn into branes seems to be a parallel.

Clinical Parapsychology: Today's Implications, Tomorrow's Applications

Martina Belz

Introduction

The relationship between spontaneous extraordinary or exceptional experiences (ExE) and psychopathology has been recognized as problematic from the earliest days of psychical research and the field is still riddled with controversy today. The question of whether ExE are merely symptoms of psychopathology that are just explained paranormally by the person and/or his environment, or whether these experiences are based on genuine 'paranormal' processes is an ongoing yet unresolved debate between parapsychologists, the clinical sciences and the people who report these experiences. The answer to this question isn't trivial at all, neither for the clinical sciences nor for the individuals who seek help and assistance to understand these experiences and cope with their consequences.

A survey of the effects of ExE on people's lives (Milton, 1992) found that there is a need among experients to receive guidance and reliable information concerning these experiences but this need seems rarely to have been adequately met. Although the necessity for a specific information and counseling service in the field of parapsychology has been increasingly recognized in the last twenty years, especially among the representatives of scientific parapsychology (Solfvin, 1995), professional counseling services dealing with such experiences are still extremely rare. This is even more the case when we look for approaches in clinical parapsychology (CPP) that meet the standards for Empirically Supported Treatments (EST) as required for other areas of counseling and psychotherapy.

The following article is intended as a contribution towards meeting standards of EST in clinical parapsychology. It starts with a historical perspective, and then goes through the relevant literature about different approaches that have tried to categorize paranormal phenomena. The following sections discuss the literature that has been gathered so far about the psychological functioning of individuals reporting ExE, especially their psychopathology as well as the

explanatory models used to understand and make sense of these experiences. Finally the possibilities to apply these data for counseling and psychotherapy for individuals reporting ExE are illustrated and implications for future research and the clinical field are discussed.

A Short Story of Clinical Parapsychology

More than 120 years have passed since the founding of the Society of Psychical Research in 1882. Since then several authors have pointed out possible connections between ExE, altered states of consciousness and dissociative states as they play a role in hypnosis and the dissociative identity disorder (Flournoy, 1994/1901), paranoia (Prince, 1927), hysteria (Mitchell, 1922), dissociation (Janet, 1886), somnambulism and amnesia. Pioneers such as Janet (1886) and James (1902/1958) did much to explore these phenomena. To the list of renowned scientists and clinicians that were interested in the phenomena we can add Jung (1902), Freud (1936/1984)—who for some time held a membership of both the British and American Societies for Psychical Research—and Japsers (1923/1963).

As parapsychology disappeared into the laboratory for some time (Rhine, 1934) and behaviorism in its early years warned scientists and clinicians against dealing with consciousness and mental processes—because if they did they were in danger of getting lost in a maze of introspection and mysticism (Paivio, 1975)—clinical parapsychology didn't advance much for some decades. So it were the psychiatrists and psychoanalysts like Deutsch (1926), Servadio (1935), Ehrenwald (1948), Ullman (1949), Devereux (1953), Jung (1955), Eisenbud (1970), Wolman (1977) who stuck with the topic. They discussed the role of psi in transference and countertransference, the concept of unconsious influences, psi events as they arise during psychoanalysis and the significance of psi for the understanding of psychoses. Quite a few papers were published by them often with inspiring ideas but the experimental and empirical basis for most of those publications was missing.

One of the first times the term clinical parapsychology was used was in 1977 by Montague Ullman (1977) in his paper 'Psychopathology and Psi Phenomena'. By the end of the '60s Transpersonal Psychology developed which added a new perspective by understanding exceptional or spiritual experiences not primarily as signs of psychopathology but of personal spiritual growth (Mintz &

Schmeidler, 1983). Hastings (1983) developed a counseling approach that was using techniques from the clinical sciences but based on the clear statement that for parapsychological counseling to be meaningful there must be an initial general presumption of the reality of psi on the part of the counselor independent of the individual case. In 1987 a Parapsychology Foundation conference on spontaneous psi, depth psychology and parapsychology was held in Berkeley, California. In London in 1989 the conference 'Psi and Clinical Practice' was organized by the Parapsychology Foundation, allowing experimentally working parapsychologists, clinical psychologists, psychotherapists and psychiatrists to meet and discuss aspects of CPP. The proceedings of this conference were published in 1993 and are still a valuable source of information about CPP (Coly & McMahon, 1993). They include the counseling approaches of Kramer (1993) who differentiates between a short term counseling approach and a long term approach based on Rogerian therapy, and Harary (1993) who viewed psi experiences as creative perceptual and communicative processes that need to be normalized and encouraged but if necessary also diagnosed as psychopathological processes. In 1995 at the 38th meeting of the Parapsychological Association a panel discussion on Clinical Parapsychology was organized to exchange thoughts and ideas about counseling strategies in the context of anomalous and paranormal experiences (Solvin, 1995). The APA text *Varieties of Anomalous Experience* (Cardeña, Lynn & Krippner, 2000) represents up until now the most comprehensive attempt to integrate psychological and parapsychological results with respect to exceptional and anomalistic experiences into the larger body of psychology and other behavioral sciences with a clear connection to clinical (para)psychology. In 2007 in Naarden, Netherlands, a 'Clinical Parapsychology Expert Meeting' was organized and chaired by Kramer and Bauer with participants from 8 different countries who discussed the state of the art in CPP. The proceedings of the presented papers will be published (Kramer, Bauer & Hövelmann, in press).

Clinical Psychology and Parapsychology as frame of reference

If we take on a clinical perspective in Parapsychology the topics we have to cover should be related to the questions that 'regular' Clinical Psychology has to deal with (Caspar, in press). These have to be

translated and adapted to the special field of CPP. From that point of view the relevant topics are:

- Phenomenology of ExE
- Epidemiology of ExE
- Etiology of ExE and a paranormal belief system
- Psychological functioning and psychopathology of people reporting ExE
- Diagnostic (e.g. overlap between symptoms of psychopathology and ExE; individual differences of persons with a paranormal belief system and ExE)
- Treatment of people reporting ExE

These areas of CPP can be subsumed under three different main aspects: First the *phenomena* themselves (description, epidemiology, etiology), second the *explanatory model* (different expert and subjective theories with their empirical basis and motivational value) which is used by the different persons involved (client, environment), health care system (also alternative) to explain the phenomena and third the characteristics of the affected *person* (inter-individual differences in sociodemographic and psychological variables). The following picture shows these aspects.

Aspects of ExE	
Phenomenon	Ordinary phenomenon Exceptional/anomalistic phenomenon
Explanatory model	Conventional model (e.g. (neuro-) psychology, cultural sciences, physics) Unconventional model (e.g.. parapsychology, weak quantum theory, model of pragmatic Information)
Person	Conspicuous characteristics Nonconspicuous characteristics

FIGURE 1.
Aspects of Exceptional Experiences

What do we know?

In order to understand ExE as well as possible relationships between ExE and the general psychological functioning especially of those individuals who struggle with these experiences it seems helpful to integrate the research results that looked for individual differences between experients and non-experients. Also relevant seem data concerning individual differences between those who score high on paranormal belief scales and low-scorers. In addition, the conditions of acquisition and maintenance of ExE have to be differentiated. Furthermore, the overlap between the ExE and mental disorders has to be identified. This might also help us to understand in which way ExEs are processed and related to emotions, beliefs, motives, behaviors and appraisal and which might be their functional value. Last not least we should have a closer look at that special subgroup of people who seek assistance in understanding such experiences or in coping with their reactions to them.

So what do we know after more than 120 years of CPP about the phenomena, the experients und the use of different explanatory models? Until now many studies have been undertaken, that tried to find out if there are significant relationships between people who preferrably use a paranormal belief system as *explanatory model* for exceptional experiences, i.e. have a strong paranormal belief system, and different *person* characteristics. We also find several methodological approaches that try to classify and categorize the *phenomena* and look for relationships between the phenomena and the psychological functioning of the experients. The following overview will start with the phenomena and then look at individual differences between experients and non-experients and shows the characteristics of people with a strong paranormal belief system. This will *inter alia* determine the use of different explanatory models.

The phenomena

In the literature we find several approaches to describe and categorize ExE. At the beginning of parapsychological research the preferred methodologies to gather data about paranormal phenomena were surveys with a description of ExE and case collections, e.g. Gurney, Myers & Podmore (1886), L. Rhine (1961), Haight (1979), Jaffé (1997). Traditionally the reported ExE were put into two

categories: 'cognitive phenomena' which were labeled as extrasensory perception (ESP); and 'Motoric phenomena' which encompassed different aspects of psychokinesis (PK). Later on different authors developed more sophisticated classification systems (Neppe, 1993; Berenbaum, Kerns & Raghavan, 2000) that included several phenomenological dimensions like the context of the phenomena as well as different conditions of the experients, like their state of consciousness or awareness, etc. The methodological weakness of these approaches lies in the fact that the perception of the phenomena and their interpretations as paranormal are not always clearly separated (e.g. when asking for ESP or poltergeist phenomena instead of asking what the person actually saw, heard, felt, etc.). Empirical data that show the applicability, reliability and validity of the classification systems are missing as well.

In accordance with current standards for documentation in counseling and psychotherapy, a documentary system for advice seeking individuals with ExE was developed in 1998 at the counseling department of the Institut für Grenzgebiete der Psychologie und Psychohygiene e.V. in Freiburg, Germany (IGPP) (Belz-Merk, 2002). Since then the instrument has been revised and tested several times and the counseling department has committed to use it.

The documentary system includes different modules which allow us to record a broad spectrum of relevant information. The socio-demographic and clinically relevant data of the clients cover one domain. Then there is a special section that allows us to document the reported exceptional phenomena in a rather elaborated way: it differentiates variables for all common internal, external and dissociative perceptions and phenomena as well as possible coincidences amongst them. Unusual coincidences between inherently unobtrusive internal and/or external factors can also be coded. This procedure allows a very differentiated phenomenological categorization of the reported ExE. The categories reflect the subjective view of the clients and their experience independent of the judgement of the counselors regarding the veridicality of the experience and their possible paranormal or anomalous nature. The documentary system requires also the coding of the frequencies, the beginning and duration of the ExE as well as different context factors like the state of consciousness, the external circumstances in which the ExE occured as well as the subjective beliefs and theories of the clients. Inter-rater

reliability has been tested several times with sufficiently good results and a Kappa-coefficient of $\kappa = 0.6$.

Between 1996 and 2006 1465 cases with ExE who turned to the IGPP for help and advice could be documented with this instrument with sufficient quality. The sample gives a representative picture of the advice seeking individuals and the reported phenomena. As the analysis of the data shows ExE can basically be divided into two main phenomenological groups (Belz-Merk, 2000; Belz & Fach, 2005; Fach, 2006):

- External phenomena (localized in the outer physical world)
- Internal Phenomena (localized in the inner psychic world)

Table 1 gives an overview over the reported phenomena and how the experients subjectively localized them as an internal or external perception. Corresponding experiences of the same sensoric and perceptual modality but different localization can be found in the same row.

Based on these empirically found external and internal variables the phenomena can be organized on the background of Metzinger's theory of mental representations (Fach, 2007). Metzinger (1993, 2003) postulates, that the human being creates a mental *reality model* as 'internal description' of parts of reality. This reality model consists of two fundamental components, the *self model* and the *world model* (for more details see Belz & Fach, in press).

Based on the above-mentioned components of the reality model exceptional phenomena can be reduced to four basic possibilities of anomalies in the reality model (see Figure 2). On the one hand it concerns *internal* or *external* phenomena. The former are related to anomalies in the self model the latter are related to anomalies that occur in the world model. On the other hand, phenomena may occur that concern the relation of the self and the world model.

While on the one hand during psychophysical dissociation a separation of normally integrated components of the self and the body model occur on the other hand when coincidence phenomena occur unusual links between representations in the self and/or world model occur. On closer inspection these four categories of ExE form two complementary pairs. One of them concerns the localization ('inside vs. outside') of the phenomena in the fundamental components of the reality model, the other the relation of these components, that is the self

and the world model with respect to the elements which are portrayed in it ('separated vs. related').

TABLE 1.
Phenomena and their localization in the context of exceptional experiences

External phenomena	Internal phenomena
Optical phenomena/apparitions	Visual phenomena/images
Acoustic phenomena/mimicry sounds (e.g. raps, steps, voices without identifiable source)	Auditory phenomena/hearing voices
Tactile phenomena/change in temperature	Somatic phenomena/Body sensations
Olfactory phenomena	———
———	Unusual emotions/moods
Feeling of a presence/atmosphere	———
———	Body paralysis
Kinetic phenomena/ (de-)materialisations	———
———	Out-of-body
Physical alterations/Stigmata	———
———	Automatism/mediumism/channeling
Anomalies in audio-/photo-products or objects	———
———	Thought/-intrusion
External facts/events in the personal environment	———
———	Immediate knowledge
External facts/events of a general kind	———
Other external phenomena	Other internal/psychophysical phenomena

Patterns of exceptional phenomena

When analyzed by means of a principle component analysis the documented phenomena can be combined to meaningful patterns that describe a well-known phenomenology (Belz & Fach, in press). The following list describes the factors with their frequencies:

1. Poltergeist and apparitions (53%)

This pattern includes anomalies perceived in the external world like unexplainable movements or alterations respectively, the disappearance or appearance of objects, acoustic phenomena, especially mimicry sounds (e.g., raps, steps, voices) without an identifiable source, visual impressions (apparitions of light and shapes, etc.), tactile and olfactorial phenomena for which no natural cause can be found. The reported phenomena are oftentimes associated with ghosts and/or deceased persons.

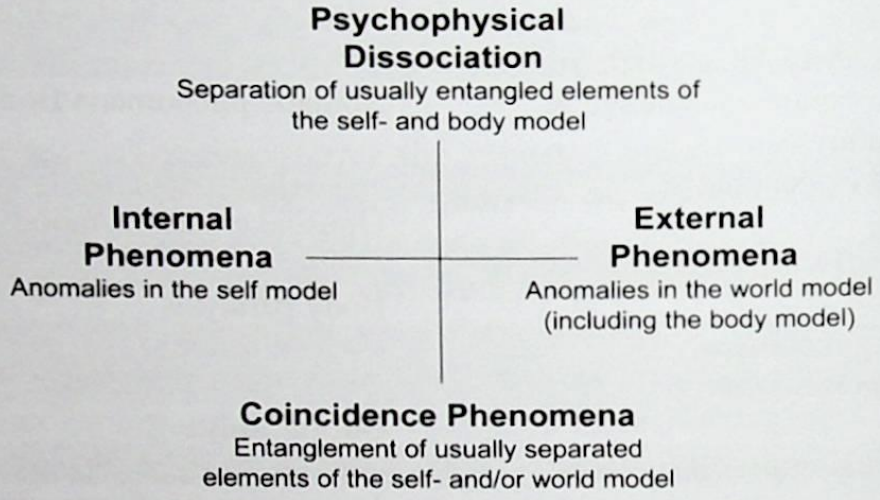

FIGURE 2.
Fundamental categories of exceptional phenomena (Fach, 2007)

2. Extrasensory perception (41%)

This pattern includes experiences in which meaningful but supposedly non-causally transmitted coincidences are reported. These are perceived by the affected individuals as occurring between internal phenomena—which can be of a usual as well as unusual nature—and inner states of other living beings (telepathy) or other external facts and events. The external facts can lie in the past or presence (clairvoyance) or in the future (precognition).

3. Internal presence and influence (38%)

Somatic phenomena (energy flux, pain) for which there seems to be no objective medical basis, hearing voices 'in the head', strange ideas and visual impressions are characteristics of this pattern which rests exclusively on internal perceptions. In many cases the phenomena are experienced as a supernatural external force on consciousness and the body. The affected individuals often assume that magic is at play or that they are possessed by external powers, ghosts or demons.

4. External presence and nightmare (15%)

The external phenomenon of 'sensing a presence' is part of a separate pattern. An invisible entity-like presence is localized in the external physical world whereby the perception is based on not qualified atmospheric sensations, less often on tactile phenomena (nightmare) which occasionally come along with a psychophysical dissociation, that is to say the inability to move the body (sleep paralysis).

5. Meaningful coincidence (10%)

This pattern refers to meaningful coincidences between exclusively external and separately considered conventional events in the environment without a satisfying causal explanation. They are mostly based on a perceived accumulation of subjectively similar events (e.g. accidents, reoccurrence of a certain number). In such cases the affected individuals get the impression they are exposed to fateful influences or heavenly messages or discover a secret conspiracy.

6. Automatism and mediumism (7%)

The exceptional feature of this pattern lies less in the inner experience but much more in a psychophysical dissociation which at least in the beginning is often induced willingly. In an altered state of consciousness without deliberate control a coordinated, autonomous bodily behavior (automatic writing, channeling etc.) appears.

Such experiences are in general understood by the affected individuals as an ability to get into contact with external forces or ghosts, therefore they are only seldom related to a feeling of being influenced unintentionally.

The three factors poltergeist (1), internal (3) and automatism (6) represent specific phenomena from the internal, the external spectrum and the spectrum of psychophysical dissociation. Factor 4 combines anomalies of the dissociative and the external area. The factors 2 and 5 differentiate between mere external coincidence phenomena and those in which internal elements correspond to external. Figure 3 shows an heuristic allocation of the six patterns with regard to their proximity to the four basic phenomenological categories—external, internal, dissociative and coincidence phenomena.

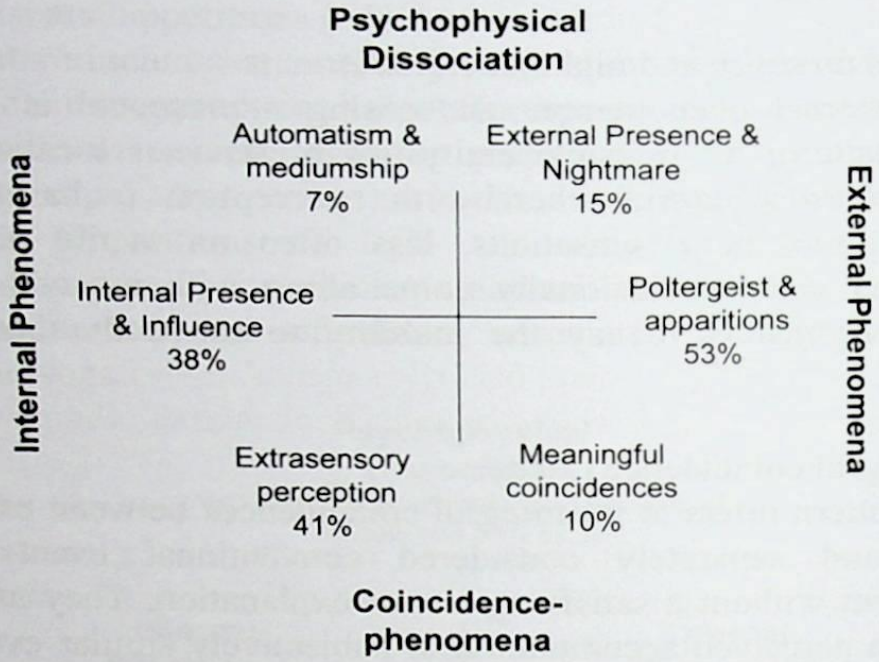

FIGURE 3.
Exceptional Experiences Pattern

Exceptional experiences, beliefs and psychopathology

A representative survey in Germany in 2000 showed that between 50% and 70% of the general population believe in the existence of paranormal phenomena and report at least one personal subjectively paranormal experience (Bauer & Schetsche, 2003). As far as we know from current epidemiological data on mental health (Jacobi, Hoyer & Wittchen, 2004) the one month prevalence of psychological disorders in Germany is 31%, lifetime prevalence is 43%. Similar results for ExE and psychopathology can be found for other western countries as well. These numbers show, that it is plain statistics to expect an overlap between individuals who have some kind of psychological disorder and the two third of the population who report their own ExE and believe in their existence. Yet several authors have noted (Broughton, 1991; Haight, 1979) that probably only about 10-15% of those who report ExE have had experiences that appear to be possible psi or actual anomalies. At least 70-80% of the individuals reporting ExE might misinterpret their experience. The motivations for such extensive misinterpretations have been explored by Irwin (2004) and Watt, Watson and Wilson (2007).

So one of the greatest challenges that CPP faces is the necessity to differentiate between a mental disorder and ExE in order to avoid

unjustified pathologization on the one hand and to identify a psychological disorder which needs adequate treatment in good time on the other hand and help those who misinterpret their naturally explainable ExE as supernatural, mystical, psychic or paranormal to come to a valid judgement.

Overlap between ExE and Psychopathology

If we compare different perceptions that are reported during ExE with the criteria for the diagnosis of a mental disorder we find quite some overlap. Table 2 shows possible phenomenological overlaps between ExE and different disorders.

Table 2 shows the most common examples of possible overlap but does not cover all possibilities. The greatest overlap can be found between the schizotypal disorder, disorders of the schizophrenic spectrum and disorders as a consequence of trauma.

Criteria to differentiate between ExE and a mental disorder

A comparison between reports of ExE of clinical and nonclinical groups show the following differences:

- Reports about ExE from clinical groups are more bizarre, more detailed and disturbing (Bentall, 2000; Jackson, 1997).
- Clinical groups report that their auditive hallucinations are uncontrollable whereas nonclinical groups have the feeling that they can controll them (Honig, Romme, Ensik, Escher, Pennings & Devires, 1998).
- Individuals diagnosed psychotic are not able to recognize the strangeness of their ExE compared to healthy individuals (Targ, Schlitz & Irwin, 2000).

If we have to decide if the reported and observed phenomena resemble the symptoms of a mental disorder or should be understood as the expression of an ExE we should take into consideration that a mental disorder is always seen as a disturbance in the functioning of the individual (Saß, Wittchen & Zaudig, 2003). So in order to classify the experience as pathological it is necessary that the experience has to cause suffering and impairment in a clinically relevant way in the interpersonal and working context. Emotion, cognitions and aspects of self-harm or being a risk for others as well as suffering and distress of the experients are relevant.

TABLE 2.
Overlap of perceptions during ExE and symptoms of mental disorders according to DSM IV and/or ICD-10

	Schizophrenia	Schizotypy	Organic Psychosis	Delusion	Hallucination	Dissoc. Disorder	Somatoform Disord.	Paranoia	Bipolar Disorder	PTSD	Borderline Dis.
External Phenomena											
Optical phenomena Apparitions	x		x		x		x				x
Acoustic phenomena Mimicry noise	x	x	x		x						x
Tactile phenomena Temperature changes	x				x				x		
Olfactory phenomena	x				x						
Feeling of a presence Atmosphere	x										
Kinetic phenomena (De-)Materializations											
Physical changes Stigmata	x					x					
Audio-/Photo-/ Object- Anomalies											
Reference to events in personal domain	x	x		x				x			
Reference to events of a general kind	x	x		x				x			
Internal Phenomena											
Visual phenomena Pictures/imageries					x	x				x	x
Auditory phenomena hearing voices		x			x	x				x	x
Somatic phenomena body sensations	x	x			x		x			x	
Unusual emotions/moods	x										x
Body paralysis											
Out-of –Body											
Automatism/Medium -ism, Channeling						x				x	x
Unusual thoughts Thought insertion	x	x		x		x		x	x		x
Sudden knowing	x	x		x				x			x

Characteristics of individuals with exceptional beliefs and experiences

Information processing

French (1992) showed in a review that high-scorers on a paranormal belief scale show a bias in reasoning and information processing that supported their belief. Brugger, Regard and Landis (1991) found that individuals with a paranormal belief system are significantly more convinced that they are able to influence chance processes. Persons with a strong paranormal belief system are more prone to expect good results in parapsychological tasks independent of the real outcome (Benassi, Sweeney & Drevno, 1979). Blackmore and Troscianko (1985) found that individuals with a paranormal belief system score far lower in tasks that test the ability to make probability judgements than individuals who are skeptical concerning paranormal belief.

In the meantime there exist numerous studies which have repeatedly shown that persons with a paranormal belief system also have elevated scores in scales that measure absorption (Tellegen & Atkinson, 1974), fantasy proneness (Rao, 1992; Wilson & Barber, 1983), suggestibility and field dependence (Hergovich, 2003) as well as transliminality (Thalbourne, 2000). This is also true for hypnotizability (Hilgard, 1974; Lynn & Sivec, 1992), imagination (Lynn & Rhue, 1986, 1987) and dissociation (Frischholz, Lipman, Braun & Sachs, 1992; Spiegel & Cardeña, 1991). Other studies have found that the same variables—that is absorption (Spiegel & Cardeña, 1991), fantasy proneness (Lynn & Sivec, 1992), hypnotizability and imagination (Whalen & Nash, 1996)—are related to dissociation and to paranormal belief (Irwin, 1994; Richards, 1991; Steinfurth, 1996; Wolfradt, 1997). For clinical groups the relationship between dissociative experiences and paranormal belief is stronger than for nonclinical groups (Wolfradt & Dorsch, 1995). Kennedy, Kanthamani and Palmer (1994) have summarized the results of studies that investigate the relationship between absorption, fantasy-proneness, and temporal lobe symptoms. They show that these factors generally correlate in the .5 to .6 range with each other and with paranormal experiences and form an interrelated cluster where all three of these personality constructs involve a high degree of imagination and fantasy.

Further variables which are related to ExE and altered states of consciousness are hypersensibility to external stimuli (Thalbourne, 2000) and 'thin-boundariedness' (Hartmann, 1991). Individuals who have rather permeable boundaries report also nightmares, sleep

paralysis but also lucid dreaming as well as especially colorful and vivid dreams and finally high scores in transliminality (Sherwood & Milner, 2004/2005). According to Jawer (2006) individuals who have a high sensitivity to external stimuli report significantly more apparitions that are subjectively classified as paranormal experience than control persons, but also more allergies, depression, migraine, nightmares and traumatic life experiences. The results of different EEG studies show that schizotypy as well as paranormal beliefs are associated with a dominance in right hemispheric processing (Pizzagalli, Lehmann & Brugger, 2001). Believers and those who report personal ExEs show high scores in schizotypical (= high preparedness to experience perceptional and cognitive contents as meaningful, which are without emotional meaning for external persons and social retention because of stressful 'emotional inoculation' by others) and rhapsodic style (critical-analytic thinking is reduced in favor of a tendency to reinterpret primarily negative events positively) (Spitz, 2005).

So individuals with a paranormal belief system and subjectively paranormal experiences seem to differ from others by their special way of processing information, which is characterized by a high permeability for external stimuli, a marked tendency for pattern recognition and the ability to dive into altered states of consciousness.

Motivational structure

The psychodynamic functions hypothesis (Stanford, 1990) assumes that a paranormal belief system serves a significant psychodynamic need of the individual, e.g. the need to enhance the sense of control over a seemingly uncontrollable world, especially after traumatic life events (Irwin, 1993).

In order to elaborate a better understanding of the dynamics involved in the structure and psychological functioning of individuals claiming ExE, Plan-analysis (Caspar, 2007) was used. Plan-analysis is a method that serves to analyze and describe conscious and unconscious instrumental strategies, starting from the level of concrete behavior up to superordinate general needs. Different aspects of psychological functioning like behavior, emotion, cognitive schemata but also motivational conflicts can be clearly arranged and reflected as far as their significance in the dynamics is concerned. As a result of several studies at the IGPP a 'prototypical Plan structure' for individuals looking for help and advice could be formulated (Berger, 2004; Spitz, 2005; Tölle, 2003).

Independent of the reported ExE pattern and possible mental disorders ExE are integrated into the psychological functioning of individuals in a way which serves the following needs and motives:

1. Externalize problems (e.g. ghosts, magicians etc. are held responsible for phenomena and negative life events)
2. Avoid stressful negative emotions (e.g. questions about traumatic life events are ignored instead reports about experiences connected with positive emotions are triggered)
3. Demonstrate exceptional abilities and show that you are a special person (e.g. extensive reports about amazing experiences are produced and explained by the assumption that the person has exceptional paranormal abilities)
4. Create meaning in life (e.g. loss and failure are reframed as test, special task etc.)
5. Show that the experiences are 'real', 'genuine' (e.g. emphasize that there is evidence for the event, that there are witnesses, etc.)

To look at their own experiences and behavior from a paranormal point of view helps the advice-seeking individuals to reduce tensions and inconsistencies (Berger, 2004) and to fulfill important basic needs to enhance self-worth and control in life, especially if only few other means and strategies are available. Gradually more and more parts of the psychological functioning are built into an ExE network. The repeated use of paranormal explanations for all kinds of experiences stabilizes this system. The ExE perspective begins to dominate the view of the world and of the self. Tensions will come back in the system as soon as the strategies which initially help seeking individuals to deal with their ExE become less and less able to fulfill their need for acknowledgement and control. This could be the case if stories about their own paranormal abilities or reports about telepathic influence don't impress others any longer but lead to loss of interest or rejection, so that interpersonal conflicts increase until a paranoid worldview develops and concrete problems remain unsolved.

This goes along with results from studies of the research group around Bentall (Bentall, Kinderman, & Kaney, 1994) which have shown, that the externalization of problems and failure are being used as regular strategies to regulate self-worth but can also develop towards a paranoid attributional style (Kinderman & Bentall, 1996).

Emotion regulation

Research results about the affective aspects of persons with a paranormal belief system point in two directions. On the one hand there are several studies which show that these individuals have a more positive affective attitude than skeptics; on the other hand there are several studies that show that believers have a more negative affective attitude. Thalbourne and colleagues (Thalbourne & Delin, 1994; Thalbourne & French, 1995) found that believers scored higher on measures of depressive experiences and manic experiences. Greeley (1975) conducted a national survey of 1,460 people that included the Bradburn Affect Scale, which measures positive and negative affect as well as the balance between the two. Exceptional experiences were positively correlated with both positive and negative affect separately, but were not correlated with the balance. Haraldsson and Houtkooper (1991) based on data from their multinational human values study that also included the Bradburn Affect Scale came up with an identical pattern. Nelson (1990) also found that people with ExE scored higher on both positive and negative affectivity than people who did not report those experiences. In two experimental EEG studies Gianotti (2003) found comparable results. Persons with strong paranormal beliefs reacted considerably more positively as well as more negatively to negative emotional stimuli and their ability to empathize with emotional information was much more pronounced than is the case with skeptics. So individuals with a paranormal belief system may be much more influenced by information directly in the situation and react emotionally more extremely than skeptics. This could be due to a special characteristic of believers as described in the section above which suggests that they are more conscious of their surroundings, take part more intensely and are much more absorbed.

Berger (2004) and Spitz (2005) analyzed how individuals seeking help and advice because of their ExE deal with their emotions. They found that help-seeking individuals can be characterized by a striking avoidance of negatively evaluated topics that might evoke difficult emotions; when confronted with them, their preferred defensive strategy is avoidance by evasion into positive emotional states.

Based on the reviewed results on emotion regulation we may presume that individuals with a paranormal belief system and personal ExEs perceive negative as well as positive emotions more intensely but then try to avoid instead of process these emotions and cope by evasion in positive affects.

Coping Style

The motivational schemata and goals which are especially important to advice seeking individuals with ExE concern intimacy/attachment and autonomy as well as the avoidance of loss of control (Toelle, 2003; Spitz, 2005). Especially in the area of intimacy and attachment remarkable discrepancies between desire and reality exist. Another important characteristic is the accumulation of social, psychological and physical stress in the actual life situation of many advice seeking individuals (Belz & Fach, 2005). The preferred strategies that these persons use to cope with the resultant inconsistencies are the following (Belz, 2008):

- Experience and interpret perceptions and cognitions as meaningful which have no emotional meaning for outsiders
- Social reservation or withdrawal in order to avoid stressful 'emotion inoculation' which results from intense empathizing
- Preference for intuitive behavior control with a holistic processing of information in combination with the reduction of critical-analytical thinking. This is a consequence of staying spontaneously too unilaterally in a positive mood. What happens is that only those behavioral routines which can be recalled automatically (i.d. without conscious consideration), which have proven to be valuable in similar situations before or behavioral routines which are expected by others (external control) are used and trained. As soon as difficulties have to be tackled that need rational analytical thinking and might be connected with negative emotions these routines fail. Experiences connected with negative emotions remain unintegrated.

Typology of individuals with ExE

The phenomena that people with ExE report can not only be organized in terms of factors and patterns, as has been shown above for the data from the IGPP counseling department, they can also be analyzed by means of a cluster analysis (Ward method). When using this method we can find out if groups of clients can be identified with typical combinations of phenomena and their relationship to sociodemographic and psychological variables (Belz & Fach, in press). In the order of the frequency the following types of clients based on N=1444 cases can be specified:

- *Poltergeist type (21%),*
- *ESP type (16%),*
- *External presence type (14%),*
- *Internal presence type (12%),*
- *Coincidence type (8%), and*
- *Mediumistic type (7%).*

In addition there are three hybrid types which are characterized by the occurrence of two patterns in each case:

- *Internal ESP-type* (internal presence and ESP, 9%),
- *Poltergeist-ESP-type* (poltergeist and ESP, 7%) and
- *Internal-poltergeist-type* (internal presence & poltergeist, 6%).

Earlier studies with smaller samples had already shown that groups of advice-seeking individuals that were formed on the basis of the ExE alone showed significant differences with regard to social and clinically relevant factors (Belz-Merk, 2002; Fach, 2006). The current results confirm the previous ratings.

The types of clients differ significantly with respect to various socio-demographic factors as well as in the degree of distress and psychopathology reported. The results show a high level of distress and psychopathology for clients with internal phenomena (Internal presence and internal poltergeist type). This is not the case for the internal-ESP type who, like the mediumistic type, is much better integrated as far as job and social/interpersonal situation are concerned even though these groups have above average experience with psychotherapeutic and psychiatric treatment.

Various types of clients with ExE also differ with respect of their interpersonal and conflict behavior (Fach & Atmanspacher, 2006). Table 3 shows the differences for the poltergeist and the internal presence type.

Clients with ExE who perceive the phenomena primarily in the external world live mostly in family systems with strong bonds where conflicts are hardly ever handled openly and emotions are more controlled. Clients that belong to the internal type despite often being much better educated are also much less integrated as far as social and job life is concerned. They are often much more distressed and involved in open conflicts with their environment. They are also significantly more likely to report experiences with psychiatric or

psychotherapeutic treatment and are more often rated as psychologically conspicuous by the counseling team of the IGPP (Belz & Fach, 2005).

TABLE 3.
Poltergeist type versus Internal Presence type

	Poltergeist Type	**Internal Presence Type**
Social Situation	family	single
	binding	isolation
	hidden conflicts	overt conflicts
EE-Characteristics	external	internal
	physical	mental
	objective	subjective
	primordiality	confirmation
	elusivity	persistence
	diffuse threat	concrete threat
	impersonal	personal
Social Behavior	adapted	unadapted
	approving relationships	challenging relationships
Conflict Behavior	avoiding	provoking
	repressing	projecting

Individual differences and belief in paranormal phenomena

A relatively large body of research has examined the relationships between paranormal beliefs (usually independent of personal experiences) with different sociodemographic, cognitive and personality traits and characteristics (Irwin, 1993; Lawrence, 1998; Wiseman & Watt, 2006). To find out if individual differences exist between those who believe and those who don't believe in the paranormal, researchers use different Paranormal Belief Scales (Tobacyk & Milford, 1983; Schriever, 1998; Goulding & Parker, 2001) with subscales for traditional religious belief, psi, witchcraft, superstition, spiritualism, extraordinary life forms, and precognition.

Irwin names four basic conceptual approaches that are basically skeptical of the existence of psi and test the relationship of paranormal belief with the following psychological attributes:

- Social marginality (Bainbridge, 1978). The underlying hypothesis assumes that members of socially marginal groups are especially susceptible for a paranormal belief system.
- The world view (Alcock, 1987). The underlying hypothesis assumes a relationship between paranormal belief and other belief systems which have a subjective and often esoteric basis.
- Cognitive deficits (Blackmore, 1997). The underlying hypothesis assumes that individuals who have a paranormal belief system have also an uncritical, illogical and irrational way of thinking, live more in a fantasy world, misjudge probabilities, make mistakes in probability judgments and can be more easily influenced and are therefore more at risk and can therefore more easily be influenced and misattribute normal experiences as paranormal.
- Psychopathology (Zusne & Jones, 1982). The underlying hypothesis assumes that individuals with a paranormal belief system are psychologically deviant and socially withdrawn.

In his review Irwin (1993) shows that the majority of the mentioned variables show either no or only minimal correlations between a paranormal belief system and the different aspects or the results are often contradictory (Belz-Merk, 2002; Wiseman & Watt, 2006).

Salutogenetic aspects

When discussing ExE and mental health we should keep in mind that ExE might also contribute to mental health and stabilization (Kohls, 2004). If this is the case and to what degree might depend on: the valence (pleasant vs unpleasant) of the ExE; if the experients could control the ExE or felt helpless and overwhelmed; if they have an explanatory model for these experiences; and if the environment accepts the corresponding way of behavior and the world view. Kohls (2004) showed that subjects with a spiritual background rate ExE much more positively than clinical subjects and have better mental health. Goulding (2004) showed in her study on mental health aspects of paranormal and psi related experiences that there is a subgroup of individuals who score high on paranormal belief and experiences also have a high sense of coherence connected with low neuroticism, which is an indicator for mental health.

The kind of the ExE also determines if an experience contributes to mental health or not. Lucid dreaming is a type of experience which has

repeatedly been described as pleasant (LaBerge & Gackenbach, 2000), because it is connected with mental clarity and control, both aspects that come with emotional stability and low neuroticism (Wallace & Newman, 1997). Mystical experiences (Wulff, 2000) as well as experiences that are described as precognition, telepathy and clairvoyance, are often interpreted as meaningful events that contribute to one's sense of spirituality. A paranormal belief system, 'Magical ideation' and 'aberrant perception' are all closely correlated with both artistic and applied creativity. Permeability of the individual ego/self seems to be a/the central variant (Goulding, 2004). Folley and Park (2005) investigated creative thinking process in relation to schizotypal personality, schizophrenia and prefrontal hemispheric laterality. Behavioral data indicated that schizotypes had enhanced divergent thinking ability compared with schizophrenic and control subjects. Divergent thinking was associated with bilateral prefrontal cortex (PFC) activation, but the right PFC particularly contributed to the enhanced creative thinking in psychometric schizotypes compared with the other two groups. This ability to implement novel associations is also typical for individuals with a paranormal belief system and reported ExEs (Brugger, 2006; Spitz, 2005).

McCreery & Claridge (2002) showed that paranormal beliefs and experiences might actually be adaptive rather than related to psychological ill health. Some believers and experients are affected in positive ways as they report an increased sense of wellbeing, sense of connection to others, happiness, confidence, optimism about the future, and meaning in life (Kennedy & Kanthamani, 1995). A survey by Kennedy, Kanthamani & Palmer (1994) found significant positive correlations between overall meaning in life and psychic and/or transcendent experiences. Very few respondents considered their ExE detrimental, and 91% of those reporting transcendent experiences and 46% of those reporting psychic experiences considered them valuable.

Clinical practice

Every year numerous people turn to the IGPP and to other institutions which offer counseling for ExE (Belz & Fach, 2005) or to academic parapsychology units (Coelho, Tierney & Lamont, 2008). This shows us that even if the majority experience these events positively or neutrally and assimilate them quite well, there is also a relevant minority who feel puzzled, irritated and confused and need

help. Another group we have to deal with consists of individuals who come with the idea in mind that they are someone special with exceptional psychic abilities because of their ExE and want to be tested. A request we have to disappoint for several reasons. Yet another group of people who report ExEs suffer from some mental disorder and look for an alternative approach in understanding and treating their pathological symptoms. So the main interventive tasks in CPP sum up to the following

- help those who look for support in understanding and integrating ExE
- clarify the role of psychopathology and refer clients to a psychotherapist or psychiatrist when it is indicated
- inform those who come with the idea of their own specialty about the elusive, nonlocal and acausal nature of the phenomena (Lucadou, Römer & Walach, 2007) and refer to psychotherapy if further problems come up

These tasks need a special kind of expertise which should be based on a solid ground of empirical data and good clinical practice comparable to the standards in clinical psychology and psychotherapy as well as a sound knowledge of the phenomena and theories to explain these processes.

Diagnosis and case conceptualization

The first step in counseling and therapy with individuals reporting ExE is trying to get a good picture of the person, his or her explanatory model, and the experience.

By dint of the following central questions these aspects can be comprehensively recorded and then summarized in an individual case conceptualization with the relevant descriptive and prescriptive information (Eells, 2001). The prescriptive component contains the central 'facts' and problems of a person and corresponds as such with the above mentioned aspects of person, phenomenon and explanatory model. Out of it prescriptive recommendations for treatment planning can be derived.

TABLE 4.
Central questions for the exploration of people with ExE

Phenomenon oriented questions:
Frequency and stability: Does the phenomenon refer to a single event or are the phenomena constantly observed, eventually with increasing frequency?
Inducement: Whereby and when is the first time that the phenomena could be observed and what followed?
***Valence*:** How are the phenomena evaluated: positive, negative or neutral?
State of consciousness: Which was the state of consciousness that the person was in while the phenomena were perceived?
Psychopathology: Is there an overlap between the reported phenomena and symptoms of a psychological disorder and if so to what extent?
Drug use: Are drugs or other psycho active substances involved when the phenomena occur?
Questions concerning the explanatory model:
Subjective view and theory: Which explanations for the occurrence of the phenomena are used by the affected person?
Consequences: Which are the consequences of the model for the life situation and biography of the affected person?
Adequacy and adaptivity: How appropriate is the used model for the understanding of the phenomena?
Obsession: How flexible or rigid are the subjective views or theories?
Self-awareness: Which explanatory models for the phenomena do I as a counselor or therapist have in my mind?
Person-oriented questions:
(Sub-)culture: What is the cultural background of the person who reports the ExE and which world view is prevalent?
Functionality/Instrumentality: Which is the intended conscious or unconscious purpose that the ExE serves interpersonal as well as intrapersonal?
Inconsistency: Are there any experiences in the actual situation or in biography which involve the violation of basic needs like attachment, autonomy, control and understanding, enhancement of self-worth, etc?
Emotion regulation: How are negative and stressful emotions being handled?
Neuro(psycho-)logical processes: Are there any neurological impairments with ExE inductive style of perception and information processing?
Impairment: Do the ExE go along with any constraints within the interpersonal or achievement area?

The three categories—phenomenon, explanatory model and person—are not and cannot be clearly distinct aspects but represent different perspectives from which the ExE can be approached and used as guidelines to the areas that should be covered with clients who report ExEs. In addition to the application of the above mentioned central questions in the exploration for a succesful intervention in CPP it is important to encounter clients with a special attitude. This is essential for the counseling and therapeutic relationship because people with ExE looking for help and advice often report prior negative experiences with the traditional health care system, where ExE are often equated with psychopathology or fraud. So it is essential to consider the following special features in clinical practice with ExE clients.

1. **Level of phenomena:** It is basically helpful to take on a positive attitude towards the phenomena and explore them thoroughly including the context and history of their occurrence. They are not an interfering variable but help to create sense and meaning in an otherwise often incomprehensible event or context.
2. **Level of explanatory model:** It is fruitful to adopt a constructivist position, which implies that the explanation of the expert is also a construction. So instead of 'explaining clients the world' it is much more promising to engage with the client's model and move from there to a preferably adaptive and appropriate model.
3. **Level of the person:** Clients with a paranormal belief system and ExE report significantly more traumatic life events than the average population. This is connected to specific characteristics in perceptional style, information processing and emotion regulation. We have to be aware of that background. On the one hand, intervention techniques that are connected to these characteristics (imagination, hypnotizability, fantasy-proneness, absorption etc.) might be very fruitful; on the other hand, we should always be aware of the necessity to deal carefully with possible traumatic life events to avoid re-traumatization.
4. **Level of relationship:** Develop individually tailored motive-oriented counseling or therapy relationships paying special attention to self-worth, control and attachment motives of the client.

Treatment planning

On the basis of the information collected along the three central aspects—phenomenon, exploratory model and person—general treatment goals and resultant technical procedures can be derived. The following list is a helpful tool when it comes to formulating an individual case conceptualization in ExE counseling. It shows in which way the different aspects of phenomenon, model and person can be knit together in a step-by-step process.

TABLE 5.
The process of treatment planning

7 Steps for treatment planning
1. To which type can we assign the client? (e.g. rather the external type with the focus on autonomy or the internal type with the focus on interpersonal problems, deficits in personal relationships and somatic phenomena)
2. Which are the central motives and unconscious/implicit plans? (e.g. avoiding negative emotions, create meaning or enhance self-worth)
3. Which basic needs and unconscious plans are threatened? (e.g. rather intrapsychic plans like the need for control after trauma or unwanted and continued "influence" by a healer or interpersonal plans and motives like the need for autonomy vs. bonding in poltergeist families
4. Which coping strategies are used? (e.g. repression and avoidance as expected for the external type or projection and externalization for the internal type)
5. Where is the natural development blocked or threatened? (e.g. blockage of the need for autonomy with the external/poltergeist type, social withdrawal with the internal type, avoidance to deal with reality for the mediumistic type)
6. Which are desirable changes and goals? (e.g. dealing with avoided emotions in connection with trauma, establishing balanced social relationships or dealing with reality if psi becomes the central topic in life)
7. Which are the strategies that can be used to reach the goals? (e.g. if the level of functioning is high information and education is sufficient, clarification if motivation is low or support to integrate ExE is needed, activating resources in case of social withdrawal or if dealing with ExE and being psychic affects ability to deal with reality)

Intervention goals with ExE clients

Based on our knowledge about the psychological functioning of ExE clients and the reported phenomena specific prototypical goals for clinical psychological interventions can be formulated for that clientele. These goals can be assigned to the above described aspects of phenomenology, explanatory model and person characteristics. Table 6 gives an overview.

TABLE 6.
Intervention goals for people with ExE

Intervention goals with ExE clients
A. Focus phenomenology
• Teach the client to evaluate the content of the phenomena as important source of information
• Impart phenomenon specific knowledge and give psychological education to enable the client to come to a sound judgment and reliable reality
• Reduce incriminating phenomena
• Control with respect to gain understanding of the desired phenomena
B. Focus explanatory model
• Develop jointly an exploratory model which is coherent and adaptive taking into consideration the state of empirical research and a constructivist attitude
C. Focus person
• Integrate the ExE into the self-concept by generating sense and meaning
• Identify etiological and functional relationships between ExE and life situation with respect to biography
• Increase self-control and autonomy (especially external type)
• Improve the ability to relate to others (especially internal type)
• Improve ability to deal with negative emotions
• Improve ability to deal with reality

Counseling strategies

The strategies and techniques that can be used in order to reach the above-mentioned goals are geared to basic principles in counseling and psychotherapy. The indication for a specific procedure depends on the stage of change the client is in alternatively his motivational situation, the involved phenomena, the current life situation of the client, his biography and last not least his request. As a general rule it could be found that in early stages of counseling and therapy and in stages when motivation is still unclear strategies using clarification and insight are more successful while in later stages coping and action-oriented strategies are more adequate.

So in the early phase of counseling it is appropriate to enhance clarification processes through interventions like active and empathic listening, asking questions and explicating. Typical questions could be the following: What is it exactly that makes the experience so irritating, so exceptional for you etc.? How come the exceptional happens just now? Why you of all the people etc.? What does it mean to you? What exactly is so important about it?

For interventions which focus on the subjective explanatory models of ExE-clients cognitive techniques as developed by Beck and Ellis are appropriate. But unlike in cognitive therapy the goal is not to identify "irrational beliefs" but check together with the client the following aspects:

1. Which are the consequences different explanatory models have for the client (advantages and disadvantages)?
2. Which of the consequences are welcome and which not?
3. Do alternative explanations exist which seem appropriate to the client?
4. Which are the implications the alternative explanations have for the client?

The intention of the use of cognitive techniques is to sensitize clients towards their own automatic thoughts that make immediate connections between experiences and explanations and go along with dysfunctional valuations and appraisals. What we should keep in mind is the possible functionality of the existing explanatory model. It might help the client to cope with difficult experiences and avoid negative and difficult emotions that he is not able to process without support (e.g. explain failure by magic and thus repair self-worth and avoid feelings

of sadness and disappointment). The counselor should not saw off the branch the client is sitting on unless he has put up a ladder to the tree!

Problems and limitations

People looking for help and advice because of their ExE have a broad range of inquiries. Some of them are not compatible with the identity of clinical experts or scientific parapsychologists. Among these are testing, training and certification of assumed paranormal abilities, help to start a career as a healer, medium, witch, shaman or parapsychologist. Others ask for support to publish books about their ExEs and their corresponding theories.

Some of the clients who ask for counseling because of personal ExEs are not necessarily convinced that a clinical psychologist can help them, but believe that only a technician or a scientist will be able to solve the problem. So one of the central tasks of a clinical psychologist in parapsychology is to establish motivation for and commitment to counseling so that the client can accept the possible link between him and the phenomena instead of externalizing the experience ('the truth is out there'), which is especially the case with people who report poltergeist phenomena or with people who report feelings of being influenced.

Another central issue is the authenticity of the perceived phenomena. Because of their intriguing character and unexpected occurrence ExEs are not self-exploratory. To de-mystify and normalize the experience might also be disappointing to some clients. So it is the counselors' task to tame the exceptional without taking away the specialty and admit that many experiences still remain unexplained. The ability to tolerate uncertainty is considered one of the biggest challenges in ExE counseling, not only for clients but also for the counselors.

Where are we going?

Taking into consideration that on the one hand there is quite some overlap between the symptoms listed in the categorical systems of the clinical sciences for psychopathology and typical elements of ExE and that on the other hand there is only marginal overlap between researchers and clinicians in mainstream clinical psychology and clinical parapsychology it is time to bring the fields closer together.

Caspar (2008) summarizes the standards and definitional criteria for professionalism that have become common for clinical psychology and the requirements which are necessary for professionalism of (clinical) psychological interventions:

- that they are based on empirically supported concepts
- professional training of those who intervene
- the use of empirically supported interventions
- an ongoing evaluation of effects.

CPP has just started to fulfill some of these requirements but still has a long way to go. So it is about time to bring together the best of two worlds if we take seriously the need for counseling and therapy for people with ExE.

The first step would be to think about the name of the field. In my opinion it would make much more sense to talk about 'Clinical Psychology for people with ExE' instead of 'Parapsychology'. This would fit in much better with fully dimensional models in the context of psychological health that are currently discussed than the actual classification systems. Another important advantage of this label leaves the judgement about the possible paranormal nature of the experience open. As we have shown above, ExEs are based on retrospective reports and the subjective judgement of the experient with all the known problems, and individuals have a wide range of non classifiable experiences for which they need help in understanding and coping. This understanding would open the field also for those clinicians who have an open or even skeptical attitude towards the paranormal. Independent of their worldview they will see clients and patients with these experiences who need help and advice. If we can offer an approach that does not demand a certain worldview and belief system but is useful for every expert with an open mind we will be more successful in moving the field forward and helping those who really need it.

At this point I could also talk about those aspects where clinical psychology can profit from the field of ExE. We might consider topics such as Weak Quantum Theory, the Model of Pragmatic Information, research on altered states of consciousness, questioning the currently dominant (but not at all the only possible) approach to mental problems and definitions of normality and abnormality, the importance to include questions of sense and meaning in life into therapy, etc., etc. But that is another story. So let's do our homework first and that is to develop empirically supported treatments for people with ExE, evaluate the

treatment effects and train clinicians for counseling and therapy of people with ExE.

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SPIRITUALITY: THE LEGACY OF PARAPSYCHOLOGY

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Background: The History and Current State of Parapsychology

When the Society for Psychical Research (SPR) was founded in 1882 it stated clearly that its mission was to find scientific, empirical evidence to disprove the claim of materialism according to which all phenomena, including those of consciousness, altered states of consciousness and pertaining to spirituality and religion must ultimately submit to the laws governing matter.

> It has been widely felt that the present is an opportune time for making an organized and systematic attempt to investigate that large group of debatable phenomena designated by such terms as mesmeric, psychical and spiritualistic. From the recorded testimony of many competent witnesses, past and present, including observations recently made by scientific men of eminence in various countries, there appears to be, amidst much illusion and deception, an important body of remarkable phenomena, which are primâ facie inexplicable on any generally recognized hypothesis, and which, if incontestably established, would be of the highest possible value.
>
> (Society for Psychical Research, 1882, p. 3)

This is quite understandable given the mood of the time. In 1842 Emil du Bois-Reymond (1818-1896)—who was to be a professor of physiology and one of the famous names at the University of Berlin—came together with a couple of other young scientists and made a pledge. Du Bois-Reymond described this in a letter to one of his friends in the following way:

> Brükke (sic) and I, we have pledged to bring to light the truth that there are no other forces active in the universe than the general physical-chemical ones. Furthermore, where those are not sufficient to explain things so far, these explanations will either have to be sought following the physical-mathematical method, or found by assuming new forces. These new forces,

> however, are to be always reduced to attracting and repulsive components and would be of the same dignity and nature as the physical-chemical ones, and thus inherent in matter.
>
> (Du Bois-Reymond, 1918, p. 108; Translation HW)

This document, describing a pact of what were to be the most famous minds in German physiology in the 19th century, is a window into the mood of the time out of which the parapsychological movement arose. It is important to note the following few facts in order to properly understand the exemplary nature of this document: Du Bois-Reymond and Helmholtz were both pupils of Johannes Müller and knew each other well. They both turned against their teacher: Helmholtz in proving Müller wrong by determining the speed of nerve impulses; du Bois-Reymond by campaigning for a materialist view. Brücke, who was to be the direct teacher of Sigmund Freud in Vienna, became the founding father of the Viennese school of physiology.

By the time the Society for Psychical Research was founded, the intellectual climate in Europe, at least in the natural sciences, had become materialist in a chilly way for every spiritualist, transcendentalist, or even dualist. And it is only against this background of materialist emphasis within the natural sciences that the founding of the SPR can be understood not simply as an innocent scientific society among others, but as a counter-movement promoting a certain world-view, namely that of non-reductionist understanding of human affairs, and of mind and consciousness in particular.

Members of the SPR included such famous figures as William James, Sigmund Freud and Carl Gustav Jung, among others. What is of course clear is that the SPR pledged to use scientific methods to substantiate its claims, and those were increasingly the methods used by the successful natural sciences. Initially, and during the first phase up to the 1930s the activities of the SPR covered large surveys, intensive case studies of mediums and séances, qualitative studies of precognitive dreams and the like (Beloff, 1993). With J. B. Rhine the picture changed (Rhine, 1964). He introduced experimental methodology into parapsychology, with the idea of replicable, experimental control, quantitative statistical analysis, control of circumstantial parameters through laboratory methods, simple research paradigms that could be repeated at will. Modern day parapsychology was born as an heir out of the marriage between the original impulse to prove the existence of phenomena beyond the world view of the natural sciences, such as telepathy, clairvoyance, precognition and

psychokinesis, and the successful experimental method used by the natural sciences. It seems a natural assumption that the child of the two should be able to do what each one alone was not able to do.

If we look into today's academic landscape, into how parapsychology impacts on research, let alone into academic decision making, allocation of research funds, number of academic departments specializing in this subject matter, future career prospects of researchers, and interfaces between academia and society, we must unabashedly confess that the 125 years since the founding generation of enthusiastic, high caliber scientists vowed to prove materialism wrong have not seen the promised turn of the tide. On the contrary, academic and experimental parapsychology as an attempt to provide the long desired proof of principle of parapsychological phenomena has come to a dead end road, it seems. This is, in a short sketch, the situation:

The long-time effort of what probably was the academically best situated department, the Princeton Engineering Anomalous Research (PEAR) Group did not succeed in proving their point finally and for good (Walach & Jonas, 2007). The Duke Parapsychology Laboratory, the erstwhile home of J. B. Rhine and once the Mecca of parapsychology, has closed. Apart from a few single researchers in the US, there is no further academic work in the form of installed professors who are able to train PhD students and create a research environment, conduct and oversee research programs in this field.

The German institution comparable to the Duke Laboratory, the Freiburg Institute for Border Areas of Psychology and Psychohygiene, founded by the late Hans Bender, once a chair at the department of psychology at Freiburg University, has lost its direct contact with the university and is operative as an independent research institution. However, classical parapsychology is only a minor part of its efforts. Major activities are historical in nature, or dedicated to clinical counseling of persons with anomalous experiences, or to the sociological understanding of paranormal claims and experiences. In the US there are a few academics that have redefined the scope of parapsychology somewhat and are working in related disciplines such as transpersonal psychology.

In the UK, the Koestler Chair of Parapsychology at the University of Edinburgh has not been filled again after the untimely death of the late Robert Morris. Although some activities are still ongoing, the work seems to be reduced and the previous chair has been converted into a more junior lecturer position. Other researchers in the UK who devote

some effort to anomalous claims seem to be more of the skeptical sort who wish to understand the question how it is possible that people fall for apparently unfounded claims, or what are the sociological and psychological frameworks that support such claims. The only sizeable research department that is also an acknowledged research center by its university is the one at the University of Northampton, Center for the Study of Anomalistic Psychological Processes (CSAPP), where actual research in the sense of classical parapsychology happens within an academic context, with the possibility to confer PhDs, develop one's career within an academic framework and get some institutional support for it. Perhaps most widely known and highly popular are the online parapsychological experiments by Rupert Sheldrake, who is a private scholar in the good old sense of the word, funding his research from small grants and out of his own pocket, but with no academic link (see for instance Sheldrake, 2001; Sheldrake & Smart, 2000, 2003).

A creative group of researchers is assembled around Prof. Dick Bierman in Holland as the only academic center on the continent, apart from the center of parapsychology in Utrecht. A new chair for parapsychology and hypnology has been recently established at the University of Lund in Sweden, held by Etzel Cardeña who is a hypnosis researcher in the first place. Apart from that, there are a sizeable number of single person scholars all over the globe, in private institutions or small home-based labs, sometimes doing incredibly fascinating and sound work.

A sober historical and sociological analysis has to conclude: Parapsychology was not able to establish itself as an accepted discipline over the last 125 years (Alcock, 2003). It has failed to grasp the attention of the mainstream. It has not stopped the main paradigm, that of analytical-materialist analysis of the universe in terms of matter-particles and their interactions. It has not had any large scale influence on the development of psychology as a discipline recently. On the contrary: researchers most interested in the phenomena frequently come from other disciplines, such as physics, engineering, or sociology. Psychologists themselves seem to be rather reluctant, or seem to be trying to incorporate parapsychology within the ruling paradigm, reducing such experiences either to mistakes in perceptions, hallucinations, or social psychological phenomena of suggestion, compliance, group pressure and distortions of experience (French, 2003; Wiseman & Morris, 1995; Wiseman & Schlitz, 1997).

In what follows, we wish to explore the reasons for this situation and especially what this situation means in historical and systematic terms for the discipline itself. Should it lay itself to rest and disassemble? Should it raise new efforts, in terms of money and manpower to continue? Have we just had too little time, too little money, and would we just need better experiments, more statistical power and more results? Just the same, but more and better, that is? Do we need a different approach altogether? Is there an inherent mistake in the approach of parapsychology (i.e. is, perhaps, the intention correct, but the method wrong)?

We wish to make plausible that without classical parapsychology noticing, the agenda has been taken over by others. In a subtle process of dialectical shift in history, what has been the perceived opponent by the founding fathers of the SPR, the materialist-experimental paradigm, has taken over parapsychology itself, or rather, parapsychology has fraternized with the erstwhile enemy, adopting its methods and presuppositions. Thereby it has both lost its thrust and its original aim. This has meanwhile been taken over by new emergent movements: by transpersonal psychology and by those researchers that are trying to understand the influence of spirituality within human lives. The agenda remains the same, but the universes of discourse and debates have shifted. Spirituality emerges as the new topic that was at the base of the founding fathers' intentions in the first place. Parapsychology now has two options: to realize this change of gears and adapt, or carry on with what seems to be quite a barren road.

A Common Framework for Parapsychological Effects: Generalized Non-locality

Seen from a very large distance and trying to oversee large patterns, rather than single events, those patterns of results which parapsychological research produces seem to have the following generic signature:

There are usually always very interesting initial results in any new experimental paradigm, let alone in field studies. If that had not been the case in the first place, it is hardly understandable, why some individuals have spent a whole career trying to pinpoint those initial effects. One might, from a classical psychological perspective, argue that such strong deviations from chance expectations are normal and can happen anywhere. It is only by the psychological make-up of those

researchers that they refused to believe their later results and invested time, effort and money into what was useless in the first place (Hergovich, 2005). Although intriguingly simple, this explanation does not really fit the pattern and the data. Some research projects, such as the PEAR lab, produced extremely exciting data, over a long period of time. What seemed to have been happening was that the data showed a striking pattern: 1) they produced too many deviations from statistical expectation to be seen only as chance variations; 2) those deviations were, however, not reproducible at will and in a stable way. It is this double pattern of seemingly contradictory results that harassed parapsychology nearly in all cases, where strict experimental research has been employed.[1] We suggest that this is the signature of a particular class of phenomena which we term generalized non-local phenomena (Lucadou, Römer & Walach, 2007). Before we describe what we mean by that more clearly, let us illustrate that particular pattern more clearly by two examples of long series of replications: micro psychokinesis (PK) experiments where human intention alone is supposed to influence a random quantum process; direct mental interaction with living systems (DMILS) experiments, where human intention is supposed to influence the physiology of another person under strictly controlled conditions.

Micro-PK and the PEAR lab series

Over nearly three decades the PEAR lab has produced many data that centered around one general idea: human intention should be able to influence random processes (Jahn & Dunne, 1987). Many experimental set-ups were tested but one was repeated many times: A random quantum process was used to produce a random series of events. A human operator was instructed to influence this process by intention only. Initially radioactive decay was used as a random source, and later on the random noise of a diode. This random noise was then sampled, converted into a binary code of zeros and ones. This was then

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[1] By experimental research we mean the following: Persons or conditions are varied at the will of the experimenter, usually employing a randomization procedure. There are control procedures, such as blinding or video surveillance, implemented such that classical psychological explanations for effects like cheating, subtle cuing, social desirability can be confidently excluded. This would allow, if successful, to conclude that the observed effect is due to an unknown agent, mediated non-locally through human intention or mind to mind relation only.

finally transformed into a kind of display that was understandable to human operators, such as light bar that had to be shifted up and down, or a sound sequence that had to be made higher or lower in pitch, or a picture pattern that had to be made complete, etc.

The PEAR lab data were very impressive using some single, highly gifted individuals (Dunne & Jahn, 2005; Jahn & Dunne, 1987). They also produced deviations over time with normal operators, when enough people were sampled. Since other groups also used this type of paradigm it lent itself to a meta-analysis quite early on, which was highly significant, although the reported effects were very small (Radin & Nelson, 1989, 2003).

A new phase of the research was opened, when a large, joint study was conducted where the PEAR lab set-up was also installed in two other, separate locations in Germany, Freiburg and Giessen, and a three-site replication study was conducted. All procedures were clearly defined in an experimental protocol. This was the largest of all studies. This final study came out flat at chance expectation (Jahn *et al.*, 2000). Interestingly, when analyzed differently the data show a wide range of statistical anomalies—change in variance, change in some parameters of non-linear behavior—that would not be compatible with pure, undisturbed random processes (Atmanspacher, Bösch, Boller, Nelson & Scheingraber, 1999; Nelson, Jahn, Dobyns, & Dunne, 2000; Pallikari, 2001). The problem, however, is that the effect did not show up in the *expected* parameter that had served well previously as an admittedly coarse grained, but robust parameter, namely mean shift. This effect can also be seen clearly in a meta-analysis of all micro-PK experiments, including the last series (Bösch, Steinkamp & Boller, 2006). If all experiments are averaged, the effect of all micro-PK experiments is not statistically different from zero. However, the data-set is not homogeneous, warranting sub-group analysis. If the final, large experiment, which contributes significantly to the inhomogeneity, is dropped, the effect is significant. The debate is still ongoing regarding the question of whether the metric of that meta-analysis was correct and other details were handled well (Ertel, 2007). Another analysis, using different weights and a different method of summarizing, arrives at a highly significant result (Timm, 2007). However, the precise outcome of the debate does not impact on our argument here. For even though the final summary statistics should be significant and micro-PK vindicated as potentially scientifically viable, the general pattern of the results is still the same: we have very strong

deviations from randomness, suggesting some effect, but the effect cannot be pinpointed down by the best replication up till now.

If the effect were due to a so called classical causal intervention—some known or unknown causal signal traveling between an active agent and the quantum source, influencing the device—one would have expected that, all other things being equal, a huge experiment would at least show the deviation with some precision, even if small. The fact that exactly this is not happening is part of the signature of such effects: something seems to be ongoing, but this something is very likely not causal and refuses to be captured by the traditional method of replication of experiments. Applying standard reasoning, one would conclude that there is nothing to investigate in the first place (Alcock, 2003). Using a broader, perhaps also more phenomenological perspective, one would be able to hypothesize that there is certainly something—why else those repeatedly strong deviations from chance expectations?—but that this something defies the net of the experimenter (Walach & Schmidt, 2005). Using Eddington's analogy one can now ask: are the meshes of the net too large and that is the reason why the effect escapes? This is the traditional parapsychological approach. It tries to react by making the meshes of the net smaller and smaller. We suggest that it is not a matter of the mesh size, but rather that the traditional experimental fishing procedure is not adequate (Lucadou, 2001; Lucadou, Römer & Walach, 2007). To continue the piscine imagery: it is like catching a quantum fish that is huge before it approaches the net, the net is cast, and when taken out the fish appears to have tunneled through the net and can be found splashing happily on the other side.

DMILS and the Influence of Human Intention on Human Physiology

William Braud and Marilyn Schlitz introduced a new research paradigm into parapsychology that was meant to experimentally model distant spiritual healing (Braud & Schlitz, 1983). Individuals were instructed to influence the autonomic arousal of another person, located at some distance (and in many experiments also in a room shielded from electromagnetic influence). This intentional influence was to be according to a random scheme of arousing and calming intentions, displayed on a computer screen by a program. The dependent variable often was electrodermal activity as a sensitive measure of autonomic arousal. The idea behind that experimental set-up is that a subtle mental influence might be measured more easily at an unconscious level. A

highly successful series of experiments were published, and a meta-analysis of this first series was significant and positive (Schlitz & Braud, 1997). At this point we decided to run a series of independent replications. We first carefully studied the method, finding out that some of the previous experiments did not use correct measurement methodology of the dependent variable (Schmidt & Walach, 2000). We then recalibrated our system, using up-to-date methodology and two electromagnetically shielded chambers that controlled for classical communication. We defined a pilot experiment—which we deliberately did not use to test for statistical significance but only for effect size calculation—and a series of follow-up experiments. Ironically, the pilot experiments showed quite a sizeable effect of $r = .35$ with 26 pairs, which, had we formally tested, would have been significant (Schmidt, Schneider, Binder, Bürkle & Walach, 2001). Follow-up experiments, using the very same set-up and similar participants, were unable to replicate this initial effect. A meta-analysis, which we then conducted using our own and all other experiments was only significant if we excluded our final two experiments; for this a case can be made because of lacking homogeneity (Schmidt, Schneider, Utts & Walach, 2004).

Again, we see the same pattern: a lot of intriguing results, even over a long time period that are difficult to nail down by a single experiment or by tight follow-up experiments, although initially replicable.

Surely, some would cite the highly successful studies by Dean Radin or Ed May as counterexamples, where parapsychological effects seem to have been replicated (Radin, 1997). While this is true from a superficial point of view, these studies do not consist of a series of strict replications as in the paradigms discussed above. Mostly, the studies by Radin are successful first shots, where a paradigm was either newly invented or adapted from existing ones, and each result was statistically significant, as a rule. But never was there a replication. On the contrary, the secret of those significant results seems to be precisely that they are not replicated (Dean Radin, personal communication, 17th July 2007).

A Common Framework: Generalized Entanglement

The overall pattern, then, seems to suggest that there are two seemingly contradictory findings in parapsychological research:

a) There is a sufficient number of positive deviations from randomness or chance expectation that seem to be incompatible with chance alone.
b) Attempts to finally prove these effects as effects of anomalous information transfer—either extracting information from a system or inserting it—in a regular, causal sense were unsuccessful as to now.

Finding a) is sufficient reason for researchers from the parapsychology community, the public and some science writers to believe that there are such effects; finding b) is normally mustered by critics of parapsychology to disavow the alleged effects. Our solution to the conundrum is different: we propose that these effects belong to an altogether different category of effects that have yet to be understood properly. But part of their signature is exactly the elusiveness under strict experimental control but the sufficient visibility such as to suggest some reality to them. We call them effects of generalized non-locality (Lucadou, Römer & Walach, 2007; Walach, 2005; Walach & Schmidt, 2005).

Locality, non-local effects, and entanglement

Traditional experimentation in parapsychology, following the experimental paradigm, assumes implicitly that there is something like a stable causal signal that can be isolated, reproduced and finally experimentally proven. In analogy to physics we call this the locality assumption. In physics this means there is an interaction between particles that can maximally happen at the speed of light and that obeys the general laws of energy exchange (Eberhard, 1978; Reichenbach, 1957). Also in analogy to physics we assume that there is another class of effects which we call non-local effects. By that term we mean that they are not mediated by a signal but, as in quantum physics, by the systemic make-up of a system. We won't go into details here, but briefly, in a quantum system a strange situation can be derived from the formalism of quantum mechanics. If a system is not yet measured, the system is described by one holistic description. If the system is measured, this measurement forces corresponding elements of the system into a particular state, i.e. the system behaves in a correlated fashion, and this collapse of the system into a certain state is instantaneous, without exchange of signals or energy. This strange correlatedness of elements of one quantum system has been termed

entanglement by Schrödinger (1935), and is also called EPR-correlatedness after Einstein, Podolsky and Rosen who have tried to use it as an argument against the plausibility of quantum mechanics (Einstein, Podolsky & Rosen, 1935). However, after empirical testing it has been clear for quite a while now that entanglement in quantum systems is a real phenomenon, and quantum mechanics is once more vindicated by experiment (Gröblacher *et al.*, 2007; Zeilinger, 1999).

We have suggested an algebraic formalism which is a generalized version of quantum mechanics, and hence applicable to other systems as well (Atmanspacher, Römer & Walach, 2002). Its basic feature is that restrictions and definitions typical for quantum mechanics are dropped, but one central element is kept: non-commuting operations are still defined. Thereby, this generalized version of quantum mechanics, Weak Quantum Theory (WQT), predicts entanglement in the system described, similar to quantum mechanics proper. This entanglement can be expected to occur *if descriptions of the system that pertain to the whole system are complementary to descriptions of parts of the system. In that case the individual elements within the system that are described by variables complementary to the variable describing the whole system are non-locally correlated.*

Complementarity

Obviously this effect of non-local correlatedness within a system hinges on the complementarity between global and local variables. In quantum mechanics complementarity is well defined and formally expressed by non-commuting operators. Weak quantum theory uses the same formal expression of non-commuting operators. However, the translation from formalism into reality is more difficult. We have little understanding of what complementarity means in everyday language (Bedau & Oppenheim, 1961; beim Graben & Atmanspacher, 2006; Bohr, 1937; Walach & Römer, 2000). It does not refer to simple opposites, where one can be expressed by the other, such as in 'hot' and 'cold'. It rather refers to maximally incompatible descriptions that are necessary to describe one reality (Meyer-Abich, 1965). The famous example is the double nature of light photons, which can equally be described experimentally as waves. In that case we have a rather clear information about their momentum, but practically no information about their location. Or light can be described as photon particles, in which case we can precisely locate them, but lose all information about their momentum.

It is not easy to translate this situation into our everyday language and into systems that are familiar to us. One way of doing so is using the well-known but perhaps complementary notion of connectedness and separation, or individuality and community. Both are necessary to describe a human being, but neither is sufficient on its own; nor can they be reduced to each other. Individuality is more than not being in communion and not being connected. And connectedness or community does not mean to lose one's individuality. Here we have perhaps natural language descriptions that come closest to complementary pairs of descriptions as we know it from quantum mechanics.

If this is true, then we would expect some non-local correlatedness, following the formalism of WQT, in systems that can be described in terms of these complementary variables, such as individuality and community. In that case WQT predicts non-local correlations between individual elements of the system. There might be other conditions governing the set-up of such non-local correlations in larger systems which we do not know at present. Gernert (2005) has proposed that there need to be something actively joining the elements, such as a common history, or a joint future, and von Lucadou (1995) has emphasized the importance of systemic boundaries.

If these assumptions bear out then we would expect non-local correlations to be quite ubiquitous. So called parapsychological effects might be instances where they can be seen more clearly, and we assume that most, if not all the effects parapsychology investigates are actually due to such generalized non-local effects or generalized entanglement.

The impossibility of information transfer

In that case the empirical pattern found is not surprising, but actually exactly what would be expected. For all effects based on entanglement correlations have one thing in common: they cannot directly be used to transmit signals. If this is done they break down. For the case of quantum mechanics this is quite clear and easy to prove (for a formal proof see the Appendix in Lucadou, Römer & Walach (2007). For the case of WQT this is less straightforward, but can be assumed as a theorem. If this is so, then we would expect those correlations to break down if they are used in such a way. Direct experimentation actually is such an attempt at direct signaling. One could imagine that the results of an experiment, where an experimental condition is tested against a control, would result in a positive, significant difference. In a

second experiment, such information could be used in a replication experiment to predict the group assignment, or to code a signal. Every new result below the mean or median of the previous result would be coded a zero, every result above would be coded a one. This situation, in quantum theory proper, would lead to a breakdown of the correlation, and we assume that this is also the case in the generalized version. Hence the actual pattern that is generated by effects due to generalized entanglement would be exactly the one observed in parapsychology: strong initial effects that cannot be replicated, but turn into strong negative ones, or decay and resurface again with further replication, with a clear lack of stability.[2]

We have therefore argued that such effects are likely due to such generalized non-locality and hence that such effects will defy direct experimentation so that no experimental series, no matter how long, how expensive and how well done will succeed in isolating these effects in the long run. This will not preclude single, strong—even spectacular—effects. Contrary to quantum mechanics proper, WQT does not define the commutation relationship. In quantum mechanics this role is taken over by Planck's constant, and thereby this commutator also defines the upper boundary such entanglement correlations can have in quantum mechanics proper. In WQT this commutator is as yet undefined, and it is conceivable that such correlations might be also quite strong in single cases. This could explain really massive events such as strong 'spuk' or poltergeist phenomena. But replications within experimental paradigms are bound to be unsuccessful in the long run, and this is exactly the situation which we seem to have in experimental parapsychology.

As yet, we are not sure whether there is a remedy. We have suggested to pursue meta-experiments. These would be experiments that obey the boundary conditions of such effects. It might be necessary, for instance, to not compare effects directly against controls, but to use a design where effects of an intervention due to generalized

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[2] One might assume that this is not true for quantum mechanics, as quantum encryption and quantum information are both possible and already tested in prototype applications. But observe that the usage of quantum correlations for the purpose of information transmission, encryption, etc. always necessitates a classical channel of information that carries the information about the meaning of the measurements in the first place. Also in quantum mechanics proper entanglement correlations cannot be used directly for information transfer, only indirectly.

non-locality—a human intention effect on a random number generator, for instance, or a healing intention effect on a cell system—is not compared to a control directly. Instead it could be correlated to an orthogonal variable, such as the potential states of mood of the experimenter, etc. What would be expected would be more significant correlations than would be expected by chance, but not a stable pattern of correlations. One should always keep in mind that in the quantum case the reality of entanglement correlations is not proven directly by comparing results of an experiment against a control condition, but by testing the outcome of a long series of measurements against theoretical expectations derived from some combinatorial argument following Bell's inequalities (Bell, 1987). For the generalized version no such argument has been formulated as yet, and hence there is no way of testing an outcome against a theoretical expectation.

A notable empirical example, where the effect has remained stable and is quite robust, precisely because there is no direct experimentation it seems are water dowsing studies that have been conducted over many years (Betz, 1995a, 1995b). In those studies, done in collaboration with government aid agencies, professional dowsers who dowse for water for government agencies in arid countries have given their predictions before drilling, and the predictions have been compared with the actual outcome. The results were correct in 96% of the cases observed. For various reasons it would be implausible to assume that such effects are due to some radiation effects, for the dowsers in question were not only able to document the precise location of drill holes, but normally also gave indications of the amount of water and the quality of the water to be found. In many cases these effects were also found at a distance, at improbable angles or following a map only. They were also possible from a helicopter, with no contact with the earth, or from within a moving vehicle. Taking those effects for granted for a moment, the only viable explanation would be, again, that these are effects of some generalized form of entanglement. Observing the boundary conditions, they seem to be robust and replicable precisely because they can only be seen and documented against a reasonable expectation, namely that of not finding water if one drills somewhere by chance in an arid area, or if drilling in an area where geological surveyors were unsuccessful beforehand.

Thus, it is direct experimentation, the direct control of experimental results with a control condition that violates the boundary condition of effects based on generalized entanglement. For this direct

experimentation would, at least potentially and in principle, allow for the coding of a signal, or, to use another analogy, to mistake the effect as effects of a stable cause independent of the full system itself.

If this is true, and the empirical situation suggests that this is so, then no amount of direct experimentation will succeed in isolating the effects, because it will be precisely the attempt at isolating them that will destroy them. Reverting back to our larger historical perspective we can now see the backlash of the subtle dialectics: by using the methodology of natural science parapsychology has actually destroyed the scientific viability of the very effects it wanted to vindicate. We can now also see why: because these effects are very likely altogether of a different nature in the first place. By mistaking these effects as the same in kind, only perhaps more subtle and more fickle than conventional causal-local effects, parapsychology has not done them justice. Not only has it lost its track in terms of the original emphasis and purpose of its founders, but by adopting the method of direct experimentation it has nearly destroyed its own subject matter.

What is the solution? We propose that it is time to refocus. If the effects are seen as effects of generalized entanglement then something funny happens: spirituality creeps back in to the agenda of parapsychology.

Generalized Non-locality is at the Core of Spirituality

Spirituality is the experiential realization of a transcendent reality (Ferrer, 2002; Fontana, 2003; Hill & Pargament, 2003; Kohls & Walach, 2006; Miller & Thoresen, 2003; Walach, 2007; Walach & Reich, 2005). This is variably called meaning or purpose, sometimes it is called a relationship with a transcendent goal or reality reaching beyond the ego. At the moment there is no common and accepted definition. Suffice it to say here that spirituality seems to have two core aspects, at least: it refers to a relationship with a reality that reaches beyond the ego—theistic traditions call this God, other traditions reserve other names, such as absolute reality or being, or Nature for it; the second aspect is about its experiential manifestation, i.e. a holistic type of knowing that includes cognition, affect and motivation.

The relationship of spirituality to religion can be, briefly, defined as that of content to form. Where religion provides a form of teachings, doctrines, rules for behavior and rituals for expressing experience and making it happen, spirituality refers to the actual experience. Each

religion has its own spiritual tradition, sometimes called mystical tradition, and each spiritual tradition can normally be referred back to a religious form or will eventually create one (Forman, 1998). Each of these traditions has a particular type of practice to offer to allow their followers to move on and grow in their experience, while the religious form supplies the containment of these experiences in the form of images, metaphors, parables, myths and rituals.

Central to all these traditions seems to be the experiential realization of some kind of unity with a larger reality. In Christian mysticism this is called *unio mystica*, and the assumption is that here a part of the soul is united with God. In Buddhist traditions the experiential realization is about oneness with all being and about the emptiness of forms, which is at the same time fullness. In other traditions the unity is with nature, with ancestors, with the group, with other deities. Often this unity is enacted in prayer and meditation, the latter being more of a receptive kind of practice and prayer being more active. In prayer the individual seeks to bring into reality wishes, either for oneself or for others. In meditation the individual seeks to be united or filled with the transcendent reality.

Each spiritual tradition also knows of parapsychological experiences that are likely to occur on the spiritual path (Eliade, 1975). That is to say that as one is progressing to the goal of unification one will have experiences of telepathic oneness with others, anomalous information transfer or even psychokinetic experiences. This is not surprising, but also not the goal. In the Christian tradition they are known as signs of holiness in a lot of the traditional trials of sanctification (Thurston, 1952), but they are not regarded as essential or desirable. For instance, miraculous healing through laying on of hands or simply through wishing seems to have been a signature of Christian saints from the time of the historical Jesus onwards (Douglas-Klotz, 1999), who presented himself as a healer to the public, before any preaching and teaching.

If we now assume that generalized entanglement is a rather general and ubiquitous process as suggested by the model of WQT, then we have a rather natural understanding at hand. Such effects as healing, as telepathic connections or psychokinetic influences would be the natural result of entanglement correlations within a natural system. The systems in question would always include the conscious activity of a person that strives to be aligned with a higher, transcendent reality. This alignment of an individual with a whole would, by necessity and

as per definition of WQT, lead to a non-local correlatedness between the individual and the whole. For 'part' and 'whole' are similarly complementary notions as 'individual' and 'connectedness' (Stillfried & Walach, 2006). We can therefore assume that an alignment of the 'part', in that case a particular individual's consciousness, with a 'whole', in that case some absolute, transcendent reality, would generate or reinforce such a non-local correlation also with other 'parts' of the 'whole', or with other individuals. The Chinese tradition has a specific term for that: 'Chi'[3]. Hence it is not surprising that in this tradition the practice of such alignment is supposed to go hand in hand with the increase of 'supernatural' or 'special' powers assumed to be due to a special force or energy. Within the framework suggested here there would not be a special unknown power at operation, but only a better non-local correlatedness between and within elements of a system.

One could speculate that the ultimate oneness experienced by some mystics or in an enlightenment experience is in fact an experiential alignment of one individual consciousness with the totality and the experiential realization of this oneness, in fact an instance of super-correlatedness in our terminology. But apart from such speculation it would make sense, in the terminology adopted here, to assume that spiritual practices such as meditation or regular contemplation and prayer actually operate via the enhancement of some generalized correlatedness between an individual and the whole and thereby between the individual and other individuals.

If this is at all plausible, then we have a brief factual description in terminology derived from the sciences of what spirituality means and achieves: it means an increase in non-local connectedness between an individual and the totality, and by the very same token also between the individual and its own subsystems and other individuals. So called anomalous events are nothing but a natural side effect of this increased non-local correlatedness. Hence, generalized entanglement would be a paradigm bridging the gap between spirituality and science, and parapsychology would be one instance within a larger picture.

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[3] Although 'Chi' is often referred to as 'energy', it can be reconceptualized along the lines indicated here and would then signal an alignment of the individual with the whole, which seems to be closer to the original meaning anyway.

Conclusion and Summary: Spirituality and Research into Spirituality is the Legitimate Heir of Parapsychology

We are now back where we started our journey; Spirituality in the meaning proposed here is the legitimate heir of what the founding fathers of parapsychology had in mind. They sought proof that the materialist description of the universe that was on the horizon of the natural sciences of those days was not all that is, and that consciousness and mind have some real influence. Parapsychology set out to find a proof for this statement. It came up with a very rich but also very inconsistent database. This database makes sense when viewed in the light of generalized non-locality as results of generalized entanglement correlations between elements of a system. If this interpretation is correct then no amount of direct experimentation would be able to provide the proof, as direct experiments destroy the effects of generalized entanglement correlations. To prove them scientifically, if this is possible at all, will only be possible using indirect methods that have yet to be derived.

Entanglement correlations in a generalized sense would also lend themselves naturally to understand what spiritual traditions try to achieve, namely alignment of an individual consciousness with a greater reality or totality. Hence parapsychology and spirituality have something in common: the processes and mechanisms that are at the base of what they study or are trying to achieve. Generalized entanglement would be the overarching category of description for both of them. Please note that this description is strictly secular; it does not need to make any claims about the nature of this total reality, about the description or about the interpretation. All it does is make a plausible assumption that there is something that can be called the totality, and the rather trivial observation that everything that is not this totality is by definition a part of this whole. By virtue of this very structure, entanglement correlations between the part and other parts are to be expected. Assuming that the conscious attempt plays a role here in aligning individual goals with the whole, one can then describe spirituality and its goals in quite natural terms. In that sense, spirituality in the description offered here would be the natural heir of parapsychological efforts of the past. Classical parapsychology, it seems, is dead. But risen is a phoenix: spirituality that encompasses the

same mechanisms and processes that are at the base of parapsychological effects. By admitting this situation parapsychology would only win: it would link its effort with a larger and growing community of researchers that are interested in spirituality and its effects. It would potentially find a way out of the experimental conundrum of lack of replicability. And it would adopt a potentially powerful theoretical tool, the formalism of WQT that allows for the description and understanding of generalized entanglement correlations.

With all the praise some caveats should not be forgotten: the formalism of WQT is still quite undefined in some areas. It only defines necessary but not sufficient conditions. That is, we can explain, after the fact, that non-local correlations were to be expected. This is sufficient for a general explanation, as it allows for a rational reconstruction of many phenomena. However, it is not sufficiently detailed to describe precisely when such effects are to be expected and precisely when they break down. Also, it is certainly possible to think about other formalisms and other ways of describing non-locality, not tied down to the algebraic framework used by quantum mechanics. However, we would submit that this is a natural and rational thing to do, as it allows us to link with efforts on the side of the natural sciences and hence to muster the force and the unity of science in our efforts to understand effects like those found in parapsychology. Also, the definition of complementary variables is all but precise for the time being. It seems to be rather fluid what is to be considered a part and the whole, or what is an individual and when this is in connectedness with a larger whole or community. All the terms have as yet only a rather intuitive meaning, but are not precisely defined. But it is at least rational to conceive a future state where such definitions are clearer, and hence also easier to understand under which conditions some individual will be expected to be in non-local correlations with other individuals within a certain system. As for now we will have to be satisfied with this rather loose description. Even so, this attempt at modeling can at least make plausible two things: parapsychological events can be reconstructed rationally as effects of non-local correlatedness to be expected in systems under certain conditions which are not clear at this point; spirituality is a concept encompassing parapsychological effects and can be construed as operating on the same basic principles of generalized non-locality. If this is so then Generalized Entanglement might be a powerful global concept that

affords us with what the founding fathers of parapsychology sought in vain: a scientific notion that allows for a non-reductive understanding of spirituality, parapsychology and human nature, while at the same time being scientific and connected with the main stream of science.

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DISCUSSION

WACKERMANN: There is little time but I would like to make one factual correction and one short comment. The correction; you referred to our replication studies as unpublished but it was published in the Proceedings of the PA convention in 2004. I agree with Etzel, this is bad practice to give things just in the Proceedings but because most of those present have access to the Proceedings they can take a look at the paper. The results were published and I would not describe them as positive. Already at this time we wrote clearly that we have no clear pattern and what we have are so-called effects with significant z-values

in conditions where we did not expect them. We wrote we have a very puzzling pattern and we have no idea what to do with it. The study of 2005 was unpublished, we know this was because of a local problem that led to a kind of 'disentanglement' in my department, the data were never analyzed in any detail so there was nothing to publish. This is the factual correction; you have misrepresented facts. Now my comment; what I can see is an attempt to utilize non-reproducibility of results in a positive way, to set a stage for experimental metaphysics. I strongly disagree with this misrepresentation of the present state of experimental parapsychology. I agree there are a lot of things we should think anew, and that with certain parameters perhaps we are at the end of their useful life, but I disagree with your characterization.

WALACH: Thank you.

LOBACH: I thought it was interesting and I am sympathetic to your thoughts and approach but I am not sure I accept the premise that experimental parapsychology is dead. There was a reference yesterday to a study by Honorton in which he compared replication rates for mainstream studies and found there was a problem with them as well. That was one of my plans, to look into replication rates for mainstream psychology effects, which I think is pretty low as well. So before we pronounce experimental parapsychology dead we must be sure that it is more dead than replication in areas of mainstream psychology.

WALACH: Well I guess that is a matter of temperament. I thought 60 years would be sufficient.

DISTANT HEALING INTENTIONALITY AND BRAIN FUNCTION IN RECIPIENTS, USING FMRI ANALYSIS

JEANNE ACHTERBERG

Introduction

Traditional healing systems in all parts of the world include beliefs about the effects of intention on heath. Intentionality itself has been called the 'crown jewel of healing' (Fahrion, 1997), and has been considered essential for healing by many authorities, including Benor (1996) and May (1969). This shared belief serves as the foundation for healing in major established systems of healthcare such as Traditional Chinese Medicine, Anthroposophic Medicine, Tibetan Medicine, and Native American practices.

The terms distant intentionality (DI) and distant healing intentionality (DHI) describe practices based on a postulated non-material connection between individuals. Distant healing intentionality includes many practices such as prayer, energy healing, healing at a distance, transpersonal imagery, Therapeutic Touch, Reiki, and Qi Gong.

Intentionality, itself, has been defined as a focused attention, or "mental projection of awareness, with purpose and efficacy, toward some object or outcome, such as promoting change in one's self or another" (Braud & Schlitz, 1991). Intentionality can be considered a nonlocal phenomenon, with quantum qualities. These could include events of acausality, nonlinearity, being unbounded by time or space, unmitigated and immediate. Writers such as Lynn McTaggart (2007), Wayne Dwyer (2007), and Carlos Castaneda (1993) have captured the imagination of their extensive readership and suggest that intention may be an actual universal force, like gravity, and when accessed be able to change the nature of the physical world.

The possibility that human intention can act at a distance to promote healing is largely dismissed by Western biomedicine partially because there is not a scientifically tenable mechanism of action for the putative effects, and partially because the studies are not trusted. The research

designs are criticized with a passion, even though medical treatments have historically been adopted with inadequate or no research at all. Scientists often make up their own minds about intentionality—or any unusual finding—without actually reading the research. The foundational research for this article was, in fact, rejected for publication by the most prestigious scientific journal because it was "entirely too novel"!

To date, efficacy studies of distant healing practices have yielded equivocal findings and many studies have been challenged on the basis of design flaws and claims that are not supported by their findings. Attempts to objectively evaluate DHI practices are complicated by the fact that healing intention is often directed at psychological or spiritual problems, thus empirical measures using medical outcomes do not adequately describe beneficial changes. Jonas and Crawford (2003) identified at least 2,200 published reports on various DHI practices, and commented on the high rate of poorly-designed studies. Nevertheless, they concluded that these provisional findings merited further investigation of DHI. Austin, Harkness and Ernst (2000) conducted a systematic review of distant healing, and discovered that 57% of 23 randomized, controlled trials showed a positive treatment effect. There were methodological limitations of several studies, making them difficult to interpret; however, the evidence merited further study.

The shared premise of most DHI or DI practices is that various mental activities have the potential to link or connect the consciousness of two or more people. Healers who practice a particular form of DHI are generally trained to serve as the intercessor or mediator for a larger, cosmic or divine source of life energy. This source is called by many names; i.e., *mana* in Hawaii, *Chi* or *Qi* in China. Healers that we interviewed for our research by and large contended that all healing is essentially self-healing and that they were merely a vehicle through which healing is allowed to take place.

The physiological evidence for intercorrelated activity between brains has been demonstrated in several studies. Over five decades ago researchers described what they termed 'extrasensory induction' in monozygotic twins who were sensory-isolated from each other (Duane & Behrendt, 1965). In two of 15 pairs of twins, changes in EEG alpha rhythms evoked in one twin correlated with EEG changes in the other sensory isolated twin. In subsequent studies, visual evoked potentials (VEPs) or visual cortical activity were recorded in one member of the pair, the so-called 'sender', by having the individual view a flashing

light or a visual pattern at random intervals. Simultaneous VEPs were observed in 10 to 15% of the sensory-isolated members of the pair.

A spate of small EEG studies was initiated by the work of Grinberg-Zylberbaum, Delaflor, Atti, and Goswami (1987), which showed that VEPs in one member of a pair of individuals took place at above chance rates in the nonstimulated subject who was isolated, at a distance, and in an electromagnetically shielded room. Grinberg-Zylberbaum and his colleagues also showed that when pairs were supposedly empathically bonded by meditating together, the intercorrelated brain activity was enhanced. An additional single case study employing a similar paradigm, but using fMRI, reported correlated visual signals (Standish, Johnson, Kozak & Richards, 2003).

Conceptually, prayer should be placed under the rubric of DHI, given the widespread belief that petitioning divine intervention or channeling a vital (or life) force results in healing for the person being prayed for. Targ (2002) reports that more than 80% of Americans believe that their thoughts or prayers can cause healing, and this view is shared by 75% of family practice doctors. However, two recent, large, randomized controlled studies failed to show any effect of prayer on physical health of cardiac patients. Krukoff and his colleagues (2005) studied 748 patients having percutaneous coronary intervention or elective catherization from nine medical centers. They were randomly assigned to one of three groups: intercessory prayer; a combination of music, imagery and touch therapy (MIT); and a control group. The end point was a composite of major adverse cardiovascular events, combined with readmission and 6-month death. No effect was seen on the composite primary end-point, although the MIT group experienced significantly less mortality within six months. (This latter point, interestingly, was ignored by the press coverage of the study, and not emphasized in the original report.) Twelve groups from Christian, Muslim, Jewish, and Buddhist faiths participated, receiving the name, age, and illness of patients who had been randomly assigned to them. They prayed according to their usual practices for a period that ranged from 5-30 days.

A similar study design was conducted by Benson and colleagues (2006) at Harvard—'the Study of Therapeutic Effects of Intercessory Prayer: A Multicenter randomized trial of uncertainty and certainty of receiving intercessory prayer'. A sample of 1,802 persons who had received coronary artery bypass graft surgery were divided into three groups: (1) a group was told they might or might not receive prayer and

did; (2) a second group was told that they might or might not be prayed for and were not; and (3) a third group was told they would be prayed for and were. The prayers were Protestant and Catholic groups who received the first name and last initial of the person for whom they were to pray, based upon randomized assignment. Prayer continued for a 2-week period. Intercessory prayer itself had no effect on recovery. Certainty of being prayed for was associated with an increased incidence of complications.

All research of this or any nature should honor both context and cultural validity, i.e., be conducted in circumstances that conceptually replicate the natural environment of the phenomena under study. In this light, Larry Dossey (2006, personal communication) makes several points about these large prayer studies: (1) nowhere in the world is prayer carried out in this fashion, i.e., randomized, double-blind controlled trials; (2) people generally pray for loved ones whom they know and care for, but these studies were designed to eliminate a relationship between the prayers and those who were prayed for; (3) prayer is normally carried out in context and with ceremony; and the studies did not meet these criteria; (4) In the Benson *et al.*, study, patients were essentially taunted and teased about whether they would be prayed for or not.

The studies of correlated brain activity cited earlier also were not conducted under the same conditions that normally exist when and where such experiences occur. The events which appear to involve correlated brain activity are subjectively and anecdotally recorded as premonitions, synchronicities, dreams, intuitions, and shared physical symptoms by people who are isolated and at a distance, and are normally associated with significant life events. The laboratory settings created an artificial environment, using checkerboard or other patterns to stimulate brain activity in one member of a sensory isolated pair. The fact that correlations were demonstrated even in a significant minority of trials is noteworthy, therefore.

Research Questions

In my opinion, for a test of nonlocal consciousness or DHI, a logical choice of participants would be individuals such as healers who had extensive training and experience in some form of distant healing. My colleagues and I initially studied healers paired with individuals with whom they felt empathically linked or bonded. We designed this

because of previous information that a close connection might result in more of a commingling of consciousness than in people who were unknown to one another. In the first study, reported in detail elsewhere (Achterberg *et al.*, 2005), fMRI technology demonstrated that DHI was correlated with a significant activation of brain functions in the recipients of the healing intention. The group significance was p = .000127; primary brain areas involved were the anterior cingulate cortex, middle cingulate cortex, frontal cortex and the precuneus. The level of probability encouraged further investigation of the phenomenon. The extent that the bonding or close personal connection may have played in such highly significant results was a primary issue for the research that will be described in this paper. The research reported in Achterberg *et al.* (2005) will be referred to as 'Group One: Bonded DHI pairs'; and the second comparative research will be called 'Group Two: Non-Bonded DHI pairs'. The research question addressed in this first aspect of the work was:

Group One: Bonded DHI pairs

"Is there evidence for correlations between DHI and brain function in recipients of DHI who are tested using fMRI?"

In the next phase of the work, which will be described in this paper, the protocol was taken one step further by studying healers paired with people whom they did not know, and had not established an empathic bond. The research questions addressed in this aspect of the research are:

Group Two: Non-Bonded DHI pairs

"Is there evidence for correlations between DHI and brain function in recipients of DHI who are unknown (non-bonded) to the healers, as measured by fMRI?"

"Are there significant differences in correlated brain function between empathically bonded versus non-bonded healers and patients?"

A limitation of addressing the second question is that only three healers in the first group were willing to participate in a second study. Therefore, the research questions address the issue of empathic bonding and DHI and not the performance of the healers under the two conditions. The reluctance to do additional sessions was generally due to the fear expressed by the healers that being so close to the source of electromagnetic energy (the MRI scanner) might be dangerous and

impair their healing abilities. I do not, personally, believe this could be true, not only because the control booth was shielded, but also because there is no evidence that DHI is related to electromagnetism. However, it is of vast importance to honor the belief systems of research participants, and I chose not to try to persuade them to participate in subsequent research.

Research Considerations and Language

Care is taken in describing my work on DHI to avoid the term 'sender' in reference to the healer whenever possible. 'Sender' implies that DHI involves a transfer of a signal, energy, or information. The MRI control booth is shielded both optically and acoustically from the person in the scanner, and sending sensory clues is highly unlikely, if not impossible. The scanner and the control booth are shielded from outside electromagnetic energy, the usual form of signal transfer described in the popular literature on DHI and by many healers, themselves.

Functional magnetic resonance imaging (fMRI) was selected as the most appropriate technology to explore research questions pertaining to DHI. Functional brain imagining technology has the capability to map brain hemodynamics that correspond to mental activities, and provides high-resolution data correlating brain function and anatomy.

Methods

Participants

Group 1: Bonded DHI Pairs. Twenty-two participants (11 pairs of healers and recipients of DHI) were recruited on the Big Island of Hawaii. Three men and eight women with an age range of 46-71 participated as healers. The recipients of the healing ranged in age from 44-61, and included three men and eight women. On average, healers had been practicing their healing traditions for 23 years. The healers represented a variety of practices, including Healing Touch (distance healing and laying of hands, conducted primarily by nurses trained in the methods); a traditional Hawaiian healing form called *pule* that consists of prayer, chant, and song by a spiritual elder or Kahuna; Peruvian shamanic healing; Reiki (a form of energy healing that may have ancient origins and was purportedly rediscovered in the 19th

century in Japan); vibration or sound healing, and three eclectic forms of DHI that did not fit into a single established tradition. Three of the pairs represented a Chinese method of healing called Qigong.

Group 2: Non-bonded DHI Pairs. Twenty-six participants (13 pairs of healers and DHI recipients) were also recruited on the Big Island of Hawaii. One man and twelve women with an age range of 50-61 participated as healers. The recipients of the healing ranged in age from 49-63, and included four men and nine women. The healers represented a variety of practices including Reiki, Christian prayer, non-Christian prayer, General Healing Intention, Healing Touch, and psychic healing. Two of the healers participated in both Groups One and Two.

The large number of women versus men is to be expected, given the interest and activities of healing that are gender related on the island and elsewhere.

Inclusion criteria for healers

1. The claimed ability to have the skills to communicate or heal in some distant or nonlocal form
2. Acknowledgement within their communities for their healing abilities
3. Fulfilling cultural requirements for training, apprenticeship, and practice
4. A stated belief in their ability to turn on and off their healing intentions within a time frame of approximately 2 to 4 minutes, which was built into the fMRI program

Additional inclusion criteria for healers:

Group 1: Bonded DHI Pairs. Ability to name an individual with whom they have a special, close or bonded connection, who understands the goals of the experiment, and is willing to have an fMRI scan.

Group 2: Non-Bonded DHI Pairs. The healer agrees to be paired with a recipient they do not know

The inclusion criteria for the receivers of DHI for Groups 1 and 2

1. Meeting the standard requirements for receiving an MRI (no implanted devices or metal objects such as pacemakers, joint pins), and no history of claustrophobia
2. Willingness to undergo an fMRI scan of 34 minutes' duration and a post scan interview

Standard Procedures for the Research

The Institutional Review Board, University of Hawaii, John Burns School of Medicine, and Manoa, Hawaii approved the study protocol. It was conducted in the Department of Radiology, North Hawaii Community Hospital in Waimea, Hawaii, from August 2003 through July 2005. Analysis of the technical information was performed by Todd Richards, Ph.D, who is affiliated with the University of Washington. Consultants affiliated with UCLA; University of California, Irvine; and University of Pittsburgh periodically reviewed the research protocol and results.

Pre fMRI scan. Both members of each pair in both groups signed an informed consent form and filled out a demographic questionnaire. A semi-structured interview was conducted with each healer within 4 days prior to the scan. The interview was designed to elicit information on their DHI practices. The healers were given information about their role, and the 'On' (DHI) and 'Off' (No DHI) procedures were described. Instructions were standardized and included information that the healers should try to connect with the receiver during the On condition in ways that are prescribed by their own DHI practice. In describing their practices, the language most often used involved notions of energy, prayer, good intentions, or thinking of the individual in the scanner and wishing for them the highest good. During the Off conditions, they were instructed to take their attention away from the person in the scanner, and they frequently chatted casually with the principal investigator or research nurse.

The instruction given to the recipients of DHI was to relax as well as they could in the scanner environment. They were provided with a call button and shown how to use it should they became distressed, had questions, or wanted to stop the procedure. No one in either group used it to make contact during the study trials. They were given general information about the nature of the study, both in the informed consent form that was signed, and again prior to going into the scanner.

The recipients in Group 1 knew about the type of DHI that they would receive because they had worked with the healer previously. The participants in Group 2 knew only that the study would involve some form of distant healing. None of the recipients were provided with any information about the timing of the On/Off conditions. In other words, they knew they would be receiving DHI of some kind, but not when. The healers were not informed about the timing of the On/Off signals

before the trials, so they could not have coached their receivers before the scan.

Timing and Experimental Conditions. The healer, principal investigator and/or research nurse, and radiology technician were in the electromagnetically shielded control room, and were physically, optically and audibly isolated from the receiver in the scanner. During the course of the experiment, the healer was verbally instructed by one of the researchers with cues to start and stop the DHI. The random pattern of the twelve 2-minute intervals was determined prior to the onset of the study using a coin toss. A single randomized sequence that had an equal number of On and Off sessions was used for each session. The pattern was Off, On, On, Off, On, Off, Off, On, On, Off, On, Off for a total of six 2-minute on periods and six 2-minute Off periods. In three instances, the length of the interval was 4 minutes because two of the On or Off conditions occurred back to back. This pattern remained the same for each healer. The reason that a unique pattern was not used for each pair is because in small scale trials such as this one—i.e., with only twelve 2-minute segments—the majority of random sequences could be quite unequal in terms of numbers of On and Off sessions. In other words, it is likely that sequences for pairs might include virtually all On or all Off trials. Since each recipient is serving as his or her own control, it is necessary to have a reasonably equal number of On (Experimental) and Off (Control) conditions for effective statistical comparison.

The total time that individuals were in the scanner was 34 minutes, which included a 10-minute structural baseline of sagittal and transverse images. During the time in the scanner, no physical or sensory contact was made with the recipient by any member of the research time.

Post scan. The scan was followed by an open-ended interview with both the healer and the recipient in order to document their subjective experiences during the trial. After the end of the trial, participants were paid $100 for their efforts.

Analysis[1]

The fMRI localizes brain activity as well as brain structures, and it is the former that differentiates it from MRI. A strong magnetic field (approximately 30,000 times that of the earth's magnetic field) is passed through the participant's head. The scanner is connected to a computer that rapidly produces images, with the most common being blood oxygen level dependent (or BOLD) images. This is the 'raw data' that other analyses are based upon. The BOLD measures blood flow that accompanies neural activity. The fMRI technique does not require ingestion or injection of radioactive or other substances, as do the PET or SPECT scans, and there is exceptionally high resolution; hence our choice of technology. The fMRI has the capacity to image 3-dimensional volumes of the brain. Software designed to analyze the BOLD images varies from center to center. We used FSL—a common program for the industry. Images were statistically analyzed, using the single voxel as a unit of measurement. Granted that this may be an oversimplification, but the fMRI is basically a subtractive procedure—i.e., activation functions and structures are compared with nonactivation functions and structures. The participant, therefore, serves as his/her own control. Statistical programs allowed for intra-individual comparison as well as group averages and comparison between groups.

Structural and functional magnetic resonance imaging was performed on a 1.5 Tesla MR imaging system (Siemens Symphony Magnetom, Software Numaris/4, version Syngo MR 2003BDHHS). The facility where the scanner is located is shielded from all known electromagnetic field signals. Blood oxygen level dependent (BOLD) functional MRI scans were acquired using a T2-weighted gradient echo version of the echo-planar imaging (EPI) pulse sequence to identify regional brain activation. Heavily susceptibility-weighted sequences were used to maximize the BOLD response. Additional parameters of the fMRI data acquisition include TR = 4.4,TE = 65 milliseconds; slice thickness 6mm, skip 1 mm; 64 x 64 acquisition matrix; 21 slices positioned to cover the whole brain; and 324 brain volumes to cover the

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[1] fMRI Acquisition and Analysis Methods: Detailed Technical Overview (conducted and written by Todd Richards, Ph.D., Neuroradiologist, University of Washington). This is a highly technical report, and readers may prefer to skip all but the first paragraph that describes fMRI.

24-minute acquisition period. Four hundred and eight brain volumes were collected per subject.

Functional MRI scans were analyzed using the FSL software program (Functional Magnetic Resonance Imaging of the Brain, Software Library, Oxford Center) which offers robust corrections for false positives, autocorrelation, multiple voxel testing comparison, and cluster size detection. Analysis was carried out using FEAT FMRI Expert Analysis Tool, Version 5.1, part of FSL. The following pre-statistics processing was applied: motion correction using MCFLIRT; spatial smoothing using a Gaussian kernel of FWHM 5mm; high pass temporal filtering (Gaussian-weighted LSF straight line fitting, with sigma = 50.0s). Time-series statistical analysis was carried out using FILM (FMRIB's improved Linear Model) with local autocorrelation correction. Z (Gaussianised T/F) statistic images were thresholded using clusters determined by Z greater than a cluster significance threshold of $p = 0.01$. Registration to anatomical images was carried out using FLIRT.

General Linear Model (GLM) regression was applied to generate statistical p-value maps based on the contrast between the On versus the Off variables. The expected response to changes in the healer/recipient protocol may be equated to the expected response to stimulation paradigms currently used in brain research. In these research paradigms, the responses follow a hemodynamic delay curve. The GLM regression can determine the extent to which the observed receiver's responses may be predicted by this model. A goodness of fit statistic (r squared) indicates the degree of fit between the hemodynamic model and the actual brain activity during the time course recorded. Both positive and negative beta coefficients can result from this analysis.

The final step was to create the group maps from the individual fMRI analyses and co-register the group z-score map to the MRIcro atlas (see ch2bet.hdr and aal.hdr from software package http://www.psychology.nottingham.ac.uk/staff/cr1/mricro.html) for the location and function of significant areas of activation. Then, software developed by one of the authors, TR, was used to quantify the average z-score and pixel activation counts within each of the 116 different brain regions in the MRIcro atlas.

Data were analyzed for both the intra-individual comparisons for the On/Off conditions (experimental versus control) and for the group effect as a whole during these procedures. Ten subjects from Group 1

and twelve subjects from Group 2 were used in a direct fMRI statistical parametric map comparison. Note that one participant from each group was omitted from the group analysis because of slight variations in the data collection procedures. For the intra-individual comparisons, all participants were analyzed, i.e., eleven in Group One and 13 in Group 2. First of all, each individual subject's fMRI data were analyzed using FSL's FEAT described above (FMRI Expert Analysis Tool) to compute the contrast of parameter estimates (COPEs) and the variance of the COPEs (VARCOPEs). The COPEs and VARCOPEs come from the regression analysis (General Linear Model) of the statistical comparison between the On condition and the Off condition during the fMRI scan. The On condition for the contrast was the time when the healer was instructed to focus on DHI, and the Off condition was when the healer was instructed to stop the DHI. These resultant individual COPEs and VARCOPEs were merged into two large files for the fMRI group analysis.

Group analysis was carried out using FEAT (FMRI Expert Analysis Tool) Version 5.1, part of FSL (FMRIB's Software Library, www.fmrib.ox.ac.uk/fsl). Higher-level analysis was carried out using OLS (ordinary least squares simple mixed effects). Z (Gaussianised T/F) statistic images were thresholded using clusters determined by $Z > 2.3$ and a (corrected) cluster significance threshold of $p = 0$.

Results

The FSL software produces a quantitative table of cluster results which includes: cluster size, probability for each cluster, z scores, x y z coordinates of the cluster in Talaraich space and contrast of parameter estimates (See Table 1). If a cluster is significant in a group-difference analysis it means that there were specific brain regions where the combined subjects in the two groups had enough activation-difference to raise the z-score above the noise level threshold. In other words, if all of the subjects had random activation at different places in the brain, then there would be no group-difference activation. The software performs a statistical operation similar to calculating a t-test (comparing the mean fMRI activation of two groups using the General Linear Model) at each brain voxel but then corrects for multiple comparisons by using the extent of the cluster size (based on both the number of adjacent voxels and the t score within that voxel). In other words, for each brain voxel, there is comparison between the 10

individual fMRI activation values from Group 1 and the 12 individual fMRI activation values from Group 2 (one of the subjects was omitted from each group analysis due to slight irregularities in the collection process, as noted earlier).

TABLE 1.
Cluster Table for the Group Comparison Group 1 > Group 2[2]

Cluster Index	Voxels	*p*	Max Z	COG x (mm)	COG y (mm)	COG z (mm)
2	1055	0.00592	3.9	10.6	-59.1	20.4
1	875	0.019	3.78	14.6	-69.2	-34.4

Analysis of Group 1 showed that 9 of the 11 individual scans had significant activation in several areas of the brain during the On condition. The areas of significance were variable in the individual analyses. In analyzing the group as a whole, one of the clusters was highly statistically significant ($p = 0.000127$), Activation in the anterior cingulate cortex, frontal superior areas, and the precuneus was identified for the group. (See Figure 1) Anatomic locations are based on the Tzourio-Mazoyer atlas. This atlas was produced as a segmentation of the MNI atlas, and is available in MRIcro as ANALYZE files.

Analysis of the 13 individuals independently in Group 2 revealed that 8 recipients of DHI showed significant differences between the On and Off conditions, and 5 did not. The analysis of the effect of Group 2 as a whole did not show significant differences.

Using FSL's statistical group analysis, there was a significant fMRI activation difference ($p < .01$, corrected) between Group 1 and Group 2, with Group 1 having significantly more activation in certain areas of the brain during the On condition. The group difference maps are shown in Figures 1 and 2; the cross hairs are centered on the significant cluster that occurred when Group 1 was compared with Group 2. The first figure shows significant activation in a cluster centered in the posterior/cingulate/precuneus region, ($p = .019$) and the second figure

[2] Column 1 = Cluster index which uniquely identifies each cluster; Column 2 = # of activated voxels within the cluster; Column 3 = probability that the cluster occurred randomly by chance; Column 4 = Maximum Z-score within the cluster; Columns 5,6,7 = X, Y, Z Talaraich coordinates of the center of gravity of the cluster

shows significantly more activity in the cerebellum (p = 006); both values were corrected for multiple comparisons using extent of cluster size. Table 2 represents a cluster table for the group comparison.

TABLE 2.
Brain Region Activation Report. These are the brain regions where there was a significant difference between the two groups (Group1 > Group 2)

Brain region	number of activated voxels within region
Rolandic_Oper_R	23
Cingulum_Post_R	30
Calcarine_R	202
Cuneus_R	559
Lingual_R	1186
Occipital_Sup_R	170
Occipital_Mid_R	17
Fusiform_R	25
Precuneus_L	1681
Precuneus_R	2065
Heschl_R	27
Temporal_Sup_R	31
Cerebelum_Crus1_R	222
Cerebelum_Crus2_R	95
Cerebelum_4_5_R	54
Cerebelum_6_R	492
Cerebelum_8_R	358
Cerebelum_9_L	16
Cerebelum_9_R	14
Vermis_7	54
Vermis_8	111
Vermis_9	26

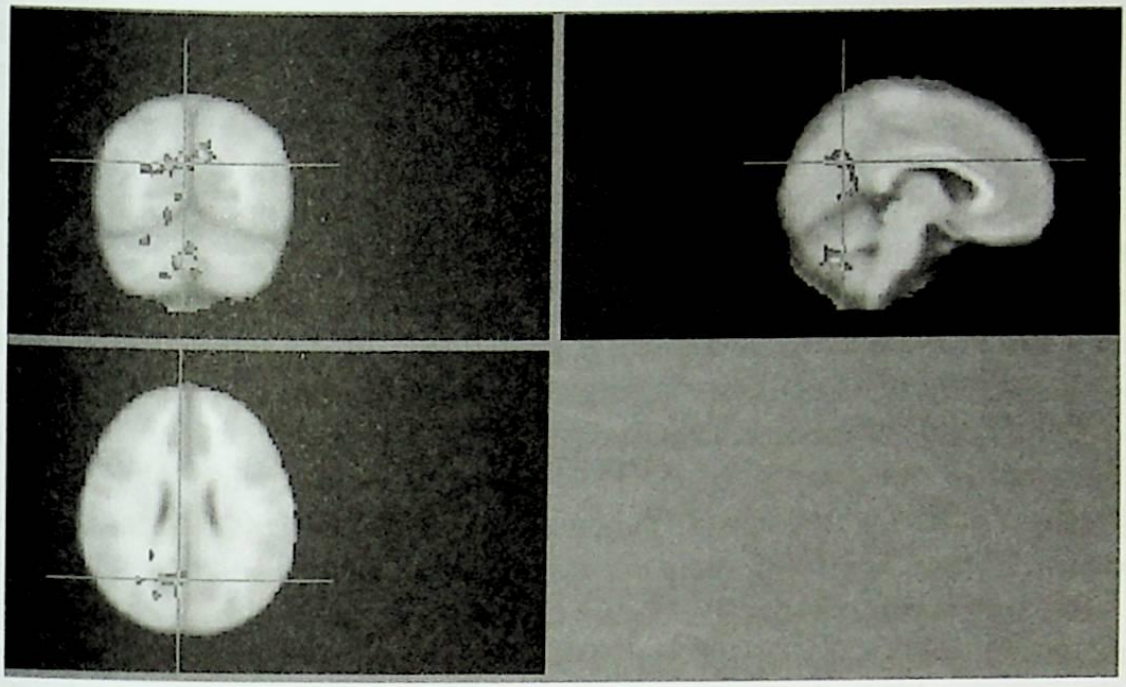

FIGURE 1.
fMRI group statistical map for the comparison test group 1 > group 2. The green cross hairs are centered on a significant cluster in the posterior cingulate/precuneus region.

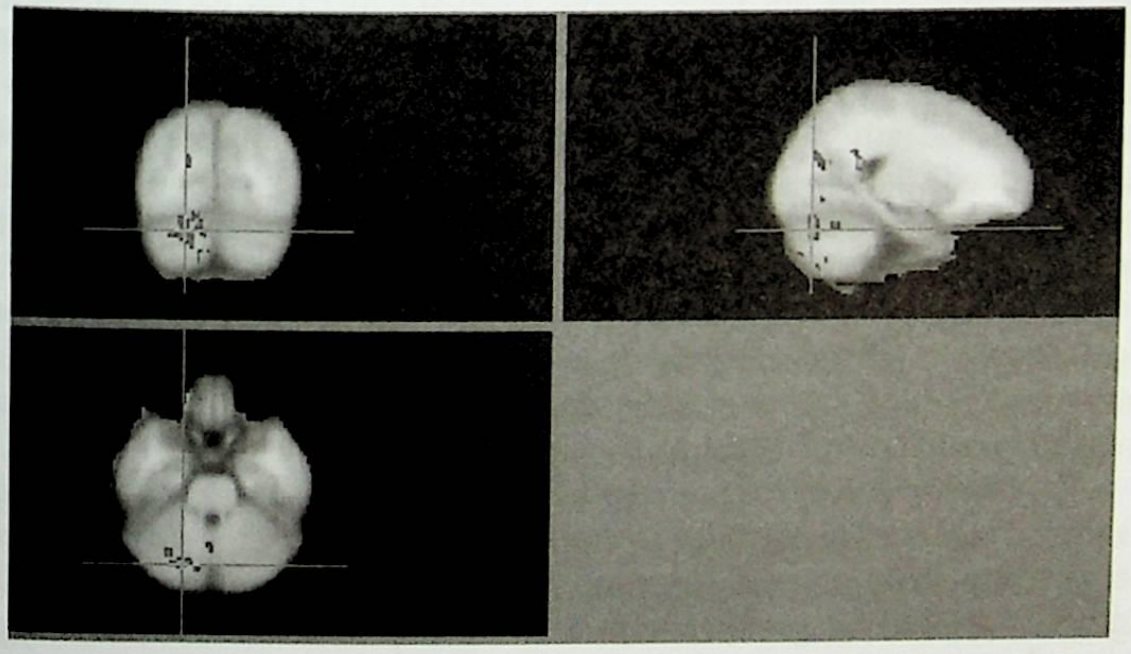

FIGURE 2.
fMRI group statistical map for the comparison test group 1 > group 2. The green cross hairs are centered in a significant cluster in the cerebellum.

Discussion and Conclusions

In this study, two groups of healers who practiced some form of Distant Healing Intentionality (DHI) were compared using fMRI changes in the recipients of DHI. The healers in Group 1 were paired with a person with whom they felt bonded or had a close personal or empathic connection. In Group 2, the healers were paired with a person they did not know. The research questions guiding the design of the study and the results in answering each question are addressed below:

Question: "Is there evidence for correlations between DHI and brain function in recipients of DHI who are tested using fMRI?" The question will be answered, below, with (A) intra-individual, and (B) overall group, or group average, correlations.

A. In the analysis of intra-individual scans, individual pairs in both groups showed significant activations during the DHI or On segments. Nine of the 11 pairs in Group 1 were statistically significant; 8 of the 13 pairs in Group 2 showed significant activation in brain areas during the On condition of DHI, as compared to the Off condition of DHI. The nonsignificant results in Group 1 were both from recipients of Qi Gong. There was significant movement artifact during the onset of the On sessions that could not be removed from the analysis of the scans and they had to be invalidated and omitted from the analysis. Ironically, the Qi or life energy associated with work by Qi Gong Masters is said to cause a motion or startle reflex. These participants may have experienced a most unusual degree of nonlocal stimulation, but had to be excluded from the significant research findings because the technology required by the fMRI could not correct the artifact.

In Group 2, two of the five non-significant pairs involved the same healer who said the DHI modality was Psychic Healing, two healers practiced Reiki, and the fifth used Healing Touch. The latter was a participant in Group 1 and had a highly significant result.

B. When pairs in each group were combined and analyzed, Group 1 showed significant group or overall activation during the DHI segments but Group 2 did not; i.e., a group effect was apparent for Group 1, but not Group 2. The group analysis for Group 2 did not demonstrate a significant treatment effect, as mentioned above, indicating that on the whole, DHI was not significantly correlated with changes in brain function. Although 8 of the individual scans for Group 2 were statistically significant when the On condition was compared with the

Off condition, the individual effect was not robust enough to achieve significance for the group.

Question: "Are there significant differences in correlated brain function during On conditions and DHI between bonded or empathically linked pairs, versus non-linked pairs?"

When the two groups were compared to one another, they were found to be functionally and statistically significantly different in outcome, with Group 1 showing greater activity in several brain areas during the On or DHI. This would indicate that a close or bonded connection would engender a more pronounced mind-to-mind connection.

Issues of Causality and Function

Although there were significant areas activated for Group 1 as a whole, and 9 of the 11 scans were significant, the individual scans showed high variation in the brain areas activated during DHI. The question that arises from these findings is whether the areas that are activated are a function of the DHI modality, the healer, the recipient, or the nature of the relationship between members of the pair. In pilot work such as this, it is impossible to address this factor with any certainty. To do so would require larger numbers of participant pairs, with a cross comparison of DHI modalities.

From the existing data, Reiki, the most frequently used form of DHI in Groups 1 and 2, was related to extremely diverse individual brain scans. Two healers—one a practitioner of Healing Touch, and the other a practitioner of an eclectic form of DHI—were in both Group 1 and Group 2. Their individual results in terms of brain areas activated during DHI were different in the two groups. These very tentative findings suggest that the brain areas activated are more likely to be an idiosyncratic function of the relationship between the healers and their paired recipients, as opposed to being correlated to the DHI modality or the skills employed by the healers.

Overall, then, the healer/recipient pairs who felt a bonded, close or empathically linked relationship with one another were more likely to have significant findings when the individuals were analyzed, as well as in the analysis of group averages, and when the two groups are compared.

Speculation on Functions of Brain Areas Activated

For Group 1 as a whole, the brain areas included the rostral anterior cingulate cortex, which has been shown to be activated during the height of opioid and placebo response. (Petrovik, Kalso, Petterson, *et al.*, 2002). The frontal lobes were also activated in this group and are generally regarded as modulating information processing, judgment, and decision-making. Little is known about the function of the precuneus; however, it has been recently argued that it, along with the anterior cingulate gyrus, may be a part of a neural network that is involved in resting consciousness and self-reflection (Kjaer, Nowak, & Lou, 2002).

The anterior cingulate cortex, active during the On condition for Group 1 also, may serve as a type of executive control. Decision making at this level determines both verbal and motor responses. It has rich interconnections with many cortical and subcortical brain areas. Mirror neurons in the anterior cingulate cortex have also been identified. Iacoboni and his research group (2005) Ramachandran (2000), Rizzolati & Craigher (2004) and other neuroscientists believe that the mirror neurons may provide a unifying framework to help explain mental abilities that have thus far eluded explanation, including what appears to be precognitive recognition of intention of others (and therefore a forthcoming goal) and other phenomena associated with parapsychological function; and, as these researchers note, reading other peoples' minds. "The psycho-constant in all remote healing studies that work is empathic bonding and compassion. Perhaps, as scientists get comfortable with mirror neurons, they may finally open up to a form of bonding that occurs even when people are outside sensory contact—mirroring without the mirror" (Dossey, 2006, Personal Communication). The importance of human inter-connectedness or relationship between members of healer/recipient pairs was suggested in the introduction to this study. If there is a demonstration of mirror neuron activity in humans for the emotions of love and compassion, this would be a highly significant contribution to the field.

When Group 1 was compared to Group 2, the cerebellum was another area of activation where there were group differences. The cerebellum, once believed to be exclusively associated with motor behaviors, now is known to have many expanded functions. It is densely packed with neurons—more than all other areas of the brain combined—and approximately 40 million nerve fibers connect it to the

cerebral cortex. The cerebellum reacts with more speed than any other part of the brain, and possibly mediates language. It may, in fact, coordinate mental activities and be involved in decision resolution when conflicting information is being presented.

Both the anterior cingulate cortex and the cerebellum have been shown to be related to empathy, which may account for the strong action in those areas for Group 1 in which the pairs felt a close or bonded relationship with one another. Singer, Seymour, O'Doherty, Kaube, Dolan, & Frith (2004) investigated brain activity in a scenario involving couples and pain-related empathy, using fMRI. The empathy pathways are therefore highly similar to what we found in the bonded healer/ recipient group.

The anterior cingulate cortical activations as well as those in the cerebellum coincident with the On sessions of DHI in the bonded Group 1, offer a preliminary demonstration of a possible biological mechanism involved in nonlocal connections.

Limitations and Deliminations of the DHI findings

The objections to this work from within the field of consciousness and fMRI have been interesting and occasionally irrational. Most of the direct criticism has come from individuals who do not have experience with fMRI, and do not understand the technology or the analysis software.

Because this study is novel in both design and outcome, the limitations and delimitations of this current work need to be seriously considered. Criticisms and caveats are given below, which generally involve internal validity—whether the healer was causing the effect or was it due to extraneous variables. For each point, I have countered or supported the critique, as necessary.

(1) One of the criticisms of the reported findings of Group 1 was that someone must have been signaling the person in the scanner/MRI unit regarding the sequence of the On and Off events. The findings therefore must have occurred as a result of auditory or visual cues, or tricks sent from someone in the control booth.

My comment: Signaling a randomized clue to a person who is in the MRI scanner and isolated visually, auditorally, and from every other sensory function that we know of is impossible.

(2) Another criticism concerned the possibility of flawed data analysis software, or software that could manipulate data at intervals.

My comment: The FSL software that was used to analyze the data is regarded as standard for the fMRI technology and as providing robust corrections for false positives, autocorrelation, multiple voxel testing comparison, and cluster size detection. Given the random series of On/Off trials, no software package could be deliberately manipulated to reflect the data at 2-minute or any discrete and random intervals.

(3) A challenge to internal validity is to question who or what was actually causing the significant effect. There were four people in the control booth for Group 1: the healer, the radiology technician, the principle investigator, and the nurse research associate. The principal investigator was not in the control booth for every trial in Group 2.

My comment: The possibility of an 'experimenter effect' must be considered. If someone other than the healer was creating the positive effect, then the results are still of interest and importance in terms of shared or correlated consciousness or nonlocal effects of DHI. This possible violation of internal validity does not negate the positive findings. It merely challenges who was creating the effect.

(4) Studies of DHI have been summarily dismissed by scientists, journal editors, reviewers, mainstream health care professionals who state there is no known biological mechanism to account for the results and therefore cannot exist.

My comment: This objection is a weak argument in that it is used to discount novel research findings that challenge the existing scientific models of the nature of reality. These models are not static. They change drastically over time and across cultures.

Entanglement Theory, posed by quantum physicists (Einstein, Podolsky & Rosen, 1935), provides a possible description or explanation for the connectedness results. This theory asserts that once photons came into contact, their subsequent activities are inexplicably connected in nonlocal fashion. Explaining the results of nonlocal human connections with quantum physics has been a fad in the popular literature for many years. Explanations at the quantum level were either 'thought' experiments or related to microsystems, but there was no evidence that complex systems, such as brains, behaved in this manner. However, Pizzi and colleagues (2004) found evidence for nonlocal connections between separated preparations of human neurons, suggesting that the theory of Entanglement may be relevant to macrosystems as well. In our study which found that people who knew and cared for one another had stronger findings than did those who did

not may provide justification for the application of Entanglement theory to nonlocal healing.

Recommendations for Future Research

The results of this study pose many questions for additional research. For instance, one frequently asked question is whether similar strong findings as were noted in Group 1 might also be found in pairs of nonhealers who sent prayers or thoughts to someone that they loved who was receiving a scan. We have completed a pilot study of this nature, and preliminary analysis shows positive findings.

Another direction for study is to investigate a single type of DHI. We cast out a broad net in this pilot research, not wishing to restrict the research reported in this paper to one form of DHI. Further research might consider a test of prayer, Reiki, Healing Touch or Therapeutic Touch. Qigong, while one of the most frequently studied forms of DHI, proved problematic in the fMRI research, as mentioned earlier, due to movement artifact. Master practitioners of Qigong are often credited with being able to cause muscle movements in recipients—even when the Qi is sent nonlocally. Therefore, other instrumentation that measures movement—such as the ballistocardiograph—is recommended.

Whether there is concordance of actual brain activity between the pair is also a question for future research. This would require the capability of measuring brain function in both members of the pair at the same time. Based on previously cited research, EEG technology might be a more appropriate and readily available form of instrumentation than the fMRI. One of the serendipitous findings of our research was that, with a single exception, the healers were not willing to have a scan. Some would not even go into the room where the scanner was located. The single healer who was willing to undergo a scan (the Qigong Master) was instructed to send Qi in a random sequence related to her by the research associate through headphones. She sent Qi to the principal investigator (Achterberg), who was in the control booth. The healer's scans were remarkable in terms of the sizable number of activated brain areas during the On sessions (Figure 3). The red and yellow areas in this brain indicate significant activation, and the areas in the cross hairs are the most pronounced.

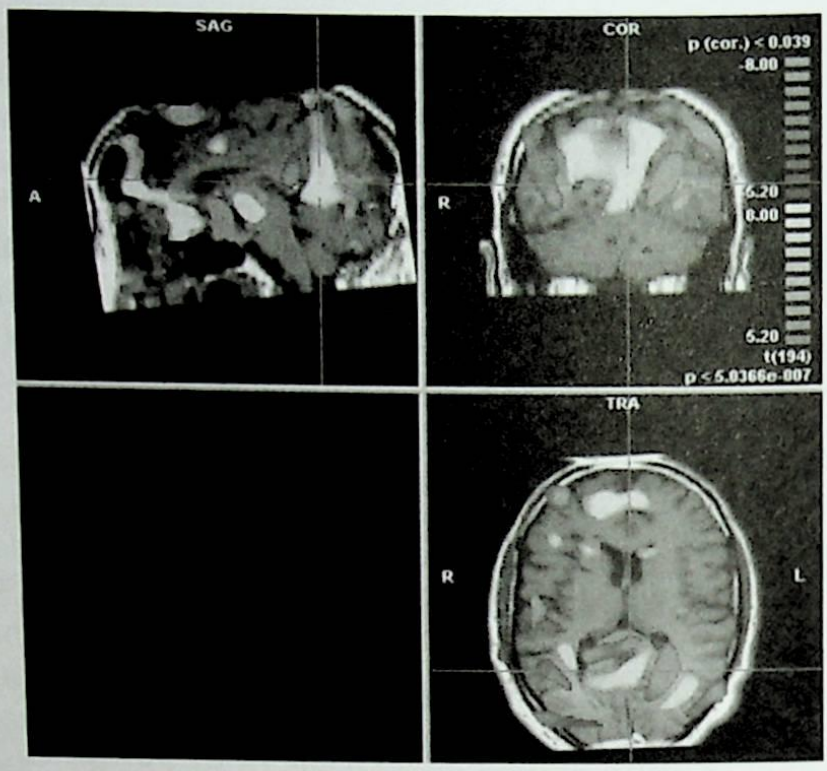

FIGURE 3.
fMRI results from a Qi Gong Master during the DHI/On condition, indicating widespread activation (yellow areas).

Another possibility is to study people who are already trained in self-healing in a standardized fashion, such as Metta. Metta is also known as 'loving-kindness meditation'. They could practice their own loving and compassion or healing intentionality on themselves while they are in the scanner to identify neural correlates and compare the findings to the research reported in this paper.

Currently, the results of these studies are being reanalyzed by Dean Radin and Todd Richards to identify whether there was an anomalous anticipatory response to the randomized outcomes. If so, this would indicate a precognitive ability to detect the On sessions prior to their onset, and validate other work that Radin has done in this area.

In summary, the work reported in this paper supports a body of previous research that suggests that human intentions may directly affect others in ways that are not fully understood. Care, compassion, and knowledge of the recipient appear relevant to have an effect of DHI on neural substrates. These findings challenge the existing paradigm of science and medicine which holds that nonlocal effects of correlated consciousness are impossible and therefore unworthy of investigation.

The implications of this research for training and clinical work in all forms of health care are monumental. If is true that intentions create a physical effect in others, even when the recipients are unaware of these intentions, then we have a certain responsibility to consciously monitor our thoughts, not only as health care providers, but as members of the human family. Whether care and compassion can be learned or taught effectively remains to be seen, but denying or ignoring the idea could rob health and healing of its most fundamental and vital ingredients.

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DISCUSSION

NARANJO: Thank you very much for your interesting presentation. I have just a couple of questions. The first one is based on my own experiences with this kind of research; when I was doing EEG studies I remember to my surprise the results of the experiments reflected mainly suppressions rather than activations. My first question is: do the colors in the illustrations you presented actually represent activations compared to the baseline or de-activation to the baseline?

ACHTERBERG: Activations. I didn't have time to go into detail, but one pair was analyzed for deactivation and those were blue areas. So there were some instances of deactivations, and they were significant deactivations.

NARANJO: My other question was relating to the activations you found that might be related to empathy. Are those activations that you found consistent over all subjects? Was it an average over all subjects?

ACHTERBERG: That was a group effect; it didn't happen in every single one, but happened frequently enough to be statistically significant.

ROLL: This is not really a question, but a comment and some sage advice concerning the area of replicability. So far the motivation in your own life and working with others has been healing; now there could be a sudden change — that your purpose may now include replicability, and that could dilute the healing intention.

ACHTERBERG: I'm not sure I could replicate it myself!

ROLL: Don't try and replicate it – just go on with your work!

KOKUBO: How many measurements are averaged in the data you presented?

ACHTERBERG: With the technology of MRI you are comparing 'on' sessions with 'off' sessions so that each patient serves as their own control. We were using 12, 2-minute segments.

VAN DE CASTLE: I would certainly go along with what you are saying about the importance of intentionality and love. In my talk I gave a brief example of the dream helper work, and that is how it always starts up there: we get together with the person, we dedicate ourselves to

them and let them know that unconditional love is going to be there for them, and we offer our dreams up for them that night. It is nothing you can get real statistics on, but knowing the kinds of material that comes up with dreams I can objectively evaluate it with the Van de Castle system, and it is unusual imagery that comes up. Usually the person the next morning is moved to tears because we made them such a central part of it, because that person is worthy, and deserves to have that love, and people think 'wow! You did that for me?' Now it's not healing in the sense of an organ change, although some do report they are less anxious or depressed, so we don't have that kind of measure but it seems clear that doing the work in a love context is so critical.

ACHTERBERG: I agree, it is a wonderful feeling, I had one of my training groups do dream work on me one night and they stayed up all night. I had forgotten the date, yet I felt itchy all night — it was an interesting sensation. I was very touched that they spent so much time using their dream work on me, and I was stunned by what they reported.

GENERAL DISCUSSION

ROE: Both presentations have drawn an analogy between the kind of connection that is hypothesized to exist between people and the kind of entanglement that has been observed with microscopic systems. But it's difficult for me as a non-physicist to see how this analogy is viable or even useful; how can we meaningfully say that the kinds of macroscopic systems that we're interested in represent entangled systems?

ACHTERBERG: Well, people who have looked at this research have looked at the initial documentation of these macro systems and describe them as acting in an entanglement fashion.

ROE: So, practically, how could you go about 'entangling' two people?

ACHTERBERG: Through consciousness.

ROE: So having them simply spend time together or engage in conversation would be sufficient?

ACHTERBERG: They did not have to engage in conversation in my research.

WALACH: Let me re-phrase the whole thing; I think it is a mistake to try and use physics to understand what is going on because it is not a physical situation—it is a general systemic situation. By virtue of us being human beings we are necessarily correlated to some extent and the stronger you make connections between certain people, through rituals and interactions the stronger that gets. Very simple.

DOBYNS: If I try to raise all of my questions about Dr Walach's presentation we would be here the rest of the night! Therefore I would like to just provide one counter-example to the no-signaling theorem, which I notice is inserted into weak quantum theory as an assumption rather than something that can be derived from first principles, the way it is in ordinary quantum theory. I expect everyone knows the story of Russell Targ's first application of associative remote viewing (or ARV) in the early 1980s. It was initially very successful, raising hundreds of thousands of dollars in the Silver Futures market, then after a while it stopped working. This is a pattern seen over and over again with many people who have tried to do this with ARV; they have initial successes that sometimes are quite impressive and then things peter out and get more difficult. This is not the way the quantum entanglement no-

signalling theory works. You do not get to send signals for a few dozen rounds until the paradox monster wakes up and says 'Ok, we're done!' —it just does not work at all. So this tail-off phenomenon is part of the non-replicability issue but it cannot be accounted for by the no-signalling principle and in fact it violates it.

VON LUCADOU: I tried to explain this, it depends on what you conceive as a signal so you have to specify this notion—there is a mass of different notions of 'signals'. To explain this, one has to go into details and to put it simply I would say we use the term 'signal' in the following ways: if you have done several experiments and if you take the information from these previous experiments to make a clear-cut prediction about the outcome of future experiments, then this theory says you cannot get the same degree of certainty as you would from classical signal enhancement theory. If you go into the detail it is very complicated because there are so many entangled systems all going on and being linked with signals, so one has to be very precise. Conversely, the advantage of this no signal-transmission axiom is that you can easily compare it with the thermodynamic principle that you cannot use energy a second time. So this is a very general principle and you can apply it in certain situations, but in this situation you have to define it very precisely. Then this principle allows you to decide very complicated systems and to make predictions. However, the remote viewing experiments are so complicated in their details, using different complex structures as targets and decoys and having different ways of describing and comparing them, and this gives so many different levels of signals present that it is impossible to make a clear prediction in this case. But if you take a very simple experiment such as a PK experiment, then the theory fits perfectly, making a prediction that fits 100% with the result of the meta-analysis of Bösch and Boller. It is utterly untrue that you cannot apply this model to real experimental situations, but first you have to define exactly which type of information you will describe as a signal. I would like to say if compare for example the remote viewing experiments to the early experiments which have been done by Sinclair and his wife, you can see very clearly the same structure that the structures fit perfectly, which shows there is entanglement, but if you go to the interpretation of these structures then there is no effect at all. I have many examples in the remote viewing case where I have the original pictures from Ed May and I have discussed the matter with him; you have no signal but you have correlations.

SHOUP: I really like these talks; you have both put your finger on the right ideas but from very different points of view. The metaphor of 'it's just a dance' is really perfect, and I shall talk more about this tomorrow in my talk. Harald, we agree on so many things and yet I have to part company with you when, as many do, we try and generalize the idea of entanglement and say that people or minds are entangled. It may appear that way, but I don't think we can play with quantum mechanics in quite that way. Unfortunately, that turns away many mainstream scientists who have a special meaning attached to entanglement. What we need to do is find a way to get from what we observe that appears like entanglement—or special relationships between human beings—to what is really going on in physics and make that connection. If we can do this in a plausible way we can make a connection with mainstream science.

WALACH: I would partially agree with you, if we can really make the connection with physics then that would be preferable to what we do here. It is very important to understand that what I have presented is not physics; it is a systems theoretical approach using theories and ideas that are available in physics and extrapolating them to a more general situation. It is just turning the whole thing around, and I understand the misgivings of people who have certain definite meanings attached to the term. I understand that perfectly and if I had a better one I would use it, but I don't. It is, of course, the attempt to use the structure and the formalism of a physical theory and apply it to general systems, and I don't think that is prohibited. You have the right to challenge the idea that we should eventually make connections with the physics of it all; that may be one step too difficult at this point, and therefore another suggestion is to use a generalized form of the formalism to see where that leads us, and it appears to give very similar predictions, which I find intriguing.

MULACZ: I think the importance of quantum mechanics and its application in this field is that we are actually forced to change our frame of thinking, that we are forced to do away with Aristotelian logic. We have learnt this from childhood, maybe it is innate, that there is *A* and *not-A* and nothing else. Apparently we have to amend this and learn that there are some situations in which there is *A* and *not-A* at the same time, and only *A* and *not-A* gives us a complete description. Actually, you already find this notion of complementarity in the

writings of Jung and others. The most important effect is that we are forced to change our way of thinking.

ACHTERBERG: To state the obvious, because I have been trying to defend my data for some time, I think we feel compelled to have a known mechanism for an effect. One of the reasons editors and reviewers give for rejecting this kind of research is that there is no known biological mechanism for the reported effects. That's the death; the article will not get published and will not be heard. Even though quantum mechanics is itself very controversial it does provide us with a small doorway into public awareness, and I feel we're justified in doing something with it even if it turns out to be not totally correct or to have been applied obliquely in a way that may or may not fit, since it provides us with a known mechanism to get the point across.

JOSEPHSON: The connection with physics was actually made in a paper by Pallikari and me in *Foundations of Physics* and Valentini in *Physics Letters*, which have both been ignored by the mainstream. We used Bohm's statistical interpretation of quantum mechanics to show that if you assumed a different kind of probability distribution then you could get action at a distance. What Valentini showed by a piece of mathematics was that there would be a tendency to go to equilibrium so he said it was like a quantum analogue of heat death. The usual situation is quantum heat death, where you have gone to an equilibrium distribution and quantum mechanics apply, but you might get non-equilibrium distributions before you reach heat death where you can get action at a distance. This fits well with what Walter was saying about how you can extract energy from a heat source to work with, but you are using up that resource and it disappears. In the same way, using a source that lets you communicate directly at a distance may be using it up and going towards equilibrium, which would explain why you could get an effect at the beginning but it would fade away. In the context of my talk this morning this would mean you discovered some source in your consciousness and you can only use it a certain amount of times before it would run out, heading to equilibrium and heat death.

VARVOGLIS: Harald, I would like to take issue with your 'parapsychology is dead' announcement, and like Mark Twain would say maybe that rumors of our death have been exaggerated. There are two ways of defining parapsychology; one is in terms of methodology, a process of study in a certain area; the other is in terms of its object of study. If the object of study is psi phenomena—or whatever attribution

you want to give it, such as nonlocal correlations between complex systems—then you cannot say it is dead, because you are continuing in that direction. There is a lot to be said for the idea that as long as such phenomena seem to keep appearing in one way or another then we will continue to study them. If we abandon all pretence to attempt a systematic study then parapsychology *is* dead, but as long as you introduce some kind of systematicity in your approach then I don't think you can say it is dead. What might indeed be dead is the attempt to squeeze parapsychological phenomena into a certain methodology, which was initiated in the late 19th century and came to its clearest definition with Rhine and his successors. We may indeed be seeing the end of the era of that approach to parapsychology. It would be interesting to tone down the 'parapsychology is dead' message because depending on who is hearing it, the message may be interpreted in very different ways.

WALACH: I am glad of the precision that you bring in your comment, because this is exactly what I want to say; I think I actually said 'classical parapsychology is dead'. And by that I meant exactly what you said: the approach of squeezing the phenomena of parapsychology into a framework of classical experimentation that is only meant to extract causal signals. Because that is what we have done *ad nauseam* and it didn't help, did it? As for your other comments, I could not agree more.

VAN DE CASTLE: I want to add to the comment that was made earlier about the work done at SRI using remote viewing to speculate on the Silver Futures market. One of the big people for that had been Keith ('Blue') Harary, and it is true when they started out they were able to do very well in predicting the Silvers market, but then it went down. You can look at that in two separate ways. One is in terms of one of the most robust findings we have in parapsychology, which is the decline effect. Most of the time we find effects that start out strong but then goes down, and frequently it is because of the dullness of the task; for example, guessing 'star, circle, star, circle' and so on is very boring and the scores rapidly decline unless you can add fresh novelty to it each time. The second explanation would be to look at it in terms of Harary getting very egotistical about it; they were using it for their own selfish needs and making big money, and some psychics would say that if it all becomes too commercial then they lose the gift. But there is an interesting way to get around that. Out at the Monroe Institute they

have a lot of people who have gone through at least a week's training, and they take groups of people out to Las Vegas to apply what they have learned in the Casinos. But each group has to agree before they start that any winnings will go to a favorite charity, so that they will not personally benefit except in the sense of the love connection, or agape, that it feels good to be able to do this for my fellow human beings. It's not yet been published but I believe that for the last 10 years or so they have always ended up making money for the charities.

ACHTERBERG: You have triggered some thinking for me with respect to the prayer studies that have been discussed at this conference, which have started so well but the last few have gotten abysmal negative results. I was wondering if that is not a similar diminishment of results, perhaps because more experimental controls have been added, which could do it, but maybe because the study of the phenomena has a natural lifespan.

VON LUCADOU: Thank you very much for the comment on the stock market experiment. It illustrates exactly what Harald was saying about global variables and local variables. In the context of this experiment, I would say that novelty is part of the pragmatic information; it is a global variable because it can only be considered from the whole system, whereas replicability—to get the same again—is a local variable. You say novelty is a psychological variable, a condition of what is going on, and therefore is always changing. The difference in this paradigm of treating this as a psychological variable is only that in the model of pragmatic information this would be considered as a system theoretical variable—it does not depend on the psychological state but it depends on the state of the whole system. There are many examples where you can show that these are different ways to describe things, but I believe the psychological description provides a kind of language to describe the systems theoretical structure behind it. In many experiments—not only in parapsychology—you find this pattern. For instance, in therapy research if you have a new method then it contains a lot of novelty so you get big effects, and after a while you get the decline. This of course does not mean that the therapy is worthless because if you wait for half a century another therapist will come along when we have forgotten about this therapy and people will think what they have is new, so you have gained novelty in the system. The model is not, as some have maintained, nihilistic; on the contrary it entails the whole dynamic of the system, where novelty and replication

play against each other and so this makes the dynamic much more interesting than the pure signal model, which says everything is stable. Nothing is stable in self-organising systems, and the model of pragmatic information takes this into account.

ROLL: A related point of view has been presented by Michael Persinger with respect to replicability, and that is if you lay out the same road again with no reason except to replicate then the brain has no interest in pursuing that, it just shuts down with respect to that activity.

ACHTERBERG: I like that, because you do get tired of the same activity.

VARVOGLIS: I have a question for both Harald and Walter. I am trying to think in terms of how there is this split between microscopic and macroscopic referentials that say you cannot apply the same rules, 'as below so above'. How you make the transition from micro to macro is not clear to me, but I am sure there are equations somewhere in physics that say—other than from thermodynamics—why all of a sudden there are no non-local phenomena occurring in macroscopic systems. Now in the weak quantum theory I understand that the claim is that there *are*. So we have these systems that spring out once in a while with these non-local type correlations but they can't last once you try and turn it into a structure, and these run alongside systems that seem to be operating in a normal structured mechanical and reliable way. So my question is: what is the distinction between these two systems? Why are there some situations where entanglement occurs but it is very fleeting and goes away whereas with other systems mechanically reliable causal events occur?

ACHTERBERG: It is a wonderful question and I am following the work of Pizzi in Milan very closely because they are reporting what look like entanglement effects in macro-systems made of human neurons, so I am watching the evolution of that research. To my knowledge that is one of the few demonstrations in what would be considered a macro-system, and it's exciting research.

WALACH: I think this is an essential question but I don't have an answer to it. I think it is complicated because we would have to understand how this entanglement situation actually condenses into something that is stable all the time. I am not even sure that for quantum physics that is quite clear and it is certainly harder to understand in the generalized situation but I am not a physicist, maybe Brian knows that.

JOSEPHSON: As it happens I had a student who got a PhD on the subject of the boundary between quantum and classical but it was all buried in mathematical models and it could not be explained without looking at the thesis, which he has had trouble in getting published. On the other hand there are things involving Bose condensates where people are getting entanglement with more and more macroscopic systems.

SHOUP: I just wanted to say that entanglement disappears when it is measured and so it may be that some of these effects are simply due to using up the entanglement that has been built up in the personal or experimental situation. The opposite case would be if we had some way of generating entanglements which naturally happen when things interact—it's kind of a romantic notion that once we've interacted sufficiently intimately we are never truly separated again. But that can be destroyed by measuring it, and so may have some relation to what kinds of experiment we do on those effects.

VON LUCADOU: The mistake we have at the moment is that we say causality and entanglement are things that fight against each other. In reality, they help each other. If you have self-organizing systems, internally it is entangled; if you open it, it becomes disentangled. In the system itself there are also causal processes, so nature has elements that need to work together, much like causality and entanglement. What we do in experiments is separate them to study the properties of entanglement. You have to do very complicated experiments to see this. Normally we look only to the causal elements. These are very simple as you have one variable and another which depends on it. So, as we have to make nature simple, in experiments we have made this separation, when nature itself does not.

THERE IS NO MIND-BODY PROBLEM IN PARAPSYCHOLOGY

HOYT EDGE

The topic of this paper is the mind-body problem and parapsychology. Let me begin by making an audacious and deliberately provocative statement: I do not believe that there is a mind-body problem in parapsychology. This issue, which has plagued philosophy and parapsychology for over a century, can now be laid to rest. We need to move on.

In denying the mind-body problem, I do not want to imply that there are no problems that need to be addressed. In fact, I must admit that there are serious philosophical issues that still need to be addressed, but the purpose of the dramatic statement above is to put one issue into the forefront: that we sometimes continue to talk about a problem in philosophy for centuries, assuming that the meaning of the concepts that have traditionally defined the problem have remained the same.

William James (1948), no stranger to parapsychology, suggested that we can eliminate some philosophical problems once we get a better handle on what the meaning of the words involved are. I am suggesting that we should take a pragmatic approach and become absolutely clear about what we mean by the concepts we use. I believe that there is no such thing as the virgin birth of concepts; those concepts that we use today have a history that is loaded down with assumptions, and we often continue to employ these concepts without understanding those assumptions. In suggesting that we need to take a fresh look at the traditional mind-body problem, I think we will see that the nature and context of the mind-body problem, particularly as it relates to parapsychology, has changed dramatically over the last decades. Thus, I want to suggest that we should stop talking about the mind-body problem. That will allow us to reconceptualize our task and focus on what the real problems are when we conceive of ourselves as persons and then see what parapsychology can offer in terms of empirical data and metaphysical grounding. Just as parapsychology previously was thought to help ground a dualistic perspective within the mind-body problem, we will see that the implications of parapsychology are just as important today.

Historical Background

It would not be entirely incorrect to say that modern parapsychology was founded as a discipline squarely within and because of the mind-body problem. This philosophical problem has formed the basis of both philosophical discussion and empirical research within parapsychology, especially until the middle of the 20th century. Carlos Alvarado (2003) has made a similar argument in relation to the concept of survival being important to the formation of parapsychology. Given the connection between survival and the mind-body problem, I believe that his paper supports this view. However, I want to discuss the rise specifically of dualism and parapsychology. You are already too familiar with the history of the rise of Cartesian dualism for me to take up the subject in detail, but a quick overview might be helpful.

Descartes and Dualism

As an important contributor to the scientific revolution of the 16th and 17th centuries, Descartes understood full well the dangers facing those who pursued the new mechanical science which he supported; this danger was brought to a head publicly when Galileo was condemned and put under house arrest by the Catholic Church in 1644. Descartes was also a good Catholic (although he was living in the Dutch protestant republic). In his *Meditations on First Philosophy*, published in 1641 (a short while after he taught a year at the University of Utrecht in 1635-6), he offered a solution to this existential problem for him—how to pursue his mechanical science without interference from the Church while also making room for the legitimate function of religion. His solution, of course, was dualism, in which he argued that mind and body were separate substances, each with its own characteristics, ones that were mirror images of each other. Schematically, his solution was:

MIND is:	***MATTER is:***
thinking	non-thinking
non-spatial	spatial
seat of values	a-valuable
free	determined
purposeful	mechanical
private	public
subject(ive)	object(ive)

The Church's legitimate area of concern was with mind, the substance that thinks, makes free decisions, especially about moral and religious issues; further, mind is eternal because it is radically simple (a mind contains no parts—we will return to this notion, based on atomism, shortly). On the other hand, Descartes argued, the Church should not be concerned with matter—a substance with the characteristics of little religious import since it is incapable of thinking, is completely mechanical and determined, and is not within the moral sphere. Further, since everything composed of matter is complex, all material things will eventually decompose and go out of existence and hence they are finite. This area, Descartes argued, should be the subject matter of science.

This solution provided the intellectual justification for the legitimacy, the separateness, and the autonomy of science. Notice also, however, that it defined the nature of science—it was formalized to pursue the study of non-thinking, determined and mechanical things. Science traditionally assumed materialism as a defining principle; its nature is to assume that the world is totally material more or less in the way Descartes defined matter. Further, mind and matter are defined in terms of each other; they are tethered together. They compose a worldview, each part conceptually dependent on the other. They are mutually dependent terms so that one cannot be decoupled without affecting the other. The important point here is that the mind-body problem in its origin assumed a particular view of mind and of matter.

However, the Cartesian view of mind broke with tradition so radically that the philosopher Richard Rorty (1979) has argued that the mind was invented in the 17th century. MacDonald (2003) also alludes to the uniqueness of Descartes' view of mind[1] when he says, "Descartes... was the first philosopher to abandon all of the conceptual baggage associated with the word 'soul' in favor of a radically new term, 'mind' and its principal attributes" (p. 2). And although Descartes' solution to the practical dilemma facing religious scientists provided an intellectual justification for their work, the Cartesian

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[1] MacDonald admits that Descartes was not consistent in the use of the terms 'soul' and 'mind', but argues that Descartes' mature view made the explicit distinction. Wallace Matson (1966) offers an excellent explanation of why the Greeks—"not the dullest people who ever lived" (p. 95)—did not have a mind-body problem.

philosophy was condemned at the University of Utrecht in 1643, only two years after the publication of the *Meditations*, and later, in 1663, the work was put on the Index of Prohibited Books by the Catholic Church.

Science continued to make astounding progress over the next centuries to a point where the Cartesian problem reversed itself. While Descartes was interested in creating a place for science to work independently of the Church in an ensouled world controlled and defined by the Church, by the end of the 19th century thinkers began to ask whether there was a place for mind—the success of mechanical science had been so great, and a satisfactory answer had never been given to the question of how there could be an interaction between mind and body. Already in the latter part of the 19th century scientists began to question whether there was any room for mind in a thoroughly material world.

Such was the situation at the founding of the Society for Psychical Research in 1882. Noakes (2008) has recently made a good case that the founders did not wall off their psychical research from the good practices they were engaged in as 'normal' scientists. Nevertheless, they were intensely interested both in supporting the existence of a robust mind and in the question of survival of the mind after death, both a legacy of the Cartesian notion of mind. Henry Sidgwick, a founding member of the SPR and its first president, was a philosopher and ethicist; as opposed to other founders who focused on survival, Sidgwick was more interested in proving the existence of a Cartesian mind that was free in order to maintain human moral responsibility. Early on in our field, then, parapsychologists focused on dualism as a support for their philosophical views and used parapsychological research to under-gird a dualist world view. As Alvarado (2003) has expressed it: "...many of these authors argue that the findings of psychical research support the concept of the soul or of non-physical dimensions of human existence" (p. 66).

Thus, the question of whether there were legitimate data in parapsychology to support dualism became a main focus. Otherwise, a thorough-going materialism seemed the only legitimate option. For instance, the contemporary philosopher Owen Flanagan, Jr., in his book, *The Science of the Mind*, wrote that the main disadvantage of Cartesian dualism was that "we have no evidence whatsoever that there are any nonphysical things" (Flanagan, 1984, p. 20). Earlier, the philosopher C. D. Broad, in his book *The Mind and Its Place in Nature*,

had come to the opposite conclusion, but he can do so only by appealing to the data of parapsychology. He writes,

> If there were no facts to be considered except the normal ones, and we rejected all the alleged abnormal facts dealing with Psychical Research, I should regard Emergent Materialism [epiphenomenalism] as on the whole the most reasonable view to take of the status and relations of matter and mind in Nature.
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> (Broad, 1929, p. 647)

Rhine and Dualism

J. B. Rhine, who helped define parapsychology as a modern experimental discipline, was a committed materialist until he read Bergson, who converted him to dualism (see Mauskopf & McVaugh, 1980). Rhine was called to Duke to engage in empirical work by the committed dualist, William McDougall, and Rhine's research led him to argue for all of the attributes of the Cartesian mind. Summing up his work and its implications in *The Reach of the Mind* (Rhine, 1947), he argued for the following description of mind:

1. The mind is different from the body—"By the discovery of an experimental sanction for the psychocentric conception of man we can be brought to think of people all over the world as being more than bodies" (p. 222)
2. The mind is nonspatial—"When ESP was found to function without limitation from time and space, this discovery was taken to mean that the mind is capable of action independent to some degree of the space-time system of nature" (p. 213)
3. The mind is purposeful and not mechanical—"PK results do not follow the laws of mechanics" (p. 116), and "PK is plainly a purposively oriented operation and therefore cannot be physical" (p. 117)
4. The mind is free—"The characteristics of intelligent, volitional action in human behavior are so different as to be in some respects contrary to those of the causal principles basic to the physical sciences" (p. 118)
5. The mind is the seat of value—"The acceptance by man of a free ruling factor over and above his physicochemical system has raised him from the brutish, selfish, quarrelsome impulses of his primitive nature" (p. 223)

Having considered these findings, Rhine says, "...a distinct difference between mind and matter, a relative dualism, has been demonstrated by the psi experiments..." (p. 205).

Others in parapsychology, such as C. J. Ducasse (1951), have argued for dualism. Whately Carington (1933) wrote *The Death of Materialism* in which he garnered all sorts of scientific and philosophical argumentation against physicalism. In contemporary parapsychology, John Beloff (1989, 1990c, 1994, 1997) and Frank Dilly (1988, 1989a, 1989b, 1990) have both argued for dualism based on the parapsychological evidence. I will return to contemporary arguments below, but I hope my preceding points will suffice to show that both psychical research and parapsychology have their intellectual genesis in Cartesian dualism and the attempt to use the parapsychological evidence to argue for dualism remains.

Atomism

It is important to remember that there was a second assumption implicit in Descartes' mechanical view in addition to dualism, and that is atomism. He employed atomism in different ways. In terms of the mind, he conceived of mind as an atomic substance. Therefore, each mind is a basic building block of the mental world and cannot be further subdivided; hence, the mind is radically simple, which guarantees its eternal survival (since it can never decompose and atoms do not go out of existence).

On the other hand, he applied atomism to his view of matter, which supported his mechanistic view of the material world. One implication of atomism is that the influence of one atom on another can take place only in billiard ball fashion, by direct contact. There was no mysterious and occult action at a distance. Therefore, any influence on a physical object was due to direct contact. A major reason for Descartes' aborted attempt to explain the mind-body interaction being due to animal spirits (although he abandoned this explanation almost as soon as he offered it) was based on atomism; animal spirits had to, mysteriously, become physical enough so that they could come in contact with the body in the pineal gland.

Atomism was a fundamental idea within modern science and not just in Cartesian philosophy. It became a basic assumption of science and of our intuitive understanding, so much so that C. D. Broad (1962) could proclaim as his third Basic Limiting Principle that we cannot affect the world except by direct contact. Broad offered his Basic

Limiting Principles as a view accepted by an intelligent Westerner who understands the world, and then he argued that parapsychological data undercut all of these Basic Limiting Principles. Both ESP and PK were viewed as mysterious action at a distance within this atomistic and dualistic view. The main thrust of the claims of ESP is that an individual mind can get information without the possibility of mechanical causation in direct mind to mind, or mind to matter connection; likewise, in PK a mind was able to affect the world without physical contact. Thus, psi phenomena were virtually defined in terms of mind-body dualism and of atomism.[2]

Dualism in Contemporary Parapsychology

On the contemporary scene, two thinkers in particular have continued to argue for dualism: John Beloff and Frank Dilly. One of the giants in our field in the 20th century, John Beloff (1990d) stated that "The focus of my interest in the paranormal has always been its implications for the mind-body problem" (p. 100; see also Edge, 1991). An unusually elegant and clear writer, Beloff (1990a, 1990b, 1990c) argued for dualism in several ways, but he offered a straightforward argument in four steps once he had come to the conclusion that only two possibilities were live options for answering the mind-body problem, dualism or physicalism[3]:

1. Our common sense, or folk psychological view, is radical dualist (or interactionist), and we should not reject this view without good reason.[4]

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[2] The traditional view of parapsychology contains the seeds of its own destruction. It asserts both that psi events exist as action at a distance and it accepts atomism, which denies action at a distance.

[3] Beloff uses the term "physicalism" rather than materialism, which he reserves for one version of a physicalist view, the identity theory.

[4] Since Cartesian dualism did not leave open the possibility of the unconscious, Beloff (1990b) rejects a strictly Cartesian version of dualism, but nevertheless he argues that mind-body dualism is "the most important single insight in the entire history of philosophy" (p. 69). And in his description of dualism, much like I (1985) have shown with Rhine's quotes earlier, Beloff also affirms that the mind has the characteristics of a Cartesian mind (i.e. mental events are non-spatial, are teleological, are private and subjective, are

2. Psi phenomena exist.
3. Since these phenomena are mental, they undercut physicalism.
4. Therefore, we should accept dualism.

Against this kind of view, scientists usually reject #2 above, denying psi phenomena. Not only do they not accept the parapsychological data, they often question whether those data are even possible, since they go against the view that the material world forms a closed system and thus it is not possible for anything non-material to affect the material world (since only matter can affect matter, and there is no action at a distance).

The philosopher Frank Dilley has taken a different approach from Beloff to the relationship between dualism and psi phenomena. Rather than arguing we have to accept dualism because of the existence of psi phenomena, Dilley (1989b) argues that the only way to make sense of dualism is to accept the existence of psi phenomena, arguing that "psi powers are needed to explain mind-brain interaction" (p. 241). Realizing that many will think that the cure is worse than the disease, he nevertheless urges philosophers to rethink the possibility of psi phenomena:

> The arguments for the truth of dualism are too strong to be denied, and so are the arguments *for* interaction of mind and brain. If the only way to salvage both truths is to admit the existence of parapsychological powers then so be it. Mind-brain interaction poses the same sorts of conceptual difficulties as do psi powers and it is time we face up to that fact.
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> (Dilley, 1988, p. 469)

The problem with interactionist views has always revolved around how to explain an interaction between two kinds of entities that are as different as minds and brains are. Dilley goes into the question of whether there is some sort of energy transfer possible in those kinds of interaction, but I will not discuss that point. Rather, Dilley's position can be taken, in simplified form, to be the following: given that no ordinary interaction can take place with some sort of energy transfer, the only way to explain the action of mind on brain and brain on mind is by using ESP (actually, he reduces telepathy to clairvoyance) and

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free, and are the seat of value) with the additional proviso that there can be unconscious mental events.

PK. He says, "The suggestion made in this paper is that, just as the mind clairvoyantly establishes a direct relation, call it a reading relation, to a material object, so the mind establishes that same reading relation to its own brain in perception" (Dilley, 1989b, p. 243).

Likewise, we can understand how the mind can affect the brain in volition if we conceive of it as a psychokinetic effect. Analogous to the mind affecting a material object, the mind sets up an active and direct relationship with the brain in an act of volition by "changing the resistances of brain synapses" (p. 243). The reason people have been so resistant to this kind of proposal, Dilley goes on to argue, is that they have an incorrect understanding of the relation between mind and brain based on a misunderstanding of minds. Since minds are not in space, there is no action at a distance using the mind and thus there is no prohibition of such a connection. The question of action at a distance (a spatial divide) arises only between physical events.

Dualism and Materialism

Let me come back to the statement I began the paper with, that parapsychology does not have a mind-body problem. Needless to say, the statement was deliberately provocative, and although I will argue that it is true, I will warn you now that I will put caveats on the statement.

How can I argue that parapsychology does not have a mind-body problem, especially since I have shown above that parapsychology has virtually defined itself in terms of this problem? Isn't PK understood as 'mind-over-matter', or telepathy as 'mind-to-mind communication'? Methodologically, don't we set up our experiments to negate the possibility that mind acts without the possibility of there being a physical cause? Therefore, isn't our understanding of psi phenomena so tied to the mind-body problem that to dismiss it has the same effect as dismissing parapsychology?

Let me call upon William James at this point, who not only was extremely interested in psi phenomena but also had a profound influence on philosophical thinking. In his pragmatic approach, James urges us to be careful of words, that we tend to use them without fully understanding them—this is as true for philosophers as for the non-philosopher—and that in order to become clear about these ideas[5], we

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[5] In using this phrase I am pointing out James' indebtedness to Charles Sanders Peirce (1958a, 1958b).

need to see what consequences follow from the ideas. Philosophical debates are often the products of sloppy thinking according to pragmatism, and if we become more careful about our words, we may either eliminate some philosophical problems or we may need to re-conceive the issues. It is in this pragmatic spirit that I want us to think about the mind-body.

If my analysis of its genesis is correct, the mind-body problem arose and thus depends upon a particular view of the mind—Cartesian dualism (at least some robust form of it). This view depended, in turn, on two assertions: 1) that there are radically different sorts of things in the world, minds and bodies, and that they have the characteristics specified earlier; and, 2) the world is atomistic. At the very least, what this view seems to assert is that minds exist in some way as ontologically different from matter, and that one should think of a mind as some sort of thing, as an entity. Like material objects, minds are things, just radically different sorts of things, with different characteristics. This view assumes a substance ontology; I'm not using the word 'substance' in the Cartesian sense (of not needing anything else to exist but existing independently) but rather merely to point out that the world is ultimately populated by things rather than processes, activities, or relationships.

I do not need to journey into the history of the mind-body problem here. Descartes quickly recognized that his proposed solution to the interaction of mind and body using animal spirits was inadequate, and a host of alternative relationships between mind and body were proposed, including parallelism, epiphenomenalism, and occasionalism. More radical solutions included an elimination of either mind or matter. Idealism, for instance, argued that matter in itself did not exist, only mental events. Of much greater importance to the history of philosophy has been the attempt to deny mental events in one form or another.

Our traditional folk psychology, the way that we (Westerners) intuitively thought about minds, reflected this dualism. For instance, in an informal survey I have conducted in Introduction to Philosophy classes for the past 40 years, I have asked my classes whether they think that any particular thought—say of fried chicken—is the same thing as a brain process, or whether such an assertion would be as contradictory as asserting that a square is a circle. In the late 1960s, the classes responded almost uniformly in a Cartesian way, voting that thoughts and brain events were radically different things, so much so

that to say that a thought was a brain process produced an inconsistent statement.

However, within a period twenty or twenty-five years I have gotten a reversal of that vote. When I ask students in my classes now, virtually all of them respond in the opposite way, saying that, of course, thinking is *nothing but* brain activity, and they wonder who could have thought otherwise; indeed, I have to work hard in trying to get them to think that such a dualistic view is plausible. Those few students who resist this materialist perspective do so on religious grounds, wanting to keep the notion of a substantially different mind so that they could retain the possibility of the soul surviving. A majority of philosophers now are materialists, and there is no question that mainstream natural scientists (qua scientists) seem to fall into this category. So, it looks as if materialism has replaced dualism as a way of thinking about thinking. Without thinking about the implications, we tend to say, 'My brain forgot that' (a phrase I heard over lunch just before I wrote this sentence) instead of saying 'My mind forgot that'.

It's amazing to me that such a radical change in our thinking could have taken place in such a short span of time. It is easy to conceive that advances in our understanding of the brain could have brought about a change in thinking among those specialists familiar with the research, but social forces also have been at work to such a degree that our cultural understanding has changed this dramatically. Of course, this change has not been complete. We still can say, when we can't figure something out, either that our mind is not working today, or that our brain is not processing (although we usually do not say 'It slipped my brain' rather than 'It slipped my mind'). But at the very least, the identification of thinking with brain activity seems natural to most Westerners. The point I want to make is that when Richard Rorty (1965) argued in 1965 from a pragmatist perspective that our language could change over time such that we could eliminate all references to mental language and many of us took such a statement to be inconceivable, the changes over the last 40 years suggest that he might have been correct; to an extent mental language is already being eliminated.

These points suggest that this is an especially interesting time for Western culture (Eastern culture is facing related issues, too, often violently so), a time when the social change in thinking may be as great as the change faced in the 16th and 17th centuries. We are caught between two worlds, dualism and materialism, and the change from a

dualist to a materialist view of reality—both of the world and of ourselves—is producing both intellectual and cultural upheaval.

It is important to note that both dualism and materialism are dependent on the Cartesian world view since materialism seems comfortable retaining the Cartesian notion of matter and simply eliminating the mental side of the dualism. I indicated earlier that mind and matter are paired terms, defined in contradistinction to each other, similar to 'day' and 'night'. Matter is defined in a particular way because mind has been defined in a particular way. This world view is a package containing these paired concepts. One remains within the Cartesian world view even if one argues that Cartesian minds do not exist but only Cartesian matter exists. The hand that fits within the Cartesian glove is composed of a set of concepts and related assumptions. Denying one of these concepts without examining and questioning the conceptual complex of ideas still retains the world view.

While our ordinary language and folk psychology seem to switch between these two views (dualism and materialism), the tide of the 20th century is toward materialism. In asserting that parapsychology does not have a mind-body problem, I want to stress the connection between the historical origins of parapsychology and dualism, because the way in which we intuitively understand parapsychological phenomena tends to bring with it the assumptions and concepts of the Cartesian world view with an emphasis on the mental side.

I want us to be careful that we do not retain a world view in which we say that there are two options, a Cartesian or Cartesian-like dualism on the one hand, and a materialism on the other hand. Such a world view, in which the mind-body problem arose, assumes a particular concept of mind and a particular concept of body (matter). Within this world view, the founders of the Society of Psychical Research and Rhine and others tried to use the data from parapsychology to show that materialism could not be correct. Accepting materialism seems to eliminate all those characteristics found on the mental side of Descartes' dualism which parapsychology conceived of itself as providing evidence for. Parapsychology has traditionally seen its task within this world view. I want to suggest that this world view—this complex of ideas and assumptions—is incorrect. In refusing to talk about the mind-body problem, which has its origins in this world view, we will be better able to answer the questions that we should be dealing with in the 21st century.

There Is No Mind-Body Problem

A Short Summary

Let us stop and see where we are. Let me present a summary of my argument so far. I have suggested somewhat contentiously that we do not have a mind-body problem in parapsychology, which seems odd since modern parapsychology has depended upon dualism as it defined itself, and parapsychology has seen itself as giving evidence for dualism. The mind-body problem, in the form of dualism, and parapsychology have gone hand in hand since the founding of the SPR. I have noted secondly that the dualism that parapsychology has supported depends on a view of mind that was invented in the 17th century, and that this view of mind is conceptually coupled with the view of matter propounded at that time; this dualism presents a coherent world view, one in which mind and matter are defined in terms of each other and reality is conceived as being composed of one or the other of them. Each of these terms is conceptually dependent on the other such that if you change the meaning of one of the terms, you will likewise affect the meaning of the other, and you will have altered this world view. This dualistic understanding has been translated into our folk psychology and our folk science, although over the last 25 years there has been a radical cultural change such that Westerners (taking my students as examples) now find it quite normal to say that a thought is a brain process, whereas before this time they thought that such a view represented a logical contradiction. The most significant cause of this cultural change undoubtedly has been the progress of science and brain physiology in particular.

The first of the questions that we have been asked to address in this paper is: what do we know? The first thing we should be clear about is that the mind-body problem is a philosophical problem, not an empirical problem. Empirical research will have implications for how we deal with the philosophical issues, but the mind-body problem is basically a philosophical one. Secondly, it seems pretty clear that our folk beliefs, reacting to the advances of science in the 20th century, have migrated—they are no longer consistently dualist in the way they were at the founding of the SPR in the 19th century, or at Rhine's taking parapsychology into the laboratory in the 1930s. Whereas our folk beliefs earlier were clearly consistent with a Cartesian dualist position, and were an important impetus for psi research, these beliefs have

migrated toward materialism, although probably somewhat inconsistently.

The situation we find ourselves in as a discipline over the last century is shown in a Gregory Bateson quote where he says that asserting the existence of psi phenomena is a mistaken attempt "at cute efforts to escape from a crude materialism that becomes intolerable. A miracle is a materialist's idea of how to escape from his materialism" (Bateson, 1979, p. 210). If we remain in the Cartesian world view of mind and matter, each defined in the mutually reinforcing way in which Descartes defined them, and we migrate toward materialism, then to assert efficacious psi phenomena in reality asserts a miracle, a breeching of materialism; notice this result also follows when we assert the existence of any efficacious mental events (remember Dilley's argument that psi is the only way to make sense of dualism). As long as we remain within the world view generally outlined by Descartes, parapsychology will have to reject the findings of science.

If my analysis so far is correct, where do we go from here? Let me return to my contentious statement that parapsychology does not have a mind-body problem. What is entailed in that assertion is that the mind-body assumption that parapsychology has incorporated into its thinking is a Cartesian (or Cartesian-like) dualism. I think that approach is tragically mistaken. If we take mind to be what Descartes (and Rhine, Beloff, Dilley and others) take it to be, and matter to be what these parapsychologists take it to be, then, in fact, I believe we should reject both concepts of mind and matter. Our continuing to talk of the mind-body problem is a case of our continuing to use a set of conceptual categories long past their usefulness.

Let me be more explicit about what I want to argue. I want to argue from a pragmatist perspective, that the mind-body problem should no longer be a problem for us since we have changed our understandings both of mind and of matter. However, I *do* want to argue that there are still issues related to an adequate understanding of *persons* that should interest parapsychology; we simply need to understand that we are not dealing with the traditional mind-body problem when we discuss these issues. I hope that you will not think this is merely a semantic distinction, a quaint philosophical move that makes no real difference. I want to argue from a pragmatist perspective that this conceptual change is significant.

We clearly have two aspects to deal with, a conceptual one and an empirical one. I want to deal with them in turn. Let me turn first to the

set of conceptual issues, the philosophical ones, and then I will turn to the data of parapsychology, see where we are, and suggest directions we should pursue in the future.

Conceptual issues about personhood

Let me make three points in this section.

1. *The first is that we have mainly continued to work within the Cartesian paradigm in thinking about the mind-body problem.* This is true not only for parapsychology but for philosophy. I ask you to think about the responses to Descartes' failure to explain the interaction between mind and body. Alternatives arose, including parallelism, occasionalism, and in the 20th century materialism became the favored explanation (in the sense that mind could be reduced to body). This approach essentially accepted the Cartesian characterization of mind and of matter and urged us either to assert their existence (but not their interaction), or in the case of materialism urged us simply to deny minds. In general, none of these approaches questioned whether we should think about mind or matter in radically different ways, and in that sense they remained within the Cartesian paradigm. In general, parapsychology did not question it either, perhaps because one of the fundamental issues in the history of psi research has been the question of survival, which assumed that mind was an entity that could survive bodily death with full mental qualities[6]; survival needed a Cartesian-like mind and it assumed Cartesian-like matter.

2. *The second conceptual point concerns our view of mind.* Since the 1970s philosophers have begun to question the Cartesian view of mind, defining mind as a function rather than an entity. Let me quickly examine functionalism. Needless to say, I cannot accomplish more than a cursory evaluation. But in outline form I would like to gesture toward two conclusions: 1) that functionalism is not completely adequate, but 2) that Descartes was correct in ascribing certain characteristics to mentality.

Functionalism asserts that attempts to reduce mind to matter are incorrect because such attempts look at minds in an incorrect way, engaging in a kind of category mistake. Rather than thinking of minds as entities, functionalists say we should think about them as functions

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[6] There were exceptions. For instance, see Broad (1958), who suggested several possibilities of which mental facilities might survive.

within a mental system. We should think about minds the same way that we think about hearts. When we define a heart we do so in terms of the function that it serves—as the cause of blood to be circulated in the body. The fact that hearts are of biological material in a normal body is not important. If the heart were to be replaced by a mechanical pump, or if a Martian had a body with blood being caused to circulate by a mechanism out of some as-yet unknown material, we would still regard it as a heart. Likewise, mental states refer to the causal function they serve in the mental system. Mental states are the intermediary between sensory inputs and behavioral outputs. So, wanting to eat an apple serves the function of producing the activity of reaching out, grabbing an apple, and eating it. According to functionalism, the 'hardware' of the cause of the behavior is irrelevant; rather, what is definitive is the function served. Thus, functionalism asserts that traditional materialism is wrong when it tries to reduce a thought to a brain process. Rather, a mental state is simply how the state relates to the system that produces behavioral outputs.

Although modern functionalists talk about mental states as functions rather than as entities, and thus they seem to take us out of the Cartesian framework, nevertheless functionalists are really materialists in hiding. In fact, although they have given up reductive materialism and thus do not reduce the mind to matter, in practice functionalists are materialists because they assume that mental states are functions of brains and, given Ockham's Razor, they think it makes sense to account for mind in purely physical terms. Unless they consciously assert that the material realm must be viewed in an entirely different way, they are still stuck within a Cartesian world view.

I believe that functionalism fails in several other respects. While functionalism has a reasonable story about mental states such as wanting or believing (and how they result in behavioral output), it fails utterly to account for the subjective aspects of mentality, the 'what it is like to be in pain' aspects of mental experience. These internal qualitative aspects have often been referred to as qualia. We know what it feels like to be in pain, or to taste something sweet, or to see the color red. It makes complete sense that one could have a zombie that could produce behavioral outcomes in the same way as a human being, such as reaching for the red apple, but who would never experience the qualitative aspects of our experience. But, we take this internal part of experience as fundamental to our mental life.

Further, functionalism seems inadequate in its computational approach because it does not account for meaning. Some philosophers have talked about this aspect as dealing with intentionality, the 'aboutness' of a belief (any belief has to be about something, has to refer to something, where no material event refers to anything—it just is what it is). I want to focus, rather, on the aspect of meaning; in thinking we express meaning and we make sense of the world.

One way to look at this issue is to examine the Turing Test, which forces us to consider what we mean by thinking and to ask if computers think. A person outside of a room slips questions into a room and gets responses out of the room. The test involves seeing if the person outside of a room can figure out if a person or a computer is giving him his answers, based solely on the responses he gets from the room. If he cannot, according to Turing (1950), we should assert that a computer can think since we cannot functionally distinguish between a person and a computer. Notice that this is a test of a functionalist view of mind because it defines thinking as being able to respond appropriately.

John Searle's Chinese Room thought experiment (or Ned Block's Chinese Nation thought experiment) questions the adequacy of the Turing Test. In Searle's (1980) version a non-Chinese speaking person sits in the room with a set of rules that allow him to take Chinese script from outside the room and convert it to other symbols; simply in following these rules mechanically the person produces symbols that answer the questions asked by persons outside the room. According to functionalists, this should convince us that thinking is occurring. Given the adequacy of these conversion rules, the linguistic responses may be adequate in the sense that they provide a set of appropriate symbols, but I want to focus on the question of meaning. We would never want to say that the person in the room understood Chinese. He is merely responding according to a rule book in manipulating the symbols and is not speaking or understanding Chinese when he outputs the symbols; he is merely following rules, such as 'If you receive x, respond with a y'.

Although manipulating symbols, the man does not understand the symbols; what is missing is meaning. In our mental life, meaning seems fundamental. Events either are meaningful or they are not. But, things do not have intrinsic meaning. Persons, however, attribute or have meaning.

Not only do material objects not carry meaning; they do not carry value in themselves. There is still a difference between kicking a chair

and kicking a person, and not simply because the person is likely to kick you back. Rather, it is morally wrong (in most instances) to kick a person, but we don't make the same moral judgment about kicking chairs. Matter does not seem to be located in the moral realm; it is a-moral. Persons reside in the moral sphere while material objects reside in it in only a secondary way.[7]

It should be noted here that my position concerning ethical value is not the same as Rhine's but our views overlap. If you remember, Rhine accepted the Cartesian description of mind and argued that the data from parapsychology supported such a mind and in turn supported dualism. I agree that there are aspects of the person—thinking, meaning-creating, seat of value—but I do not think that dualism is correct. Rhine was correct in rejecting materialism, because it is hard to affirm these characteristics of a person in a world of only Cartesian matter. But asserting these characteristics of the person on the one hand, or asserting the existence of psi phenomena on the other, does not necessarily imply dualism. Rather, it should cause us to question the whole worldview of Descartes, where reality is divided between minds as he defined them and matter as he defined it. While experience contains some of the characteristics Descartes used to describe mind, it fails in others, notably in accepting an atomistic view of the world. I can point this out best by turning our attention to the traditional description of matter.[8]

3. *The third conceptual point concerns our understanding of matter.* In the rise of the new science in the 16th and 17th centuries, thinkers began to reject an Aristotelian view of the world (for both scientific and cultural reasons) and replace it a mechanistic and atomistic description of the world. According to this view of the world, atoms (from the Greek *atomos*, meaning 'unable to be divided') were the building

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[7] This conclusion may be more complicated; a stark bifurcation between minds and matter can be viewed as one traditional cause of the environmental crisis. Some ethicists have argued that we should begin talking about the material world as having inherent value, also.

[8] Although I have criticized functionalism, I think that there is much to be said positively about it. It is correct, I think, in affirming that we should not talk about mental entities, an idea that has its roots in atomism. Functionalists do not draw the conclusion that we should question atomism when they assert that mental processes are functions, and thus I don't think they completely understand either how radical their approach is nor the potential implication of a non-atomistic approach.

blocks of the world, the basic stuff out of which everything is made. The basic notion here is not whether atoms really are indivisible (obviously, they are not) but atomism asserts that the appropriate way to characterize reality is to assert that it is composed of entities, bits of matter (or of minds if one applies it to minds, or of individual citizens if one applies it to political philosophy as Locke did).

One implication of this view of the world is that action happens only by contact, and there can be no action at a distance. I described this characteristic earlier when I discussed Broad's Basic Limiting Principles and how this characteristic essentially undercuts the possibility of psi events, since psi *does* seem to occur at a distance.

A question that needs to be discussed is whether physics still retains this notion of matter. We have already seen that there is reason to question the Cartesian notion of mind, and if there is also reason to question the Cartesian notion of matter, then it follows that we should question the whole Cartesian worldview. And, insofar as the mind-body problem arose in the Cartesian worldview and became a problem in that worldview, then we will have good reason to argue that there is no mind-body problem in its traditional sense, even if we want to clarify that some issues still remain for philosophy (and parapsychology) to retain as subject matter for investigation.

Subatomic physics has been offering dramatically different descriptions of matter from those of classical physics. In particular, atomism has been called into question. As I began to argue about 15 years ago (Edge, 1994), I think that physics is requiring us to reject an atomistic view of the world in favor of a relational one. In this world objects are no longer thought of as objects existing in space, but they are space-time entities. Further, the equivalence of mass and energy has also called into question any simple atomistic view of the world. Finally, the Copenhagen interpretation of quantum mechanics predicted the possibility of objects becoming entangled such that when one is affected, the other is simultaneously affected, something Einstein thought impossible. The Einstein-Podolsky-Rosen (EPR) thought experiment was proposed to test this interpretation. In the 1960s John Bell proposed mathematics that allowed experimental testing, and beginning in the summer of 1982, Alain Aspect produced evidence that supported the Copenhagen interpretation: 'objects' seem to affect others in a 'ghostly action at a distance', as Einstein described it.

Notice I put 'objects' in quotes. The traditional connotation of that word in an atomistic world is that an object is an entity that exists in

space, where space has the function of separating objects. Hence, if one object affects another, it cannot do so over space but must engage the other object spatially—it must contact it. However, the results of the Aspect experiment (and now dozens of others) show that we can no longer think, at least exclusively, in terms of this atomistic world. Rather, objects seem to be in relationship, are 'entangled' with one another so that causal effects occur over a distance (given our ordinary notion of space). Given that these entangled objects no longer have the atomistic properties of traditional objects, the classical notion of objects has been undercut. This is the sense in which I want to argue that the concept of matter has changed.

Thus, I want to argue that, given our fundamentally different notions of both mind and matter, it is dangerous to slide back into the 17th century question of the mind-body problem. Both the history and logic of the mind-body problem assumes a certain view of mind and of matter. In that sense, I think that we no longer have a mind-body problem, and so long as we continue to pose the question in this traditional way, we run the risk, as William James has pointed out, of not being clear about our philosophical investigation.

This is not to say that there are no fundamental philosophical issues that we have to deal with. Indeed, there still are fundamental issues, but we must examine them in a way that does not presuppose an outdated and inaccurate set of concepts or world view. If we want to talk about issues relating to the traditional mind-body problem, how are we to talk about it since, in a fundamental sense, neither minds nor bodies exist?

A Pragmatic Approach to the Mind-Body Problem

One approach to solving this problem might be to ask how dramatically the concepts of mind and matter have changed to argue that they are different concepts such that we can no longer use them without confusion? I simply do not see how we can be successful in this approach. I cannot even make a reasonable start in answering, 'How much change is enough?' The answer to that question either turns on a personal intuition, or it boils down to a pragmatic approach, which I recommend. As James asks us to do, I suggest we ask which ideas really are at stake when we are concerned about mental functioning[9]

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[9] I may use terms like 'mental functioning' and 'mentality' rather than 'mind' in this section. I do not want to prejudice the discussion by using a term that I

and why they are at stake. And, I suggest that we take a minimalist approach, taking those characteristics that seem most basic.

I should let you know that my own cross-cultural studies will help guide me in this choice. There is no reason why we should take an exclusively Western notion of mentality or the person; rather, we should be guided by what appears to be important for the most part across cultures. The caveat of 'for the most part' must be added because I am not attempting an exhaustive cross-cultural study of mentality or personhood. Such a study reveals that there are sometimes highly unusual approaches in describing persons. For instance, Fajans (1985) says that the Baining of Papua, New Guinea, don't have a folk psychology, and Myers (1986) asserts that the Pintupi of Australia show an extraordinary lack of interest in trying to know the motivation for others' actions; if we had to take all of these anomalistic ideas into account, we would never progress.

I am going to focus on mentality rather than the physical side of the equation since I assume that physics will offer a more adequate understanding of the physical world. Plus, parapsychology has viewed itself as defending and supporting the mental side of the equation, so we should get a fuller understanding of mentality to see its connection with parapsychology.

Let me say from the beginning that I think that the unit of understanding is not mind, as I have argued, but the person. Thus, it is not mentality itself that I am concerned with; I have used this term simply to gesture toward the area of our discussion and as an attempt to bridge the gap between focusing on the mind and focusing on the person. Therefore, it is the characteristics of the person that I want to analyze—those things that make a person a person.

I will quickly and in outline describe the characteristics that I see most important in an analysis of the person; I am not attempting to argue adequately for these conclusions or to give a sufficient explanation of them. Such arguments would require far more detail than I can offer in this paper. However, I will mention several characteristics that seem important to attribute to people:

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have argued is loaded down with baggage, but I want to gesture toward that area that we have referred to as mental. As I will argue, some of the traditional characteristics that we have attributed to mind are fundamental and they must be explained or accounted for in an adequate philosophy.

1. *Reflective subjectivity*. It will be very easy to fall instantly into a conceptual morass as I describe these characteristics. For instance, I might have called this characteristic 'consciousness', but since I am questioning the hegemony of atomism, and 'consciousness' suggests that it is an entity, I want to avoid it. I might have used the more labored 'consciousing', which has the advantage of implying a process rather than a thing, but I think we run a danger when we create words to designate this area. Although much work in contemporary philosophy has been about consciousness, I prefer to keep our focus on what it means to be a person. I think it is persons, not consciousnesses, which are natural kinds. We naturally pick out a person in the world, not a consciousness.

In examining subjectivity, some philosophers, for instance, have talked about qualia. I will avoid this term, but I want to affirm there is a felt quality of what it is like to, for example, see red, or see a sunset, or believe a proposition.[10] We do not think that a chair has such a first person point of view or experiences this kind of subjectivity. We do not think that a chair experiences anything like tasting an apple, nor do we want to say that any objective description of an apple fully grasps the experience of biting into an apple. However, this is a minimal characteristic. I assume that many animals experience such a felt quality. If I want to focus on the person, I need to go deeper. I not only can feel pain but I can think that the pain might become unbearable in the future. I can think about how I might react to that pain and think that I might even die from the illness. In other words, I not only look at the world from a certain point of view, but I can also ruminate on that point of view as a point of view. I can reflect over myself and my experiences; I can place myself as the object of reflection. Lynne Ruder Baker (2007b) describes this as a first person perspective. As far as I know, no other animal takes a first person perspective; they do not experience the world in this robust first person way.

2. *Meaning-Creation*. Since Brentano, philosophers have argued that intentionality is a defining quality of consciousness, pointing out that mental events are 'directed toward' something, or they are 'about' something in a way that physical events are not. For instance, a belief can be about the kind of weather we will have tomorrow; the act of belief directs us toward what the belief is about—that water is

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[10] Philosophers usually don't place belief in this category, but I think there is a felt quality in believing that is typically overlooked.

composed of oxygen and hydrogen, or that the moon is made of green cheese. Further, we always desire something, or know something, or feel something. We cannot say the same of physical objects; they simply are. I take intentionality to be a part of the meaning-creating aspect of the person, but I am describing something broader here than intentionality.

In addition to holding a propositional attitude about the world, humans understand the world. We make sense of our experience and of the world; we put ideas together in ways that satisfy our need for an explanation. Things hold significance for us. We do not simply passively record experience or mirror it; there is an active ingredient of putting things together, making sense of things, attributing meaning to objects. We can describe a person waving in purely physical terms, but we make sense of this physical activity as a sign of welcome, or a sign that the automobile wants to turn left, or a police officer telling us to slow down. We seem to be meaning-seeking and meaning-creating beings. We create theories, formal and informal, and these provide us with expectations about how the world will act and how we should act; we also tell stories and see patterns in data. We are in a world that is intelligible and in which we create/attribute meaning.

3. *Moral Responsibility*. Descartes argued that minds were the seat of value—because of minds, humans not only had inherent value but through their free choice they made decisions and therefore should be held morally responsible for those choices. So the ideas of choosing, acting and taking responsibility fit into this category, and I include them together because they are interdependent, tethered in a conceptual relationship. And there are indications that these concepts are related cross-culturally (Edge & Suryani, 2002). However, as I have indicated above, I want to take the discussion away from Descartes' focus on mind and instead use the category of person as the category to which these characteristics belong. I believe that we have empirical evidence from cognitive science to support this move. It is people who choose and recognize value and respect other people.

If these characteristics are ascribed to people, then we should expect that people belong to a basic category of our understanding of the world. The Yale psychologist, Paul Bloom (2004a) and a team of researchers (Kuhlmeier, Bloom, & Wynn, 2004) have produced empirical evidence that I think indicates that the category of person is hardwired into us. Essentially, he has argued that infants react differently to material objects than to humans when presented with

disruption of continuity. Bloom and his team have tested how infants react differentially to seeing box-like objects on the one hand, or humans on the other hand, fail to proceed from right to left in continuous motion in a video played to them. Bloom says that infants do not respond the same way to discontinuity in humans as they do to objects; in essence, 5-month old infants applied the principle of continuity to material objects (boxes) but not to humans. This result suggests that even infants have hardwired into them a difference between material objects and persons. We pick out humans as a basic category.

It is instructive to notice that Bloom (2004b) has gone so far as to write, based on this and other data, that we are common sense dualists, seeing bodies and souls as separate. However, I believe that Bloom takes an unsupported leap from his data to saying that dualism is hardwired. What his evidence supports, I believe, is that we are social creatures, and we respond differently to other persons than we do to mere objects. The intuition that persons are different from mere material objects seems basic. Bloom's research takes us a step closer to saying that personhood is hard-wired in us, showing this response is not merely conditioned; it is not simply a cultural artifact. There seems to be some implicit mechanism where we are able to recognize persons and treat them socially, as a special part of our experienced world, when we do not do so with inanimate objects. But, to say we are hard-wired to distinguish humans from material objects only supports the view that we are social creatures, that we recognize other people as a basic category; it does not support any sort of traditional dualism. There is, indeed, a social response that is part of us, and Bloom's work is fascinating in that it implies that this response is hard-wired in us rather than simply being a cultural response, something taught to us in our early years. But to suggest that this response is based in dualism ignores the history of the concept and is an example of the confusion that arises when we continue to use terms without enough sensitivity to their historical origins; indeed, I think it is an example of the problems that can arise if one continues to use the categories of the Cartesian world.

Also included in this concept of personhood is that persons are worthy of special respect. We treat persons differently from objects and, as persons, we belong to a moral realm. Additionally, persons share responsibility for their actions; we hold others and ourselves accountable. Different cultures parse the idea of responsibility

differently, applying more responsibility to families or groups rather than individual actors, but persons are always included in this category.[11] As one example of a first-person point of view, the moral perspective is also constitutive of us as persons.

Let me stop at these three minimal characteristics of persons. Persons have first-person perspectives, they create meaning, and they take moral responsibility. Once we eliminate the assumptions of dualism and atomism, and the need that all explanations must be in terms of these two categories, it becomes natural to explicate what seems most human in terms of persons rather than minds; a fact that does not lessen their importance in any way. These are characteristics that make us who we are as people, and any account of us must take them into consideration; no account would be adequate without them.

I should also point out that if we look cross-culturally, there are a great number of ways in which cultures have conceived of persons. I (Edge, 2002) have talked about some of these and their implications for dualism, so I will not repeat them here. But the range of descriptions of persons found cross-culturally is vastly more varied than the Western focus on mind. I will return to some cross-cultural considerations later.

Because of my analysis above, I believe that any account that is strictly a third person account will not be adequate as a philosophy. I have no objection to science working on models that use only third person categories and testing those models. I believe that those attempts will yield important insights into aspects of us. But, I do not think that science alone will be able to give a full and adequate explanation for the characteristics of the person that I have listed.

In this section, I have argued two things: 1) that although we should refrain from talking about the mind-body problem, there are certain characteristics formerly assigned to mind that are still important philosophically to understand who we are, and 2) that these characteristics belong to persons and not to minds.

Having changed the focus of our inquiry, and in the spirit of the traditional mind-body problem, one can legitimately ask two questions: first, we can ask what the relationship is between a person and the

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[11] Since the modern era, philosophers at this point turned to the idea of freedom and argued that there must be individual freedom of choice in order to have moral responsibility. Since I think that past discussions about freedom have been imbedded deeply in a modernist (dualist and atomistic) view, I refrain from including it my analysis of moral responsibility.

body, and second, we can ask what the relationship is between people and the world. I do not want to discuss the very interesting topic of the relationship between the person and the person's body. Baker (2000, 2007a) has defined it as a constitutive relationship, as the person having a body (or being constituted by it) but not being identical with it. Her arguments are careful and extensive. All I want to say now is that I favor a naturalism, which means that there is no double-decker universe. There are not two stuffs that the world is made of that either interact or are parallel to each other. I will return to the idea of naturalism later.

While the question of the relationship of person to body is important to philosophy, I believe that the second question is more important to parapsychology, the question of the relationship of the person to the world. Philosophically, the simple point can be made that the person is already in the world as a part of the world. However, more important to parapsychology, I will argue that parapsychology can show that persons are intimately related to the world and to each other. Psi phenomena display a unique kind of relationship of the person and the world that shows how intrinsic and intimate that relationship is.

Application to Parapsychology

I would like to turn now from a purely philosophical analysis and discuss several ways in which I think that my conclusions about personhood have implications for parapsychology.

First, remember that parapsychology seemed to be defined in terms of mind-body dualism. J. B. Rhine was right in thinking that certain of the characteristics of the mind were fundamental to being human, although my list of characteristics differ somewhat from his (and from Descartes'). His intuition was correct but, as a product of his time, he understood these characteristics as belonging to a mind in a dualistic world. I believe we should understand mentality in a more naturalistic way.

It seems to me that parapsychology has been moving in the direction of naturalism. Although physicists were among the founders of the SPR and physical theories of psi were offered already at the end of the 19th century, attempts at connecting parapsychology and physics accelerated in the last decades of the 20th century. Two of the most recent examples are Carr (2008) and Clarke (2008). These articles do an especially good job of outlining the various possible approaches that one may take to

connect physics with parapsychology. I am not a physicist and I am not qualified to argue for one approach over another. I do not know how much and in what way quantum theory would have to be extended to offer an explanation of how psi phenomena function.[12] But, parapsychologists have argued that these new theories can offer an explanation of how psi can be fit within physics.

Although there have been more technical explanations of this view (Atmanspacher, Roemer, & Walach, 2002; Lucadou, Roemer, & Walach, 2007), Dean Radin (2006) has recently given a popular argument for the idea of entanglement offering an explanation of psi phenomena. I want to use the following discussion merely as an example of how physics might potentially offer a naturalistic explanation of how psi works physically. Again, I should emphasize that I am a philosopher and my aim is ultimately a philosophical one in this paper; I will leave it to physicists to discuss which theory will ultimately be able to accommodate psi phenomena (which I assume can happen). My intention in discussing entanglement is simply to employ it as an example and to use this application of physics to point out a larger implication for parapsychology.

Radin (2006) argued in his *Entangled Minds: Extrasensory Experiences in a Quantum Reality* that psi phenomena should be interpreted in terms of entangled minds. His interpretation is based on advances in physics that undermine atomism. Radin says, "I propose that the fabric of reality is composed of 'entangled threads' that are consistent with the core of psi experiences" (p. 19), and "this is why I propose that psi is the human experience of the entangled universe" (p. 235).

The idea of entanglement derives from the EPR experiments. Entanglement essentially says that information can be shared between two or more entangled objects (photons, electrons, etc) in a non-local way; since the 'communication' takes place faster than the speed of light, there is no way that classical physics can explain the phenomenon.

The concept of entanglement has assumed growing importance in certain areas of physics. Two practical areas in which entanglement research is being pursued are quantum cryptography and computation.

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[12] Psi phenomena display the first two characteristics of personhood, subjectivity and meaning; it is not clear that they fall under the characteristic of moral responsibility, however, except derivatively.

For instance, error detection is basic for information processing, but using classical procedures reaches a fundamental limitation. Introducing entangled photons allows one to overcome these classical limitations.

> Entangled qubits [quantum data] are a remarkable species that come in linked pairs. They are created when (for example) high-energy photons go through certain materials, which convert them into two lower energy photons.
>
> These twin daughters remain linked even when they are separated, so something that happens to one is echoed in the other, instantaneously, no matter how far apart they are, an effect that, even though it's predicted by quantum mathematics, remains so strange and counter-intuitive that it is sometimes called 'spooky physics'.
>
> Entanglement has attracted intense interest as a way of encryption, since any attempt to intercept a message carried on entangled photons is immediately shown up on the remote daughters as a warning.
>
> (University of Southern California, 2006)

If we take seriously the idea of quantum entanglement, Radin argues, we can conceive of minds being entangled, and we can use this idea to explain telepathy, for instance. What one person of an entangled set of persons knows might also be known by the other. Notice that entanglement does not assert that information is going from one person to the next one. The traditional atomist interpretation of how telepathy works assumes that there can be no 'ghostly action at a distance', that there has to be a causal connection between objects A and B such that one contacts the other in space. But in entanglement, information does not flow from one object at place x to another object at place y; rather, there is a non-local connection such that what happens to an object at place x simultaneously happens to an entangled object at place y. Notice that the idea of entanglement presents a relational view of the world as opposed to an atomistic one. We will return to this idea shortly.

I find it fascinating that over the past two decades a series of thought experiments about quantum information processors have been created that are called 'pseudo-telepathy games'. In the games, two players play. They are entangled in the sense that they have shared history (information). In the game each of the players is given a series

of questions (these have been chosen randomly from a set of questions); the other person must supply a correct answer to the questions without communicating with the first person. They cannot do so (more than one would expect by chance) under assumptions of classical physics, but the scientists creating these thought experiments offer mathematical formulas showing that they can do so under the assumptions of quantum physics using the idea of entanglement.

These games have several assumptions, as pointed out by Brassard, Broadbent, and Tapp (2004); these conditions seem to be the ones that parapsychologists use in setting up their own experiments:

1. It cannot be possible for two people (e.g., Alice and Bob) to communicate with each other. Parapsychologists have worked with this constraint by setting up physical barriers, such as shielded rooms. Brassard, Broadbent, and Tapp think that no shielding can be trusted to block communication given their acceptance of entanglement. However, they solve this problem in another way—if an answer can be given without the time intervening that it would take information to travel at least at the speed of light then this shows that no communication in a classical sense is possible.
2. Alice and Bob cannot know, prior to being told what to ask, which question to ask. This, of course, simply means that we must randomize the process.
3. The answers given must be correct more often than is possible under classical conditions. Using mathematical formulas these scientists are developing, they purport to show that correct answers can be given in this thought experiment.

Do the physicists think of this as a telepathy experiment? Brunner, Scarini, Methot, and Gisin (2007) write: "the right answer is not telepathy, but quantum mechanics. These games are at the heart of understanding the quantum phenomenon of non-locality; for they are the strongest proof of it".

Why do they deny that this thought experiment can be viewed as telepathy (and hence call it pseudo-telepathy)? The assumption these scientists are working with is that telepathy has to be viewed as miraculous, as something that will not conform to any possible physics. But, it should be clear that telepathy has traditionally been defined as a problem within the context of classical physics with its assumption of atomism (and, thus, locality). Psi phenomena did not fit within the

classical world and yet psi events were reported to exist and so, as we saw earlier, people like Bateson (1979) thought of psi phenomena as miraculous. However, quantum mechanics offers another option, one in which psi phenomena are not miraculous (at least seeming no more so to our untutored ears than other quantum events).

The physicist Guy Vandegrift (1995) more recently offered the same conclusion as Bateson but saw the implications of entanglement. He says, "It appears that elementary particles act as if their behavior were linked by channels of communication that can be best described as 'psychic'" (p. 471), and "I did not intend to write an essay on psychic phenomena, and made this analogy because it is the most direct description of what the EPR experiment is actually doing. I do not believe in mental telepathy, miracles or any other occult phenomenon. This affair with Bell's theorem has shaken me to the bone" (p. 476). In his response Vandegrift seems stuck in an intuitive world of classical physics when it comes to psi phenomena, but the thought experiment concerning entanglement seems to describe how something like telepathy could occur within an expanded physics.

Whether entanglement is the best way to bring psi phenomena within physics is not one I can answer. But, I want to use entanglement as an example and draw two important conclusions.

1) The first is that parapsychologists seem far more open now than they were in their past to view psi as a natural phenomenon, not necessarily a mind-dependent phenomenon. The direction of physics seems to offer avenues in which such a connection is possible. While most of the history of psychical research and parapsychology has taken it as a task to provide evidence that psi was independent of natural processes, that it supported dualism and thus a non-natural mind, parapsychologist today seem open to naturalize psi and bring it under an extended physics.

2) The second conclusion is that the idea of entanglement and other aspects of quantum theory seem to present a far more relational understanding of the world and not one ideologically tied to an atomistic conception. The idea of non-locality certainly suggests that traditional atomistic causal conceptions are limited.

Before leaving our quick examination of Radin, it is worth noting that Radin (2006) takes an additional ontological step to suggest that "the fabric of reality is composed of entangled threads" (p. 18). Given the connection between entanglement and psi, he suggests that "our

brains and minds are in intimate connection with the universe" (p. 263). Radin suggests that since entanglement exists to such an extent, we can think of each of our minds/brains[13] as entangled with all others and with all aspects of reality in a kind of quantum Jell-O, which goes beyond the boundaries of space/time. Another metaphor Radin uses is that the quantum level is like a vast ocean which encompasses all persons and things and that individuals are like waves rising out of the ocean; we can call the ocean 'consciousness' and the wave 'mind'. He views mind as attention and intention, and the unconscious as attention without intention.

Radin has not attempted a detailed elaboration of his view, but it harkens back to Ramakrishna Rao's (1998) discussion of Hindu philosophy in which he approvingly suggested we needed to understand both an Eastern perspective, with a greater emphasis on consciousness, and a Western perspective, with a greater emphasis on mind. Both systems incorporate a distinction between mind and consciousness, but in different ways. In Western systems mind and consciousness are not exclusive, but coming out of the dualist tradition, the concept of mind is more focused on rational activities, while consciousness is viewed as encompassing both conscious awareness and unconscious mental functioning (for instance, what happens in blind sight or hypnotic suggestion). In the Indian tradition, mind is considered a more subtle form of matter. Consciousness is completely incorporeal. Mind is able to interact with both matter and consciousness. As opposed to Western thought, Hindu thought emphasizes consciousness in which, in its pure state, there is no object and no intentionality; it is this state that Hindu philosophy describes as the highest and purest state.

I do not want to engage in a discussion of this Hindu approach, but simply point it out for two reasons. The first is to reinforce the movement from an atomistic to a relational view of the self and world, and that with this approach, more options become open to us to think about the relationship between persons and the world. Second, I want to use this discussion about the (traditionally Eastern) Hindu world view as a transition between Radin's explanation of psi and the next section, which considers my suggestions for what work parapsychology ought to engage in for the near future.

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[13] Radin continues to use the concept of mind, but he also does not analyze the mind-body problem *per se*; gesturing toward naturalism, he says that when he refers to 'mind', he is actually referring to mind/brain (p. 261).

The Implications for the Future

Let me summarize where we are. I have suggested that our concern for the mind-body problem grew out of a Cartesian dualist framework, which itself was produced to answer certain questions in the 16th century, a time when the mind was invented, according to Rorty (1979). Understanding this, I agree with Tarteglia's (2004) characterization when he writes, "The mind-body problem may be a crease in our understanding of the world which arose through certain historical contingencies, and by isolating and understanding them, we may be able to iron it out" (p. 745).

The mind-body problem formed the basis for physical research and parapsychology, both its philosophical basis and, in turn, its experimental program. Parapsychology conceived of itself as providing empirical proof of mental processes in a dualist system. The progress in brain physiology has called this dualism into question, and because of this advance our folk psychological ways of thinking about minds have changed so much that the person on the street seems comfortable (at least most of the time) assuming that thoughts are brain processes. From an information processing point of view, functionalism has suggested that we can think of the mental in functional terms, although I think contemporary mainstream functionalism fails to explain the three aspects of the person that I listed: reflective subjectivity, meaning-creating, and moral responsibility.

However, the move in our thinking away from dualism to naturalism seems to me to be correct, and I think parapsychology, in general, has also made this move; most of us no longer think that we are out to prove dualism but rather we assume that a science of the future—into which parapsychology can be incorporated—will be able to give an explanation of how psi works. But, the future may already be present. The idea of entanglement in quantum mechanics offers one way of thinking of psi as correlation and as non-causal. Both thought experiments (using the idea of pseudo-telepathy) and experimental results seem to confirm this 'ghostly action at a distance'. Therefore, since the mind-body problem arose out of and assumes dualism, I have argued that we no longer have a mind-body problem but suggested that we still have an explanation problem; philosophically, we are still interested in what it means to be a person and what our relationship is to others and to the world. Parapsychology can play a role in helping us answer these questions, especially the latter.

Let me suggest some areas that parapsychology might be concerned with in the future. Fifty-five years ago, we were interested in pursuing a dualist agenda. What agenda should we pursue now? My own quick answer to this question is a naturalistic one, one in which we do not set out to prove entities which are necessarily transcendent, where there is a double-decker universe. What does this mean? What are specific suggestions for directions parapsychology should take in the next decade? Let me mention four, all of which have already received some attention in parapsychology over the years.

1. *Cross-cultural research.* We saw that Radin suggested a notion of consciousness that fits more with Eastern thought than Western thought. Since the Western understanding of the world and of ourselves has focused for centuries on a dualistic view of the world, examining other cultures' understanding of non-dualistic approaches might be helpful. Elsewhere, I have tried to explain some of these approaches that are different from Western approaches in my rejection of Beloff's dualism (Edge, 2002).

If I am correct in my argument that we need to approach the person in a more relational and naturalistic way, we can find significant differences in concepts of self by comparing Western and Eastern cultures.[14] I (Edge, 2002, 1994, 1995, 1998) and others have written extensively on the differences between individualist and collectivist cultures, each focusing on different ways of explaining what it means to be a self or person, with collectivist cultures approaching the self in much more relational ways, consistent with the direction that the idea of psi phenomena and entanglement takes us, as opposed to an atomistic Western notion of mind. The different ways in which different collectivist cultures approach the explanation of self are vast, and good work needs to be done to help us better understand notions of self that encompass: our felt experience, ourselves in a quantum reality, ourselves given the existence of psi phenomena, and other extraordinary experiences that are part of human experience (e.g. trance possession, mystical experience).

For instance, Nisbett (2003) has argued that even basic cognitive functions such as perception are different in the East and West; that the

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[14] I will continue to use 'Eastern' and 'Western', but it should be understood that there are vast differences within both Western and Eastern cultures and so these terms are at best markers for general differences that need to be pursued in greater detail.

perceptual experience of SE Asians when they look at a picture is different from what Americans see. It is still not clear how great the differences are among the world's cultures, but it seems clear that there are basic assumptions that lead people in different cultures to conceptualize and explain their experience and the world in different ways.

We need to ask how these differences affect our investigations in parapsychology. We can explore psi cross-culturally in two beneficial ways. First, we need to see which psi phenomena appear cross-culturally and which appear only in the context of a particular culture. There are reasons to think that basic psi phenomena (such as ESP, PK, and OBEs) are widespread. Dean Shiels (1978) found, for instance, that some sort of out-of-body experience can be found in 95% of cultures (based on the Human Relations Area Files). If psi is processed through a physical process such as entanglement, it would make sense for psi to appear cross-culturally, but it is an empirical question as to how widespread these phenomena are.

Just as important as uniformity or potential universality of psi experiences are the differences we may find among cultures. For instance, compared with American and Scottish subjects in a cognitive DMILS experiment, Balinese subjects responded significantly fewer times, indicating that their attention had wandered from focus meditation (Edge, Suryani, Tiliopoulos, & Morris, 2002). We found evidence of a difference between how Westerners and Balinese conceptualized the focusing task, even when given exactly the same instructions, indicating important cognitive differences between the two cultures. There are further questions about cross-cultural differences that might be important. For instance, if belief in psi events affects its production (such as in the experimenter effect), can we see a correlation between different levels of belief in psi and psi functioning in different cultures?

The second way in which we can investigate psi cross-culturally is to examine the explanations that different cultures offer for psi phenomena. We have seen, for instance, suggestions by Radin and Rao that an Eastern explanation of the world might accord better with understanding psi phenomena. The implication of this view is that the way in which we in the West have understood and explained psi may be inadequate. So, we should find out how other cultures explain psi. Do psi phenomena, for instance, attach to minds, or to spirits, or to consciousness, or to nature? Are psi phenomena viewed as normal or

paranormal? How do psi events fit into their understanding of the world and into their understanding of the self? Given that non-EuroAmerican cultures have a tradition of relational understandings of reality and of the self, it makes sense for us to investigate their world views and how psi and how self fit into them. The direction of this paper has suggested that the West's dualist approach to psi phenomena has been flawed and a more relational view seems to be more appropriate. If non-Western cultures have developed systems to understand the self and the world in relational ways, then it makes sense to examine traditional relational systems for clues as to how to re-conceptualize psi.

2. *Psi as a Property of Nature or Personhood.* At one time in parapsychology the question of whether psi was expressed by mind or by nature was an important one. It spawned interesting research, particularly in animal psi, but the Levy affair seems to have caused a halt in animal studies. There have been a few experiments in the last 25 years that have approached this question, but we should revive this line of research. If entanglement is a fundamental aspect of quantum life, and further if some sort of monism is an appropriate world view, then it is quite conceivable that psi is a property of nature, a function of how all things work at the quantum level. But it is also conceivable that that there are parameters that affect some or all of psi functioning. Conformance Behavior Models (Edge, 1978a; Stanford, 1978), for instance, assert that there must be a teleological aspect to psi behavior, but it seems neutral whether one can understand this only in terms of human functioning or whether we might understand it as a function of nature.

I suggest a return to animal studies, as well as a development of methodologies to question whether psi may apply to plants or even to inanimate objects. The Global Consciousness Project, run by Roger Nelson since its inception, is now expressed in terms of studying consciousness. According to his theory, sufficient amounts of shared human focus or emotion or attention produce a coherence in the physical field that will bias random number generators located in different locations on earth. However, it may be a methodology that could be adapted to examine whether psi might extend to regions beyond human attention.

3. *Contribute to other scientific disciplines.* Olival Freire Jr (2006) has detailed the history of quantum mechanics from 1962 to 1985 in its

initial belief that Bell's theorem was purely philosophical speculation—from a time when an investigation of the issue was viewed as 'crackpot physics' and had nothing to do with completing quantum mechanics to the realization that entanglement was a basic empirical question that was fundamental to understanding quantum physics. Freire is careful in evaluating the claim that extra-intellectual issues (that are discussed in the sociology of knowledge) were at work in this process; she rejects more extreme views, but one aspect she discussed is important. Two of the initial empirical investigators of entanglement had training in both physics and philosophy, Abner Shimony (who was actually hired by MIT's Philosophy Department in 1959) and Michel Paty.

The point Freire made and I want to emphasize is that because of their training in theoretical areas outside of physics, they brought a perspective to the question of hidden variables and were willing to engage in empirical research with approaches that the majority of physicists rejected. Training in parapsychology also brings a new perspective to normal science. There are those among us who know both parapsychology and physics, and their understanding of how psi phenomena seem to work might suggest approaches to the understanding of entanglement or other quantum approaches that would move that field forward. For instance, if we pursue one of the questions I mentioned above, whether psi seems to be more a function of consciousness or of nature, we might be able to set parameters to how something like entanglement works. If physicists talk about entanglement, for example, as a kind of pseudo-telepathy because it seems to fulfill the action-at-a distance aspect of telepathy, it is quite conceivable that advances in parapsychological understanding might be helpful in understanding this quantum mechanical phenomenon.

But it is not just physics that might benefit. Let me mention my own field, philosophy. The relational approach to the person and the world that is found in parapsychology has been under-appreciated, in general. Phenomenology and Existentialism, as well as Process Philosophy and systems approaches, have often presented methodologies and world views that are relational. But such relational approaches have been a minority in Western thought. I can well imagine an influence on philosophy coming from a naturalized parapsychology that not only produces a robust set of data but which also stresses the relational approach to the world that parapsychology assumes. If persons are

naturally related at a deep level, there are fascinating implications for social relations and for ethics.

And, of course, as Caroline Watt (2005) has pointed out, parapsychology has already made significant contributions to mainstream psychology in our history. I believe that we can continue to make contributions in several areas, particularly to the qualitative methodology of investigating anomalous phenomena.[15]

In particular, we should continue our studies of the psychological variables of psi and psi-like experiences. Of course, we are interested in investigating whether psi is genuine and those conditions under which it appears, but it is just as informative to our field and to psychology, in general, to understand the processes involved in those experiences which either appear to be parapsychological or which a person believes to be parapsychological. As Bob Morris was fond of telling us, we should be just as sharp in understanding what psi is not and the processes involved in these experiences as we are in understanding psi and its physical and psychological concomitants.

4. Survival. I have ignored an area of parapsychology that has been central in its history—survival research. In doing so, I am not implying that this area is unimportant. Indeed, this is one area that attracted my attention to the field and was the subject of some of my first articles (Edge, 1978b; 1976) concerned with the philosophical issues surrounding survival. These philosophical issues become more difficult with a more naturalized understanding of the person. Are there ways to conceptualize personal survival within this framework, and if so, are such conceptions robust and interesting enough to have the same implications we found under a dualist conception?

Further, Stephen Braude (2005) has pointed out that our concepts of person and personal identity are "as loose and elastic as most of our concepts" (p. 231). Philosophically, therefore, it may be difficult to identify which aspects of personhood would need to survive in order for us to have a robust enough concept of survival. Would we have to have continuing first-person experience or simply retain in memory the

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[15] Irwin and Watt (2007) mention an example of the positive influence of parapsychology on another discipline. Medical investigators have been the principle investigators of Near Death Experiences, but many of these experiences have an out-of-the-body component. The authors argue that in their initial phase NDE experimenters intentionally avoided the OBE literature for fear of being connected with the 'occult' (p. 173).

experiences that we had before death? What kind of agency would suffice for us to retain the idea of a postmortem person? While our ordinary loose concept of person suffices for ordinary circumstances, it is not clear that it would be sufficiently clear for the extraordinary case of survival (for us who have experienced only pre-mortem existence).[16]

In addition to these philosophical issues, there are methodological ones that have plagued survival research. I do not think that we have been able to solve the problem of the Super-Psi Hypothesis (i.e. the This World Hypothesis); that is, I do not think that we have been able to designate what would count straight-forwardly as evidence for survival and not be thought of as an example of psi on the part of the living.

However, having pointed out these problems let me say that there is no more fundamental set of issues concerning parapsychology and the nature of the person than these. Parapsychology always has conceived of itself as contributing something important about what it means to be a person; we should continue to keep this a goal of our work.

Conclusions

I began the essay with an intentionally provocative statement—that we no longer have a mind-body problem in parapsychology—having considered the history of parapsychology and of philosophy over the last several centuries. Given the data from parapsychology and the idea of entanglement from quantum physics, I have suggested that we should no longer think of the mind-body problem as a problem, although there are certainly philosophical issues to be discussed around our understanding of personhood. But, given the dualist underpinnings of the history of the mind-body problem, we should focus our questions elsewhere.

Considering the history of parapsychology, this may be a particularly difficult thing to do for many interested in parapsychology. I know that it was the dualist interpretation of psi events that first drew me into parapsychology, and I am sure that this is the case for many of you. However, much like significant changes in our folk psychology, I think that there has been a significant change in thinking among many

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[16] Lynne Baker (2007c) has argued that a concept of person consonant with the one proposed in this essay would be most consistent with the possibility of resurrections. It distinguishes between personal identity and human identity and supports the possibility that the former could survive.

parapsychologists. As opposed to trying to assert a dualism, and in doing so pitting ourselves against science, I believe that both science and parapsychology have changed. Science began moving from a crude materialist stance because of the findings in quantum mechanics. Because parapsychology virtually defined itself in reaction to this dualism, parapsychologists now have shown more flexibility in conceptualizing how we explain our data. Some will undoubtedly choose to remain dualist in some fashion, perhaps with a notion of consciousness that is in no way material, but it is also possible to adopt other perspectives. But, I believe that when we participate in more cross-cultural studies, including that culture called quantum mechanics, we will find more interesting and productive ways of thinking about how we live and how the world works. The number and creativity of theories being presented in parapsychology suggest that parapsychology has a solid future.

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EMPIRICAL PICTURES OF TIME

RICHARD SHOUP

Empirical Pictures of Time[1]

It is probably fair to say that nothing has been more perplexing or controversial in the study of psi phenomena or in modern physics than the concept of *Time*. We have measured Time using the motions of the heavenly bodies, the changing of the seasons, the swinging of pendula, the beating of our hearts—and today the oscillations of tiny particles to an accuracy of better than 1 second in 60 million years (NIST, n.d.). We often feel that Time is of supreme importance in our daily lives, and to prevent 'wasting' it even wear a measuring device on our wrists. Yet no one seems to know exactly what Time is or what brings it about.

Time seems to 'flow' only in one direction, and we use forward cause-and-effect reasoning pervasively, yet experiments sometimes seem to show influence and information moving backwards in Time. The fundamental laws of both classical and quantum physics are symmetrical in Time, yet we routinely expect our experiments to produce their results, not the other way around. To make matters worse, the concept of Time is seriously tangled and confused with those of causality, randomness, and even consciousness.

In this paper, we discuss the meaning of Time and its importance in physics and in psi research, some relevant experimental evidence, and an explanatory theory that does not seriously conflict with existing physics. We also attempt to untangle things a bit, and suggest a fundamental definition and origin for Time itself. Finally, we make some suggestions regarding future research directions.

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[1] Two meanings of the word 'empirical' are intended by the title: 1) 'Derived from observation or experiment', that is, suggested or observed in experiments in the real world; 2) 'Capable of being verified or disproved by observation or experiment', i.e., suggested by theory and testable in the real world. Both meanings have relevance here, as they encompass the complementary activities of derivation of theoretical hypotheses, and testing them via experiment. The word 'time' is capitalized frequently to emphasize it as a special concept.

Time in Physics

> To be perfectly honest, neither scientists nor philosophers really know what time is or why it exists.
>
> *Paul Davies, physicist*

Symmetry of Physical Laws

In general, the laws of physics, both classical and quantum, are invariant under time reversal. This means that the equations representing these processes are symmetrical in the time variable, and any solution for $+t$ is also a solution for $-t$.

Despite these strong time symmetries, we routinely assume only forward causality in much of our thinking about the real world, and in nearly all of the experimental sciences. Yet, despite our everyday experience, there is a considerable body of both anecdotal and scientific evidence suggesting retrocausal phenomena. Confusing matters further, the part of quantum theory that would prevent actualization of time symmetry and backward causal effects remains the most perplexing and poorly understood aspect of the theory, and is still enigmatic and contentious nearly a century after its initial discovery—*measurement*. To better understand quantum measurement, and especially its relationship to Time, we must delve a little more deeply into the basics of quantum theory.

Asymmetry of Quantum Measurement

Quantum dynamics in the orthodox formalism includes two entirely different types of evolution or change—*unitary evolution* described by the Schrödinger equation (linear, reversible, lossless), and *measurement* or projection (nonlinear, irreversible, information losing) by the Born Rule—the notorious 'collapse of the wavefunction' (Peres, 2003).

From a superposition of two states of a binary variable such as

$$\Psi = \frac{1}{\sqrt{2}}\left(|\mathbf{0}\rangle + |\mathbf{1}\rangle\right),$$

one alternative is selected (projected) during measurement according to the implied probability distribution (Figure 1). The outcome is traditionally assumed to be *entirely random*, causeless, and thus unpredictable. Only statistical statements can be made about the result, in this case a uniform distribution of **0** and **1** states. Once the

measurement is complete, the resulting state has become a classical one, and subsequent measurements of the same variable will continue to give the same result. This scenario applies whether the situation involves the spin (up or down) of a particle, the path through a two-slit arrangement, or any other binary observable.

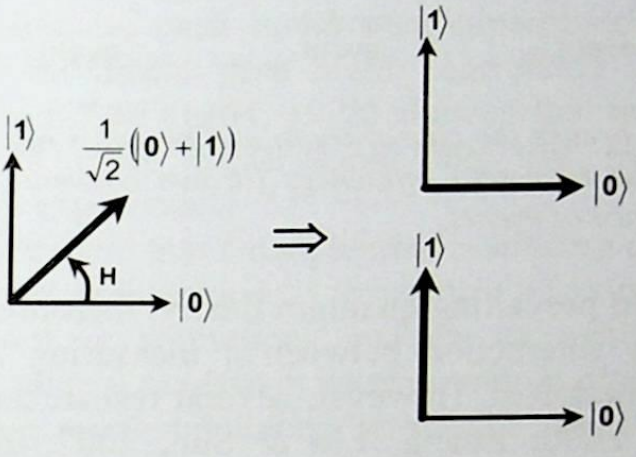

FIGURE 1.
Superposition and measurement as vectors in Hilbert space. A particle initially in state ***0*** *is placed in superposition of* ***0*** *and* ***1*** *by a unitary transformation (rotation) H. Measurement, according to orthodox theory, projects the superposed state non-unitarily and unpredictably onto* ***0*** *or* ***1****.*

This non-unitary collapse, or *decoherence*, of the wavefunction due to measurement has been the most vexing and controversial aspect of quantum theory from its inception, and yet it has been supported by many experimental tests to great precision. A wide variety of interpretations of the quantum formalism have been proposed to mitigate the practical and philosophical difficulties of this part of the theory, but none have met with complete acceptance. We give our own modified version of quantum theory below, and discuss its potential explanatory power.

The consequences of the assumed 'collapse' are severe. We focus here on two of them: 1) the prevention of backwards or *retrocausal* influence due to non-linearity; and 2) the introduction of a fundamentally random element at the core of physical reality. By a 'random' process or event, we mean one that is *uninfluenceable* (no inputs, no external effect possible) and *unpredictable* (no internal memory, no pattern possible). A random event is *isolated from the past*. While unitary evolutions are time-symmetric, and thus inherently permit reversed or backward influences to propagate, a lossy or random

(causeless) event breaks the dependency connection (causal chain) and prevents backwards influence (Figure 2).

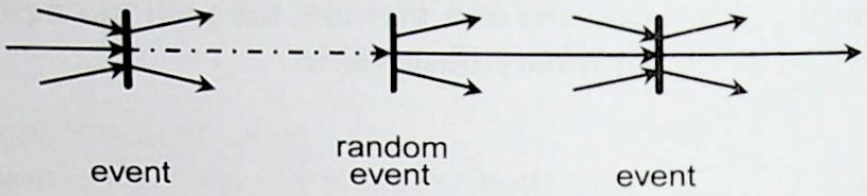

FIGURE 2.
A random event breaks the causal chain and prevents any possible backwards influence. Arrows represent influences (drawn conventionally forward) that connect a sequence of events.

According to prevailing quantum theory, measurement is thought of as a two-party interaction between a measuring apparatus and the system being measured. However, several researchers (Cerf & Adami, 1996; Shoup, 2006; Zhang, Liu & Sun, 2002; Zurek, 1981) have postulated that quantum measurement should properly be considered as a *three*-way interaction among the measuree (the system of interest), the measurer (the experimental apparatus), and the environment (everything else), with each described as a fully general quantum object. A measurement really just entangles the measured system with the measuring apparatus and the rest of the environment, spreading prior entanglements around, a process known as *decoherence*. In this view, classical correlations and apparent randomness seem to appear in quantum measurement *only when the environment is ignored*. This is quite significant in that, *by this theory, measurement is entirely unitary*, reversible, lossless, and thus can allow backwards causal influence, at least in principle.

In orthodox theory, the result of a quantum measurement is random. But if a 'random', unknowable, non-deterministic choice did in fact occur in measurement, we would naturally ask where the information came from that determined this choice, and how did it enter the situation? Before the measurement, there are two possibilities extant, and afterwards one has been chosen and realized. Does a bit of information come from God himself for each 'collapse' of a binary variable? This choice requires a bit of information, a reduction in entropy of the measured system. If, however, measurement is actually unitary, we can give up the assumption of fundamental randomness, and look for the source of the choice elsewhere, see below.

Entanglement

Quantum *entanglement*, a special state available only in the quantum realm, has often been suggested as a possible mechanism to explain anomalous phenomena such as telepathy and clairvoyance. In an EPR (Einstein, Podolsky & Rosen, 1935) experiment, for example, two particles become entangled by an initial interaction, fly apart an arbitrarily large distance, each in its own superposed state, and yet when measured individually give highly correlated (but apparently random) results. John Bell (1964, 1978) showed that no local realistic theory can explain these correlations, and this result has been confirmed by many experiments.

It is now well accepted that entanglement *cannot* be used to transmit information, but can only impose a constraint of correlation. In psi research, correlation is sometimes enough to explain anomalous phenomena where several random or nearly-random data sequences are being compared (e.g. symbol guessing between a subject and a random target generator). But correlation alone may not be enough to explain other phenomena where real information transfer may be required (e.g. remote viewing of distant current events).

Entanglement is time insensitive in that either member of the entangled pair can be measured first, and the correlation will still be exhibited in comparison of the results. (In fact, by Special Relativity, it is possible for two observers to move so that each thinks they measured first.) Some theorists argue that EPR is best explained using a 'zig-zag' of forward and backward Time (Costa de Beauregard, 2001), while others (Suarez, 2003) claim that quantum entanglement is best thought of as occurring outside of Time entirely. Still other physicists insist that Time is merely an illusion or a derivative of something more basic (Barbour, 2001; Hellmann *et al.*, 2007; Price, 1996). Significantly, it has also been shown that *entanglement in the present can be created by means of an interaction in the future* (Elitzur & Dolev, 2006; Elitzur, Dolev & Zeilinger, 2002). This latter possibility has important implications in psi research, as will be discussed in more detail below.

Time in Psi Phenomena

> The notion of time has to be introduced if only to distinguish cause from effect: Cause must always precede effect.
>
> *Gerard 't Hooft, physicist, Nobel laureate 1999*

There are two significant ways in which Time is relevant, even central, to the study of psi: 1) some phenomena such as precognition directly contradict our usual notions of forward causality, and strongly suggest that information sometimes flows backwards in Time—a better understanding of Time is essential in explaining these effects; 2) other phenomena such as present-time telepathy, while not directly evidencing backwards Time influence, may be better (only?) explained using a combination of backwards and forward influence. Both of these relevancies are explored further below.

Characteristics of Psi Phenomena

From the significant body of experimental evidence, a few striking and salient properties of these anomalous phenomena have emerged. We list a few of them for reference, but without thorough discussion, since the first has special significance for us here.

1. Time/order independence (clairvoyance vs. precognition)
2. Complexity independence (goal orientation)
3. Selectivity (resonance, 'tuning in')
4. Experimenter dependence (attitude, audience)
5. Small effects, difficult to replicate (at present, and perhaps fundamentally)

Perhaps the most shocking, problematic—and thus the most profound—characteristic of psi phenomena listed above is their *apparent independence of Time*, including especially precognition and even retroactive psychokinesis. In a common example, it does not seem to matter much whether a remote viewing takes place before, during, or after the viewed event (Braud, 2003; Radin, 1997; Targ & Katra, 1999). Persistent evidence suggests strongly that thinking about these phenomena in terms of the usual notions of cause and effect is likely to be counterproductive—and may in fact be *the* major impediment to a deeper understanding.

Evidence of anomalous correlations (which are in principle timeless), as well as retrocausal effects that apparently involve information transfer, are relatively common in the literature. Schmidt (1976, 1987, 1993) conducted innovative experiments that appeared to show retrocausal effects, including retroactive PK on pre-recorded targets. Radin (1997a, 1997b) and Bierman and Radin (1998) have

shown presentiment effects repeatedly in experiments involving measures of autonomic response.

Many specific experiments in the literature have been designed to elicit and to test retrocausal effects, but for further hints about an explanatory mechanism, we now look at two examples of retrocausal effects that are not typical, nor even intentional.

Unintentional Backwards Influence on Random Processes

A network of random number generators (RNGs) has been developed by the well-known Global Consciousness Project (GCP), and has been in continuous operation since 1998. Approximately 70 generators exist today scattered around the globe, each sending 200 random bits per second back to a central server for storage and later analysis. Details, analysis methods, and all generator data are available on the GCP website (Nelson, n.d.). According to the GCP, behavior of the network sometimes deviates significantly from chance, especially around times of notable human events.

In particular, highly unusual behavior was seen in the network on and around the tragic day of September 11, 2001. This behavior has been discussed in detail elsewhere (Nelson, 2002; Nelson & Bancel, 2006), so we present here only one less-publicized view of the data.

On and around 9/11, over 30 random generators ('EGGs') were operational and functioning normally.[2] Figure 3 shows the standard cumulative χ^2 statistic for several groupings of 32 generators for the full day. Data have been aligned (pinned to zero) at 0600 hours (this begins a period of relative quiescence for all four curves and significantly precedes the key events of the day), and filtered with a one-hour sliding window.

As can readily be seen, beginning approximately four hours before the events, variance of the generators dramatically increased. Four different groupings are shown to emphasize that significant deviations were seen simultaneously across most if not all the generators, not just one or a few. The disturbance continued for more than 12 hours, gradually subsiding later in the day similarly among the groupings. No deviation of this magnitude has been seen anywhere in the entire database collected from the GCP generator network. Note that the four-

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[2] Note that no disruption of data collection was observed during or around that day, and no data was lost or compromised due to the social upheaval that took place following these events.

hour prior response rules out any of the usual prosaic explanations such as electromagnetic or other known effects on the generators or the data due to unusual human activity following the tragic events of that day.

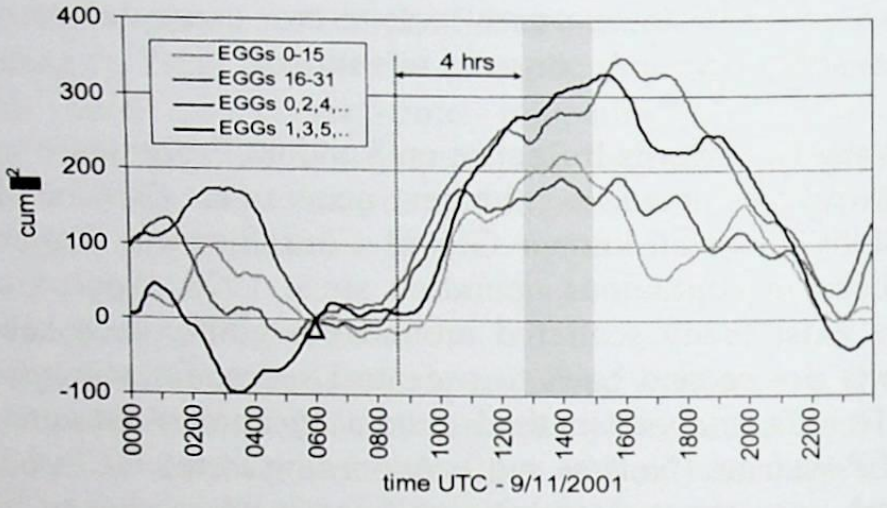

FIGURE 3.
Correlations among 32 random generators (EGGs) on September 11, 2001, combined in four groupings of 16 each for comparison. Deviations beginning 4 hours prior to the principal events (gray area) of that day suggest retrocausal influence, and thus no prosaic explanation. Curves are pinned to zero at 0600, a common quiescent point.

It is difficult to interpret such behavior in many supposedly random devices, but evidence has been amassed by the GCP project from this event and many others for correlations with global human concerns (Nelson, Radin, Shoup & Bancel, 2002). For our purpose here, we simply emphasize that 1) these random devices deviated significantly from expected chance behavior during the events of 9/11, and 2) that the deviations began well prior to the events with which they were presumably associated.[3] We take this behavior as (anecdotal, not formal) evidence of *mutability in supposedly random uninfluenceable processes*, and of *retrocausal influence* upon them, *without any direct intentionality* or focus on the generators by any humans (except perhaps the project researchers and observers after the fact).

Were the generators somehow altered by the events or the outpouring of attention and emotion on that day, or were they

[3] May and Spottiswoode (2001) have claimed that the generators behaved normally on this day, but if the report is read carefully, it actually gives strong evidence for the reality of the effect! See especially Figure 10.

influenced by a retroactive experimenter-mediated effect, or something else? Since there is no evidence or known mechanism whereby this attention and emotion could have *directly* affected the RNGs, it seems that the only remaining plausible path for this influence to have taken was through the one place where the data are collected together and any deviations become apparent—the collation and analysis of the independent data streams by the experimenters—and then backwards to the data generators themselves.

Unintentional Backwards Influence on Subjects

Another unexpected precognitive effect related to 9/11 was seen in a completely separate dataset. On our public *GotPsi?* experimental web site (Shoup & Radin, n.d.), a simple 5-choice card guessing experiment has been running since August 2000. As of August 2008 over 30 million trials have been recorded, and the site is currently logging N = 15-20,000 trials per day from users all over the globe. Details of the "Card Test" and several other experiments on the web site along with some preliminary results can be found in an early report by Radin (2002).

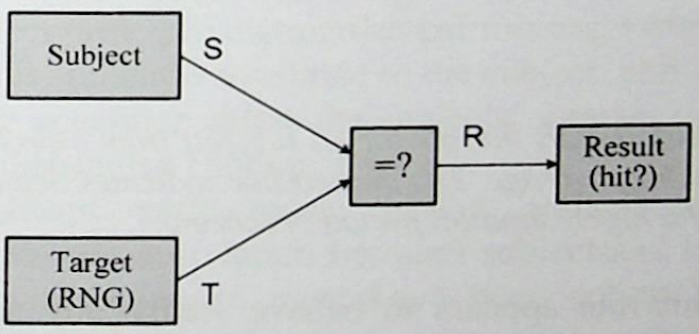

FIGURE 4.
A simple forced-choice card-guessing experiment.

In the on-line Card Test, shown schematically in Figure 4 and pictorially in Figure 5, the subject is asked to guess which of five displayed playing cards has a picture "on the other side" by clicking on that card. His guess (S = 1..5) is compared with the target card (T = 1..5) and the result is either a hit (R = 1) or a miss (R = 0). The result of each trial is shown to the user, and runs of 5, 10, 25, or 100 trials are tallied and the odds against chance displayed.

Figure 6 shows the daily hit rate for all subjects (typically 100-200 users each day) over two years of operation of this experiment, March

2001 to March 2003, filtered with a sliding average window of 30 days, and normalized as $z = (n_{hit} - n_{exp})/(\sqrt{N}*0.4)$.

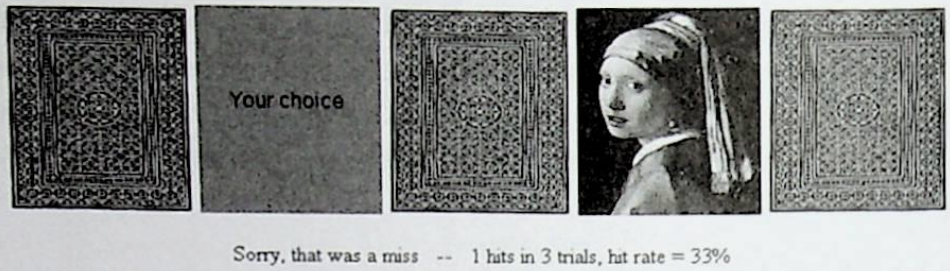

FIGURE 5.
One completed trial in the on-line Card Test.

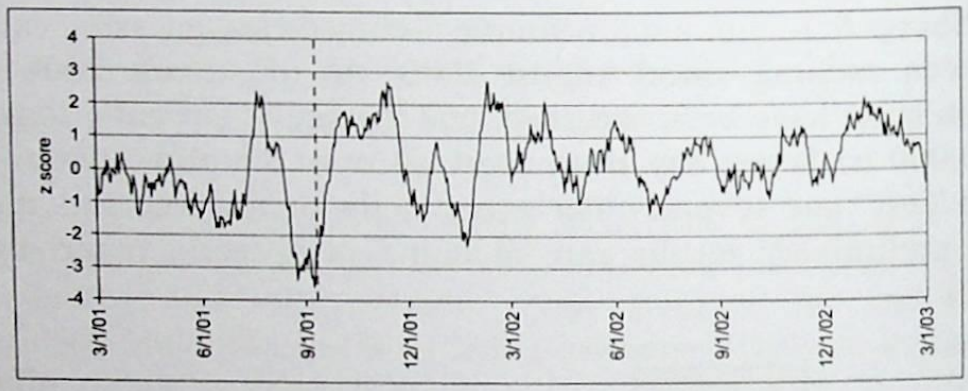

FIGURE 6.
Card Test normalized daily hit rate for all subjects from March 2001 to March 2003, 30-day trailing average. The dashed line indicates September 11, 2001, and the center of a highly unusual period of scoring.

The daily hit rate appears to behave statistically as the expected random walk except perhaps for the striking peak and deep notch in July through October of 2001. For unknown reasons, the hit rate rose well above $z = 2$ (max $z = 2.4$, $p = 0.0082$) and then fell dramatically during the period just prior to September 11, 2001, staying below $z = -3$ (min $z = -3.6$, $p = 0.00016$) for over two weeks.[4] (This almost continuous drop of nearly 6 standard deviations is itself highly unlikely, of course, no less given its timing.) Then almost immediately after 9/11, the hit rate rose steeply again, and returned to ostensibly

[4] These curves are the result of a trailing average filter, so each point sums data from the entire previous month. Bearing in mind this caveat, the behavior of the data and the sharp movements in the curve are striking.

random behavior, which continues to this day. Nowhere else in the entire database of the Card Test is any similar excursion found.

Although all of this behavior could be purely due to chance, it seems that two weeks before 9/11, subjects participating in the Card Test suddenly became quite poor at guessing cards, selecting incorrect cards far more often than chance would predict. Remarkably, almost immediately following the key date of 9/11, performance vastly improved for a time, and then returned to varying randomly in seeming accordance with expected probabilities. If it is assumed that results during this highly unusual period are somehow related to the tragic events of September 11, 2001, then they are precognitive by several weeks, and constitute evidence for retrocausal influence on this experimental task completely unintended by anyone.

Unlike the GCP random generators, the target distributions in the Card Test during this period show no significant departure from chance expectations. This does *not* necessarily mean that the (single) RNG in this case was not perturbed, but only that the resulting target distribution continues to appear random. So another hypothesis must be that the generator and the subject were somehow anti-correlated during this period, each still appearing approximately random by itself. Note that we cannot in principle distinguish psi missing (where information from the target is somehow available to the subject, and is used to miss) from PK on the generator unless the subjects' guesses are significantly non-randomly distributed (Shoup, 2002).

Both significant extra-chance hitting and missing constitute evidence for non-zero correlation between subject and target, and thus some coordinating influence must have been acting between them. Since there is no known channel connecting subject and target generator directly, and no plausible common influence in the past, it is again suggested that this anomalous correlation is mediated by the only remaining path available—their interaction in the future result comparison and analysis, as discussed below. This interaction could presumably produce such correlations whether S is chosen prior to or after T, and we separate these two cases below to look for any difference.

Evidence of Time/Order Independence: Offset Targets

Looking more deeply into the previous example, it is the case that target generation in the Card Test is actually done in a way that allows further testing of the hypothesis of the Time independence of these

anomalous effects. Unknown to the subject, several different means of target generation are employed in the Card Test on a trial-by-trial basis, as depicted in Figure 7.

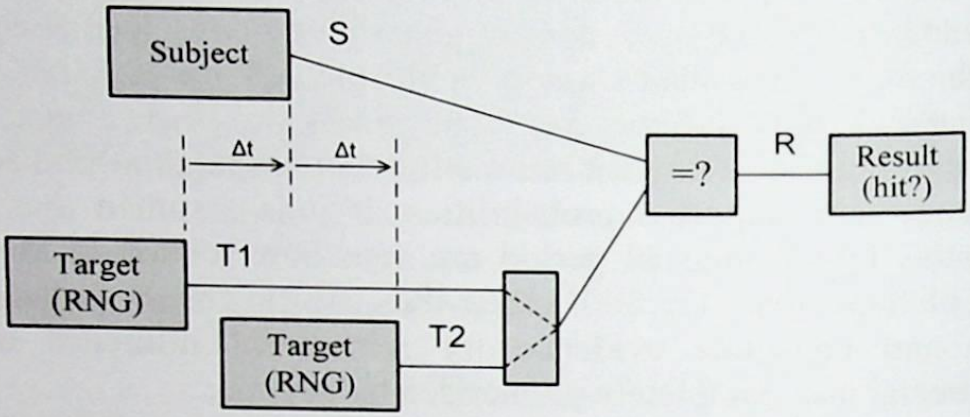

FIGURE 7.
Card Test experiment with target card T1 chosen before, and T2 after, the subject's guess.

For each trial, *two* target cards are potentially generated, one prior to the subject's guess (T1), and one following it (T2). An additional random choice determines which of the two targets will be selected and used to compare with the subject's guess. Figure 8 shows the daily hit rates separately for those trials using the T1 targets and those using the T2 targets.

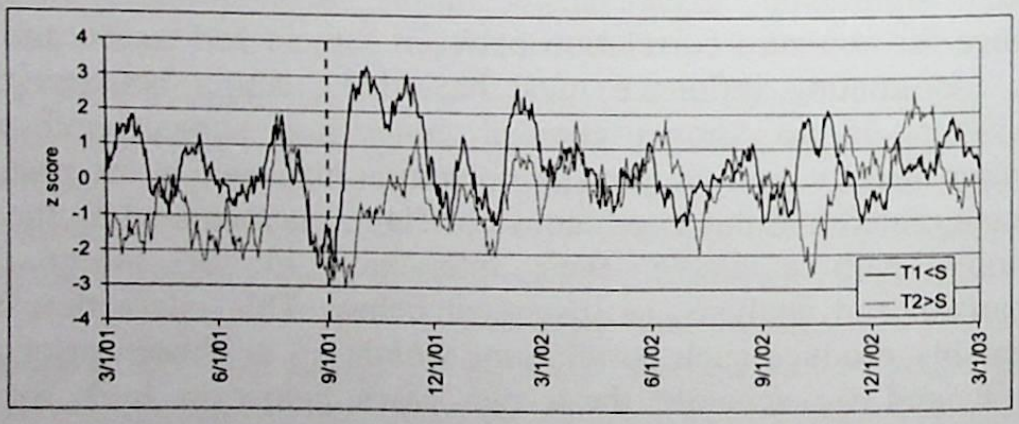

FIGURE 8.
Normalized Card Test hit rate for trials with targets T1 (generated before the subject's guess) and T2 (afterwards). Note the unusual behavior of the two curves surrounding 9/11/2001 (dashed line) and through 2002.

As with the combined daily hit rate shown previously, each of these curves represents the success rate for the subject's guess matching a randomly generated target. By the null hypothesis (chance behavior), these curves should represent two *independent* random walks. Contrary to the expected behavior, however, beginning in July 2001 the two hit rates behave remarkably similarly and apparently non-randomly for most of an entire year. They rise together to a peak near $z = 2$ in mid-July, then fall closely together to a low below $z = -3$ through the beginning of September. Then shortly after 9/11, the T1 (prior) hit rate rises dramatically while the T2 (post) hit rate remains low for several more weeks, and rises more gradually. The curves appear to rejoin in November, track each other closely again throughout the first six months of 2002, and then begin to diverge and revert to the expected independent random walks (which continue to the present day).

Anomalous correlations are apparent between the T1 and T2 hit rates, and show variation relating to the special date of September 11, 2001. During a one-year period surrounding this date, the hit rates for prior and post targets behaved remarkably similarly and anomalously, suggesting that a common influence was affecting performance in both conditions, with a time constant measured in weeks—yet the subjects were only intending to guess correct cards throughout. But interestingly, there is also a clear (but temporary) distinction between the two conditions for a period of about two months just after 9/11, suggesting that the time offset was somehow discernible to the subjects or affecting the targets during this period.

The data from these two experiments is tantalizing, not only for the precursory effects, but also the long-term effects reminiscent of those seen in the GCP data analyses (Nelson, n.d.). Further study is certainly indicated. We can only hope that with improved analysis techniques, calamities of the magnitude of September 11, 2001 will not be necessary to test new hypotheses.

Can Physical Theory and Psi be Reconciled?

> For us believing physicists, the distinction between past, present and future is only an illusion, even if a stubborn one.
>
> *Albert Einstein, physicist, Nobel laureate 1921*

Most physicists still assume—largely without examination—that psi phenomena cannot be taken seriously because they *appear* to violate well-confirmed and accepted physical laws, and especially offend our notion (assumption) about forward cause and effect. In particular, precognition would seem to require information transfer from the future, supposedly a logical impossibility. In fact, it has been asserted by many scientists that a *complete rewrite* of physics would be necessary if these phenomena are real (e.g. Gell-Mann, 1994; Kaku, 2008a, 2008b). But as we suggest below, this is simply not so, and only modest changes in the foundations of quantum theory are needed to allow and to explain many of the psi phenomena that have been observed consistently in laboratory experiments as well as anecdotally for many years (Broderick, 2007; Broughton, 1992; Radin, 1997).

Some theories that have been created to deal with the stranger aspects of quantum phenomena could perhaps permit psi as well. Transactional interpretations of quantum mechanics, the possibility of entanglement due to future interactions, the idea of unitary measurement, all mentioned above, contain aspects of both forward and backward interaction. Observational theories of psi by Houtkooper (2002, 2006) and others argue that the outcome of a measurement remains indeterminate until later constrained by a comparison with another quantity. As also mentioned above, a proper understanding of Time may be needed not only to explain precognition, but also to explain other phenomena that do not appear at first to require any backwards influence. With particular reference to the present subject of retrocausation, the volume of papers produced from the 2006 AAAS conference *Frontiers of Time: Retrocausality—Experiment and Theory* contains many relevant contributions. (See references.)

Analysis of a Simple Experiment Involving Anomalous Correlations

Let us return to the simple guessing game in canonical form given above in Figure 2, but now considered in the abstract. Typically, the subject tries to make his guess (symbol S) match a target generator (symbol T), and an equality test produces result (R) indicating success (1, a hit, or 0, a miss).

Suppose an experiment of this type has been conducted, and the results show a hit rate higher than that expected by chance. We assume that all forms of ordinary signaling or leakage between subject and the target generator have been prevented, and ask how this physical anomaly could be explained. Consider four possible paths by which

information or influence could have traveled to bring about this result, as highlighted in Figure 9.

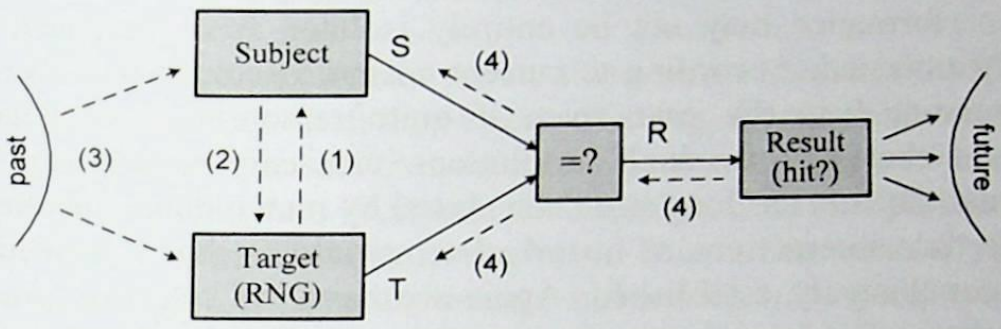

FIGURE 9.
A simple forced-choice experiment, with four possible paths of influence (dashed) by which an anomalous correlation between Subject and Target could have been brought about.

1. *Influence directly from T to S.* The subject was somehow able to sense the target symbol directly and adjust his guesses accordingly. This was at one time a typical explanation involving clairvoyance (S chosen after T) or precognition (S chosen before T). The associated mechanism would have to include a previously undetected means of information transfer such as a new force or field, and an unappreciated human sensitivity or sense organ. This hypothesis seems highly unlikely, especially since there may be many target generators in the world from which to select. Moreover, if the target is chosen after the guess, then the subject must possess some precognitive ability, and this would require information flow backwards in Time, contrary to the 'law' of cause and effect.

2. *Influence directly from S to T.* The subject was somehow able to affect the target generator directly. This is the usual psychokinetic explanation, again difficult to mechanize, yet apparently consistent with many careful real-world experiments (Jahn & Dunne, 1997; Schmidt, 1993). Quantum processes in the RNG that are now considered to be fundamentally random would have to be influenceable, and information conveyed in some currently unknown way. Again a new or unnoticed force or field would seem to be necessary—unlikely in the face of existing and well-tested physical theory—along with the selection problem as above.

3. *Correlations due to past interactions.* The subject was somehow able to make use of correlations or entanglement between himself and

the target generator due to interactions in the past. Or perhaps the subject somehow knows something about likely target sequences due to past common origins. After all, real RNGs have a physical history, and their performance may not be entirely isolated from the past, as is usually assumed. According to current cosmological theory, all matter has interacted in the past, even if quite remotely. It is generally assumed that any residual correlations that existed between well-separated objects has long ago been erased by unavoidable decoherence and particle interactions at historical temperatures, but this seems far from conclusively established. Again we have the selection problem, since there may be many such generators. And while this hypothesis might explain clairvoyant or precognitive effects, some way to *affect* the target generator would still be required in order to explain psychokinesis implied by any deviations from randomness seen in the target sequence.

4. *Correlations due to future interactions*. The subject was somehow able to make use of correlations or entanglement between himself and the target generator due to interactions in the *future*. It might be that the presence of anomalous results in the future exerted influence backwards through the equality constraint to bring about such correlations, with S influencing T or vice versa or both. Or perhaps the subject and target streams became entangled via their interaction at the equality test, while both streams remained independently random.

It is hypothesis 4 in which we are most interested. For hypotheses 1 and 2, an additional unknown path of influence or information flow would be necessary between S and T, and this seems to be in serious conflict with well-established physical theory as well as experimental data. However, alternatives 3 and 4 may not require any direct or indirect information transfer between S and T, instead relying on correlation alone, and thus do not imply such a mysterious path or the difficulties associated it.

In addition to providing the necessary property of Time independence, hypothesis 4 (correlation due to future interactions) can arguably account for many if not all of the so-called telepathic, clairvoyant, precognitive, and psychokinetic anomalistic effects apparently exhibited in such experiments.[5] It provides good agreement

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[5] We hope the reader will agree that the standard terminology of telepathy, clairvoyance, precognition, and psychokinesis is antiquated, misleading, and in reality an impediment to progress. These phenomena are not separable, and by

with the characteristics of psi listed above as well, without requiring new paths or mechanism or any major insult to well-accepted physical law—*if* we allow symmetrical (bi-directional) causal influence as argued above.

From this theoretical viewpoint, there is no essential difference between precognition (a future event affecting the past) and retro-PK (the same). We emphasize that the latter does *not* imply *changing* any previously recorded data (Schmidt, 1976), but retrocausally influencing how it was generated 'in the first place'. This is the essence of Time independence or backwards-in-time influence. (Compare with the Transactional Interpretation of quantum mechanics (Cramer, 1986, 2006), and Decision Augmentation Theory (May *et al.*, 1995).)

Order Independence and Quantum Nonlocality

There is a non-obvious connection between the Time independence shown in the simple abstract experiment above and the Time independence associated with quantum entanglement and measurement. With a simple rotation, the experimental diagram above gives something quite suggestive of an EPR configuration (see Figure 10). If we constrain the Result stream to be all 1s (hits), then the S and T streams must be perfectly correlated (identical), just as in the case of the spins of an entangled EPR particle pair measured at the same angle.

In the usual EPR arrangement, this constraint on the 'Result' would be considered as a *preparation*, since it creates the entanglement, and occurs prior to measurement of the S and T spins. But the real point here is that the entire situation *in either rotation* can be viewed in terms of timeless bidirectional relations, and the order of the S and T measurements is not relevant to their correlation. Because of the equality constraint, the S and T streams are jointly constrained no matter what their time sequence. With respect to the simple guessing experiment above, the usual EPR arrangement is just a special case, namely complete entanglement at the 'source' (R) followed by identical measurements at S and T, in either order.[6] (In this analogy, we are

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all indications are very likely to be explained by variations of the same physical mechanism.

[6] While the similarity here is not trivial, we hasten to add that this simple binary configuration does not capture the full generality of the non-local

assuming the subject to act essentially as an RNG too, producing an approximately random distribution of guesses.)

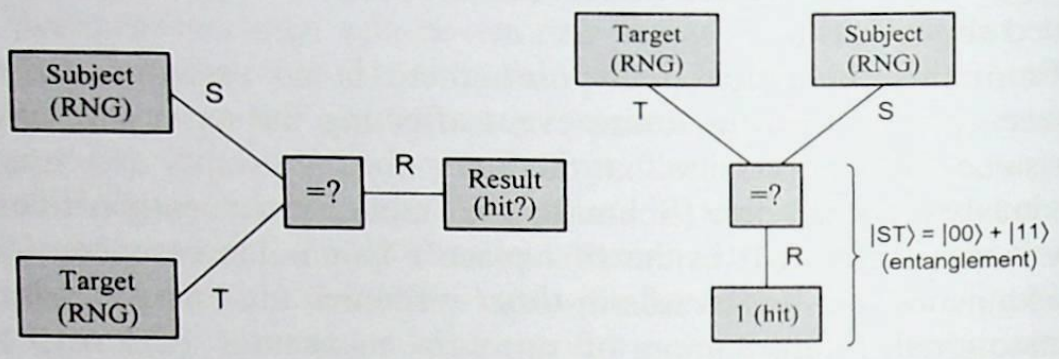

FIGURE 10.
Rotation of the simple psi experiment suggests an EPR configuration.

Randomness and Causality

A quantum-random process or device is considered to be completely uninfluenceable and unpredictable, a box with no inputs, producing only an output stream. In view of the preceding discussions, where we theorize that all interactions are bidirectional and relational, causal influence can be thought of as flowing backwards in Time as well as forwards. In this view, an RNG is not solely an output device, but can be affected backwards in Time from observations and other events to which it is connected. By this theory, the *output* of a truly random source is actually more like an *input*, and its internal mechanism is vacuous or merely cooperating.

We conjecture that the output of a real-world quantum RNG usually appears to be totally random because in typical situations it is connected to macroscopic devices operating at room temperatures, and the apparent randomness is a reflection of the highly disorganized microstate of the observing equipment. We know from experiments that indeed such a device *can* be influenced, and by far the most parsimonious explanation seems to be that the device is being manipulated *via its output*. Thus to understand deviations from randomness seen in an RNG, we should focus not on its inner mechanism, but on retrocausal influences from the attached measuring processes and observers.

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phenomena demonstrated in EPR arrangements and relevant to Bell's Theorem, notably in rotations of the measuring angles.

What is Time, and What is a Clock?

> Time is nature's way of keeping everything from happening at once. Space is what prevents everything from happening to *me*.
>
> *(attributed to) John Archibald Wheeler, physicist*

We return then to the central question of the nature of Time. Fundamentally, *Time is an indicator of change*. Time "passes" when something changes, or to be more specific, when the value of a variable is different from its "previous" value. The previous value must be something we have remembered, i.e. stored in a memory or delayed from the "current" value. So, in looking for change in some variable, and thus the passage of Time, we constantly compare the value of the variable to its value remembered, or "last time", see Figure 11.

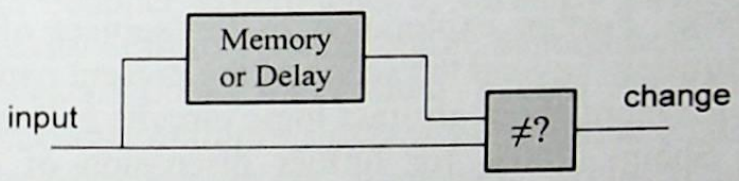

FIGURE 11.
A change detector compares its input value with the previous stored value. When the values become unequal, Time has 'passed', as with a single tick of a clock.

The *memory* element needed here may be created in abstract terms by a self-confirming (looped) logic expression such as $\mathbf{X} = \mathbf{X}$, or, using complement (inversion, logical NOT) as the simplest basic logical operation, $\mathbf{X} = \sim\sim\mathbf{X}$. Interestingly, the similar but paradoxical self-denying logic expression $\mathbf{X} = \sim\mathbf{X}$ creates a *clock*, an oscillator. Figure 12 shows two simple logical circuits with even and odd feedback that implement respectively a memory and a clock, the most fundamental and essential time-related elements of any computer.

The memory element merely retains its current value indefinitely, never changing, while the clock (oscillator) is a source of constant change, its logical values akin to the tick-tock of a mechanical clock. By counting these changes, we can determine a discrete time interval or 'length' of time, just as a conventional watch or clock does, whether mechanical or electronic.

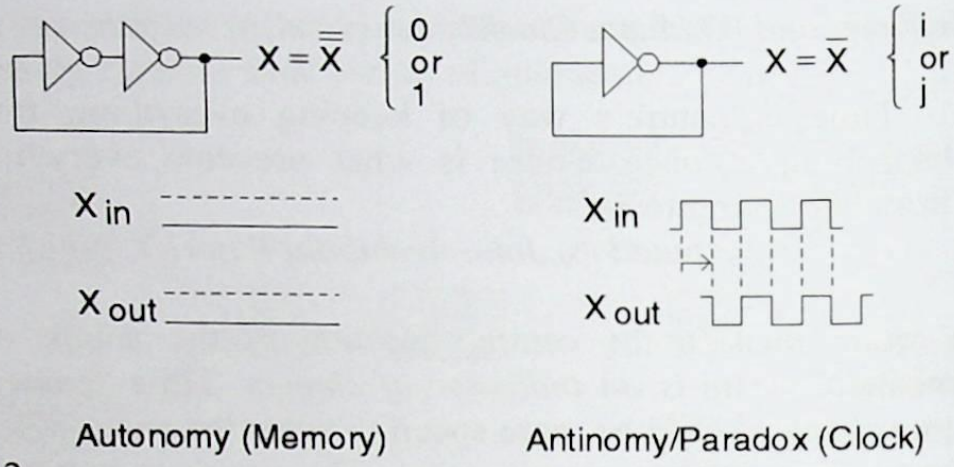

FIGURE 12.
Memory and Clock as simple logic circuits. The triangular elements represent inversion, or logical NOT.

These simple abstract models are suggested as an indication of the ultimate origin of Time, and how it can be generated and understood at the deepest level. Further exploration of the subject of Time at this level is unfortunately beyond the scope of the present paper. See Shoup (1993, 1995) for more about abstract logic circuits and their relation to physics, and Shoup (2008) for further discussion of discrete time, clocks, motion, and relativistic velocities derived from notions of primitive 'pre-space' and 'pre-time'.

Next Steps: Experiment, Theory, and Acceptance

> The way to do research is to attack the facts at the point of greatest astonishment.
>
> *Celia Green, philosopher, psychologist*

Toward New and Illuminating Experiments

Allowing for the possibility of retrocausal influence, the potential exists for some new directions in experimentation, with emphasis on Time and related physical variables. Suggested:

1. Further analyze already-conducted experiments (especially large ones such as the GCP network and GotPsi) for retrocausal effects on RNGs and on subjects. These could include investigating probable vs. actual futures, propagation of influence in Markov chains, and backward information transfer, even if unintentional.

Use factoring in large datasets to look for experimenter-related effects.

2. Search for and analyze other experiments already conducted, preferably by other scientists in other fields, looking for presentiment and other possible backward effects in their experimental data.

3. Create a new much larger RNG network using easily-downloadable software (timing- or noise-based randomness). Search for deviations in other supposedly random databases such as lotteries, astronomy (SETI?), particle physics, etc.

4. Tease apart more of the already known physical variables such as local sidereal time, lunar phase, and local geomagnetic parameters in both new and past data.

5. Design and conduct experiments to carefully manipulate the size and complexity of observers and future dependencies to a stream of random events. Look for related effects backwards on the generators. Focus multiple subjects on a single RNG stream and look for interference effects. Implement a closed loop (bilking) arrangement conditioned on a trial-by-trial basis.

Toward New and Deeper Theory

A timeless, relational outlook brings the opportunity for new theoretical explorations from which experimental tests may be derived. Suggested:

1. Further explore measurement as a unitary 3-way interaction, clarifying for wider acceptance, and looking for additional testable predictions. Take inspiration and experimental ideas from the delayed choice and quantum eraser experiments.

2. Explore the further implications of backwards-in-time influence including a model of sequential processes in a Markov chain, for example.

3. Explore further consequences of retrocausal influence on random processes, predicting behaviors of different RNG types, configurations, recordings, etc.

Toward Acceptance and Mainstream Participation

The current circumstances with regard to acceptance of psi phenomena and psi research in mainstream science might be likened to a 'perfect storm'. Consider:

1. *The phenomena are small, elusive, difficult to replicate, and seem to depend upon various poorly understood or ephemeral variables including even the attitude of the experimenter.* Experimental designs and technique will continue to improve, but a theoretical breakthrough is necessary to overcome the real problem here.

2. *The phenomena appear to violate well-established physical laws.* They don't, but physical theory must be modified somewhat, and a different viewpoint taken in order to allow the jettisoning of past incorrect assumptions. See above.

3. *The 'noise level' in the area is quite high*, perhaps greater than in any other area of human experience, with many odd phenomena being claimed by wishful or incautious thinkers (or worse), and with proposed explanations that defy common sense as well as science. Unfortunately, the fear of 'guilt by association' is high among most scientists today, and this high noise level dampens interest. In decades long past, study of anomalies may have been considered a sign of progress seeking and of scientific courage, but academic and funding agency politics make this more difficult and rarer today.

4. *The phenomena are claimed and investigated mostly in the realm of psychology*, and tentative explanations often involve vague or poorly understood concepts such as 'consciousness', 'intention', and 'mind'. This is a serious continuing problem for the field of research called '*para*psychology' (outside of or beyond psychology, which itself does not often receive the appropriate respect from the 'hard' sciences), the study of the '*para*normal' (not normal—whatever that is), and possibly invoking explanations in '*para*physics' (outside or beyond existing physics). In the author's opinion, all such nomenclature is a significant barrier to acceptance by and involvement with mainstream science, the Parapsychological Association's long membership in the AAAS notwithstanding.

However, if psi phenomena were to become accepted and widely studied in mainstream science, the outcome could be very positive. Consider:

1. The significance to many fields of science, especially physics and cosmology, would be very great indeed. The scientific method itself would be rethought.
2. The significance to society would also be large in terms of a new viewpoint on reality, causality, Time, and ultimately ourselves. The philosophical effects of such a change in science would subtly but profoundly permeate culture, similarly to the revolutions associated with relativity, quantum physics, and evolution.
3. These changes in science would at long last bring about a substantial, meaningful bridge between Eastern and Western thought and culture. Over time, the two apparently conflicting, but actually complementary, ways of looking at the world would become unified, and could contribute to greater harmony world-wide.

Summary and Concluding Remarks

We have tried to show how the concept of Time appears, and the role it plays, sometimes directly, sometimes subtly, in modern physics. Our current understanding of Time surely is inadequate, challenged by recent developments in particle physics and cosmology, by quantum phenomena, and perhaps most of all by apparently retrocausal phenomena such as precognition. The two experiments discussed above show that backwards influence can have unconventional and important effects in experiments even if they are not based on intention in the usual manner, and can yield important clues about psi functioning.

Our understanding of the nature and the experience of psi phenomena are deeply intertwined with our technological culture and its worldview. The habit of cause-and-effect thinking and the everyday experience of the one-way 'flow' of Time still dominate. But, as we have tried to show, by altering our notion of causality and taking full account of both forward and backward influence, many of these seeming mysteries will become understandable.

The theory we have presented in summary is as follows:

1. Quantum measurement, when properly viewed, is unitary, lossless, reversible, and thus in principle permits propagation of retrocausal (backwards in time) influence. Measurement is exactly symmetrical with preparation, and there is no 'collapse'.
2. Quantum measurement does not give a random result, and its apparent randomness comes from backwards influence of future observers and the environment. There is no fundamental randomness at the core of physics.
3. All interactions are therefore bidirectional and relational. Cause-and-effect is an assumption that should be applied symmetrically in Time, or discarded.

A further reconsideration of these matters leads us to a deeper understanding of Time:

1. Time is discrete, and 'passes' whenever a variable changes.
2. A clock is created from a self-referring (oscillating) logical device.
3. A Time interval is defined by counting the ticks of a clock.

With the stakes as high as causality, randomness, and Time itself, it is difficult to overstate the importance of research into psi phenomena and the related physics necessary to understand it. To this end, we need to foster greater acceptance and involvement with mainstream science by 1) exceptionally careful experimentation and reporting of results; 2) better theorizing that recognizes and meshes with existing physics; 3) focusing on simple core phenomena over the stranger, more difficult to investigate; and 4) recognizing the importance to skeptical scientists in other fields of careful naming, descriptions, claims, and speculations.

Our goal in this research should be to build a better bridge between science and the entire human experience, and thus to expand the reach and the intellectual power of science, and thereby to bring about a better partnership with the whole of nature.

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PHYSICS WITH AN OPEN MIND

YORK H. DOBYNS

Introduction

The relation of physics to parapsychology is complicated. Some physicists find the phenomena intriguing and at least vaguely plausible. A few physicists in this camp have gone so far as to perform parapsychological experiments of their own, often with groundbreaking results (Jahn & Dunne, 1988; Schmidt, 1974; Targ & Puthoff, 1974).[1] At the other extreme, a number of physicists—some quite eminent—contend that parapsychology is pseudoscience and that its subject matter is nonexistent. A full review of the literature, pro and con, would be an article or more likely a book in its own right. The scope of the current work is more modest; it is to consider what the current state of knowledge in physics actually has to say about the claims of parapsychology, with samples of the relevant literature used to illuminate commonly-used arguments.

Terminology and Taxonomy

Although this paper discusses parapsychology, the current sentence marks the only appearance of the word 'paranormal' in the text. It is increasingly clear that the phenomena examined by parapsychology, however rare or poorly understood, are among the normal capabilities of human beings (and perhaps of other living things). To use a misleading term that is widely seen as a pejorative synonym for 'supernatural' seems completely inappropriate. For referring to these phenomena as a general class, the terms 'psi' or 'psychic phenomena' will be used throughout.

However, the general topic of psi is a grab-bag of anomalous observations, not all of which necessarily operate in the same ways or even constitute the same phenomenon. Various attempts to organize categories of psychic phenomena have been made in the past. The

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[1] These citations are not intended to be an exhaustive bibliography even of the authors cited, but merely illustrate some early work by the respective authors.

utility of this has been arguable since phenomena can usually be found that either cross between categories or cannot reliably be classified as one or another. In order to consider psi from the perspective of a physicist, this paper will employ a taxonomy based on concerns relevant to physical theory; these categories should not be taken as fundamental or even necessarily appropriate to psi research as such.

The label 'ESP' will be used for all observed phenomena in which human beings apparently acquire causally accessible information by means other than their known senses. 'Causally accessible' means that it would be possible in principle for some signal of a type known to exist (e.g., radio) to convey the information from its source to the place and time at which the percipient acquired the information. This covers a broad range of experimentally distinct phenomena, including clairvoyance, telepathy and retrocognition as defined by Rhine, as well as some forms of remote viewing, dowsing, psychometry or 'object reading', Ganzfeld studies, veridical OBEs and NDEs, etc.

The label PK ('psychokinesis') will be used for all phenomena in which human beings appear to be altering the behavior of physical systems external to themselves. It is immediately obvious that this is a questionable term for the analysis of experiments: for example, is the now widely-known DMILS (Direct Mental Interaction with Living Systems) experimental paradigm a case of ESP by the study target, or of PK by the active agent? For purposes of physical analysis, however, such phenomena are conceptually quite different from ESP. It will be useful, moreover, to distinguish between micro-PK, in which only the statistical parameters of a distribution of unpredictable behaviors are altered, and macro-PK, in which bulk movements of objects of appreciable size, or measurable energy flows or emissions, are observed. Micro-PK phenomena include parapsychological RNG, dice, and perhaps DMILS experiments. The extensive recent research on 'intentional healing', especially 'distant intentional healing', must also be classed from a physical point of view as micro-PK with biological targets, even though they are generally being studied by scientists other than parapsychologists. The canonical example of macro-PK is, of course, the RSPK phenomenon formerly referred to as 'poltergeist' activity. Intentional healing case studies which fall completely outside the normal range of responses for the patient (e.g., spontaneous remission of a condition which is incurable without treatment) would also seem to fit the criteria for macro-PK, as would multiple well-

attested cases of mediumistic levitation, and the recently popular demonstrations of 'external qi gong' in China (Braude, 1997). [2]

The terms *retrocausation* or *retrocausal* will be used for observed phenomena regardless of other categories in which there seems to be a reversal of the usual order of cause and effect. This includes some forms of remote viewing, all forms of precognition, premonition, and presentiment, and PK experiments in which the target's behavior was established before the PK effort was made. The issue of causality is of sufficient physical importance to merit a separate category even when the phenomena within it do not seem to differ, except for the time delay, from other psi phenomena.

The Basic Problem of Physical Relevance

Contrary opinions of many physicists notwithstanding, current knowledge of physics places relatively few constraints on the possible nature or extent of psi phenomena. The primary reason for this is that the observations from which modern physical theories are constructed do not include observations relevant to psi hypotheses.

To see why this is important, consider the 'revolution' in physics that took place in the first quarter of the 20th century. Newtonian physics, which had been the unchallenged paradigm for over two centuries, was suddenly shown to be inaccurate in two distinct regimes: that of extremely small masses and energies, and that of velocities comparable to that of light. These two domains gave rise to two distinct generalizations from Newtonian mechanics: quantum mechanics in the domain of the small; special relativity in the domain of the fast-moving. Both of these theories give the same answers as Newtonian mechanics for bodies of humanly observable scales moving at moderate velocities.

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[2] The strongest evidence for modern, reproducible macro-PK experiments is apparently contained in a paper by Leping Zha which was presented at an IONS conference but seems never to have been formally published. The information "Zha, L (2001). 'Review of History, Findings, and Implications of Research on Exceptional Functions of the Human Body.' Presented at the Third IONS Conference on the Science of Spiritual Healing, Dec. 2001, Hawaii." is therefore relegated to this footnote rather than given as a regular citation. The existence of this paper is attested online at the URLs http://www.noetic.org/publications/research/main.cfm?page=frontiers_59.htm and http://www.ramcconnell.com/selfdeception.htm

As long as observations were limited to such bodies neither alternative could be discovered.

Similarly, since experiments in physics have not been designed to examine psi hypotheses, the current structure of physics describes the behavior of physical systems in the absence of appreciable psi effects. Theoretical physical arguments against psi are therefore extrapolations into an experimentally untested regime, and therefore can be expected in principle to be as unreliable as trying to compute the behavior of elementary particles from Newtonian premises.

Aside from the general lack of empirical support in the relevant areas, there are a number of additional considerations which undermine any attempt to criticize psi based on physical laws.

Built-In Bias of Instrumentation

Upon even brief reflection it begins to seem obvious that the equipment and experimental designs of physics will automatically be as hostile to psi effects as possible. Given the wide range of observed effects and the tendency toward erratic replication that are common knowledge in the parapsychological literature, it seems reasonable to suppose that different experimental designs and apparatus may be more or less psi-conducive. What, then, can be expected during the design and test phase of physics experiments, or of the equipment intended for use in such experiments? Since the designers are not considering possible psychic effects, equipment that responds with variant readings or other forms of erratic behavior in response to stray thoughts of experimenters or passersby will be interpreted as behaving erratically *for no detectable reason.* As anyone who has ever attempted to construct sensitive equipment will appreciate, such unpredictability cannot be tolerated; if the source of error cannot be found and controlled with a reasonable investment of effort, the current design will be discarded and a different one adopted. From a parapsychological perspective, modern equipment in the physical sciences is the end product of many generations of development which has unwittingly minimized its sensitivity to any kind of psi influence. At face value this would appear to be relevant only to PK experiments of various sorts, but various attempts to design instruments to detect psi influences must also be subject to this effect.

Self-Affirming Theories

Almost every controlled study of psi has been inspired by anecdotal reports of a similar or related phenomenon occurring spontaneously. Several categories of such phenomena are targeted by physical criticisms that share a common, and circular, structure. (Specific examples will be discussed elsewhere; this argument is given in general terms to avoid repeating it at each appearance.) The hostile physicist claims that the phenomenon is impossible because it conflicts with some feature of physical theory. Since the phenomenon is known to be impossible, the anecdotal evidence, no matter how extensive, and no matter how commonplace it may be in human experience, must be dismissed as due to selective observation, selective memory, and the generally poor ability of human beings to understand probabilistic phenomena. Confirmation in controlled experiments must be due to sloppy procedure or outright fraud. If, however, the theory on which the criticism is based should be challenged, the critic replies that the theory enjoys overwhelming empirical support and has no empirical counterexamples. Naturally not, since all potential counterexamples have already been dismissed on the grounds of their theoretical impossibility. This particular phenomenon extends and intensifies the problem created by the fact that physics experiments generally disregard psi; once this vicious circle becomes established, the everyday practice of physics does not merely ignore potential psi effects but actively dismisses them from serious consideration and disregards them even when they appear.

Physics Experiments May Provide Evidence of Psi

Some entire categories of physical experimentation seem to have the potential of providing support for psi even though the majority of physicists have not recognized them as such. The 1980s saw the emergence of two dramatic controversies in physics: the alleged discovery of 'cold fusion', and the possible existence of a fifth fundamental force suggested by re-analysis of old data from gravitational studies. The cold fusion controversy still drags on with some experimenters claiming significant progress; the 'fifth force' issue is widely regarded as settled. In both cases, especially in the early years, an overview of relevant publications seemed to show a remarkable correlation between the experimenters' attitude toward the phenomenon and their ability to produce it (or at least to report it) under laboratory conditions. To establish whether this apparent

correlation is real and statistically significant would require an immense meta-analytical investment which has not yet been made.

In summary, there are abundant *a priori* reasons for regarding physics-based arguments against psi as suspect. The following, more detailed discussion of several such criticisms should be taken with the caveat that it may be giving these arguments more credence than they deserve.

Erroneous Criticisms

Most physicists who have a positive interest in psi phenomena tend either to perform experiments or construct modest theoretical models. Their publications are therefore likely to be technical and evidence-laden. In contrast, physicists hostile to the field are generally more concerned with indicting the whole of it and are therefore prone to make sweeping philosophical statements in writings for a more general audience.

When, in contrast, a hostile physicist attempts to make specific criticisms of particular experiments or programs, the result often indicates that the physicist failed either to understand or to correctly analyze the experiments in question. To review all such cricitisms and evaluate the frequency with which they err would require an encyclopedia; therefore only two examples drawn from the author's personal experience will be offered to establish existence of this phenomenon.

The paired articles Freedman, Jeffers, Saeger, Binns and Black (2003) and Dobyns (2003) illustrate one instance of such a failed criticism. One of the authors of the first article (Jeffers, a physicist) contributed to it a methodological criticism of experiments at Princeton Engineering Anomalies Research (PEAR), of which their experiment was a partial replication. (Jeffers' identity as the source of this part of the paper can be concluded from his having raised the identical issue in prior correspondence with PEAR.) Sections 1 and 2, and Figure 1, of the commentary article provide a detailed demonstration that Jeffers' suggested 'improvement' of the methodology in fact weakens the experimental controls rather than strengthens them.

Stenger (1990) mentions what he sees as methodological vulnerabilities in the experiments reported by Jahn and Dunne (1988). The descriptions and illustrations of the equipment and protocol in the latter reference, however, immediately make it clear that the

experiments were fully protected against every source of artifactual interference proposed by Stenger.

The fact that physicists can make mistakes is fundamentally not very interesting, even if it should turn out that they are unusually prone to making such mistakes when trying to find flaws in a psi experiment with positive results. It seems more instructive at this point to turn to physicists' general criticisms of the subject, and consider whether they are as theoretically compelling as the authors seem to think.

The Argument from the Unity of Science

It is widely held that, even though we do not know all the details, there is some universal body of natural law that explains all phenomena in every branch of science. Explanations in one science can be grounded in knowledge from another science considered more fundamental. This concept is not unique to physicists; the underlying unity of scientific knowledge was been widely popularized by biologist E. O. Wilson (1998) under the name 'consilience'. As applied against psi, this argument roughly states that even though we don't know all the details of this universal 'theory of everything', we know enough to be sure that psi is innately incompatible with this framework.

Anderson (1990) calls science a 'seamless web' in the process of declaring PK experiments fundamentally wrongheaded; he argues that our ability to perform precise measurements at all proves the phenomenon can't exist. Weinberg (1992) goes so far as to declare: "... our discovery of the connected and convergent pattern of scientific explanations has done the very great service of teaching us that there is no room in nature for astrology or telekinesis or creationism or other superstitions." (In the preceding discussion Weinberg consistently uses 'telekinesis' to refer to psychokinesis.)

Despite its endorsement by two Nobel Laureates, the argument from the unity of science actually carries very little weight. The reality is that any comprehensive system of natural law must be essentially modular in structure, and profound changes can be made to portions of it without the slightest effect on the remainder.

This can be demonstrated immediately by the fact that physics itself does not comprise one framework for natural law, but two. Quantum mechanics, in its special-relativistic form of quantum field theory, has demonstrated enormous predictive power and has been confirmed to very high levels of precision. It has been rivaled in its success only by the power of general relativity in dealing with astronomical phenomena

involving large masses and intense gravitational fields. Unfortunately, these two very successful theories are fundamentally incompatible with each other. At least one of them must be fundamentally revised before we can have a genuine 'Theory of Everything' just in physics, let alone in science as a whole. (For the moment, the claims of string theory to this 'Theory of Everything' mantle can be ignored, since it has yet to make even one empirical prediction.) Yet somehow, this inconsistency at the core of physics fails to create similar fissures throughout the rest of science. To show an example from another perspective, physicists have not felt obliged to revise their theories in response to the geologists' development of plate tectonics in the 1960s, despite the fact that this constituted a profoundly revised understanding in a science which would seem on the face of it rather closely related to basic physical principles. Although it seems reasonable to assume that there is indeed a single universal system of natural laws, it also seems clear that the details of any particular branch of science can be revised without requiring wholesale revision of the whole of science.

The Argument from Historical Success

A closely related argument against psi is sometimes raised not from the presumed unity of scientific explanation per se, but from its history. Although the overarching framework of natural law that encompasses all observed phenomena is not known, it is contended that the current framework has enjoyed steady success in being extended to cover more and more phenomena. The preponderance of evidence therefore would seem to be that the continued extension of this paradigm will meet with continuing success, until it ultimately encompasses the whole of science and it will be directly seen that there is no room in it for phenomena such as psi.

This argument from historical success can easily be refuted by historical evidence. Long-term success of a scientific model cannot be extrapolated into an indefinite future. The Ptolemaic model of the solar system enjoyed uniform success and steady observational refinement for over a millennium; as late as 1450 a reasonable individual might have concluded that some refinement of an epicycle-based geocentric model would ultimately account for all astronomical observations to the limits of accuracy. Instead, the work of Copernicus, Kepler, Galileo, and Newton replaced that model within a time span that was quite short compared to its previous longevity. Newtonian dynamics in its turn was unchallenged for over two centuries until it was found necessary to

replace it in the first quarter of the twentieth century. Contrary to the expectation of historical stability, the history of science makes it quite clear that even the most fundamental concepts are subject to revision, and that revision may occur with very little warning.

Genuine Problems

Although general indictments of psi on principle can be seen to be vacuous, some of the observed features claimed for psi phenomena are difficult to reconcile with current physical theories. A review of these features will provide context for consideration of the extent to which current physical theories need revision to accommodate psi.

Distance Independence

For almost as long as ESP and PK phenomena have been subjected to serious investigation, they have appeared to be unaffected by intervening distance, at least over distances comparable to the Earth's diameter. Simple remote viewing studies achieved noteworthy hits over distances from kilometers to thousands of kilometers (Dunne & Bisaha, 1979; Dunne & Jahn, 2003; Puthoff & Targ, 1976). The US government 'Stargate' program, declassified in the mid-1990s, appears to have achieved impressively accurate remote viewing descriptions at intercontinental ranges (Puthoff, 1996; Targ, 1996). Systematic surveys of both remote perception and remote-REG (PK) data at PEAR could detect no evidence for distance dependence in the effect size (Dunne & Jahn, 1992; Dunne & Jahn, 2003). Although a high-profile ESP experiment conducted during the Apollo 14 mission attempted to extend this distance baseline, this single episode does not provide sufficient data for drawing firm conclusions about longer distances (Mitchell, 1974). More recently, the growing popularity of remote viewing has led to an explosion of almost hobby-like efforts to remote-view extraterrestrial targets, but the accuracy of such attempts is for obvious reasons hard to verify, and has produced such spectacular failures as the alleged presence of a large extraterrestrial spacecraft attached to, or perhaps comprising, Comet Hale-Bopp in 1997.

Although firm conclusions cannot be drawn for distance scales longer than about 12,000 km, the evidence is clear that psi effects (or at least ESP and micro-PK) are at most weakly attenuated by distances up to that scale and may be completely unaffected by them. They certainly do not show the $1/r^2$ distance dependence which is the longest-range

interaction for any known physical field, and which geometrical considerations suggest is the weakest distance dependence possible for any physical field effect.

This particular problem, however, does not seem to require any fundamental revision of physics, for numerous physical models of non-attenuating phenomena exist. Correlations produced by quantum entanglement, for example, retain the same statistical strength regardless of the distance intervening between particles. More prosaically, while the signal power of a broadcast message falls off as the squared distance to the transmitter, the informational content of the message does not; as long as the signal is strong enough to be distinguished from noise, its full content remains accessible at any distance. Since ESP and micro-PK both seem to be informational processes, the model of signal transmission seems adequate to explain their apparent distance-independence. It may be significant, in this light, that reports of macro-PK phenomena in general do not involve large distances between the phenomenon and its presumed originator.

Further consequences of these two analogical models for distance independence, entanglement and signal transmission, will be discussed later.

Energy Conservation

Apparent macro-PK incidents that have been captured on video, such as 1970s-vintage scenes of Nina Kulagina psychokinetically manipulating small objects or more recent Web-based presentations of qigong masters moving objects without touching them, tend to show the presumed originator apparently undergoing considerable physical strain while inducing modest motions in a target object of no great size. The visible effort of the video subject is more than adequate to explain the observed motion of the target object, although the means of connection are not obvious. Some reported macro-PK incidents, on the other hand, have apparently involved considerable energy input (e.g., levitating an entire human body or a massive table by a considerable distance) without concomitant signs of physical effort (Braude, 1997). If these incidents actually occurred, there is no obvious source for the energy that appeared as work on the object. It is, of course, possible to dispute the veracity of these large-scale macro-PK incidents, since regardless of the quality of attestations the accounts are decades old and the phenomena have never been reproduced under laboratory conditions. If, however, the evidence for the existence of these

phenomena is accepted, either macro-PK taps into a currently unknown energy source or the law of conservation of energy needs revision. Either of these possibilities requires a substantial change to current physical theories.[3]

Non-Detectable Energy

In certain genres of apparent psi manifestation, references to some form of energy unknown to, and currently undetectable by, standard scientific instruments are almost ubiquitous. Such unknown energy more or less defines the discipline of qigong; however, references to it in one form or another are almost inescapable in the field of intentional healing. As a reminder, from the perspective of a physicist intentional healing—distant or otherwise—appears to be a form of micro-PK with biological targets, even though the healers themselves do not use the terminology and the experimenters conducting the studies are generally not parapsychologists. Even a cursory examination of such phenomena makes it clear that this 'energy' is a perceived and experienced reality to the practitioners, for all that no currently known instrument can detect it (Goldner, 1999; Rand, 1998). It may, however, have detectable indirect effects in PK experiments (Jahn, Dunne, & Dobyns, 2006).

Other concerns with energy appear when macro-PK is considered. Even if we disregard cases where energy conservation seems to be violated, the less spectacular cases still leave the mystery of how exertion over here is conveyed into motion over there, without contact or any detectable medium. (If there were a detectable mechanism to transfer the force, the incident would not be classified as macro-PK!)

For micro-PK and ESP explanations there is a ready reconciliation that whatever practitioners are experiencing as 'energy' is their subjective perception of an informational phenomenon in which no physical energy difference is actually involved. This requires no revision of physics since there are currently any number of ways in

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[3] With regard to energy conservation it has been proposed that the 'cold areas' sometimes associated with RSPK reports are the source of the energy; thermal energy has been extracted from the environment and converted to kinetic energy to move the objects. I have seen this notion brought up innumerable times in casual discussion and correspondence but have failed to locate a single refereed publication containing the idea; I therefore cannot provide a normal citation. In any case, this possibility still requires some rewriting of physics, since it simply replaces a violation of the First Law of Thermodynamics with a violation of the Second.

which information can be encoded in the detailed structure of material objects while remaining completely invisible to all current instrumentation. This escape hatch is not available if macro-PK is to be taken seriously. There would appear to be two basic possibilities for a physical model of this 'energy' that is not detected by instruments; either potential energy is stored in some arrangement of known matter and fields that is unfamiliar to current physical models, or an additional field beyond the known ones must be posited. Both of these explanations would seem to require at least some new physics.

The 'new field' approach runs into a possible conflict with observational as well as theoretical physics. If the basic approach of quantum field theory is accepted, every fundamental field has a quantized excitation, which manifests as a particle. In general, collisions in particle accelerators will produce, at least briefly and with some measurable probability, every species of particle which the collision has sufficient energy to create. At the present time all particles that have appeared in such experiments fit comfortably into the so-called 'standard model' of quantum field theory. For an additional field to exist, either its excitations are so massive that they cannot be produced by any collision energy yet studied, or its coupling to other fields is so weak that even decades of experimentation have not been sufficient to produce a significant number of excitations, or some symmetry principle prevents its creation in the sorts of collisions examined.

Of course, physicists are fairly confident that at least one form of real substance has thus far eluded detection by one or more of the mechanisms just described. Astronomical observations seem to require that considerable amounts of 'dark matter' must be present in the universe, to explain the observed properties of galaxies and galactic clusters. Indeed, the total mass of this dark matter would seem to be perhaps four or five times the mass of the ordinary 'baryonic' matter from which stars and planets are made. Most models of dark matter require that some considerable quantities of it are present in the Solar System and presumably passing through Earth without interacting, or that it was created by physical processes early in the Big Bang, or both. Nevertheless, none of the minor zoo of potential dark matter candidate particles has ever been detected.[4]

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[4] This is not strictly true in that neutrinos are one of the dark matter candidates, and they have been detected abundantly enough that their existence is not in

Unfortunately, unlike the astronomers' dark matter, if a currently unknown sort of field is the source of psi effects, it interacts strongly enough with ordinary matter to be generated by biological entities and to affect bulk material objects. Problems with such a field will be discussed in more detail later.

The other alternative, that psi 'energy' might be stored or transmitted through manipulation of known fields or particles, suffers the drawback that the known behavior of these entities is not consistent with observed properties of psi. Using this as an explanation for psi therefore requires a specific rewriting of the laws governing, for example, electrodynamics. Although I present the reasons why it is not unreasonable to consider such a rewrite, it still constitutes a revision of known physics.

Retrocausation

Possibly the most difficult psi phenomenon for anyone, including physicists, to accept is the apparent retrieval of information from the future, or imposition of information on events already past. Even Stapp (1994), a physicist who accepted the evidence for psi and was writing an article to provide a theoretical model for it, flatly declared that "Such an influence of an observer backward in time on atomic events seems completely at odds with physical theory." The pervasive assumption in physics is that the past may determine or constrain the future, but the future cannot determine the past.

The trouble with retrocausation is that it appears to open the door to time paradoxes, in which a sequence of events becomes inconsistent with itself: it happens if and only if it does not happen. Obviously the potential for creating a physical instantiation of the Liar's Paradox is a matter of considerable concern. In the recent past the mere possibility of retrocausal effects was deemed sufficient grounds to reject a speculative phenomenon as nonphysical; for example, Benford, Book and Newcomb (1970) considered the mere possibility of a retrocausal

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dispute. However, neutrinos are a form of 'hot dark matter' since, even at the current level of cosmological expansion and cooling, the kinetic energy of primordial neutrinos would be large in comparison to their very small mass. The astronomical community is currently of the opinion that some form of 'cold dark matter' is required for models to succeed in representing the actual structure of galaxies and clusters, despite minority analyses to the contrary (Dobyns, 1988).

'tachyonic antitelephone' as an adequate disproof of the existence of tachyons. At the time this article was published the attitude of the physics community toward retrocausation was an archetypal instance of the "'Self-Affirming Theory' problem discussed above.

Since that time the attitude of the physics community has liberalized. A paper by Echeverria, Klinkhammer and Thorne (1991) presented a strong argument that retrocausal phenomena cannot, in fact, create time paradoxes. Although their analysis was based on the general-relativistic concept of traversable wormholes, it generalizes quite readily to other retrocausal phenomena. Their conclusion can briefly be summarized by saying that for any sequence of retrocausal events which is self-inconsistent, there must exist at least one family of closely related event sequences which are internally consistent and therefore possible.

Further liberalization of physicists' attitudes toward retrocausation is evident in an article by Hawking and Hertog (2006) invoking explicitly retrocausal models for cosmology, and in the convening of a 2006 AAAS symposium specifically to discuss retrocausal phenomena (Sheehan, 2006). Although the community as a whole may not have caught up with these developments, it seems safe to conclude that there are no physical reasons for rejecting retrocausation *a priori*.

Moreover, there are any number of physical models that suggest that retrocausal phenomena should occur. In electrodynamics, the study of radiative effects leads at first to the conclusion that any radiating body should generate both 'retarded waves', which radiate outward into the future, and 'advanced waves' which radiate into the past. (From the viewpoint of an outside observer, 'radiating into the past' means that advanced waves would be seen as converging from infinity onto the radiating object.) While advanced waves are often dismissed by fiat as an unphysical solution of the equations, a highly successful version of electrodynamic theory has been constructed by presuming that advanced waves are real (and are not observed because they are normally cancelled out by interference effects) (Wheeler & Feynman, 1945).

The success of this 'absorber theory' led to the 'Transactional Interpretation' of quantum mechanics, which, while it does not specifically predict retrocausal effects, offers a completely natural framework for analyzing their occurrence (Cramer, 1986).

The notion of tachyons—particles which, unlike normal matter, must always move faster than light and slow down when they gain

energy—has been a speculative topic in relativity since the 1960s. While the aforementioned Benford *et al.* (1970) article more or less stopped serious study of tachyons in its tracks, the new understanding of the fairly innocuous nature of retrocausation may allow such study to be revived, and in particular might defuse the standard assumption that any quantum field theory that predicts the existence of a tachyonic particle is *ipso facto* nonphysical.

General relativity abounds with causality-violating solutions to Einstein's equations of space and time. The anisotropic cosmological solution found by Gödel (1949) contains 'closed time-like curves', meaning that by moving in an appropriate trajectory an object can enter its own past. Certain configurations of spinning masses were found by Tipler (1974) to induce the same phenomenon. The traversable wormholes of Echeverria *et al.* (1991) are of course a general-relativistic construct.

Such phenomena as these involve masses and mass densities that are quite literally astronomical, and therefore might seem irrelevant to the question of psi. However, the potential for causality violation appears to be present even in empty space if one considers the still mostly speculative properties of quantum gravity. As noted above there are profound incompatibilities between quantum field theory and general relativity; every attempt to construct a full quantum theory of gravitation has failed. However, it is generally believed that in any such theory the geometry of spacetime itself must, due to the quantum-mechanical uncertainty principle, break down at very short scales into what has been called 'spacetime foam'.[5] Spacetime, at this scale, should be seen to exist as a quantum superposition of every possible geometry, including every possible topology of wormhole connections between closely separated point-instants. This would seem to imply a non-vanishing probability for particles to carry information along grossly non-causal paths through space and time.

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[5] The spacetime foam effect is eliminated by some formulations of string theory, which purports to be a successful quantum theory of gravity. However, given the current lack of empirical content in string theory, levying string-based criticisms against other physical models would seem premature.

The Observation Problem

One of the thorniest physical conundra for psi phenomena emerges indirectly from recent experiments by Radin.[6] These emerged from an attempt to produce a psi-based version of a classical physics experiment. It is well known that when quantum particles, such as photons, are projected through a pair of adjacent slits they will produce an interference pattern, depending on their wavelength and the slit spacing, which is quite different from the broad diffraction spread produced by a single slit. It is also well established that a 'which-way' measurement—that is, one that establishes which of the two slits a particular photon travels through—destroys the interference pattern. It has furthermore been established that what destroys the interference pattern is the possibility of making a measurement; if an appropriate instrument is in place, the interference vanishes, even if the instrument's outputs are simply being discarded without examination (e.g. the data leads are wired directly to ground).

In response to the experimental claims of the PEAR laboratory Jeffers decided to conduct an experiment (ambiguously PK or ESP) using double-slit interference as his random source (Ibison & Jeffers 1998). The goal of the experiment was to have subjects attempt to psychically view which slit individual photons were passing through; if successful this observation should disrupt the interference pattern. Jeffers found no effects. Subsequently, however, he brought his apparatus to the PEAR laboratory for a replication in which PEAR would deploy its own population of operators. The replication at PEAR found statistically significant effects (Ibison & Jeffers 1998). Aside from differences in operator pool and experimental ambience, however, PEAR made an important protocol change: operators were not instructed to psychically observe the photons inside the sealed chamber, but simply to shift the reported on-screen feedback in the intended direction. In other words, from the operator's viewpoint the PEAR version of the double-slit was simply a micro-PK experiment with a different random source. This protocol change was made because one of the experimenters (Dobyns) found Jeffers' proposed mechanism incredible and saw the experiment as 'designed to fail'. The contrast between the photon throughput of the device (ca. 50,000

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[6] This experiment was presented in "Gazing at the Mind's Eye", Radin's presentation to the 2008 Annual Meeting of the Society for Scientific Exploration, and appears not to have been published at the time of this writing.

photons/sec) and the rate at which human consciousness can process stimuli suggested that the number of individual photons which an operator could hope to observe as specific entities would be a negligibly small fraction of the total; hence the dilution of the interference pattern, even if operators were maximally successful at the assigned observational task, would be undetectably small.

Radin proceeded to perform his own version of a psi-interference experiment with a single subject, a trained meditator with experience in remote viewing. The experimental setup involved a Mach-Zehnder interferometer, which uses a beam-splitter rather than a double-slit and creates interference between photons sent down two widely separated beam paths before being reunited. Statistically large effects were seen when this meditator focused his remote-viewing attention on one of the beam paths, psychically performing a which-way measurement.

The reason this experimental result of Radin's creates physical quandaries is the nature of which-way measurement. As noted, simply having the detector in place is sufficient to destroy interference. However, remote viewing employs no apparatus. Insofar as spots indefinitely remote in both space and time can be remote-viewing targets, the 'detector' must be construed as always being in place, or alternatively it can be deployed retroactively (and retrocausally) at a remote viewer's whim when he chooses to examine past events. Combining the effects of which-way measurement with the potential for performing one psychically, it suddenly becomes mysterious that any double-slit interference pattern should ever have been observed in any experiment whatsoever. In this case consistency of physical observation, rather than physical theory, seems to require that remote viewing has a limited reach in time and that interference-based experiments are generally successful because no remote viewer directs his conscious attention to them before the information becomes unavailable.

In summary, most of the features of psi phenomena that appear physically problematic turn out to have models available from known or theoretically accepted phenomena. The worst problems appear to be energy transmission issues in macro-PK and the quantum observation problem for interference phenomena.

Potential Revisions to Physics

There is a fairly obvious hierarchy to the ways in which data on psi phenomenology can be reconciled with known physics.

1. It is possible that existing physics requires no revision. At a minimum, this appears to require rejecting all reports of macro-PK as in some way erroneous.
2. It may be possible to accommodate psi phenomena with a minor extension such as adding a new field to the known set.
3. Incorporating psi into physics may require a paradigm-shifting theory comparable in scope to quantum mechanics or general relativity.
4. Psi may be ultimately inexplicable by physics; this is the position most often held by dualists who consider psi phenomena, and the mind itself, to be mediated by a different kind of 'stuff' from the physical universe.

Detailed discussion of these possibilities will progress most naturally in descending order, from most to least sweeping.

Psi Is Not Physical

This approach to the problem tends to lead to questions that are quite literally metaphysical, such as what exactly we mean when we speak of something being physical. One point of logical attack presents itself in the fact that all evidence for the existence of psi is to some extent physical in nature. The role of the physical in PK experiments is obvious, but even in ESP experiments the manifestations of psi are ultimately reported and recorded through physical actions such as speech or writing. Psi is thus something that can interact at least indirectly with physical things.[7] It is sometimes proposed as a

[7] If PK effects are discounted or explained in alternate terms then one may construct a dualistic model in which consciousness or mind is the only nonphysical entity that directly interacts with physical things, and psi is a purely nonphysical interaction between minds. Since this model attaches psi inextricably to consciousness it seems merely to add a taxonomical complication to the 'nonphysical' without appreciably changing the logical structure discussed in the main text.

definition that anything that interacts with physical things must itself be a physical thing. Accepting this as a definition of physicality is a perfectly legitimate logical step so long as one recalls that such 'physicality by definition' does not allow one to infer that something should have any of the other qualities usually associated with physicality, such as having mass-energy content, occupying a definite location in space, etc. The possibility of this logical step demonstrates that insofar as the 'nonphysical' realm can interact with the physical, its description is part of the legitimate domain of physics, even if that description requires entirely new concepts and principles of 'physical' law. Category 4 of 'nonphysical psi' can thus be seen to be a special case of Category 3, 'paradigm shift in physics'.

Psi Requires a Paradigm Shift in Physics

In this context a 'paradigm shift' is taken to mean a complete restructuring of the fundamental concepts of the theory. Examples of previous paradigm shifts include General Relativity, in which the absolute space and time of Newton are replaced with the dynamic spacetime whose curvature explains gravitation; and quantum mechanics, in which the real particles of Newtonian mechanics are replaced with abstract states (of which Schroedinger's famous wave functions are only one particular representation). A significant proportion of the parapsychological community seems to expect that the explanation of psi will ultimately require some sort of mind/body dualism. As discussed above this seeming retreat from physics is actually the first step in constructing such a paradigm-shifting theory, although few advocates have thus far carried it beyond that first step. More specific theories of this class, but still not sufficiently developed for quantitative evaluation, have been proposed by Jahn and Dunne (Jahn, 2002; Jahn & Dunne, 1986, 2001, 2004).

The earlier discussion of the ways in which physical experimentation has been directed away from psi phenomena suggests, by analogy with the cases of quantum mechanics and relativity, that the paradigm-shifting approach may in the long run be the most parsimonious way of constructing a physical theory that includes psi. An important constraint of such a theory, however, is that it must reduce to the earlier version of physics when the conditions that forced the revision do not apply. In other words, just as relativistic physics gives the same answers as Newtonian dynamics in situations where the speed of light can be treated as effectively infinite, and quantum

mechanics does likewise in circumstances where Planck's constant can be treated as indistinguishable from zero, any version of 'psi-dynamics' that incorporates psi in physics as a fundamental quantity must give the same answers as conventional physics in cases where psi effects can be ignored. In general, this constraint of reduction places severe restrictions on the mathematical forms that such a theory can take. These constraints may be less severe for a theory that incorporates psi, in that they can be seen as applying only to the interface in which psi interacts with the more mundane components of physics.

A consideration that may work in favor of the paradigm-shift approach is that physicists currently expect at least one more change at this level to occur in the not-too-distant future. The irreconcilable conflict between general relativity and quantum field theory means that at least one theory, and possibly both, must be fundamentally revised for there to be a coherent explanation of all of physics. A successful theory of psi at this fundamental level might just be able to reconcile this conflict. Even if this is too ambitious a hope, the ability of physics to survive this incompatibility between its two most fundamental theories suggests that a comparably paradigm-shifting theory of physics with psi might be forgiven a comparable degree of incongruity, provided it explained a broad range of phenomena, made quantitative empirical predictions which passed experimental test, and correctly reduced to simple Newtonian physics when psi terms were set to zero.

Psi Requires Extensions to Current Physics

Far and away the most popular method of reconciling psi with physics is to stipulate the existing structure of physics and add a new feature capable of accommodating psi. For a few examples, Tiller (2003) attempts to accommodate healing effects by extending the number of dimensions of spacetime; Rauscher and Targ (2006) attempt to explain precognition and remote viewing by proposing that spacetime coordinates should be complex-valued rather than real-valued; and Beichler (2001) provides a bibliography and review of numerous attempts at a psi-based extension of physics spanning much of the 20th century.

In general, such attempts have not been broadly persuasive for at least one of a variety of reasons. They may, for example, fail to account for the broad range of psi phenomena, being designed with only one type of empirical effect in mind. From the perspective of physics, there are two related types of problems which render such theories

unpersuasive. First, theories that invoke new fields, new particle species, or additional spatial dimensions do not in general offer a satisfactory explanation for why these physical features of the universe appear only in psi-related contexts and not in ordinary physical observations. Second, such theories tend to make psi 'too easy'. It is difficult to see, in most such theories, why strong psi manifestations should be a rare phenomenon requiring either special circumstances, exceptional individuals, or extensive training to elicit. Yet another problem does not apply to any particular extended-physics theory in isolation, but emerges from their very multiplicity. Most of these theories are mutually contradictory; for example, they may posit a different total number of dimensions for spacetime. In such a situation it is obvious that at best all but one of the proposed theories must be wrong, and it is entirely possible that all of them are. Finally, relatively few of the theoretical proposals have been developed sufficiently to make testable empirical predictions. The exceptions have generally seen only small-scale empirical testing by their own proponents, which will not be persuasive to wider communities until other experimenters take up the task.

Currently, quantum field theory (the special-relativistic form of quantum mechanics) has been applied to elementary-particle physics so successfully that the resulting model is routinely referred to as the Standard Model. This provides a complete explanation of electromagnetism along with the strong and weak nuclear forces, and has been verified to extraordinary levels of numerical precision.[8] Making a strong change to the Standard Model, such as adding a new field or changing its spacetime dimensionality, is very difficult to do without in some way contradicting this extensive and sensitive match to experiments. (String theory gets away with proposing extra dimensions by having them 'compactified' such that the total extent of the Universe in the added directions is too small to change the Standard Model's

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[8] Small but vocal communities of dissidents dispute the premises of the Standard Model, and with sufficient effort one can find publications even to this day arguing that quantum mechanics, relativity, or both are mistaken. Since such contrarian positions are not only held by extremely small minorities, but are also in my opinion ill-founded as regards both theory and evidence, I choose neither to cite them nor to describe them in detail, but simply mention their existence to avoid giving a false impression of monolithic unanimity in the physics community.

predictions at any testable scale.) This places an additional hurdle on the integration of any proposed extension with the rest of physics.

Psi within Current Physics

Previous sections have pointed out that the contradictions between known physics and psi phenomena are not as profound or fundamental as is generally assumed. However, the energy conservation and transmission issues indicate that macro-PK, at least, seems to have no acceptable explanation within currently known physics, forcing us either to adopt some form of revised model or to assume, along with the skeptics, that all reports of this sort of psi manifestation are erroneous. It seems contrary to the principles of empiricism to discard an entire body of well-established data because they are incompatible with theory, so it seems one is forced to the conclusion that physics will eventually be revised either by extension or by reformulation to account for macro-PK effects.

On the other hand, there is no guarantee that all psi manifestations are the same phenomenon or operate through the same mechanism, so it seems worthwhile to examine possible models of other types of psi manifestation under currently known physics.

It should be mentioned here that a number of proposed models for certain types of psi known under the general category of 'observation theories' propose that the explanation of psi lies in the quantum-mechanical phenomenon sometimes called 'collapse of the wavefunction' under observation. One fundamental problem with these theories is that the so-called 'wavefunction collapse' is currently a complete mystery. While its consequences are exactly specified by the laws of quantum mechanics, there is no formalism that describes what actually happens during this process or even whether it is properly to be described as a process, and physicists are still divided among multiple 'interpretations' concerning its physical meaning (Penrose, 1989, 2004 —the citations given here are to explanations which will be accessible to educated non-physicists). Given its status as an enigma, wavefunction collapse seems to be itself in need of explanation, rather than something that can explain other phenomena. For this reason observation theories shall not be discussed further here despite their currency in some branches of parapsychology.

ESP

For the current analysis ESP covers the acquisition of information which could have reached the percipient by some known form of signaling, leaving retrocausal cases (precognition, presentiment, etc.) in a separate category. It was noted previously that both signaling and entanglement can account for the distance-independence which is one of the physical criticisms of ESP.

If ESP is mediated by a signal, there seem to be three basic categories of possible explanation, listed here with their physical consequences.

1. The signal is of some known physical type but which human sensory mechanisms are not believed to detect. (If the percipient is receiving a signal that known sensory mechanisms do detect, the phenomenon is not ESP but a sensory confound resulting from poor experimental design or implementation.) This is implausible for the longer-range cases, but possible in principle. The implications for physics are nil, although the implications for biology and physiology may be profound.

2. The signal is of a known physical type but is propagating in ways that avoid the usual obstacles such as distance, sensory isolation, etc. This is compatible with known physics in the sense that it involves processes which might be possible in principle but are not definitely known to occur, and will be discussed further in the section on retrocausation.

3. The signal is of unknown physical type. This requires an extension of physics as discussed above.

4. The signal is 'nonphysical' in the loose sense discussed earlier. This requires a paradigm-shifting physical model.

An alternative to signaling for many ESP cases can be found in the quantum-mechanical phenomenon of entanglement, which has been demonstrated in numerous experiments in recent decades (Aspect, Dalibard & Roger, 1982). Figure 1 illustrates a schematic structure that is isomorphic between experimental demonstrations of entanglement and many, perhaps all, experimental tests of ESP. The block labeled "Preparation" represents, in an entanglement experiment, the preparation of two particles in an entangled quantum state; in an ESP experiment it represents the decision to perform an experiment and the

recruitment of any necessary participants. In an entanglement experiment Events A and B are typically detection events at two separated detectors. In an ESP experiment Event A would be the attempted ESP task and Event B the features of the target of the task; this might be a remotely presented image in a ganzfeld trial, the outbounder description in some RV trials or the properties of the target site in others, etc. Finally, in the box labeled "Correlation" the data from the two events (or sets of events) are collected and are found to contain correlations that cannot be explained by classical data transmission.

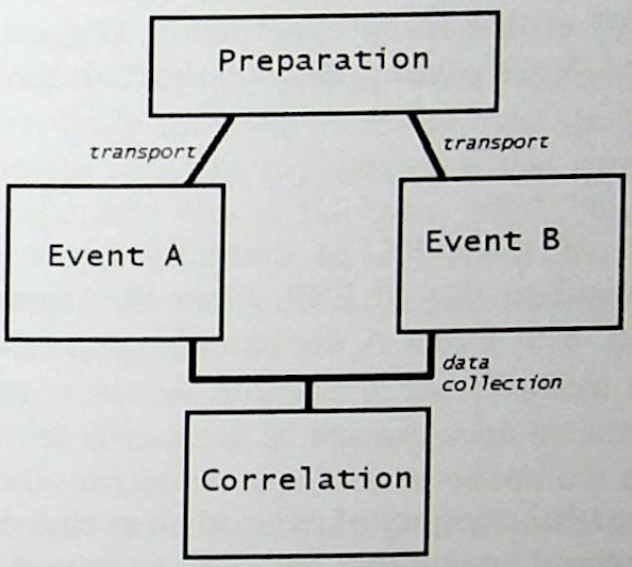

FIGURE 1.
Isomorphic structural framework for entanglement experiments and psi experiments.

In principle any interaction between quantum particles entangles at least some of the variables describing their states. In practice entanglement experiments require some care in preparing a state where two particles are measurably entangled and extreme care in preventing that entanglement from being disrupted by outside interactions. It is generally believed that the dense, high-temperature environment of a biological entity precludes the persistence of an entangled state for any length of time, so it is problematic to suppose that the entanglement-like correlations of ESP data are the result of literal entanglement of particles. However, the problem with preserving entanglement is that when one particle of an entangled pair interacts with its environment, it becomes entangled in turn with the particles of that environment, and

the single item of information relevant to the state of the original entangled partner becomes impossible to find among the vast number of environmental states. Every instant of life for a biological organism, on the other hand, involves similarly astronomical numbers of quantum interactions, each of which creates its own entanglement, and organic brains are known to operate by a form of massively parallel processing. It thus seems at least a viable speculation that living organisms may be able to solve the 'environmental entanglement' problem at least partially for large amounts of entanglement information. Some additional credence might be added to this speculation by the observations of von Lucadou (2006) that psi phenomena are entanglement-like in that many experiments suggest that they cannot actually be used for signaling despite the correlations between the separated events.

Micro-PK

The relation of micro-PK to currently known physical law is essentially identical to that of ESP. Even the isomorphic relation to Figure 1 applies, with Event A the human agent and Event B the PK target.

Retrocausation

The third physical category of psi is in an extraordinarily ambiguous position with regard to current physics. As noted above, numerous physical theories show the potential for retrocausal phenomena. These have traditionally been dismissed as unphysical due to the need to maintain causality, but the analysis of Echeverria *et al.* (1991) has proven that there is in fact no such need. In consequence of the perceived need, however, possible evidence for retrocausal physical phenomena has been dismissed or ignored, leaving physical science in the peculiar position that retrocausal phenomena are theoretically expected, widely disbelieved by the community, and empirically almost completely untested. Such empirical tests as have been performed, however, strongly support the existence of the phenomenon (Bierman, 2006; Broughton, 2006; Dobyns, 2006; Nelson & Bancel, 2006; Radin, 2006).[9] For purposes of the current analysis it therefore seems

[9] To cite all references on this topic would require an article in its own right, or perhaps a book. The citations given, together with the references they cite in turn, provide an overview of recent research.

reasonable to stipulate that retrocausation occurs and can be accommodated within known physics by one or more of the mechanisms discussed earlier.

Interestingly, this makes it easier to explain both ESP and micro-PK in terms of current physics. For ESP, the relationship is obvious: in any experiment in which the percipient receives eventual feedback of the target, hypothetical mechanisms for perceiving a distant site or object can be replaced by direct retrocausal awareness of an experience in the percipient's personal future.

At least two mechanisms exist for accounting for micro-PK, including its distance independence, via retrocausation. One is the DAT model proposed by May, Utts, and Spottiswoode (1995), in which operators attempting a PK task adjust their behavior so that data collection is preferentially initiated when the outcome will correspond to intention. A problem with this explanation is that some experimental databases are incompatible with it. (Dobyns, 2000: Dobyns & Nelson, 1998) However, the mechanism of Echeverria *et al.* (1991) provides another means by which the existence of retrocausation seems to imply the existence of something akin to PK.

The solution given in that analysis to the problem of paradoxical, self-inconsistent event sequences is that any such sequence implies the existence of a slightly different sequence in which the events are self-consistent and therefore possible. Usually, in fact, there will be many such alternatives and one must resort to quantum mechanics to assign probabilities to the possible outcomes. The situation allows a Holmesian paraphrase: 'Once the impossible is eliminated, whatever remains, however improbable, must come to pass'.

Consider, as a *Gedankenexperiment*, a reliable computer equipped with a reliable retrocausal information channel. Let it be set up in a paradoxical configuration, e.g. that at 10 a.m. it will send a signal to itself at 9 a.m. which will cause it to shut itself down. Since this is an impossible sequence, some alternative event, such as a failure in the computer, its power supply, or the communication circuit, must happen instead. Even though such events are very low probability for a 'reliable' system, their probability cannot be reduced to zero, and the vanishing probability of the paradoxical sequence means that one of the alternatives must occur in its stead.

Now expand the apparatus to include a physical RNG connected to the computer, and expand its program to query the RNG and to send the shutdown signal only if it receives a 0 bit in response. There is now

a paradox-free course of events that has probability 0.5, as opposed to the minute probability of an equipment failure. It would appear to follow that an unbiased RNG connected to such a system will (almost) always produce 1s, even if it produces a proper even mix of 1s and 0s in isolation.[10]

If the computer connected to the RNG is replaced by a human being with some capacity for retrocausal communication between past and future selves, most of the essential ingredients still seem to be present. The reliability of the communication is presumably fairly low, but this is a quantitative rather than qualitative change. Any retrocausal communication presumably takes place at a subconscious level, since the agent is attempting to conduct a PK experiment rather than a precognition experiment. What does seem to be missing is the conscious intent to create a time paradox. However, the desire for a specific outcome would seem to create a possible substitute in that the PK agent only wants to know about positive outcomes. While, presumably, most of the time a paradox will be avoided by a failure of the erratic precognition channel, sometimes it will be avoided by the RNG producing the desired outcome. This differs from the DAT mechanism in that the statistical unit becomes the element of observer feedback rather than the initiation of data collection, changing the statistical signature of the outcomes.

Summary

The relation of current physical knowledge to psi phenomena as discussed above seems to lead to the following conclusions;

- Most physics-based criticisms of psi phenomena are inadequately supported by theory and may actually be contradicted by evidence.
- Most forms of psi for which there is observational support can therefore be accommodated within the current theoretical framework of physics, particularly if at least one of several possible theoretical channels for retrocausal effects can be confirmed to exist.

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[10] For proper rigor this analysis should be conducted in terms of quantum mechanical amplitudes rather than classical probabilities; however, there does not seem to be anything in the theory of quantum measurement that would significantly alter these conclusions.

- Macro-PK is an exception in that it cannot be accommodated with existing theory and can only be explained by an extension or reformulation of physics.
- Other psi phenomena will become easier to explain under such extension or reformulation.
- Since at least one paradigmatic reformulation in physics is both expected and required by the current state of conflict between the most fundamental theories, it is not unreasonable to consider such reformulation as a way to incorporate psi. A single reformulation might even succeed in accomplishing both tasks.

Future Directions for Research.

These are the questions about psi that seem most interesting from the perspective of physics, along with some suggestions for how they might be answered:

a) Will adding psi to the mix allow physicists to achieve the 'Theory of Everything' that will successfully incorporate gravity into quantum mechanics? Although it is a difficult challenge to get mathematical physicists to take psi seriously, the worst roadblock to answering this question is the lack of quantitative information regarding psi effect sizes, which derives in no little part from the erratic nature of replication in psi experiments. Gaining a good working understanding of the replication problem would be a prerequisite for theoretical unification.

b) Has psi been making unrecognized contributions to physics experiments? This could be answered by retrospective meta-analysis of the physics literature on contentious issues where experimenters had significant emotional or personal investment in specific outcomes. A challenge of this meta-analysis would be to distinguish an actual experimenter psi effect from simple methodological sloppiness.

c) Are sensitive experiments vulnerable to psi influences via a PK mechanism? Addressing this prospective version of the preceding retrospective question would require collaboration between a physics research team and parapsychologists; it is also pointless unless human participants who can reliably be expected to produce micro-PK effects can be recruited.

d) Is macro-PK a real phenomenon, and if so what conditions are required to elicit it reliably? (The reliability question is of course crucial to all forms of psi, but macro-PK is of special interest on physical grounds.) Serious and systematic investigation of current claims, even or especially anecdotal ones, seems to be needed to answer this question.

e) What is the relationship of psi sensing, whether as remote viewing or any other mode, to issues of quantum measurement? Can Radin's 'which-way' interference effect be confirmed with other researchers and remote viewers? Can continuous RV observation impose a quantum Zeno effect? Can RV observation prevent a 'quantum eraser' experiment from working? The subsequent questions are simply specific instances of the first one; these can all be answered by direct experimentation, although fairly delicate and expensive physics equipment may be required to set them up.

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GENERAL DISCUSSION

CARDEÑA: I have a couple of quick questions. Hoyt, what is your opinion on the Pantheist position? A couple of people here have mentioned Whitehead, and his vision that maybe consciousness is imbued in everything so that you do not have to work out how you get consciousness into the system. The more I thought about it, the more it seems worth considering. And Dick, you gave a very interesting model with implications for how we might look at macro PK effects, and I wondered if you had any recommendations with respect to other kinds of parapsychology experiences—given your retrospective model, would you suggest doing some things that we don't already do, and by 'we' I mean not physicists but us poor old lowly psychologists.

EDGE: I was tempted to ask you if you could define 'consciousness'! One of the reasons that I suggested that we ought to find out whether psi is a function of nature or of persons is essentially trying to get at this question. I am quite open to the idea that there is a level of consciousness—whatever consciousness is—and I think it is not filled with the kinds of assumptions that the idea of 'mind' has, so I am quite open to the possibility that down to the smallest levels one could find some sort of consciousness. In thinking about consciousness, however, we tend to think about it as a thing and this is something that comes naturally to us in the atomistic rather than dynamic universe in which we think we live. What I want to do in talking about consciousness is to emphasize the dynamic quality; that is, I would want to say just as persons 'walk' so they 'conscious', and I would want to make sense of that idea of 'persons conscious' as opposed to 'persons *have* consciousness'. I think if we can make sense of the notion of consciousness in that sense we are better off, and if you want to then apply it to other areas than persons then you could.

SHOUP: Etzel, you asked about new kinds of experiments. Well, my problem at the moment is trying to find resources to analyse the 110 million trials I have in the can, so I haven't thought about new experiments too much recently, but it is certainly the case that there are things that we could do that follow from the kind of discussions we have had here. In particular, I would like to see more Helmut Schmidt-type experiments where the data are observed by observers in certain points in the chain of data collection and analysis, and not observed in other cases. I would like to control the dependencies in observers; that

would be a first step. As far as macro PK is concerned, I am not too interested in trying to evoke that or do any table-tipping, but it does seem there is nothing in principle to prevent this lectern from rising into the air simply because all the molecules in there that are bouncing around could decide in a highly highly improbable way to go in the same direction for a while, and similarly for the surrounding air molecules. There are, I think, ways this could happen without terribly violating existing physics—it would just be extremely unlikely, so that could be a place to look.

DOBYNS: Just as a quick comment on what was said a moment ago, the notion of macro PK as micro PK 'writ large' where we would co-ordinate a large number of random fluctuations has a great deal of conceptual appeal, but it has the problem that you still end up doing work on a macroscopic object and that energy has to come from somewhere, it can't simply be created by fiat.

SHOUP: But the room gets colder.

DOBYNS: Now we are violating the Second Law instead of the First; I don't see that as an improvement.

SHOUP: To recap if some of the audience didn't hear, York said that the energy must come from some place and I said it must get colder; we all know reports suggest it gets colder with poltergeist effects, and that is a plausible thing to explore. Then York said, well then you are violating the Second Law of thermodynamics, which says that these things tend to disorganize. From my point of view I have the perfect response: the Second Law isn't really a law at all, I call it a tendency; things tend to disorder. But there is nothing to stop them from going in the other direction; if you prepare the situation with sufficient initial conditions a system can become more ordered, and there are examples of that. So I don't see a big problem there, but the unlikeliness of such things might be questionable.

VON LUCADOU: I want to make a comment concerning macro PK; I have investigated a lot of cases and Friederike Schriever has written a nice article about more than 60 cases, which were collected by Hans Bender. I think these cases show us something very interesting. Firstly, such events occur rather often so they are not as rare as one might think. Secondly, they show a certain lawfulness, so not everything happens but only certain things which are rather specific. But what is most important is that they follow a certain time development and they

show a certain structure. We have developed a phenomenological model derived from the model of pragmatic information and weak quantum theory, and this has been published. What came out of this was really astonishing: Fredericke did a cluster analysis of all these historical cases and it turns out there are two main clusters, and these fit perfectly with the prediction of the model, which says that there is a complementarity between the structure of the cases and the behavior reported in cases. This shows up a certain complementarity in terms of weak quantum theory. Another feature is the apparent elusiveness of the phenomena. I gave a paper at the last Euro PA in which I formulated an uncertainty rule for macro PK that simply says whenever you have perfect documentation in a case, then the effect size of what is happening goes down and vice versa; it depends on the organizational closure of the whole system. So there seems to be a rather clear lawfulness behind the phenomena indicating that the main problem is not energy but it is the organization of the whole system. You are right, we do not know how the system works, but I am really astonished about how precisely you can predict the development of these cases. I have never found a case where the model doesn't work. So my question is, this would say we have found a model that is equivalent to other physical models so where is the problem you mentioned? We do not know the mechanism in detail but we have so many processes in nature where we do not know the mechanism in detail but we know how it works. This is maybe sufficient.

DOBYNS: In response to this I would simply comment that while phenomenological studies are all well and good and I approve of any increment in our understanding of the phenomenon, this does not alter the fact that if the phenomenon occurs at all it is violating what we consider to be fundamental laws of physics and therefore those laws require revision.

HÖVELMANN: Just a remark concerning Hoyt Edge's paper, which I very much agreed with. However, I was not completely convinced by the parallel that you drew when you compared the mind-matter relationship to the relationship between night and day. I think the relationship between night and day is publicly perceivable and communicable, whereas mind is a private phenomenon and is difficult to communicate, and I don't believe the parallel is as obvious as you made it out to be.

EDGE: My point is simply that when you look at the characteristics of mind and matter as they are defined by Descartes, they are defined in terms of each other. So the question is not whether they are the same sorts of things, with one more private than the other, but that conceptually they seem to be so close together.

ROLL: This question is mainly directed to York but also applies to some of Dick's observations. York stated there is no known physical mechanism for macro PK and also that energy has so far only been detected subjectively by psychics. These two points seem to be mistaken. Blanchard, in a book with his associates some years ago, described how the location of a microphysical object is due to four quantum numbers of which one can be referred to as the spin of a fundamental particle. If one of these quantum numbers is altered, the object—Blanchard theorised—becomes unstable in this location and moves to another location where it is again stable. The quantum number that refers to the spin of a fundamental particle can be affected by an applied magnetic field, and we have observed that the beginnings of poltergeist experiences are associated with an increase in human magnetic disturbances. In other words, there does exist a quantum mechanical model to account for the movements of large scale objects. Secondly, York claimed that energy has not been detected. In fact it has been on two occasions; first by William Joyce at Duke and secondly by Bill Joines and Steven Baumann and others. In these studies two gifted individuals were able to produce effects that were detected by photomultipliers, and it is said that these people could emit photons that were visible in a darkened room. If this is taken together with the Blanchard claim then we have empirical and theoretical evidence that macro PK takes place.

DOBYNS: In the first place regarding Blanchard's physical model, the model you describe is not part of current physics and, frankly, if it is postulating a change in macroscopic location due to manipulation of spin quantum numbers in a macroscopic body then I at least would find it difficult to integrate with what we know of current quantum physics. I would certainly not regard it as a credible physical theory until some of its prediction had been tested and verified with experiments in domains other than macro PK. Regarding the matter of energy, considering the ubiquity of reports of these energy sensations by all manner of talented individuals, I do not find reports of photo production by two specific talented individuals to be all that

illuminating. One of the major reports of macro PK is the well-recorded incident in which Ingo Swann caused a magnetometer to register through its shielding—does that mean that psi is *magnetism*? I am afraid that I at least would classify the measurements described as simply another instance of a PK experiment with a very interesting and valuable result, but not necessarily relevant to all the thousands of sensitives and practitioners who report feeling something they call 'energy' and projecting something they call 'energy', but who do not physically glow in the dark.

VAN DE CASTLE: This is not exactly glowing in the dark, but years ago there was a lot of interest in Kirlian photography and there got to be a lot of legitimate complaints regarding its lack of reliability. There has been a version now coming out in Russia by Konstantin Korotkov called the gas discharge visualization technique. An electrical pulse is passed through your fingertips which releases a gas vapor and it shows up as a kind of aura around each fingertip. Then you can take a computer-enhanced imagery that allows you to get 18 different parametric measures from that, so there have been huge advances. I spent some time over there with them in the lab and saw some things that I felt were fairly outstanding. My second comment is for Hoyt. You started out by saying no-one in the audience did cross cultural work then you recognized that I had done some. This includes six trips that were sponsored by the Parapsychology Foundation to do some field work in Panama with the Kuna Indians down there. I had made ESP cards with jaguars and sharks and so forth for stimuli. I found I got results that were successful enough for me to be happy about publishing them. The last year I was there one of the Kuna said to me, 'why do you think we can relate to you? You are a white guy and you are not one of us'. It seemed obvious that I didn't have the right kind of connection to be their sender, so I had them choose who they would prefer to be their sender. They chose and we went through the study and got results that were so outstanding that I still have a hard time believing them.

EDGE: Thanks, I meant to say that I was surprised no-one had said anything about cross cultural work, not that no-one had done it. I agree with you about the importance of working within a culture rather than coming in from outside; this is why the work I have done in Bali with the support of the Bial Foundation has always been with Dr Suryani and she has always been the one directly working with the Balinese

precisely for this reason. But not only is Dr Suryani so well known to the people who participate in our studies, she is a figure of high regard. I have often wondered—but did not have a chance to investigate formally—how much the experimental evidence has been affected by that kind of relationship as opposed to another Balinese working with these people. But you are right; we have to be sensitive to that.

WALACH: I have a question for York Dobyns. You said that the EPR measurement was analogous to what happens in parapsychology; you have entangled photons for instance, you measure one and the other, and then compare the measurements. I am not sure that is what is happening here, and wondered what the views of other people are here. As far as I can see what is going on there is you have streams of measurements in both detectors, and the way you find out whether they are correlated is not that you compare them but you compare the streams of measurements you have to a theoretical distribution which you derive from the Bell inequality. That is a fundamental difference because we don't have a theoretical model to find out whether measurements are correlated or not, is this not the case?

DOBYNS: No, that is not the case. You cannot establish whether Bell's inequality has been violated or not except by comparing the two streams of measurements with each other. The inequality provides limits as to what the correlation can be under different conditions of detector angle orientation in the standard polarization approach but there is no theoretical distribution to compare the individual streamer against. If you only look at one of the detectors you have literally no way of knowing whether an entanglement is present or not.

WALACH: No, you are looking at two, but when you compare the two to the boundary conditions of the Bell inequality that is the decisive point.

DOBYNS: You compare the correlations to the Bell inequality. However, for purposes of the experimental structure, the fact that the entanglement is violating a certain mathematical limit based on classical assumptions is actually not all that important. What is important is that a correlation exists which can only be established after the fact by comparing experimental results from the two different sites, just as in a PK experiment we cannot attest that PK has happened or not until we compare, for example, the list of intentions or activity periods of the agent with the output of the target. In a remote viewing

experiment we do not assert that successful remote viewing has taken place until we compare the transcript of the viewer with the properties of the target.

WALACH: But would it not be the case that without the Bell inequality you wouldn't be able to tell?

DOBYNS: The Bell inequality is merely what tells us that the correlation is not resulting from classical fixed information transmitted separately to each location by classical particles. It is not needed in the parapsychological equivalent where we do our information exclusion by making sure the REG box does not have external control on it, or that no-one is slipping your viewer a piece of paper saying 'the target is the museum in Anaheim'. In the physics experiment we have a mathematical proof of information exclusion provided the correlation displays certain mathematical properties. In the parapsychology experiment the absence of classical information is hopefully imposed by our controls, if we are doing them right.

VASSY: I have a question about mind-body dualism. I always find it very instructive to think of the time in physics when there were four different kinds of forces: gravity, electromagnetism, strong nuclear and weak nuclear forces. None of them could be reduced to the other three, so none of them could be explained by the other three. Yet nobody thought at the time that reality somehow is divided into four different realms and we should think ontologically in terms of four different things in nature. Therefore the moral of this situation for me is that when we tend to think of mind and body as two ontologically distant things, is it not because they cannot be reduced operationally or in any other way to each other, because this need not imply that they are fundamentally different. Maybe we are culturally conditioned to think mind and body are so different, and subjectively we feel that they are different. But if we can extract ourselves from all of those subjective and cultural assumptions and try to think objectively about them, then perhaps we would not take those differences so seriously as we do.

EDGE: I think this is a response that is interesting and logical. If we have been conditioned to think of them as separate, one can argue we can also be conditioned to think of them as un-separate; if we begin to use the language of conditioning, you have to apply it in both cases. I was interested to hear you say in your delivery of Ed May's address that you are able to teach parapsychology because your colleagues

knew that you wouldn't fill students' brains with heresies. When I started teaching as a graduate student philosophy in 1968 I asked my students, "is the thought of 'fried chicken', for example, a brain process, or is that statement equivalent to the logical contradiction that 'a square is a circle'?" Out of 25 students, 23 accepted that claiming that a thought is a brain process is equivalent to saying a square is a circle — it is a logical contradiction. But 20 years later when I ask the same question, I get the reversed conclusion; 23 said not only is it possible but it has to be true. Now these are 18 year olds, who are not yet particularly sophisticated in the ways of science, but they reflect a cultural shift within a period of 15-20 years that was dramatic enough to move from saying something is completely illogical to saying it has to be true. The point I was trying to make in my talk is if that kind of cultural shift happens so quickly, then one should not be surprised if a lot of conceptual baggage is brought into that conceptual change.

VASSY: I wanted to show with an example from physics that just because two things are not reducible to each other does not mean that we have to think of reality as divided into two different things.

EDGE: An interesting analogy, I think the dis-analogy is that these two things have been defined in contradiction to each other. If one has any characteristic of the other then it is automatically the other—you don't have that with the four principles; this is a unique characterization of mind and body.

SHOUP: I think it was 't Hooft who got his Nobel Prize for unifying weak nuclear force with electromagnetism. But unifying mind and brain is the ultimate problem for us because we live here. My identity exists here, connected to my brain. This is part of Eastern tradition and we are trying to build that bridge. As an exercise, take an afternoon and try to avoid all personal pronouns; instead of saying 'I think XYZ', or 'my brain thinks XYZ', say 'this brain is thinking' and try to remove your identity for a while. It is really instructional and to the point that you are making—there is a strong bias that we can't give up.

EDGE: I agree with the latter part of what you said, but do you really mean that 'your experienced identity is with your brain'? I have to admit I don't know what that means. I can understand that theoretically, but I have to admit that is not my experienced identity, which has nothing at all to do with my brain. It has to do with my personhood and it is only a theoretical construct that allows me to talk about the brain.

Interesting, and important, and perhaps we ought to be talking that way, but let's not think that is the way we experience the world.

CARR: In discussing the connection with physics, York gave us three possibilities: one, that there need be no change; two that you need some extension; and three that you need some giant revolution. I would like to push the idea you really do need revolution; you can't just make small extensions. He also made the point that such a revolution is bound to come from within physics itself, and I think that's a really crucial point. I think attempts to explain psi just in terms of entanglement and quantum theory cannot be the full explanation. Physicists do not understand quantum theory, so to try and explain one mystery in terms of another does not get the bottom of it—there has to be a more fundamental picture that will enable us to understand both psi and quantum theory. I think in a way that York was not sufficiently ambitious in just confining his attention to ESP and micro PK, because actually it is a more fundamental problem; it is not just the matter of explaining psi in physical terms but of explaining any mental process at all that is the challenge. Physicists never talk about the theory of everything; they're just talking about a theory of physics. But half my experience is not in the physical world, it is in this other domain, the mental domain, which doesn't just include ESP but also dreams, out of body experiences, near death experiences, a whole range of phenomena that I think are intrinsically linked to any theory of psi, indeed a theory of mental processes. My view is that there is a good prospect of that, especially in the higher dimensional approach which is beloved of string theorists at the moment. I think that is the way to go, but whatever the solution, it will require going much deeper, and whatever change we must go through is going to evoke a lot of pain on part of the physicists. I think there will be this great paradigm shift to incorporate psi, but it won't be easy, even for the physicists.

DOBYNS: The problem with the grand revolution approach, which I think is going to be called for, is that getting the revolution—or to get the right one, as opposed to something that ignores part of reality—will require someone who is profoundly mathematically adept, capable of handling all the intricacies of the current physical theory that things must reduce to, and simultaneously encompass the whole phenomenology of psi and incorporate it. I am not up to the task; I hope no-one is offended when I say I don't know anyone who is. We are

probably looking for the next Newton or Einstein to be produced by the fates.

JOSEPHSON: I would like to get back to the energy question. First, the boundary thinness dimension that Christine talked about may be relevant, in suggesting that there are features of our environment that most of us are unaware of, but the psychic may have thinner boundaries and so be more sensitive to them. This could provide a source of energy that would not violate physics directly.

SHOUP: I do suspect that there are connections among all of us to a degree that are totally unappreciated, either in terms of quantum entanglement or some residual connection or togetherness from our common origins. It is maybe worth pointing out that correlations can be entirely invisible if all the particles involved are not measured simultaneously; you can have entanglement between 2, 3, or N particles such that you cannot see any effects if you examine N-1 or fewer of them, and so that might mean such things exist even in weak entanglement among very large numbers of particles you may not see it all and yet under the proper invocation in some fashion that could become manifested. We ought to all look at this.

VARVOGLIS: Hoyt, I'm generally supportive of your arguments about naturalism, and think that is a good way to go, but I am less convinced by your historical and cultural arguments. The fact that Descartes might come up with a theory and that you can show its cultural and historical relativism is not an argument against its validity. We can have insights for different reasons. The fact that students now see no problem of conceiving of a thought as a brain process does not argue for there being any greater insight there. It could be students are now more culturally conditioned by Dennett and the Churchlands and so on into thinking that thoughts *are* chemical interactions, and in itself does not show evidence of progress. This is not an argument as to one being more valid than another.

EDGE: I would love to think my 18 year olds had read Dennett and the Churchlands! The serious point is that the traditional philosophical approach of simply abstracting and looking at ideas fails to recognize that these ideas are proposed to do work within a context, and this approach does not let us understand all of the assumptions these concepts have. I have not presented an argument—and I agree with you on that—but what I was trying to say is if you understand the work that

that concept is doing in that context then you will understand more about its assumptions.

VARVOGLIS: Dick, the idea of retrocausal influence is a very important idea, which has considerable data starting with Schmidt's experiments. How would you close a system in terms of future observers, how would you decide you were discovering lawfulness in nature as opposed to some future observers' hypotheses coming into the system and continuing to introduce new information, which you could no longer call lawful, independent of that observer?

SHOUP: It is a very good question—where does the experiment end? How can it be isolated from the future or the past? I don't have a good answer to that; I can only think about the connectivity, the dependence relationships and the structure of the experiment, whether you let the information affect any further in the future for example by publishing or by allowing other entities to be affected by results of an experiment or the outcome of a particular measurement.

VARVOGLIS: The reason I bring it up is because I'm taking into account York's position about whether or not we have to disturb science too much. It may be true that with retro-causal and time symmetric postulates we do not disturb science too much, but *methodologically* it is a disaster for science in terms of epistemology. It is a big shift, maybe not in terms of assumptions but it is a big shift.

PERSONAL REFLECTIONS ON UTRECHT II

CHRIS A. ROE

It is a thankless task to attempt to summarize the issues raised at this special conference. If you share my thoughts then they will seem obvious and simply recapitulating them here could seem something of a waste of time. If you don't share my thoughts then I run the risk of seeming wrong-headed or, worse, willfully ignorant of the principal lessons to be drawn—especially if they were given in the paper that *you* presented. On the other hand, it allows me the indulgence of pronouncing on the future of parapsychology without the constraining briefs the other presenters have had to work to—I can take a broader view, so long as I can relate my thoughts to the papers we've heard.

How Have Things Changed?

My first thought was to contrast our approaches and speculations against those of Utrecht I, as recapitulated at this conference by Carlos Alvarado, but 55 years is a long period of time by most human-calibrated criteria, and is an age in terms of scientific advancement, where innovation can sweep through very rapidly. It is no surprise that the methods—and even the phenomena as we measure them—have changed out of recognition. Although I do have some sympathy for those who complain that we are not sufficiently concerned to work more intensively with gifted individuals, I think that this reflects a more or less conscious re-orientation of the field to position us as inside the scientific community looking out rather than outside looking in.

Hoyt Edge talked about the profound shift away from assumed dualism in our conception of our phenomena and our speculation on appropriate explanations of them. We see now more serious efforts are being made to accommodate psi phenomena in our understanding of conventional—if at times spooky—physical constructs such as entanglement or retrocausality rather than positing some aspect of self that is fundamentally non-physical in nature. Even in Fatima Machado's presentation on haunting and poltergeist cases, the spiritist position is described in relation to beliefs of the *experient* rather than of the investigator. Martina Belz has described how clinical para-

psychology similarly commits to understanding clients' experiences in their own terms but does not commit to explanations that invoke new entities.

This may not be what the founders of psychical research envisaged for the field, and it may not meet Eileen Garrett's aspirations, but it does confer some advantages:

- Allows a rapprochement with establishment 'science'
- Counters concerns over parapsychology as 'deviant', 'mysticism', or 'pseudoscience'
- Places psi effects in a context of recognizable (and generally accepted) methods and procedures
- Makes available to us terminology, constructs and models that may have some value in explaining observed patterns or in suggesting hypotheses

We can turn to available physical or naturalistic theories to explore how they might accommodate psi—and to this end we must be reassured by the authoritative pronouncements from York Dobyns that there need be nothing inherent in our phenomena that *necessarily* places them outside the sphere of conventional physics. Bernard Carr's recent monograph[1] similarly explores how psi phenomena might be understood in terms of current physical theory.

In such a recasting of psi phenomena we don't hear so loudly the cries for a scientific revolution or paradigm shift—our science is no longer deviant (in Jim McClenon's terms) or mystical or different from other scientific endeavors in its operation. It can safely be accommodated in university departments, as Deborah Delanoy described, without evoking fear that students will be indoctrinated in heresies. This must help to defuse the antipathy that parapsychology has faced at times from the scientific establishment.

We use generally accepted methodologies and adopt standards of good practice to be found in related fields such as neuroscience or psychophysiology as described at this conference by Thilo Hinterberger, Eva Lobach and Jeanne Achterberg. These have a sense of comfortable familiarity for mainstream audiences that help to

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[1] Carr, B. (2008). Worlds apart? Can psychical research bridge the gulf between matter and mind? *Proceedings of the Society for Psychical Research*, 59(221), 1-96.

'normalize' the surprising data that they have to offer, and makes our interpretations of those data more difficult for them to dismiss out of hand. As subsequent generations of researchers come through who have a grounding in these approaches (so that they are psychophysiologists with an interest in parapsychology rather than parapsychologists who have learned some psychophysiology along the way[2]) then they are better able to establish their credentials by contributing competently to other, more mainstream concerns. And when they have demonstrated their methodological or technical competences in work that focuses on mainstream problem areas—work that has been generally accepted by that mainstream, as evidenced by presentations at their conferences, published papers in their journals, and so on—then it becomes increasingly difficult to dismiss any parapsychological evidence they present that has been gathered using those same methods and apparatus; either all their work must be rejected or none of it.

Another advantage of overlap with related disciplines is that their terminology and concepts become available to us. This can provide new ways of thinking about our phenomena, of organizing otherwise disparate observations, and—taking such models to be intended as metaphors—can direct attention to neglected features or allow us to pose otherwise inconceivable hypotheses and design programs of research to test them.

Dangers in Adopting Concepts and Terminology

However, there are also dangers in appropriating terminology from other disciplines. We can unwittingly inherit semantic confusions, or can stretch the application of respectable terminology to distortion point when we move it from one context to another. We have seen examples of this over the last few days. Etzel Cardeña made a very persuasive case for conceptual confusion in the use of the construct 'states of consciousness' (SoCs). He argued that such terminology encourages the unwarranted assumption that consciousness can be thought of in terms of relatively discrete and fixed states. In practice the validity of these states is rarely tested, and instead tends to be defined operationally in a deeply unsatisfactory way that results in no guarantee

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[2] Or they are neuroscientists with an interest in parapsychology, or anthropologists…, or physicists…, etc.

that any of one's participants represents the variable of interest and makes no requirement to check that they are. It seems as if parapsychologists have followed suit.

I referred to this in a paper I gave on *ESP and Altered States of Consciousness* (ASCs) at the Perspectives on Anomalous Experience Conference held at Liverpool Hope University in 2005. I was developing there comments made originally by Rex Stanford, who criticized the assumption that ganzfeld stimulation was a one-size-fits-all method of inducing an ASC. Very surprisingly, after the initial work by Braude, Honorton and Parker few had bothered to gauge whether and to what extent participants had in fact entered an ASC during ganzfeld. This is frustrating given that anecdotally many of us know of participants who, for whatever reason, experienced no change whatever from their everyday waking state. Where indirect measures have been taken of participants' subjective shift in consciousness, such as Carl Sargent's estimates of time distortion and Jim Carpenter's qualitative analysis of mentation content, it has been those who presented as experiencing the greatest shifts who performed best at the psi task. The possibility that many of one's participants might not have entered the supposed psi conducive state *at all* during one's experiment clearly could be a major contributor to variation in outcome across studies. I will return to the issue of replicability later.

A similar argument can be made with respect to meditation studies, where it has been tacitly assumed that an equivalent SoC will be achieved irrespective of the particular practices undertaken or the degree of proficiency achieved by participants prior to the study. Serena Roney-Dougal and Jerry Solfvin's recent work with students and teachers at ashrams in India[3] illustrates how important is the degree of proficiency and type of practice upon psi performance. In particular, her work draws attention to the importance of the delicate interaction between person and situation variables; a point that was made in Christine Simmonds-Moore's presentation at this conference, and one which I believe should become the central concern of psychological work in parapsychology.

But if we are worried about the inappropriate application of relatively narrow concepts like SoC, then there are other terms that

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[3] e.g., Roney-Dougal, S.M., & Solfvin, J. (2006). Yogic attainment in relation to awareness of precognitive targets. *Journal of Parapsychology*, **70**, available at http://www.psi-researchcentre.co.uk/article_9.htm.

have been used quite liberally at this conference that should also be of concern, and may undermine attempts at a rapprochement. I will give two examples: 'consciousness' and 'entanglement'.

'Consciousness'

I must caveat what I'm about to say by being clear that my concerns do not relate to the data or to their claimed relevance to parapsychology. But in interpreting or describing findings in terms of evidence of 'effects of consciousness' it seems to me that we are reflecting a lay or common sense understanding of the term—at which level who can deny that consciousness is implicated in the processes that we are interested in, such as coherence in EGGs or positive healing effects? But what we mean technically by 'consciousness' in these situations is quite nebulous. It isn't clear how an explanation in terms of 'consciousness' can be used productively—for example, to specify necessary and sufficient conditions for the action of psi—when its meaning varies from speaker to speaker (for example, in some cases the attentional system is implicated but in others it is not). References to consciousness at this conference also seem to eschew the quintessential properties of consciousness as argued by philosophers such as John Searle and David Chalmers (and *contra* Dennett and the Churchlands)—qualia and intentionality. I think there's a need for some conceptual clarity here.

Similarly, when speakers have alluded to psychological intentionality[4] as having some role (for example in determining the outcome of otherwise random or stochastic events), it is not usually clear what form this intentionality takes — does it necessarily require active volition or reflect some need, or can it reflect the passive contents of awareness; can it be understood simply in terms of the action of cognitive and attentional systems or does it necessarily involve semantically deeper tagging of the target system as personal and important?

[4] Psychological intentionality reflects the familiar lay sense of purpose or will, whereas the philosophical sense of intentionality (mentioned in the previous paragraph) refers to the referential status or 'aboutness' of mental contents, that they make reference to external objects and events.

'Entanglement'

There seems to me to be a real danger that talking in terms of entangled systems makes use of a quite superficial metaphor that is intuitively satisfying as a label to capture the essence of the posited relationship between entities precisely because it is vague enough to be fitted to a variety of qualitatively very different relationships and situations. I accept that as a nonphysicist I could be completely wrong in my suspicions, but this descriptor does seem to be credited with much more explanatory power than it actually has. It is not clear to me how it helpfully limits our thinking as well as opening it up—for example, what does it have to say about the necessary or sufficient conditions for entanglement? Compared with other accounts, what surprising (and as yet untested) predictions does an entanglement explanation make? And, importantly, what outcomes would serve to refute it?

If the metaphor is intended to draw attention to similarities in mechanism between the macro- and the microscopic, then as a nonphysicist I find it difficult to see how the entanglement of subatomic particles (perhaps even single neurons) is, in important ways, analogous to human beings having an emotional connection or shared history. I can't see how the realization of states in an entangled system through the act of observation of parts of that system has anything to say about the kinds of target system discussed at this conference, such as Thilo Hinterberger's analysis of neurological responses to pictorial stimuli in persons that are separated from those stimuli by space or time. To my eye, the physical systems that exhibit entanglement involve correlated elements that share some basic properties, but in the remote perception experiments described here the to-be-correlated elements seem to have nothing in common with one another; for example, in Dick Shoup's *GotPsi?* on-line card guessing experiment, my subjective experience of an image of the girl with a pearl earring and its neurological instantiation have *no* properties in common with its pictorial representation on screen or on a computer memory chip. The only point of correspondence is at a *semantic* level, a level of meaning that does not exist in the world but only in the person or people involved in the experiment. It is not clear to me how this discrepancy is to be resolved in a manner that gives us something that can direct our thinking and our empirical efforts.

So, although there are clear advantages in emphasizing methodological and conceptual links that we have with other

disciplines, this can be undermined if we inappropriately apply or extend terminology or concepts that we borrow from them. I think we need to adopt the philosopher's care and precision with our technical language so that we are clear and consistent in what exactly we mean by them.

So What Have We Learned?

Notwithstanding these concerns about theory and terminology, this conference has given us a chance to reflect on what we have learned thus far. We have heard a number of papers that summarize particular areas of empirical activity and that provides a useful service, since we can't speculate on where we're going as a discipline without knowing where we have been and what we have discovered on the journey. I'd like in this section to complement those earlier contributions by looking for other patterns in our data or picking out methods that haven't yet had so much airtime at this conference.

Before I get to data I must also note that in my view scientists tend to be either conservative or liberal. Conservative scientists tend to be more concerned about Type I errors, where an effect is concluded to be real when in fact it reflects the operation of chance or error. I think parapsychology is mainly populated by these.[5] Liberal scientists are more concerned about Type II errors, where an effect is concluded to be due to chance when in fact the hypothesized effect is real. I am a liberal scientist. I think there are hardly any drawbacks to erring on the side of being more prone to Type I errors than Type II — *so long as the conclusions drawn are in relation to how they direct further work rather than in terms of definitive judgements about theory or even world view*. (For the latter I doubt we have any databases that have accumulated sufficient robustness while exhibiting comprehensible or characteristic internal properties to justify this.) My comments here reflect that liberalism. In terms of the most promising avenues for further research, then, I would highlight the following working hypotheses:

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[5] I think that this in part accounts for Ed May's complaint that as a community we are reluctant to assert the reality of certain phenomena, despite reasonably strong evidence for their occurrence.

- Psi may largely be an unconscious process; our most consistent results accrue from protocols that take direct physiological measurements (such as DMILS, presentiment), use behavioral indicators rather than self-report (such as time-reversed effects), or incorporate 'hidden' psi tasks (such as tests of the Psi-Mediated Instrumental Response theory, global and field RNG effects, and cases of RSPK).

- Where psi information is brought to awareness it is mediated by altered states of consciousness (such as in protocols using dream ESP, ganzfeld stimulation, meditation, and hypnosis). However, these elements need to be understood as part of an interacting system—there's no one size fits all—so that properties of the person need to be taken into consideration.

- The best individual difference predictors of spontaneous experience[6] are variables that themselves are intercorrelated (such as creativity, schizotypy, dissociation, boundary thinness, fantasy proneness, transliminality, proneness to sleep anomalies, etc.) and reflect an underlying factor pertaining to more fluid or ready access to nonconscious processes. The best predictors of performance in the laboratory will be derived from these.

These are not particularly new, and indeed the claims would be familiar to such as Myers and James, but innovations in technology and discoveries in other disciplines have opened up opportunities for us to devise approaches that could empirically confirm those field observations. Where these have been utilized (as referred to above) they give rise to some of the strongest effects in parapsychology. Yet we have heard from a number of presenters here that parapsychology has a poor record when it comes to replication of its basic effects, with some authors speculating that psi is so evasive and capricious that this elusivity is its defining feature, and so needs to be accounted for in theories of decline effects, models of pragmatic information, or skeptical accounts in terms of the inconsistent effects of fraud and error. In the next section I'd like to speculate on why replication might seem so difficult.

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[6] I focus here on experience rather than belief because the data suggest that belief is a consequence of experience rather than a cause of it.

Parapsychology's Apparent Problem—Why Might Replication Be So Difficult?

I should begin by agreeing that we do have a replication problem in that our expectations about levels of replicability are not being fulfilled empirically. A number of explanations for poor replication have been put forward that I am sure we are all familiar with but that I should like to acknowledge here. Note that these explanations need not be mutually exclusive; they are organized in terms of utility, from what I consider as least to most relevant to my own research within a psychological tradition:

- Dualists might propose that this variability is a property of the phenomenon. Psi is mystical or capricious, and is not compelled to covary with or be affected (only) by material (measurable) factors. It may reflect some nonmaterial agency or intelligence that we do not have direct awareness of. This kind of explanation precludes scientific study so it is not a promising basis from which to direct research! We need to have exhausted all other possibilities before we must default to this kind of explanation.
- Physicists look for a physical explanation for variability—psi effects reflect the properties of systems and current expectations for the nature of relationships or effects we might observe are simplistic. Consistent replication may not be possible in principle if it constitutes signal transfer. This has been a primary focus of presentations and debate at this conference, particularly in terms of nonlocal effects. Of course, it is proper that this explanation is considered seriously, but in my view it would be premature to presume that it is *solely* or *primarily* responsible for the pattern of replication that we observe.
- Statisticians point to effect size and study power. Jessica Utts has compellingly argued[7] that given the effect sizes we are dealing with it is naïve to expect replication on demand. Meta-analyses may give a truer picture of effect sizes, but these can also be misleading. The ganzfeld debate gives a case in point, where more recent summary statistics imply a heterogeneity of approach across studies

[7] For example, see, Utts, J. (1991). Replication and Meta-Analysis in Parapsychology, *Statistical Science*, **6(4)**, 363-403.

that is simply inappropriate once researchers have moved beyond proof-oriented work to explore process issues, for which they deviate from the 'standard' protocol with inevitably mixed results.[8]

- Social scientists would note that the effects we are interested in are a product of complex open systems in which many factors remain uncontrolled and unmonitored. Expectations for replicability are overstated given the complex systems we work with, and any inconsistency is likely a product of the interaction between extraneous variables than some capricious property of the phenomenon.[9] This is not uncommon in psychology; anyone who has taken an undergraduate research methods class knows full well that even the most robust of psychological phenomena do not occur to order—I have been teaching practical classes for approaching 20 years and in that time I suppose that virtually every data set has thrown up at least one idiosyncratic outcome. Such an explanation would also accommodate some forms of experimenter effect, which would be understood in terms of Robert Rosenthal's interpersonal expectancy effects.

- Transpersonal psychologists might argue that psi phenomena are a product of living organisms, specifically those with a capacity to create or attribute meaning — they do not occur in a semantic vacuum. I have been to the last two Transpersonal Psychology conferences organized by the British Psychological Society and have got to know a surprising number of people who are doing work on what I would class as parapsychological effects, although they would not describe themselves as parapsychologists. Virtually all their work is oriented around the subjective meaning of the experience, typically investigated qualitatively (ideographically) and considering causes and consequences in person-centered

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[8] An empirical demonstration of this is given by Bem, Palmer and Broughton (2001) Updating the ganzfeld database: A victim of its own success? *Journal of Parapsychology*, **65**, 207-218. I discuss this point at greater legth in Roe (in press). The role of altered states of consciousness in extrasensory experiences. In M. Smith (Ed.), *Developing perspectives on anomalous experience.*

[9] In this respect, at Northampton we have tried to follow Gertrude Schmeidler's recommendation (e.g., Schmeidler, G. R. [1988] *Parapsychology and psychology: Matches and mismatches*. Jefferson, NC: McFarland) to conduct studies with ANOVA designs that allow us to look for interaction effects as well as simple first-order effects.

semantic terms. This asserts a wider context for the occurrence of exceptional experiences, which don't just happen but are understood as having reasons or purposes for the person that might reflect antecedents to or consequences of personal growth and development. It is certainly the case that spontaneous experiences can be profound and radically alter the experient's metaphysics—*but where do we see that in our study designs?* It may be difficult to engender that sense of personal meaning within a protocol that values control and imposes uniformity, but it is possible. In my view, Bob Van de Castle's description of helper dreams is the most grounded ecologically embedded approach to research discussed at this conference. Unfortunately, much of parapsychological research is as far removed from this as it is possible to be, given our preoccupation with precluding fraud and error. With so little effort to make our tasks personally relevant to our participants, we open ourselves up to displacement effects (where they find meaning for themselves) or experimenter effects (where the experimenter's meaning attribution is far greater than the participants'). With this in mind, perhaps we should not be asking why our replication levels are so low, but rather should ask whether we get more replication than we deserve.

Parapsychology's Real Problem, and Its Consequences

Parapsychology's real problem is not with our phenomena: it is that there is almost no money in the field. It is virtually impossible to prosper as a full-time research-contract worker in parapsychology, and many of our number have to take other jobs to subsist. That is why many UK-based parapsychologists have adopted the strategy of working within the university system, usually based in psychology departments—we have seen too many colleagues forced out of the field because grant applications were unsuccessful or benefactors had lost interest in their work, and we were determined not to have the same thing happen to us. As Deborah Delanoy graphically illustrated, this does have costs, particularly in terms of the demands that are made of our time that restrict the amount of research we can conduct, but there are ways in which this can be managed, and the benefits of stability allow researchers to take a longer view, to initiate programs of research that consist of 3, 4, or more studies that bear on the same problem in the knowledge that the infrastructure for that work is not dependent on

the whims of potential funders. Given the current economic climate and pressures from government for universities to be more cost effective, there is a danger that some of this freedom will be lost as departments look to recoup their overhead costs, but for now at least such planning is possible.

Another benefit is that the university-based researcher may be able to work *pro bono* on projects, since time for research is built into their academic timetable (at least so long as their contract stipulates that they must be research active). Ed May mentioned that our principal funders have placed a ceiling on the amount of money they can award, and that makes it difficult to propose anything other than short-term studies with relatively simple designs, since anything else would be too expensive. But where the principal investigator is university-salaried, then more of that money can be used to service direct research costs rather than paying wages. Thus limited funding can be stretched out to allow for a more systematic exploration through a number of related studies.

This strategy can create opportunities for graduates with an interest in parapsychology to get their first step on the academic ladder. For example, previous grants of mine from the Perrott-Warrick Fund, the Bial Foundation, and the Society for Psychical Research have been used with some success to bring through PhD students, and some of these have now graduated and secured tenured positions at other UK universities despite their research interests primarily involving parapsychology. Three of my current PhD students have the security of three years' funding from such sources. But these opportunities are rare and many people find themselves having to finance their own way through Master's and PhD programs. Ed May mentioned a 'talent filter' in his talk,[10] but to my mind *this* is the talent filter—one that filters out those with talent but limited material resources. I disagree that this inevitably leads to a dilution of quality, as the brightest are drawn away

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[10] While Ed may be right to suggest that parapsychology is populated by 'second tier' researchers who are unlikely to win Nobel prizes (though to be considered subordinate to such an elite hardly constitutes 'mediocrity'!), when I look around at the people here today, and when I think of those I have met at other parapsychology conferences, my enduring impression is of a collection of very bright enquiring minds, unafraid to wrestle with some of the thorniest problems that empirical science has to offer, earnestly and without prejudice. That has not always been my impression of the delegates at other, mainstream conferences, perhaps because in having greater numbers *in toto* they are better able to accommodate some mediocrity in their community.

to better funded and more personally lucrative pursuits. Rather, in my experience this talent filter tends to act as a kind of natural selection, favoring the most tenacious, most committed, most independent-minded researchers, who are not discouraged by the very limited rewards because their interest is in contributing to our understanding of perhaps the most fundamental questions we can ask, rather than in becoming a competent technician for whom scientific research is an interesting occupation that pays the bills.

A consequence of such limited funds (even with creative strategies to make most efficient use of them) is that parapsychology simply cannot afford to be a large discipline; the money we have can sustain only so many. The Parapsychological Association currently lists 108 full members, but of these I suspect perhaps only half are professionally engaged in parapsychological research at this moment. Although this figure might have fluctuated over the years, I suspect that the current situation is fairly typical; for example, at the first Utrecht conference, Robert Thouless warned that "experimental workers in our subject are so few that we cannot afford wasted effort" (p. 23). This impression is consistent with Sybo Schouten's well-known calculation (presented at an earlier Parapsychology Foundation conference)[11] that the person-hours invested across the lifetime of parapsychology from its beginnings with the establishment of the Society for Psychical Research in 1882 equates to only two months' of research in conventional psychology in the United States.

Yet at this conference a number of commentators have used phrases of the sort "despite over a hundred years of research what progress have we seen?" This is disappointing since it seems we have adopted one of the skeptics' most cherished rhetorical devices, intended as it is to have the audience assume that extension over time also translates into extensive and intensive activity, so raising their expectations about the degree of progress that might be reasonable. Against this our actual progress pales and the audience is moved to conclude that the phenomena are non-existent rather than elusive.

We know, of course, that the truth is far removed from this impression of frenetic activity. I recall hearing presentations at a Bial conference some years ago that included a couple of sessions of papers

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[11] Schouten, S. (1993). Are we making progress? In L. Coly & J. McMahon (Eds.) *Psi research methodology: A re-examination.* (pp. 295-322). NY: Parapsychology Foundation, Inc.

from neuroscience laboratories that had received support to explore aspects of consciousness. At the end of each talk the speaker acknowledged assistance from other members of their research group, listing them on a final slide, and it occurred to me that more people had been involved (however indirectly) in those half dozen studies than were professionally engaged in parapsychology *altogether* at that time. Yet if those presented studies had been inconsistent or delivered null results, I doubt that the laboratory directors would be wringing their hands over lack of progress or inherently irreproducible results. Thus do we need to temper our expectations for parapsychological research to take into account the circumstances in which our research is carried out.

It also seems likely that the limited number of persons involved in parapsychology has consequences for the nature of the research that is undertaken. For example, parapsychology seems to attract innovators, who have been successful in developing new protocols or adapting methods from other areas and in demonstrating 'proof of principle' by reporting significant psi effects using such methods. We also have a number of 'early adopters' who are quick to seize on new approaches and technologies and are responsible for the first wave of independent replications. However, relatively quickly the innovators lose interest in simple confirmations and move on to develop yet more methods and approaches, with the early adopters soon following suit. I am sure that this pattern also occurs in other disciplines, but with their greater numbers they also include many able technicians who are willing to conduct the kinds of modest replication extensions that Kuhn would have called 'normal science'. For reasons given above, I suspect that parapsychology does not include many able technicians, so that interest in the original protocol (and effect) seems to wane. This gives parapsychology the appearance of a 'butterfly science' that flits *en masse* from protocol to protocol as they fall in and out of 'fashion' much as a butterfly flits from flower to flower. At best this is frustrating in diverting resources away from a potentially fruitful avenue of research; at worst it looks suspicious to the outsider, who expects to see continuing and systematic work using a particular method for so long as it is productive, particularly where great claims were initially made for it—why are there now so few micro-PK studies? So few ganzfeld studies? Has Hyman's prediction of an as-yet undiscovered fatal flaw been fulfilled? As a community we need to better coordinate our efforts to give rise to a more systematic program

of research, one that goes beyond proof of principle and first wave independent replications.

However, we are not helped in this endeavor by our funding streams. I mentioned earlier some of the difficulties posed by the limited funding opportunities and the restricted funds available even if successful. These also encourage the pattern of research that I sketched above. Bidding for such small amounts encourages short-termism because the funds could realistically only support one- or two-year projects, and insularity in planning research projects, since it clearly isn't viable to split such projects over more than one institution. The bidding process involves acute competition (by virtue of the very few funding opportunities that are available to parapsychology), and this encourages distinctiveness in those bids, both with respect to bids from other researchers and with respect to background research. Ironically, choosing to fund the most creative and innovative work could possibly be counter-productive for the community as a whole. What we need is for senior members of the community, perhaps through the Parapsychological Association, to approach potential funders to see if they might be willing to support work that establishes an infrastructure for parapsychology that could enable more extensive integration of efforts across laboratories, or support more systematic research over more meaningful time scales (5-10 years), perhaps collectively tapping into European funding. That, I would argue, is our biggest challenge at this point in our history.

Conclusion

I think we have reason to be proud of the advances that have been made since the first Utrecht conference; despite being strangled of resources (and it is here that we need to direct our attention, particularly in supporting opportunities for new researchers), we have developed more sophisticated conceptual frameworks that more clearly overlap with mainstream thinking, adapted and refined methodologies that combine security and rigor with greater appreciation of the ecological boundedness of the phenomena, and have slowly built up databases with replicability of a degree that is not out of place for the complex open systems that are the subject matter of the social sciences.

We have attracted some bright young minds who have training in mainstream approaches and methods, and I am confident that they will be capable of carrying forward our agenda. The organizers of Utrecht I

sensed that they were at the beginning of an exciting new phase in the evolution of parapsychology. Looking around the room I echo that excitement for our own future and look forward with great expectations to the next Utrecht conference. Thank you.

DISCUSSION

KOKUBO: I want to give you a suggestion from QiGong research. Many researchers study ESP but the effects are small power and so it is very difficult to replicate. We usually study bio-PK, healing, and the effects are much larger so they are easier to replicate. Of course the study of ESP is important but, I recommend that we should devote more resources to study bio PK rather than ESP. The effects we see suggest that many healers have some control over their ability, for example they can stop what they do at will. However many researchers confuse consciousness with will, so that their studies are based on free will; but this is not a property of consciousness, and some of the difficulties associated with ESP studies are a result of this confusion.

ROE: And the people who are successful in these bio-PK studies, do they go through any particular practices in preparation, or have they had any particular training to make them able to perform at will?

KOKUBO: The evidence I have from discussions with strong healers suggests that there are two types: one type uses energy healing, whereas the other works by sending out warm intentions and their focused will activates the patient's own autoimmune response. Energy healers emit a kind of power that might be measurable while intention healers do not, and so we should consider these two types separately. In healing studies we have to differentiate between psychological and physical healings, and QiGong is physical. Also, some people claim that imagination is important in the process of energy healing, but this is not true. Imagine you have damaged your hand and can't use it. In the first stage of rehabilitation it may be useful to have you imagine the action of picking up a pen and writing your name, and this imagination might help you achieve the goal. But in later stages of rehabilitation you should be able to use your hand without the need for conscious will or imagination to guide it. Similarly, in the early stages of development energy healing training usually involves having the subject imagine an energy probe, but in later stages this is no longer necessary. Bio-PK

may, then, be like other bodily functions, and the source of its power is our body.

WEST: I noticed particularly that Chris began by saying that not only our methods but also perhaps the phenomena seemed to have changed in 50 years, and this brings me to my point. What are we actually studying? There is a great deal of talk about psi, but this includes various things, including macro PK. We have heard talk at this conference of theories that might conceivably include macro PK but we have not heard anything about any recent research on macro PK, which leads to my questioning whether or not we should include macro PK as part of parapsychology. I think there are parapsychologists who are pretty skeptical of macro PK, and there are reasons for this. If you look at the history of macro PK, originally I think it was mostly concentrated upon séance room phenomena, at a time when things that went on in dark séance rooms were technically difficult to control. Nowadays of course we can use instruments to see in the dark and more effectively control conditions, and this seems to have coincided with a diminution of that kind of phenomenon—some people would claim because it never really occurred in the first place. Historically, people used to investigate macro PK by trying to produce fairly delicate but macroscopic tests such as causing a pendulum to swing or a balance to move, but now all that sort of thing seems to have disappeared, and I just wonder whether one ought to just forget this strand and say either that it is impossible to investigate or conclude that it no longer happens. We see similar macroscopic and microscopic effects in ESP experimentation; the Rhinean effects were almost macroscopic in that you didn't actually need statistics in order to see them, the effect sizes were enormous in comparison to what we see now. But is this because the earlier effects were fallacious because of poor methodology, or is it perhaps somehow part of the phenomena that makes it more difficult for them to occur now?

ROE: My response to the first part of your question is that macro PK certainly is still part of the curriculum of parapsychology. I think our primary obligation is to provide explanatory frameworks that help people understand their anomalous experiences, and they do still report these kinds of experiences. Whether those explanations turn out to be in terms of 'normal' causes or less normal ones is another matter and does not affect their status as part of our field. I think the kind of work we conduct means that we find the effects we are looking for; if designs of

experiments are such that you are looking for subtle effects then the system is constrained in a way that that is all you will capture, you will not see these larger effects. This occurs in part because of our paralyzing concern with committing the 'wrong' type of error, a Type I error where we claim an effect that later turns out to be a result of error or even fraud, and a conception that this kind of error is less scientific than a Type II error and potentially fatal for a career, which seems to me to be a little bit wrong-headed. So that would be my position.

JOSEPHSON: I think that macro PK is a real thing. Perhaps it is a matter of fashion that it is not being investigated these days. I think Matthew Manning's demonstrations were almost certainly real. It may also be that a skeptical atmosphere inhibits the effect and perhaps some people become more skeptical over time.

ROE: That leads nicely into something else I intended to say, which is that one of the consequences of there being relatively few active parapsychologists and those people who are active being very fashion conscious is that we have a discipline that does seem to flit from preferred protocol to preferred protocol, and from phenomenon to phenomenon, following trends. And because there are so few of us, that takes up much of the resources of the field. Skeptics I think have latched onto this as an indication that the approach or phenomenon that gets left behind when it is no longer the fashion has somehow become suspect. For example, I cannot see any compelling reason why the approaches that arose out of PMIR and were very successful nevertheless have been relatively neglected for the last 20 years; macro PK may be similar. If we were a more substantial body of people then there would be some among us who would be actively investigating phenomena such as macro PK.

MULACZ: Carlos Alvarado started his historical review in 1921 when the first psychical research conference was held. When I compare the changes that occurred between that conference and the first Utrecht conference in 1953 I see much greater changes than I see between 1953 and the parapsychology of today. The reason why those changes occurred is as Brian Josephson has mentioned, it came out of fashion. If we allow those fashions to be that important then we lose a lot of phenomena. I think a historical perspective is very important in reminding us of the strengths of those approaches, such as working with gifted subjects. We could take up the old stuff again but

incorporate new advances such as using technology that could use infra red to monitor what goes on in the dark. That way we can arrive at new solutions to old problems.

ROE: I think it is difficult because I think there is an encouragement for people to attempt to replicate new approaches, particularly where the initial findings are especially promising. Clearly we have limited resources and if people are occupied doing that then they are not able to do something else.

DOBYNS: I notice that you remarked that parapsychology would be more successful if it conveyed the impression of not being mystical, of being respectable science done in a non-threatening mainstream style. That may very well be true in terms of attracting funding and gaining normalized acceptance, but if so I think you are looking at a Catch-22 situation because even at the rather superficial level that we have been discussing experimenter effects it seems abundantly clear that if we want to have a reliable experiment then we need to be mystics. We need to take responsibility for the content of our own minds and regulate our expectations and attitudes in a fashion that no other branch of science feels obliged to do.

ROE: I am not arguing that we need to throw out everything that we feel is important and that includes the semantic context, the importance of which I tried to emphasize in my reflections. But there have to be points of contact with the mainstream if we are to demonstrate that we are credible and legitimate and that the data we are reporting are valid. This is essentially the philosophy that Rhine advocated in adopting an approach that was in its essentials familiar to other experimental psychologists and which didn't require researchers who were especially gifted in some mystical way or who had been through some obscure initiation. Rather, he made the implicit promise that anyone who followed a set of clear methodological steps would have a good likelihood of producing an effect. Now that isn't to say that ritual doesn't have a place within a wider fixed framework. Someone like Marilyn Schlitz, for example, in conducting her DMILS experiments will have a personal ritual which for her gives that session personal meaning, but this is set within a larger context in which her experiment includes lots of features that are recognizable and familiar to other psychophysiologists so that when she is presenting her data to those people they can see that indeed this study was done appropriately, that

it meets recognized standards, and if all those things are in place they are more likely to attribute competence for the aspects that are unfamiliar to them, and are more likely to at least consider the data she is presenting. One of our problems is that we tend to be dismissed out of hand, which was one of Ed May's points, and this strategy at least gives us that airtime.

VARVOGLIS: I hadn't thought of it the way you presented it, but I think I'm going in that same direction. To address the question of why we are not studying macro PK so much these days, one thing we didn't mention yesterday when Deborah gave her talk about parapsychology in universities is that as we try to get more politically correct and acceptable so that we can communicate better and so we can have access to better funding and can spread the word softly, there is a price to be paid that is more than that we have teaching and administrative duties. We also have to 'mix water in with our wine' as they say in French, which is that we are studying safer topics, going about our business in a safer, more acceptable way, and so macro PK is a victim in part of our own desire to get into the system and tendency to leave behind the fringe stuff that is too threatening. At the Institute Métapsychique, for example, we haven't touched instrumental transcommunication because we think it is too hot a topic. There might be something going on there with what is a technological equivalent of mediumistic phenomena, but we don't want to touch it because it doesn't go along with the respectable staff that we are building up. As a field we are tending toward being more Type I paranoids than Type II paranoids just making sure we don't make any type I errors. We've been squeezed into that position by skeptics; it's appropriate but there's a very real danger that we're losing something there. Another point I would like to make is that your presentation is in a sense a very classical perspective on what we're studying and how we go about studying it, and in a way it doesn't at all reflect another perspective that's been coming through this conference, that maybe we're studying inherently unstable phenomena and we just have to accept that and get used to it and change our methodology to suit.

ROE: I think that for me personally parsimony dictates that where I need to consider these different accounts or explanations for why psi might be capricious it is sensible to concentrate my initial efforts on those explanations that invoke factors that are familiar to the mainstream (such as interpersonal experimenter effects and poor

control of extraneous variables) rather than those that rely on spooky properties of nonlocality or entanglement, if only because the former are likely to be more straightforward to rule out if incorrect. I should also comment that I think the kinds of shifts in interest we have been talking about in this discussion are not, in my view, the result of self-conscious political maneuverings aimed simply at making us less threatening to the mainstream. Rather, they reflect the fact that we now have conceptual systems, approaches and methods, and technologies available to us that hold greater promise of solving some of the problems we face and so we have concentrated our efforts on those areas. For example, detecting the occurrence of psi by asking for subjective reports is extremely unreliable because of all the cognitive processing, motivational and emotional layers that stand between the initial 'signal' and the report. Using nonconscious measurements such as EEG and fMRI potentially overcomes those problems and so we might expect these methods to bring us closer to the phenomenon and may show it to be more consistent and lawful than before.

VARVOGLIS: So when will we see a more consistent psi? Is there a deadline for when you would move on to a different class of explanation? Fifty more years? A hundred?

ROE: We have to decide that on the basis of what we are hearing back. What I'm hearing back is that when we look for nonconscious responses to psi stimuli we find them, we have situations where participants are apparently oblivious to what is going on in the psi task but their bodies are reacting and their brains are reacting. That suggests to me that this approach is far from exhausted. I don't think we're in a position to build theory on that, which I think would be premature, but there's sufficient promise for us to continue to look and to look more intensively and to map out some of its properties. Then theorize.

HÖVELMANN: Mario asked in his first question why we are not bold enough to present macro PK to the public, and this could be because it would be perceived as a step backwards to the séance investigations of the 1920s and 1930s. Another reason why we might not see so much PK phenomena today is if skeptics can have an adverse effect on performance and in the meantime many of us have become much more skeptical about such phenomena, so we may be part of the problem!

ROE: Another part of the problem is that I'm not sure what you are getting in return for all that investment of time and effort. We have

many situations on record where people have been discovered some time later to have been fraudulent, or there are enough ambiguities around the way that séances were conducted for the community to be quite ambivalent about how we should interpret those data—take the polarized reaction to the Scole case as a modern example. I don't want to put myself in that situation. I want to have study designs that are sufficiently clean that the interpretation of outcome is transparent. Hopefully the design will be invested with meaning for the participants, but it will also use some of these formal approaches and methods. As such it may be a watered down vintage but it will still be a legitimate form of psi and will be more widely palatable.

JOSEPHSON: Returning to the exchange with York, there seemed to be a suggestion that there was some heavenly deception needed as regards to the rituals that some of us use to get things to work. But if these secrets are not advertised as part of the procedure then this would make replication by the uninitiated very difficult indeed. Another way we could improve our cause with respect to the mainstream is if we could advance a theory, perhaps around a framework for self-organization, which will be convincing to scientists and can deal with some of their problems. Physics is in trouble in various ways—the entropic principle and various ambiguities of string theory, for example—so that getting a more credible theoretical approach may be the way we could gain recognition.

VON LUCADOU: You responded to Mario by saying first do measurements and then create a theory, but of course you can never do any experiment without having a theory. And you have a theory in mind if you do a classical parapsychology experiment; you assume there is a signal behind it, and this is very crucial. You also assume that we live in a somewhat Newtonian environment—only in physics is this different—and this is a very strong theory. Psychologists may not use the Newtonian paradigm in detail but they act as if it was true, yet we know that nonclassical physical models are very successful. So, what I ask myself is why do normal psychologists maintain that theirs is a better view of the world, and when would they stop and consider an alternative worldview such as weak quantum theory?

ROE: Well, I have never made that claim at any stage, but what I have said is that there are other approaches and other levels of explanation besides the physical ones. This conference has been dominated by a

consideration of explanations in physical terms, and that is perfectly justifiable. However, I am personally not particularly interested in concentrating my research efforts on accounting for phenomena in terms of physical constructs, and prefer instead to focus on psychological explanations that speak more directly to people and their experiences. Of course there is a physical reality that underpins those experiences, but this is of much less relevance to the experients and therefore to me. Of course, it could be argued that when physicists working in parapsychology do research involving human participants they show a reciprocal neglect, and don't typically consider the psychological space in which their experiments take place, or fail to treat the participants as intentional beings with idiosyncratic needs and motivations. So each 'side' can probably legitimately criticize the other.

CARDEÑA: You spoke about how the mainstream literature has shown that there can be huge experimenter effects, and these are 'normal' experimenter effects that highlight the impact of variables like experimenter gender, attractiveness, or way of dealing with participants that can have a huge effect on the study outcome in traditional psychology. And so it is a scandal that our journals and mainstream journals don't require us to provide information about the experimenters. There may be an issue of credibility if we have to describe in detail some personal ritual that involves the experimenter schmoosing around with smoke around or doing a seven veil dance! But it would be appropriate to say the experimenter made sure to remain focused and feel that he or she was connected to whatever was the purpose or intention. I think that is what is essential rather than the specific detail. But at present this is the kind of thing that a new researcher won't know if they don't come to these conferences and hear from more experienced researchers.

DELANOY: I could be going over old ground here but in response to Walter's comment I think we shouldn't lose sight of the need for both top-down and bottom-up approaches. Yes, theory is important, but it has long been argued that the results are coming from the bottom up. If you want to see the strongest indications of psi you see that coming from ganzfeld, from DMILS, from presentiment, and other 'classical' experimental designs. We are trying to get a better understanding by considering moderator variables, and that's one of the ways forward. But I think that both approaches are needed and both need to have more

respect for one another; theory needs to take account of data and vice versa.

ROE: I want to emphasize that I'm extremely happy that physicists are interested in the same problems that I am, and I'm very interested to find out what they can tell me, but I'm quite happy at the same time to be asking different questions and having a different research focus.

IANUZZO: I agree with the sentiment of your talk, but I think something is lacking from parapsychology that you did not mention. We have heard about psychological aspects of psi and also physical aspects of psi, but we have not heard about anthropological aspects. Yet psi can be understood as a cultural system, a belief system, a thinking system, and it seems to me that an anthropological approach could be very important for parapsychologists in the future. Such field investigation looking into traditional beliefs and practices is taking place in my own discipline of psychiatry, but do you think that these studies may be important for the future of parapsychology? My second comment is more practical. I would like to stress the importance of collaboration to ensure the future of parapsychology because we have great difficulties of communication—not only because of language barriers but because we are too insular in our approaches and this can have negative consequences. There is a saying in Italian that the left hand is doing now what the right hand has already done.

ROE: I think that is an important question that I hope we will spend time on in the discussion period to come. People have started to recognize the need for the community to integrate and consolidate our efforts, and it resonates with things that Carlos and Hoyt have said at this conference about learning from different approaches and epistemologies, which are many layered and capable of offering different insights that may be complementary. However, we should not expect parapsychologists with training in psychology to become anthropologists overnight; all we would get is very inexpert work. What we need instead is a strategy for recruiting young anthropologists to come into the field already armed with the requisite skills.

VARVOGLIS: I want to revisit the models we use to explain psi phenomena and mention again Marilyn Schlitz. Her series of three experiments with Richard Wiseman to test the experimenter effect personifies our dilemma between a theory-laden model versus an empirical bottom-up approach. They started out with a good concept to

just test out the experimenter effect as if there's no experimenter doing that questioning. They did the first experiment, then a second, and as if that wasn't enough because there was still that itchy variable there that they had to figure out they did a third one, which was the biggest and by then they'd nailed it down to death because there's no effect left. Now the question is this, we have started out with a very good psi-conducive experimenter and another who is guaranteed to block any effects so it should be an ideal classical experiment, and it is to begin with. But when it finally declined to null practically, did that decline occur because of normal moderator variables, such as fatigue or boredom which is plausible, *or* did they in a more weak quantum theory interpretation keep squeezing down the system to something like a signal so that it could no longer continue to yield data by repetition, which would not be possible with the model of pragmatic information. So the dilemma that we have is a real dilemma not just a theoretical one, because according to which of the two models you use to explain those three data points you are going to design future experiments in a different way.

ROE: Can you design a study that tests between them?

VARVOGLIS: You can't get away with it that easily, it's not that simple.

ROE: But I could design a classical experiment that I would expect would revive the effect, and I would need to be sure that the changes don't also lead your theory to make the same predictions. If the approaches are inseparable in practice I would still stick with my explanation in terms of conventional factors. In any case, I have a problem with that reading of the Schlitz-Wiseman outcomes because it relies on an over-interpretation of just three effect sizes that just happen to give the appearance of a decline. How meaningful are the differences in those figures? If I concede that they do differ I can still criticize the assumption that in all other respects these three experiments are identical when we know that it is impossible to cross the same stream twice; so many things will have changed subtly from occasion to occasion that can have conventional effects, not least that the experimenters come to those experiments with the memories of having been through that experiment before, with the effects that that has upon expectancy, motivation, the manner in which one interacts with participants or with equipment, and so on. Many apparent declines in

parapsychology could be explained at least in part in these relatively simple terms.

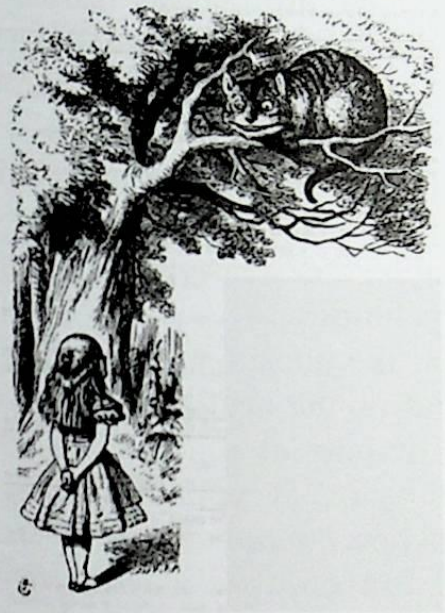
VARVOGLIS: I agree with you, Chris, but what you say is not atheoretical. You are slipping in theoretical components—these are good, common sense components but at some stage you have to ask is there something else going on beyond this.

ROE: I wouldn't like to give the impression that the factors I mentioned offer the only explanation for observed declines. There are likely to be a myriad of causes and this could easily extend to those suggested by weak quantum theory or the model of pragmatic information. But I am uncomfortable with our automatically assuming that these latter are necessarily the best ways of explaining these effects.

VON LUCADOU: Chris, you would be right if we had no other alternative experimental designs, but I have been proposing for some years now an alternative design that takes into account my model and it would be no more expensive to try it. If you could do both it would be best.

ESCAPE FROM WONDERLAND[1]

GERD H. HÖVELMANN



ALICE: "Would you tell me, please, which way I ought to go from here?"

CHESHIRE CAT: "That depends a good deal on where you want to get to."

(L. Carroll, 1865, *Alice's Adventures in Wonderland*)

Introduction

Alice is seeking re-orientation as she encounters the Cheshire Cat in Wonderland. In more than one respect, the field of parapsychology as a

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[1] The following ruminations were basically written on the day before the Parapsychology Foundation Conference in Utrecht commenced. They have profited from the fact that, as a member of the Advisory Committee, I had the privilege of seeing the full manuscripts of all presentations (except one) a couple of weeks before the conference. However, unlike the recapturing (and captivating) personal reflections Chris Roe presented in his 'moderator's summation' in the final conference session, mine were not intended to distill or reflect upon the essence of the individual contributions. Rather, they were meant to remind participants of the general conference rationale and to provide some suggestions, starting points and, maybe, ammunition for the concluding general discussion. The chapter to follow here is an edited, semi-formalized version of that presentation. While I have filled in some gaps, expanded a few sections and turned it into a coherent, legible text, I have tried to keep it suitably brief and to preserve its original character as an incitement for the final discussion period.

whole and the distinguished participants in this meeting arguably find themselves in a situation that is not dissimilar to Alice's.[2] The general theme of the present conference, "Charting the Future of Parapsychology", is to ask where parapsychology ought to go from where it currently is. This requires that we ascertain — and reach some mutual agreement about — where in fact we are. And it also requires that we come to a general decision on where we actually want to get to. Our task therefore has been both diagnostic and prospective. We have been trying to identify, with at least some degree of confidence, the place that we have reached after all those years of wanderings; we have been trying to define possible goals for parapsychology's future; and we have been pondering recommendable and reasonably slip-proof ways of reaching those goals for parapsychology.

It is important to realize, though, that the questions about the goals or the aims we want to pursue, or about or the destination we would like to get to, themselves are *not* scientific ones at all (Kemeny, 1959). Rather, they are philosophical questions, or, if you like, ones that reflect our intellectual preferences. Science *cannot* tell us where we ought to go. This is for us to decide. But once we have made that decision, once we are sure about our goals, science is the best way to go – *methódos* being the Greek word for "way" (towards an end). In fact, it is the only way to go as long as we have not given up on the science game.

Three Traditional Attitudes

Traditionally, there has been a great variety of attitudes, both inside and outside the field, on the question as to what aims parapsychology ought to pursue. I would like to mention and illustrate three different perspectives that most of us will have frequently encountered inside the field of parapsychology. I will briefly discuss them in turn.

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[2] Equating the field of parapsychology with Lewis Carroll's (1865) 'Wonderland' of course is not my invention. It has a veritable tradition (e.g., see French, 1975). It may be of some relevance to recognize that the creator of Alice, Lewis Carroll [i.e., Oxford mathematician Charles Lutwidge Dodgson], was among the very early members of the Society for Psychical Research (Shaberman, 1972). He was convinced of the reality of thought-transference and psychokinesis, but remained skeptical towards survival issues (Cohen, 1979, vol. I, pp. 471-472).

Miracles for the Sake of Miracles

Some researchers apparently have satisfied themselves that paranormal phenomena ultimately cannot (or should not) be subjected to meaningful experimentation and theory building in traditional scientific terms. They are content to enjoy miracles for their own sake. A few years ago, Carlos Alvarado provided an apt characterization of that orientation, when he wrote:

> Our field needs to learn something about the phenomena, not to continue sharing marvelous stories for their own sake. My impression over the years is that a segment of those concerned with the study of spontaneous phenomena are not interested in explaining or understanding the phenomena. They seem to be happy to maintain the mystery for its own sake. In their view, the phenomena are something sacred that should not be probed too much.... My impression is that some of those interested in survival of death or in conceptualizing psychic phenomena as manifestations that point toward nonphysical or spiritual aspects of human beings are generally not interested in showing how cases relate to aspects of the natural world. For some interested in these views, studying whether mediums have dissociative experiences in their daily lives, studying the psychological profile of out-of-body experiencers, or studying the relationship of personality variables to ESP experiences is irrelevant. These researchers feel it is more important to establish survival, spirituality, or the like because of the implications of these concepts for the nature of human beings. Perhaps those who see the study of spontaneous phenomena in this way do not want the topic associated with mundane physical, biological and psychological correlates because such correlates undermine the more spiritual views they prefer.
>
> (Alvarado, 2002, pp. 117, 119)

Those who share the attitudes so eloquently described by Alvarado seem deeply stuck in Alice's Wonderland, presumably enjoying themselves. Connoisseurs of Alice will remember that the little girl, after leaving behind the slowly vanishing Cheshire Cat and its epistemological inquisitiveness, encounters the Hatter and the March Hare, one as mad as the other. Seeking 'miracles for the sake of miracles' basically is not a scientific attitude. Those who have adopted

it have entered a different intellectual game. Admittedly, this may be a legitimate turn in terms of personal orientation, and it may be fine for those who can appreciate a 'mad tea party' with the Hatter and the Hare. But it does not, and cannot, form part of science. Parapsychology as a scientific endeavor is all about "showing how cases relate to aspects of the natural world".

Disillusionment and Resignation

Those who, like this author, have been spending major parts of their intellectual lives and scientific careers in the exceedingly difficult and challenging field of parapsychological research and theory building, will almost certainly have experienced a few disappointments and disillusionments along the way. And we all have known various other once-committed scientists who started off with a great deal of enthusiasm and a couple of interesting ideas, but then were not seen or heard of anymore after it had become apparent that the phenomena in life and lab flatly refused to fit their respective favorite ideas.

Science-fiction writer Philip K. Dick of *Blade Runner* and *Ubik* fame, in an interview on an entirely unrelated topic, once provided what seems like a pertinent description of that feeling of disillusionment. He remarked:

> Now how do you explain that? You're building a jigsaw puzzle, and you keep building it, and you have only one piece left, and you have one hole in the puzzle. You take the last piece and you start to put it in the last hole. And it doesn't fit. And since it doesn't fit, the whole puzzle is screwed up. You can never complete it. And while you're standing there looking at it, it just slowly falls into a million parts.
>
> (Philip K. Dick, quoted in Williams, 1986, pp. 98-99)

I guess there are few among us who have not had that kind of experience once in a while. Some of those who have experienced such disillusionment evidently have turned their backs on Wonderland and its peculiar inhabitants, and they have devoted themselves to easier, more comfortable and obedient (and maybe more rewarding) tasks. Some of them may even have joined the ranks of those who prefer the 'miracles for the sake of miracles' approach.

Others apparently have become card-carrying skeptics instead. Thus, Susan Blackmore regrettably left the field of parapsychology

after much swaying to and fro (Blackmore, 1985) when the desired effects failed to turn up in her own experiments and those that were found in the experiments of her colleagues did not quite live up to what she expected them to be.

London mathematician John Taylor, on the other hand, mercifully dropped out when he had satisfied himself that his electromagnetic pet ideas,[3] which his parapsychological peers had never taken very seriously anyway, could not be corroborated. It is highly instructive to compare Taylor's early trust in fairly transparent wonderworkers and his related EM enthusiasm (Taylor, 1975a, 1975b) with his final retreat only a few years later (Balanovski & Taylor, 1978; Taylor & Balanovski, 1979), which culminated in the conclusion:

> ...neither EM nor any other scientific theory can explain any of the above mentioned ESP phenomena. In particular there is no reason to support the common claim that there still may be some scientific explanation which has as yet been undiscovered. The successful reductionist approach of science rules out such a possibility [....] We can only conclude that the existence of any of the psychic phenomena we have considered is very doubtful.
>
> (Taylor & Balanoski, 1979, p. 633)

There are quite a few similar stories – some of them told, some not – in the not-so-distant history of parapsychology.

Leave Alice Behind

Let me briefly mention a third attitude[4] – that of attempting to normalize the paranormal. Those who have chosen that approach are trying to identify, isolate, describe and explain seemingly wondrous phenomena and to present them to and discuss them with their scientific colleagues in related fields outside of Wonderland. My impression is that the vast majority of those who have addressed us during this conference and of those in the audience decidedly belong to this latter group that insists on having explanations according to the rules of science.

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[3] "...on theoretical grounds the only scientifically feasible explanation [of paranormal phenomena] could be electromagnetism (EM) involving suitably strong EM fields." (Taylor & Balanovski, 1979, p. 631)

[4] The following is essentially adapted from Hövelmann & Krippner (1986) and Krippner & Hövelmann (2005).

However, this means that we ought to follow the phenomena and our experimental results and study the entire range of anomalies in consciousness wherever that may lead us, even if it may force us to adopt an increasing range of non-psi approaches and eventually bring us back into mainstream science. If we really are interested in scientific answers to the questions that have plagued most of us most of the time we will have to follow wherever our well-considered scientific methods will lead us. Otherwise, parapsychology might turn into an ever-shrinking field with ever-shrinking competence and relevance, and it eventually might lose its subject matter altogether once the other scientific disciplines have taken over and provided the non-psi (or maybe even the sort-of-psi) explanations we have been searching for all along.

So let us first get very serious about answering the Cheshire Cat's question of where we want to get to. Let us consider ways that hold out some promise that eventually they will get us there. And let us leave Alice to her fate (Figure 2).

FIGURE 2.
Alice in the sea of tears—from Alice's Adventures in Wonderland, Ch. 2

A Plea for Concerted Efforts

No doubt, previous attempts to present empirical findings of parapsychological research and related theoretical ideas to the scientific community at large have not been received with open arms, much less with open minds. And we cannot reasonably expect that our future approaches will be welcomed with any greater enthusiasm as long as our feet remain more or less firmly planted in Wonderland's soil. I may

remind you of the pessimistic perspective, for which no doubt there are at least some valid reasons, that was sketched out in Ed May's slightly grumbling contribution to this conference. But at the same time I may also remind you of the enthusiastic 'optimism' expressed by Roger Nelson. And, again, please remember York Dobyns' and Richard Shoup's well-argued cases that our findings may not be as incompatible with current physical knowledge as some would have us believe. Please also consider what Hoyt Edge and Etzel Cardeña have described, during this meeting, as opportunities for the 'naturalization of psi', as long as we manage to avoid conceptual vaguenesses and obfuscations.

Again, Carlos Alvarado in his historical talk during this meeting has reminded us of the fruitful attempt to install a couple of committees after the first Utrecht conference more than half a century ago that were to coordinate international research efforts in parapsychology. I venture to suggest that, as one way of charting the future of parapsychology, we adhere to that somewhat successful 1953 model and try and establish other committees or working groups this time that might develop some kind of *master plan* for a new and well-coordinated approach to the scientific community. Below are listed a few areas to which such committees or work groups might direct their attention. And I have added a few thoughts and suggestions that hopefully will lend some plausibility to the areas selected:

- Coordinate empirical research efforts in prominent areas, and set up an empirical register. The development of more testable theories, both conventional and innovative, is certainly needed.
- Encourage publication and re-scheduling of our professional journals. Currently, some of our long-standing periodicals are way behind their schedules. In some specific cases, this may be the fault of the respective journal editors or, rather, of the issuing organizations or societies. In others, probably the parapsychological community itself is to be blamed, because there is an apparent shortage of submitted quality papers, communications and reviews. If we are unable to produce material of sufficient interest to be discussed among ourselves in the parapsychological periodicals, how can we seriously expect the outside world to feel any inclination to take a closer look at our experimental data and the conclusions and theories that may be based on them?
- Coordinate related research and other activities. Thanks to the generous support of Het Johan Borgmanfonds Foundation that, in

May of 2007, sponsored the "1st International Expert Meeting on Clinical Parapsychology", well-coordinated efforts on an international basis in the field of clinical parapsychology and of counseling individuals with exceptional experiences have had a very promising new start. This will soon become more openly visible (Kramer, Bauer & Hövelmann, 2009) and, hopefully, have some long-term effects.

- Design and coordinate appropriate ways of presenting empirical findings and theoretical reflections both to the colleagues in the field and the scientific community at large. This may also include providing educational opportunities for interested colleagues in other fields.
- Rapproachment with establishment science may also profit from the sometimes neglected insight that parapsychological research is of value regardless of whether its outcome is supportive of 'paranormality'. Even in the extremely unlikely case that all experimental work in parapsychology could be accounted for by methodological errors, this would provide useful information that helps to re-design research in this and other areas.
- Also, including purely psychological or other non-psi components into some (or even many) of our psi experiments would lend additional legitimacy to our research. Even if we find no significant psi effects, we would nevertheless be likely to find psychological or other phenomena that merit our interest and that of the scientific community at large. The parapsychological literature would be all the richer for the inclusion of such results, and indeed scientists from other fields would be drawn to these results and delve into the parapsychological literature, whereas they would have had no apparent reason to do so if such results were not presented (for more details, see Thalbourne & Hövelmann, in preparation).
- The need for conceptual clarity has long been recognized in the parapsychological literature (Hövelmann, 1983). But, in fact, little has been done in coordinated ways to prevent conceptual vagueness, semantic confusion, and the misappropriation of terms. Various concepts such as "altered states" (Cardeña, in this volume), "psi-conducive states" (Hövelmann, 1988), and others are not properly defined and their meaning is not remotely as clear as their ubiquitous usage seems to suggest.

- Some colleagues recently (e.g., Moddel, 2008) and not so recently (e.g., Broughton, 1988) have insisted that application issues might eventually decide the future of parapsychology as an organized scientific endeavor. As Garret Moddel (2008, p. 2) observed: "We need psi applications — preferably some that make some money — and then everyone will take notice". In a sense, clinical parapsychology, mentioned before, can be considered a field of applied parapsychological expertise. Clinical parapsychology, however, is concerned more with psychological (and maybe social) improvements than with economic profit. Application-oriented workshops and other projects may prove to be useful in the long run.

No doubt, many of you will have additional suggestions for concerted actions that might help to chart the future of parapsychology, and we all are eager to hear about them.

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FLOOR DISCUSSION: CHARTING THE FUTURE OF PARAPSYCHOLOGY

ROE: We now want to open the floor for comments or questions, particularly from people who have not had an opportunity to speak so far to see what we can learn from the presentations and discussions over the last three days, and particularly what we might agree as a mandate for the future of parapsychology. Gerd, the recommendations that appear on your last slide, are they your recommendations or are they taken from the 1953 conference? Can you expand on them?

HÖVELMANN: Those are my recommendations. I think you made a similar point in your summary; that we should try to coordinate efforts across the community, particularly for the strongest research areas, perhaps having small committees to ensure that we are not all doing the same kind of research but that it is complementary. For example, inter-laboratory collaboration might mean that one laboratory would study personality aspects of a phenomenon while another would study situational aspects, and so on. The second recommendation refers to related activities, for example there was a meeting of 15-20 participants, some of whom are here today, to discuss clinical parapsychology. There will be a Proceedings of that conference and there is a small group that continues to discuss how we might provide basic information to therapists and medical practitioners and other interested personnel about clinical aspects of exceptional human experience. Thirdly I mentioned the need to publish. Even if we don't have the results we predicted we should put them in the public domain. But at the moment some of the parapsychological journals are almost nonexistent. Often it is not the fault of the editors or publishers, but that the journals are starving for good material, and we need to support them more. We also need to find a way to present our results to the scientific community in a more coordinated way and in the most prestigious places.

NELSON: There is also a huge interest in what we do among the general public, not just the scientific community. A tremendous number of people who read newspapers and magazines would like to hear about what we know about consciousness, not just our results but also what we think are the most appropriate questions to ask and what are the best ways to approach those questions. If this engagement with the media is

done correctly it could do a great deal to strengthen the field. I would support the idea of having a group of people within parapsychology thinking about how to make sure that the right stories about our work get in the press and to check that they are reported accurately.

ROE: Can you suggest how we might turn that into an action point to come out of this conference? What specifically can we do? For example, should we have a designated person to deal with the media on behalf of the PA, or of having a shared system for generating publicity each time an issue of one of our journals comes out?

NELSON: In particular it would be very useful to have a small working group that could advise. There are a number of us who sometimes are contacted by the media when they want to do a story or TV special focusing on what we do. In my case this might be the *Discovery Channel* or one of the public service channels doing a program on human consciousness and they want to have the parapsychologist's perspective. What we need to do is talk about how to do that well. Now I have some experience of this and some of us are very experienced and I think it is potentially very valuable to share what we have learned from that, because good media coverage can be very valuable to us.

HÖVELMANN: It is also potentially dangerous, and many of us have had very negative experiences with the press that could have been improved by having more control over the final copy.

ROE: An additional point to note is that the SPR did have a publicity officer in Montague Keen, who had close connections with people in the media and used to be proactive in being kept informed about what was in press with the SPR's Journal so that he could feed that to his contacts (who often were more interested in work that was still in press since they could get 'the scoop', unlike with material that was already in the public domain). I wonder if we might benefit from a similar proactive person who could be kept informed about imminent publications and forthcoming conferences.

NELSON: One of those media contacts is a British journalist named Danny Penman who once wrote a piece on my work entitled "this black box knows the future". Now I would never have phrased it like that, so I learned from that that his intentions were very good but in translating it for public consumption something important is lost. But we can control that final product more than we think we can; if the goal is to have an accurate representation then we don't deal with the media until

we ensure we have sufficient control, and now I make it a condition of my agreement that I have some power to check the facts that they report.

VARVOGLIS: There are companies now who will use software to pick up news items and provide you with a complete cuttings list on topics you specify so at least you know what is coming out in the media. I am trying to get this for the Institute Métapsychique so we have an ongoing awareness of what's out there and we can choose what to respond to. I assume that a similar service would be available in other countries. The other point I wanted to make is that we're thinking—and I'd like to propose this as a collaborative project—of devising an appropriate educational package for high school or early university students. This is not the same public as the scientific community but it might be important to get them while they're young and still open to possibilities! So it would be good to collaborate on what we should put in that package. I know others have done something in this direction, for example here at Utrecht and the *PsiPioneer* package, but a good modern updated package with experiments and so on to be distributed in schools would be a good tool for educating the public.

LUKE: There are a number of media alert systems available on the internet, such as *Google Alert* and *Zetoc Alert*. These services track the internet-based media and provide a summary for free. And in terms of establishing working groups that was mentioned earlier, I'd like see a working group that focuses on altered states and I would be happy to set one up as a Wiki or blog where people could get involved in an online discussion if anyone would be interested in that.

MULACZ: Coming back to what Roger said, of course I'm sure many of us will now demand to see the transcript of an interview before it goes into print. But even if you okayed this you might be surprised to see it lumped in with a page on ghosts, or for reasons of impartiality alongside a statement from a psi-denier that changes the perception of your contribution. There is little we can do against that unless we try to make some proactive statements within our interview to address issues not specifically asked about but part of that wider context. Secondly, I think we need more professionalism in this area; for the PA in Vienna we had a professional PR person managing things and we did not see any negative coverage of the event in the papers.

ROE: I think sometimes we worry too much about the reasons for and impact of what we see as 'bad publicity'. Parapsychology isn't the only science that has the media misrepresent or distort the stories they're telling but it doesn't deter others from wanting to publicize new stories. Secondly, readers of the press are sophisticated consumers of that medium and they realize that a tabloid account of an experiment is not the same as reading the original journal articles, so they are unlikely to hold it up to the same standards. I wonder if at times we are too afraid of the *hypothetical* negative effects and this is preventing us from publicizing our work.

ALVARADO: I like very much Mario's suggestion to become involved with early education since we usually emphasize university level education, which of course is basic. Surveys have shown that a lot of people that are involved with parapsychology start to have a serious interest in the subject at the ages that Mario mentioned and it is very important for us to direct accurate and representative information towards them. On another matter I would like to remind you that we could use the resources of the Parapsychology Foundation to work on some of these issues, for example hosting blogs on our website, and a lot of material could be posted there. We are open to suggestions for how we might carry forward the ideas that come from this conference.

VAN DE CASTLE: With regard to publicity I agree that it can be very difficult. I always find it frustrating when someone in the press wants to do a piece but insists that you provide them with names of critics; they don't just want the story but want to see the dogfight. I totally agree with adopting a proactive approach and want to give a plug for dreams since it is the free altered state that every one of us has every night and has a substantial database going for it but hasn't featured as much as I would have expected. We tried to be proactive in that we got a series of 15 one-hour shows on dreams sponsored through the *Voice of America*. These deal with different aspects of dreams; dreams and healing, dreams of the future and so on. When we did these radio shows it didn't cost us anything; we provided a service to them and they got some great material but we are now able to download them and we now have them available from our website and we are getting 500 people a day listening to them, so there's obviously the interest in parapsychology and in dreams out there. Unless you take a proactive stance and get out there to tell them what you want them to hear through your expert's eye, then you can't complain about what people are getting.

CARDEÑA: I completely agree with all of the statements about providing information for young people who are new to the field. I have both a recommendation and a question. The recommendation is that as far as I know the Rhine Research Center's Summer Study Program is now defunct. I was very fortunate to have been a student there many years ago and to have been corrupted by it! So if the Program has ceased then what can we recommend to prospective students who want to find out more about parapsychology. I would like to make everyone aware that Caroline Watt has an on-line course on parapsychology and I would suggest that people redirect interested young people to her. There could be other courses that I am not aware of, for example in Spanish or Portuguese or other languages that help spread the word, and I would be interested in finding out about them and to see if the Summer Study Program is resurrectable.

ROE: I should mention that also in the UK there is an MSc in Parapsychology at Coventry University and the MSc in Transpersonal Psychology at the University of Northampton has a strong component in parapsychology. As far as I know these are not available on-line, but if people are looking for more intensive bursts of material and nationally recognized qualifications that might help prepare them for a PhD route, then these might be of interest.

BELZ: Christine Simmonds-Moore reminded me of something Bob Morris said in Vienna shortly before his death when he predicted that the future of parapsychology will be clinical. I am not in a position to be so bold, but I would like to describe one experience we have in Freiburg of how parapsychology can make a valuable contribution to society. We offer regular courses and workshops for licensed therapists who are very interested in learning how to deal with people who have exceptional experiences.

FEATHER: I would like to respond to Etzel and in fact before he spoke I was thinking that it is good to know that there are on-line courses and helpful to mention them to one another here, but we need some publicly available listing that directs the public to where they are; we can add that to the Rhine Research Center's website and our on-line newsletter. When we get enquiries from around the world asking about courses we are often in the position of not being aware of anything we could recommend to them. I believe that Dick Bierman's on-line course is no longer running so Caroline's may be the only one available. To answer

Etzel, we would like to restart the Summer Study Program—it won't be next year but hopefully the year after. The Rhine Center does continue to try and keep engaged with the public and so runs at least two programs a month, sometimes three. These vary from having a panel of healers discuss their methods of healing, which is of great interest to the public, through to having many of the experts in this room come and present more formally as part of a public program. We hope to make those lectures available on line which would be one step towards educating the public. Our website has much more information content than in the past—and I must thank Carlos for material he's allowed us to reproduce—and now have an extensive on-line bibliography provided to us by Charley Tart. We'd like to keep the site up to date, so do please send us any material you think would be useful.

DOBYNS: This conversation about outreach and education has been focused mostly on trying to reach new students and the general public, but while listening I have been reminded that parapsychology is important in all of the sciences. It's somewhat of a pity that it is currently classified as a sub-discipline within psychology alone. My talk included some ways in which it would be relevant to physics and I believe that it would apply to all the hard sciences. In anthropology I can cite an anecdotal report that is much easier to understand if you assume that the Shaman is a telepath, and can cite a book about the rise of the witchcraft tradition and ceremonial magic in England which included a statement from the author that these are rational people whose opinions about their practices would be perfectly reasonable if only it were possible for the human mind to affect physical objects. I suspect that most of us may have some level of personal connection with scientists in other fields who do not personally think we are crazy, and if these people have established careers with a history of successfully publishing their work, then they may offer the means of breaking the mainstream publication logjam that many of us are familiar with. We could talk with these people to see if they would be willing to consider the relevance of some parapsychological effect or other, to include it at some level in their publication. It might be possible to sneak in small elements as peripheral components of substantive solid research in a way that would start getting mainstream science publications used to the basic concept.

HÖVELMANN: This also works the other way around. I'm currently preparing a paper with Michael Thalbourne that will argue that it makes

good sense to mix parapsychological experiments with a normal psychological slant so that even where there is no psi we still have a meaningful paper to present to the world rather than just having to tell the readers that it just didn't turn out.

ZINGRONE: I was going to speak on the issue of education but now that you're talking about always trying to have a 'normal' aspect to the research you do, I should mention that this is something that Carlos and I try to do all the time and we think it's really important to design projects from which you can split off bits to publish in mainstream journals. The more you are able to do that the more you can raise your credibility as a fellow researcher and it helps the field as well because it puts the names of those of us who publish mainly in the parapsychology literature out there in the mainstream literature as well. On the subject of education, the Parapsychology Foundation maintains five websites, including Rhea White's Exceptional Human Experiences website that contains her dictionary and so on. On the main website we have selections from the glossary of terms, over 40 bibliographies that Carlos has prepared, links to all the on-line libraries we can think of. On the Lyceum side that is dedicated specifically to education in the field we not only have the records for this conference, but the blog series, information on courses, and are almost finished with posting the content of the biographical dictionary of parapsychology that was originally published in 1964. Once that's finished we hope to expand it to become an up-to-date 'Who's Who'. Thirdly I wanted to mention that Loyd Auerbach also has an on-line course that's been going on for a number of years and you can get accreditation through a hypnotherapy training institution in California when you take that course. I would also like to encourage people who are in more or less precarious financial situations to think about mounting more on-line courses around the world using support software like *Blackboard* where you just put up the content and it deals with the structure. This is something like Ciáran O'Keeffe is doing with his Parapsychology School, which is an offshoot of theparapsychologist.com. And the more that the websites of all the different organizations such as the Rhine Research Center and the SPR have this material the better will be the experience of students and young researchers when they come in. It will cut down on the number of approaches that ask 'where do I go?', 'what should I do?', 'how do I get trained up?'

SIMMONDS-MOORE: I'd like to follow up on both of Nancy's points. Firstly on education, we do already have a large number of very well designed courses within universities covering parapsychology and anomalous experience, particularly in the UK. At Liverpool Hope, for example, our year-long course looks at perspectives in anomalous experience. Interestingly there is already a move in those institutions towards virtual learning environments that provide an ideal vehicle for shifting provision to a wider more accessible environment beyond the university. So perhaps these universities could work in conjunction with the PA and the PF to make these course more readily and internationally available. Liverpool Hope has the same pressures as Northampton in terms of departments needing to generate income; parapsychology can earn the institution money, so it is very possible that universities will want to get into this. But we also need to think about bringing parapsychology into other subjects, through research methods, for example, or when teaching critical thinking for example. With respect to Nancy talking about normalizing psi, it is important that several researchers in parapsychology, such as Daryl Bem have taken established experiments from psychology and given it a twist that introduces a psi component in such a way that you can 'sell' the studies to mainstream journals and get published, and that seems the way forward.

NELSON: I agree with everything that has been said so far, but I have a request, which is could someone compile the resources that have been discussed and send it to me. It may be summarized some place but I don't have it. I have compiled some useful information but don't have information to pass on if someone asks where they can go to get on a Master's course, much less a degree. And people are asking for that kind of thing a lot.

ROE: The obvious place for that repository would be with our professional body. Rather than trying to network amongst ourselves, perhaps an action point to arise out of this could be to ask the PA to set up a working group to look into educational opportunities in parapsychology and to compile the educational resources list we are all looking for. I'm sure there would be no problem with other organizations linking to that content or even including it on their own web pages.

CARDEÑA: I think it is an excellent idea, so as the current PA President could I ask you to send me details of any courses that you feel the PA should be publicizing and we can make sure that the information is made available.

GERDING: I want to change the subject now and raise the question of the experimenter effect. I'd like first to give you and example and then raise a question. On the first day of this conference we were discussing experimenter effects and I mentioned the work we did in Utrecht in which we used a self-invented magical procedure as a preparation for the experiment. That work used the ganzfeld method. In the first half of the 90s we as a small research group entered the field of experimental parapsychology and after discussions with Chuck Honorton and Bob Morris we decided to conduct a ganzfeld study exactly as they said it should be done. We conducted two experiments each with 50 sessions, so totaling 100 hundred sessions. These gave 25 hits, which was exactly at chance. At that time Dick Bierman figured out that there was some kind of macro decline effect in the ganzfeld work. We were disappointed that we had entered the field in the dip of this decline and we felt that we had to do something, so we carried on with ganzfeld research but invented a new procedure, doing it in a totally different way. I can't give you all the details here, but for example we used only three participants, who were also the experimenters (so there were no subjects from outside) and we were constantly changing roles, deciding trial by trial who would take which role. We conducted our experiments in the evening when the institute was closed so we were alone in the building. Before starting the session we had at least one hour consisting of those magical procedures to prepare for the session; this preparation included smoking some marijuana (which is legal in the Netherlands), singing and drumming. We also made a 'sacrifice'; on the day of a session I would buy some lottery tickets and that evening, before the lottery was drawn, we would burn the tickets as an offering—nobody knows how expensive that sacrifice might have turned out to be! After this magical procedure we conducted one or two trials and, to make a long story short, ended up with very good results. We believe we achieved these results by creating something new and turning the experiment in to a ritual. Mario mentioned the importance of this, as did Jung in his writing on synchronicity. We found that ritualizing the experiment made us more open to the phenomena occurring and in this respect we were inspired by the work of Kenneth

Batcheldor. I should also acknowledge my teacher, Professor Tenhaeff, who predicted that in the future of parapsychology the experimenter and the subject will be one and the same person. So now I come to the point: what were our true intentions with these experiments? Simply, we were fascinated by psi phenomena and wanted to experience them for ourselves; we wanted to be involved in the mystery of life and we saw that as our contribution to parapsychology. I want to quote Harald from his talk last night when he said that spirituality is the coordination of an individual with some larger whole. I would add that from the traditions of wisdom we learn that spiritual practices bring you closer to paranormal phenomena. So from these experiences my question is: do we have a better understanding of the experimenter effect in Harald's claim that spirituality is at the core of parapsychological phenomena? Should we think of experimenters as mystics?

VON LUCADOU: I completely agree that ritual and especially novelty are essential to make good experiments. I had the opportunity to take self-selected subjects who had come all the way from Northern Germany to our laboratory in Freiburg and so were highly motivated, and could compare them with other participants who took part simply because the experiment was part of an exhibition they were attending so they could just take part without much effort and found highly significant differences in how successful they were. I also found differences in the way that they approached the experiment, with those recruited from the exhibition being much more casual, while the former people really devoted the whole afternoon to the experiment; some danced, some prayed, whatever they wished. In doing this they created what I call an organizational closure with the apparatus—it was a classical PK experiment. So I don't think that we should only use ourselves as participants, since this creates methodological problems and would seem suspicious to skeptics, but agree that we should allow the subjects to develop their own style. In many published experiments the participants simply don't have the time to invent their own rituals or the flexibility to do what they want so they don't have that connection. We should be more trusting of our subjects, to allow them to generate the novelty you spoke of; I always thought of it as a collaboration between me and the subject, and that we each contribute to our discovering something about nature. In our model we call this creating organizational closure within the system and that is the most important thing.

DELANOY: To follow up on Walter's comments on allowing participants to do their own thing, I was wondering, Hans, whether it was truly the ritual aspect that was important in your experiments or, thinking about what Jeanne Achterberg said last night, could it be context validity? Are we not instead creating a context that has importance and makes sense to the participants in that setting? This would extend to cultural issues of the sort that Hoyt and others have spoken about, so that if we establish an environment that is valid for the people concerned, be it a ritualistic setting as it worked for Hans, or allowing participants to do their own thing as Walter describes, or taking account of what is meaningful within a cultural context, then that might be something we should start attending to more carefully.

MULACZ: One more example of the importance of ritual comes from the work of Raymond Moody with the psychomanteum, which many of you will know involves use of a curtained and dimly lit room containing a reclining chair positioned in front of a mirror that is set at an angle so that they cannot see their own reflection. Participants were informed that they might have encounters with deceased friends or relatives after spending some time looking into the psychomanteum. Now for the ritual aspect: he asked them to bring some belongings of the deceased persons they hoped to encounter and he spent time with them before sessions, looking through their photo albums and discussing the people in the pictures. Sessions happened when the person was ready—there were no clocks on the walls so that people were in a kind of timeless environment. In Basle they attempted a replication in which they religiously adhered to the physical set-up with the same curtains, chairs, mirrors, but the participants were scheduled as if they were attending the dentist's; one at 3 o'clock, one at 5 o'clock, and so on. And what happened? Zero result.

DOBYNS: Any alumnus of PEAR has to approve wholeheartedly of the concept that you can't do an experiment properly unless you've generated data within it yourself. With all due respect to Walter's concerns about skeptical criticism, I would say that if you've properly designed your experimental controls against deception by your operators then it shouldn't be possible for you to cheat at it even if you're doing it yourself. In fact, with all the evidence we have seen for experimenter effects, the case is really much stronger than that. As long as we insist on trying to hold ourselves apart from the generation of data and regard ourselves as outside the process, the field is going to be

plagued with non-replications and bizarre erratic results—we may still have those once we accept our role within experimentation and actively make ourselves part of the data generating process, but we will never be able to overcome those obstacles without doing so.

NELSON: I have to say that the ritual aspect is very important and we did work on that at the PEAR lab. But I want to go back to something that both Hans and Gerd mentioned, which is the goal—where do we want to go—and one of the places that I think we probably all share as a goal is what is important to people. What is important about being human that we're studying, the edges of what's known about being human is where we're working and I think we should acknowledge that.

ROLL: I want to go back to Hans's suggestion of using experimenters as participants. Batcheldor apparently got spectacular macro PK effects when they were the subjects themselves so this is really promising and Hans' approach is ingenious; the question is how we can do it in America, where some aspects are illegal!

VAN DE CASTLE: I've always been impressed by the work that Bill has done on psychometry, where you can take an object and it seems to have memories of the owner built into it. Mrs Garrett was a very successful person with such objects, and seemed able to tell you the history of the owner, what they looked like and so forth. This sounds a bit like superstition but the concept of there being some sort of energy that can be transferred to objects is very powerful. We had the idea of trying to use it in a group setting with our dream helper work; we would all come together as one and spend a lot of time with the target person, doing whatever was suitable for them, sitting in silence, chanting, holding hands, whatever would unify us according to their beliefs. We then asked them if they would loan us some object that had her energy—if I were going to do one tomorrow I might have someone wear the conference badge that I've been wearing for the last three days because I feel it now has my energy in it. When we would do that we asked the person to focus on some problem, not a trivial question, for us to pick up impressions about. One woman came up with the question of what her occupation should be. Through psychometry we had a lot of negative mother energy around her and imagery of baby ducks being drowned in water. When we finally pushed her on it and said there's more to this than just changing jobs she then acknowledged that her

mother had been a psychiatric patient and had at one time tried to drown her in the bathtub. So you can get a lot of highly specific information if you put that ritual aspect into it. If you're comfortable being loose in an effort to get results then you can always go back and re-center after the session is done.

NARANJO: Firstly, I believe that one main aspect that must be present is love and passion, because if we don't love the subject and are not passionate about it to the point where our captivation and enthusiasm is palpable and is felt by others, then it will be difficult to bring new people into the discipline, particularly young talented students. My second point is that my Grandfather used to say to me when I was still in Cuba that sometimes we have to take two steps back in order to go forward, and I think it might be the moment for the discipline to come back to its roots and really carefully reconsider what were the assumptions that were made when bringing psi into the laboratory. I think that some things should be revised, and I shall give one example, which is the assumption that we must impose fixed solid barriers to communication at all times in parapsychology experiments. Of course I appreciate the need to prevent classical signals, but it may be important to allow participants to continually exchange classical information most of the time and put in place the barriers only at the point of testing for psi. This will emphasize the sense of communication rather than separateness between the people and overcome their sense that they are being asked to do something that is impossible. And the other thing I would like to mention is that the discipline needs to dramatically and fundamentally revise its image in front of mainstream science. I work in a university setting and I talk to many people about parapsychology and they are very interested, but they are not happy with the way that parapsychology is characterized as an island in the middle of the ocean with its own unique problems and no connection to any other sciences. We should portray ourselves as scientists from traditional fields, psychology, biology, chemistry, whatever, who have specialized in these particular problems.

ROE: And that's a nice way to bring us back to the goal of this session, which is to see what we can do in the future to bring together these observations and reflections and have some sense of direction for the future. One thing that seems to come out of this for me is a sense that there are tacit understandings of the necessary conditions for psi to occur, and so long as they remain implied rather than fully reflected or

honored in our publications then other people are clearly not going to have an awareness of them. For example, a number of people here have described how they invest great importance in ritual but there seems to be a clear rationale for why that should be so for them; we need to communicate more effectively not only what we do but why we do it. Perhaps the PA or PF can play a role here in providing a forum that allows us to share good practice, describing the things we do, what has worked well and what hasn't, in a detailed and personalized way that doesn't easily sit with the conventions for journal articles or even conference papers.

ZINGRONE: I was going to ask to shift gear slightly. In putting together the list of graduate students that we could invite to this conference as a representative cross-section of that group what we found was that we were mainly inviting psychology graduates and a few history graduates. Since we have a room of established researchers that is bifurcated basically between psychologists and physicists, we should have those same two groups among the graduate students but we don't. So my question is where are the physics postgraduates? What are the obstacles that have kept us from having postgraduates in physics? Is there some reason why there is a dearth of physics graduates and can we come up with a strategy to overcome it?

CARR: Thanks for raising that point. It's really crucial because a lot of people in this room feel very strongly that this subject will not gain mainstream respectability until it is accepted by physics; there has to be a theory that has some link with physics and there has been a lot of discussion of this at this conference. There is a remarkable contrast, at least in the UK, between the situation in psychology where we have in the region of 80 PhDs and 16 permanent positions in psychology departments, and the situation in physics, where there are no PhDs in physics and no professional paraphysicists in the UK, perhaps even in the world. There are quite a lot of physicists who are interested in the subject, but they are not in the academic community and are not paid to do this work. Part of the reason for this is that, unfortunately, we have not had someone like Bob Morris, who was wonderfully successful in normalizing the subject and thereby seeding people throughout UK psychology departments. That has not been possible in the physics department, where there is a deep seated prejudice against parapsychology. This may be surprising since survey data suggests that physicists as a community seem more open to psi phenomena than

psychologists, but there is a tremendous prejudice from a few key physicists and these unfortunately can have a big influence. There is no money for the subject, with huge competition even for mainstream topics, and also few departments who would be willing to supervise a project in this area. Even if this were overcome, the chances of a paraphysicist getting an academic position at the moment are very, very low. Until you ensure parity I don't think the subject will secure mainstream respectability. We need a Bob Morris in physics.

LOBACH: I'm very happy that there are more possibilities to connect on-line with other researchers, but that is no substitute for face-to-face meetings like this. I'd like to see multidisciplinary conferences organized around particular themes of interest to parapsychology, such as the relationship between states of consciousness and psi, or the role of ritual. These conferences could bring together people from different fields to discuss how we might generate testable hypotheses and develop experimental designs to conduct that would reflect current understanding in all those areas.

COUSINS: I just want to agree with what some of the previous speakers have said on the importance of interdisciplinary research, but possibly we shouldn't just concentrate on just physics or psychology; there are many more disciplines that would have relevance for what parapsychologists do: anthropologists, for example, healthcare professionals such as nurses. I am surprised at how often I read about spiritual aspects of nursing in nursing journals, some of which seem to incorporate psi aspects. So look beyond the physicists and psychologists, there's more out there, and I speak as a sociologist.

ROE: Thank you, this seems an appropriate point to bring this session to a close. Do reflect on the issues that have been raised here today and indeed over all three days of this conference, and pass on any suggestions you might have to Etzel Cardeña, the current President of the PA, so that we can make sure that the discussion we have had and the ideas that have emerged are followed by action. God willing we will secure a grant to help pump prime this process and can include members of the community who were unable to attend this conference. Hopefully we will be able to change the course that parapsychology is following so that we can find ourselves in more hospitable waters.

CLOSING REMARKS

CHRIS ROE: Before we close the conference I should like to thank a few people. I'm sure you will all agree that things have gone extremely efficiently; they certainly have from my perspective. Firstly I should like to express my thanks to our two fantastic helpers, Emel Sebüktekin and Annemarie Visser, who have both worked so hard to ensure our smooth progress through the range of activities and venues that make up this conference. Despite having no background in parapsychology they have been so interested in and enthusiastic about everything that has been going on, and have made the whole experience a very pleasurable one. Susan MacWilliam as well has our gratitude for her efforts to film and record all our deliberations that will be included hopefully in a conference proceeding volume. Also thanks to our local host, Wim Kramer, for his warmth and hospitality, for introducing us to this beautiful city, and for discreetly keeping us organized and on time. Of course I want to thank the Het Johan Borgmanfonds Foundation for its financial support making this conference possible in the first place and last but not least I want to thank the Parapsychology Foundation, Mrs Eileen Coly and most especially Lisette Coly, for conceiving a vision of what this conference could be and protecting and nurturing that vision through the planning process, from mapping its content to populating it with a body of people who represent a broad cross section of our discipline, from senior figures to those just starting out in their careers, from various nations and cultures, and from different academic disciplines and perspectives concerning the phenomena of parapsychology. Without her vision and hard work, her sense of the field, its needs, and its future, there would not have been a Utrecht II. But more than this, I'd like to thank Mrs Coly and Lisette Coly for being fast friends of parapsychology for over half a century. Many of the people in the field—myself included—owe you a considerable personal debt for all the support and encouragement you have given us, and without which some of us literally would not be in parapsychology today.

LISETTE COLY: Thank you Chris for your generous comments which touch us greatly. It's very hard to believe that all our deliberations are over. The last three days have been gruelling but challenging and stimulating. All of us within the Parapsychology Foundation and the

Het Johan Borgmanfonds Foundation who have worked to coordinate this conference must thank, of course, our staff, but above all you, the attendees of Utrecht II, who have given of yourselves and of your expertise so generously as we have all sought to chart the future of parapsychology. Our moderator, Dr Chris Roe, has been of inestimable value to keep us all to the task at hand and on time—which keeps Wim and I very happy—with the able assistance of Utrecht II's advisory committee. As we close this second Utrecht International Conference it is with the very strong hope that our deliberations here will be taken up once again by future generations. In the spirit of Utrecht I, it would be good at this time to recall the timeless words of Gardner Murphy from 1953

> We need fuller understanding from the scientific public, fuller support for long and strenuous investigation, a much more effective way of finding the young men and women who will be the parapsychologists of the decades ahead. Personally, I doubt whether parapsychology will bring us this larger understanding of human nature by our own efforts alone. Rather, it seems to me, a scientific spirit working through the mediums of history and the social sciences, through the insights of the physical sciences, biological sciences, medicine, psychiatry, psychology, and catching the spirit of the Rembrandts, the Beethovens, the da Vincis, who have described man not in sober prose alone but in color, in tone, and in eternal principles of form can lead us to an integrated conception of the nature of man more satisfying than that which we now possess. But in this integrative effort, of all the modes of understanding man parapsychology will, if our work be well done, play a vital role.

On behalf of my mother, Eileen Coly,—and, I presume, my grandmother, Eileen Garrett—I can say that it has been a privilege through the years, an honor to work with you and see you doing such good work. I just wish that my own organization still had the wherewithal to push you over the top. But we have confidence in your tenacity; you are all too obstinate to give up, and that gives us hope for the future. We have done our best to play a vital role, but it is to future parapsychologists that we will pass Utrecht II's baton, and we should take heart and take with us as we leave these hallowed halls of Utrecht the words of Frances P. Bolton, PF's co-founder and benefactress, who addressed the first Utrecht conference as follows

> Man's knowledge is limited only by his all too great readiness to stay within the boundaries of the proven. Here, men dedicate themselves anew to the task of the search for knowledge, and for truth in the realms beyond those upon which science as such has set its seal of approval.

And so the proceedings of *Utrecht II: Charting the future of parapsychology* is adjourned.

PARTICIPANTS' NAME INDEX

ACHTERBERG, Jeanne: v, vii, viii, xi, 173, 181, 201, **387**, 391, 407, 409, 411, 412, 413, 416, 418, 419, 534, 579

ALVARADO, Carlos S.: v, vi, vii, viii, x, xii, xiv, xvi, 81, 110, 125, 126, 128, 130, 131, 132, 135, 137, 140, 141, 146, 162, 164, 175, 201, 215, **245**, 246, 248, 249, 272, 273, 274, 276, 277, 286, 287, 315, 317, 422, 424, 459, 533, 550, 561, 565, 567, 572

ASPREM, Egil: v, viii

BANCEL, Peter: v, viii, 5, 13, 17, 19, 20, 61, 67, 71, 238, 469, 470, 488, 514, 520

BAUER, Eberhard: v, vi, viii, xii, xvi, 328, 336, 356, 357, 359, 566, 568

BELZ, Martina: v, vii, viii, xi, **326**, 332, 333, 343, 345, 347, 356, 533, 573

BERENDS, Hans: v, vi, viii, xiii

BIONDI, Massimo: v, viii, 273

BUSH, Martine: v, viii

CARDEÑA, Etzel: v, vi, vii, viii, xi, xii, xvi, 65, 66, 208, 213, 304, **305**, 307. 308. 309. 310, 311, 312, 314, 315, 316, 317, 319, 321, 322, 328, 339, 356, 361, 366, 522, 535, 555, 565, 566, 573, 577, 583

CARR, Bernard J.: v, viii, 447, 460, 530, 534, 582

CLAUDEWITZ, Kaare: v, viii

COLY, Eileen: v, vi, viii, x, xiii, xiv, xv, 277, 584, 585

COLY, Lisette: v, vi, viii, x, xi, xii, xiii, xv, 243, 273, 274, 277, 278, 284, 320, 328, 357, 358, 359, 360, 584, 585

COUSINS, Wendy: v, viii, 305, 583

DELANOY, Deborah L.: v, vii, viii, xi, 66, 111, 221, 237, **289**, 534, 543, 555, 579

DOBYNS, York: v, vii, viii, xi, 20, 21, 69, 220, 238, 241, 242, 369, 383, 384, 413, 488, **490**, 495, 500, 505, 514, 515, 518, 519, 520, 523, 524, 525, 527, 528, 530, 534, 551, 565, 574, 579

EDGE, Hoyt: v, vii, viii, xi, 291, **421**, 427, 439, 443, 445, 453, 454, 455, 457, 460, 461, 522, 524, 525, 526, 528, 529, 531, 533, 565

ERTEL, Suitbert: v, viii, 369, 383

EVRARD, Renaud: v, viii, 274, 275, 279

FEATHER, Sally: v, viii, 5, 19, 171, 204, 217, 573

FERRARI, Danielle: v, viii

GANZ, Nadir.: v, viii

GERDING, Hans: v, vi, viii, xii, xvi, 69, 243, 577

GLICKSOHN, Joseph: v, viii, 67

GOULDING, Anneli: v, viii, 156, 183, 184, 186, 205, 304, 345, 346, 347, 358

HINTERBERGER, Thilo: v, vii, viii, x, 29, 33, 34, 43, **46**, 48, 51, 59, 60, 61, 62, 63, 64, 70, 534, 538

HOUTKOOPER, Joop: v, viii, 125, 144, 243, 342, 358, 476, 487

HÖVELMANN, Gerd H.: v, vi, viii, xi, xii, xvi, 132, 144, 321, 328, 356, 357, 359, 524, 553, **559**, 566, 567, 568, 569, 570, 574

IANUZZO, Giovanni: v, viii, 556

JOSEPHSON, Brian: v, vii, viii, xi, 70, 71, 238, **323**, 324, 416, 420, 531, 550, 554

KLOOSTERMAN, Ingrid: v, viii

KOKUBO, Hideyuki: v, viii, 81, 111, 411, 548

KRAMER, Wim: v, vi, viii, x, xii, xiii, xiv, xvi, 243, 274, 328, 356, 357, 359, 566, 568, 584, 585

KRAMER, Tim: v, viii

LOBACH, Eva: v, vii, viii, x, **22**, 29, 30, 32, 33, 43, 44, 45, 63, 64, 65, 67, 386, 534, 583

LUKE, David: v, viii, 218, 571

MACHADO, Fátima R.: v, vii, viii, x, **115**, 116, 117, 119, 122, 123, 125, 126, 127,

128, 135, 137, 145, 150, 217, 218, 220, 222, 533

MacWilliam, Susan: v, vi, viii, xvi, 584

May, Edwin: vii, x, 16, 20, 29, 30, 35, 43, 44, 45, **224**, 233, 238, 371, 387, 414, 479, 488, 489, 515, 520, 528, 544, 552, 565

Michels, Hans: v, viii

Modderman, Loes: v, ix

Mulacz, Peter: v, ix, 129, 146, 242, 274, 415, 550, 571, 579

Naranjo Muradas, Jose Raul: v, ix, 60, 70, 411, 581

Nelson, Roger: v, vii, ix, x, **1**, 11, 12, 13, 16, 18, 19, 20, 21, 45, 62, 63, 64, 67, 70, 112, 197, 208, 241, 342, 360, 369, 382, 383, 384, 455, 469, 470, 475, 488, 514, 515, 519, 520, 565, 569, 570, 576, 580

Nelson, Reinhilde: v, ix, 19

Rabeyron, Paul-Luis: v, ix

Rabeyron, Thomas: v, ix

Roe, Chris A.: v, vi, vii, ix, x, xi, xiv, xvi, xvii, 44, 45, 60, 63, 65, 66, 81, 157, 212, 215, 216, 239, 240, 242, 297, 306, 311, 313, 318, 320, 323, 413, **533**, 548, 549, 550, 551, 552, 553, 554, 556, 557, 558, 569, 570, 572, 573, 576, 581, 583, 584, 585

Roll, William G.: v, ix, 60, 61, 70, 111, 116, 119, 120, 125, 128, 130, 131, 132, 133, 134, 136, 142, 146, 147, 148, 149, 162, 171, 211, 239, 240, 244, 256, 263, 274, 288, 290, 324, 411, 419, 525, 580

Roxburgh, Elizabeth: v, ix

Sebüktekin, Emel: v, vi, ix, xii, 584, 589

Schriever, Friederike: v, ix, 345, 360, 523

Shoup, Richard: v, vii, ix, xi, 20, 38, 39, 40, 42, 44, 415, 420, **463**, 466, 470, 471, 473, 482, 488, 489, 522, 523, 529, 531, 532, 538, 565

Si Ahmed, Djohar: v, ix

Da Silva, Fabio Eduardo: v, ix

Simões, Mário: v, ix

Simmonds-Moore, Christine: v, vii, ix, x, **151**, 156, 157, 159, 160, 165, 167, 170, 172, 176, 178, 179, 181, 183, 184, 186, 195, 196, 197, 206, 209, 212, 213, 217, 218, 219, 536, 573, 576

Snel, Frans: v, ix

Tresfon, Ad: v, ix

Vaitl, Dieter: v, ix, 307, 310, 314, 320, 357

Van der Sijde, Peter: v, ix

Van Luijtelaar, Maurice: v, ix

Van de Castle, Robert: v, vii, ix, x, **72**, 75, 79, 98, 102, 111, 113, 146, 148, 216, 219, 222, 287, 412, 417, 526, 543, 572, 580

Van Egmond, Klaas: v, ix

Varvoglis, Mario: v, ix, 19, 61, 68, 324, 416, 419, 531, 532, 552, 553, 556, 557, 558, 571

Vassy, Zoltán: v, vii, ix, 25, 29, 43, 44, 45, 238, 240, 242, 528, 529

Verbrugh, Hugo: v, vi, ix, x, xiii

Visser, Annemarie: v, vi, ix, xii, 584

von Lucadou, Walter: v, ix, 321, 324, 348, 359, 368, 370, 372, 374, 383, 414, 418, 420, 447, 461, 514, 520, 523, 554, 558, 578

Wackermann, Jiri: v, ix, 46, 47, 48, 51, 56, 60, 320, 385

West, Donald J.: v, ix, 21, 218, 244, 271, 274, 275, 286, 288, 549

Walach, Harald: v, vii, ix, xi, 29, 43, 58, 59, 60, 65, 69, 302, 348, 359, **363**, 365, 368, 370, 371, 372, 373, 374, 377, 379, 382, 383, 384, 385, 386, 413, 415, 417, 419, 447, 459, 461, 527, 528

Zangari, Wellington: v, ix, 122, 123, 125, 128, 137, 141, 145, 150, 222, 274, 276, 286

Zingrone, Nancy L.: v, vi, ix, xii, xiv, xvi, 125, 141, 162, 201, 215, 274, 277, 286, 575, 582