# PARANORMAL COGNITION: ITS SUMMARY AND IMPLICATIONS

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It has often been assumed by both the general public and parapsychologists that the primary function of telepathic phenomena is communication. Indeed, a great many of the theories proposed to attempt an explanation of telepathic occurrences have forwarded the notion that telepathy is the residue of a primitive precursor to language. Although this approach appears to have gained a wide appeal it has not, on the other hand, engendered any particularly successful experimental predictions nor has it truly "explained" the telepathic phenomena.

In 1974[2], Kreitler and Kreitler published a paper entitled "ESP and Cognition" which shifted the psychological emphasis of telepathic phenomena away from communication and towards cognition. Because my own academic background and interests at that time lay in the area of intellectual development, the paper helped to shift my own assumptions concerning the primary purpose of the telepathic phenomena. As a result, I formulated the beginnings of a theory of paranormal cognition which proposed that the telepathic phenomena can be most clearly understood if they are seen to originate from the same process which allows for the presence of those phenomena associated with normal cognition.

I concluded that the primary function of both processes is to connect hitherto distinct meanings to form a new meaning and, hence, aid the organism's survival. The sole ostensible difference between the two processes is that telepathic connections (i.e., paranormal cognition) appear to be partially non-sensory in origin, whereas normal cognition connections are purely sensory. However, philosophers such as Broad[1] have clearly demonstrated that *all* cognition is, in fact, non-sensory. If this is the case, normal and paranormal cognition can be seen as being aspects of the same process.

In neither type of cognition is there any clear explanation by qualified researchers as to *how* such "meaning connections" take place. Until such a time, one cannot unequivocally state that the two phenomena are one

and the same. But since, once again, in neither case have we any direct proof of the existence of such a process or processes, I thought it useful to propose a combined theory which I hoped might add further clues to the nature of one or both of these processes.

A hypothetical process which could account for these observed phenomena was presented. This process carries the familiar term "mind," but, in this case, "mind" is not seen as an entity, but rather as the interaction between energy (labeled in this case, mental energy) and the brain or central process of an organism. This interaction is the source of consciousness and, hence, of cognition.

Kreitler and Kreitler (with, I suspect, some aid from Jung's theory of synchronicity) argued that the mechanism of this process can most easily be explained by employing the metaphor of "lightning." Just as lightning is the result of the sudden connection of equally charged bodies, so too can cognition (either normal or paranormal) best be seen in terms of a sudden connecting process between two meaning-related contents which—if and when connected—prove to be complementary with regard to their meaning. The "lightning" model is also of importance because it dispels the generally accepted view that in the process of cognition, information is carried from one location to another. The "lightning" model instead suggests that the process of cognition is, rather, a sudden connection between meaning-related contents. It is only as the result of this connection that the observable processes and physiological changes that we have associated with both cognitive systems take place. This new perspective makes much more understandable a large number of phenomena which could not be easily explained by previous cognitive theories.

I further argued that, as systems become more and more complex, meaningful connections become further internalized because the "meaning of a meaning" is dependent on an infinite variety of values that an individual imposes and, as well, on the infinite number and variety of meaning values of the referent itself, both of which would be severely restricted were paranormal cognition alone available to us. The probability that the cognitive process can detect distinct meanings which, if connected, prove to be in all respects congenial enough to produce a new meaning, is much higher within the cognitive realm of one organism than between two or more organisms. The more cognition becomes internalized or self-centered, the more necessary it becomes in terms of the survival value of the self-consciousness (i.e., the individual) to keep it internalized and not allow it to be overtaken by paranormal cognition. Indeed, seen from this viewpoint, our sense of individuality is the evolutionary price we've had to pay for the limiting of our telepathic abilities.

Scientists now concede that one of the brain's major functions is to act as a "selection filter" for incoming information. My suggestion would be that this same process occurs for the selective filtering of outgoing information as well.

I have suggested that if this approach is, in essence, correct, it should allow for paranormal cognition to be most easily observable in man under four main conditions:

1. when information is so trivial that it contains little, if any, self-survival value and hence passes through the brain's filtering process;
2. when the survival value of paranormal cognition becomes greater than that of normal cognition;
3. when internal (i.e., self-value) connections are somehow impaired due to naturally or chemically induced altered states of consciousness;
4. when the organism's maturational level has not yet reached the level of complex internalized connections its species is capable of generating, but is, as yet, largely engaged in learning to make internalized connections (i.e., the organism has a poorly defined sense of self).

Categories 1 and 2 are most clearly observed in spontaneous cases of *paranormal* cognition. While clearly of major interest and importance as indicators of paranormal cognitive phenomena, such cases are highly difficult, if not impossible, to replicate in an experimental situation, although it is arguable that the early Rhine studies and the thousands following which employed Zener cards, dice, etc., might fall into a combination of category 1 and category 3.

The greater part of modern day interest in paranormal cognition is in association with studies in category 3. The large number of GESP experiments in personality studies, dream research, hypnosis, meditation, hallucinogens, specific types of brain-waves and, most recently, the Ganzfeld effect all seem to more or less produce examples of paranormal cognition. My suggestion is that the common experimental factor in each of these techniques is the minimization of the subject's self-conscious directed thought. I have termed this effect "relaxation" and designed and carried out a successful experiment[3] (pp. 424–438) which supported the hypothesis that relaxation is an important variable in the study of paranormal cognition. My main area of interest, however, is in category 4 and specifically, in its application to human development.

A series of experimental predictions were made with regard to this, all of which were then rigorously tested. I have described these at length elsewhere[3] (pp. 180–440), so I will now briefly summarize the main results obtained. First, it was predicted that child subjects who have not yet matured to the stage of forming complex internalized connections

will show a greater tendency to employ paranormal cognition than adult subjects who have already formed complex internalized connections. I argued that within the general Piagetian framework of intellectual development it would be reasonable to argue that within our society and culture the majority of children reach the stage of creating and employing complex internalized connections at around the chronological age of eight. Following this chronological age, the observable amount of paranormal cognition employed should stabilize itself to levels of chance, under most conditions. In three separate paranormal studies involving a large number of subjects between the ages of 3 and 70 it was repeatedly found that only the three youngest chronological age (CA) groups (those aged between three and eight years) consistently scored *as a group* at levels far above those predicted by chance, while all the remaining CA groups scored at levels within those predicted by chance.

Second, still in keeping with cognitive theory, it was predicted that the tendency to employ paranormal cognition is inversely proportional to intellectual development as measured by chronological age. This would imply that within the three–eight year old CA groups, the younger subjects would obtain better scores than the older subjects. Once again, the results of the three studies reported clearly indicated this. In all cases, the younger the CA of the group, the higher the paranormal cognition score—with highly significant inter-(and in some cases, such as the five–eight CA group, intra-) group differences.

Third, in a series of three experiments, this time concerned with the IQ of subjects as a variable for paranormal cognition, it was predicted that subjects of lower intelligence levels within the same CA group will show a greater tendency to employ paranormal cognition more successfully than subjects of the same CA but of a higher IQ. Testing subjects aged between three–eight years, this prediction was once again clearly borne out, indicating another measurable variable, i.e., IQ as a determinant of the successful employment of paranormal cognition.

Fourth, it was predicted that the manipulation of both these variables, CA and IQ—either in combination or independently—would cause observable changes in the paranormal cognition scoring of subjects. It was found that those subjects paired for both CA and IQ obtained the highest paranormal cognition scores, indicating a clear inverse relation between paranormal cognition and both CA and IQ. It was also found that those subjects paired for IQ but not for CA showed a clear inverse relation between paranormal cognition ability and IQ, but not between paranormal cognition ability and CA. Finally, it was found, that those subjects paired for CA but not for IQ showed a still clear (but much reduced in

terms of successful scoring), inverse relation between paranormal cognition and CA and, to some extent, IQ.

Fifth, it was hypothesized that although both paranormal cognition and normal cognition can be employed together, if a normal cognition task of some complexity is introduced at the same time as a paranormal cognition task, then the successful employment of paranormal cognition will diminish. Further, the more complex the secondary normal cognition task, the lower the level of successful paranormal cognition scoring. An experiment employing the use of secondary word-association tasks of varying degrees of complexity which were administered during a paranormal cognition task was designed. It was found that those subjects given both tasks at the same time obtained significantly lower paranormal cognition scores than those expected and that the more complex the word associations were, the lower was the group paranormal cognition score.

Sixth (as described earlier) in contrast to this, it was predicted that if subjects in a paranormal cognition task were given an additional task geared to relax normal cognition, then they would demonstrate a higher employment of paranormal cognition. An experiment employing the use of headphones and various types of music being played at the same time as the paranormal cognition task showed that subjects in the relaxed environment made significantly greater scores on their paranormal cognition task than expected. It was also hypothesized that the more relaxed the subject groups were, the higher would be their score at paranormal cognition. Results from the experiment indicated that the more relaxation stimuli available, the significantly greater was the group paranormal cognition score.

Seventh, it was predicted that no sex differences would be present as indicators to paranormal cognition. In all the experiments and studies conducted, no sex differences were found.

The indication is strong, therefore, that paranormal cognition is clearly being utilized to varying degrees by man and that it is strongly related and affected by both CA and IQ—both indicators of the level of complex internalized intellectual development in humans—and by the relaxed state of the subject.

It seems to be the case that when self-directed internalized connections are being minimized or are not yet being fully employed because of the maturational level and intellectual development of the subject, paranormal cognition is much more actively and successfully employed. I have argued that paranormal cognition can best be seen and studied as an alternative system of cognition closely related in its mechanics to normal cognition, although quite clearly unable to combine with normal

cognition without impairing either one or both cognitive systems. All my experiments have pointed to the general validity of this theory. It is hoped that even with its limitations and assumptions it might provide the springboard for new types of experiments both in paranormal and normal cognition which will clarify both systems and, hopefully, the process or processes that initiate them.

It is important to note, for example, that although I have restricted my discussion to the telepathic phenomena, there is no reason to limit the implications of the proposed system to that phenomenon which readily falls under the telepathy (or GESP) label. My own feeling is that clairvoyance, precognition and even PK phenomena can—and should—be analyzed as primarily cognitive events and are, thus, most clearly open to hypotheses and experimentation when considered from this framework.

I believe that, by altering our approach toward the cognitive, parapsychologists will begin to be able to answer what I consider to be one of the most important objections raised by non-parapsychologists to accreditation of our work within the realm of science, namely that parapsychology appears to be a collection of data with no cohesive theory around it.

What my work also suggests is that, on the basis of the large and successful number of experiments on paranormal cognition conducted with child subjects, a whole new and easily accessible group of gifted subjects has become available for parapsychological study. There are, however, several limitations and necessary moral obligations to be considered in conducting research with children, as well as a number of practical problems which must be considered before an experiment with child subjects can begin. I have noted several of the major problems and obligations facing such a task elsewhere[3] (pp. 601–609) and it is strongly suggested that any serious researcher wishing either to replicate or even extend the ideas and experiments described herein should make due note of them.

There appears to be another problem with this last point as well. Although a number of researchers (predominantly in America) have reported successes with child subjects, two independent attempts at detailed replication of some of my work with children by British researchers have failed to obtain significant results.

At the conclusion of my doctoral thesis I wrote ". . . to use Jung's phrase, it should not be concluded that the author believes that paranormal cognition is NOTHING BUT what he has written. The field is still so uncertain and unknown that to make such a statement would be folly—both scientific and personal"[3] (p. 549). I'm glad I had the fore-

sight to include that proviso. For, since the writing of my thesis, the major issue of the experimenter effect has arisen and predominated parapsychological discussion.

A number of associates have suggested that the different results obtained by myself and the British replicators are clear instances of this experimenter effect. I am loath to accept this explanation, principally because no one really understands what the experimenter effect is or how it functions; it is not any true explanation.

Instead, my own viewpoint is that this discrepancy in results points to another aspect of the difficulties involved in the experimental study of paranormal cognition. Namely, that if, as I have suggested, paranormal cognition occurs when there is a relaxation of "self-oriented and directed" thought, so that a sudden meaningful connection occurs, then, in any experimental situation, one important variable which might aid or impair this event is the experimenter him/herself—or, to be more precise, the experimenter's own cognitive state. In other words, my interpretation of the experimenter effect, while not dismissing the many social and personality variables that other researchers have proposed, nevertheless views them as secondary offshoots of the real issue: the experimenter's own entry into the paranormal cognition state wherein for the duration of the experiment he/she accepts that there is no reason or purpose for what is occurring, there is merely an open-ended experiential event.

I suggest that this is one way of understanding the phenomena associated with this effect and, more specifically, of explaining the discrepant results obtained by myself and some replicators. What I am admitting to, I suppose, is that the state I've just described is the one I experienced in the course of my research. It is, I believe, an "alchemical" approach to the experimental situation in that the experimenter recognizes his/her lack of distinction from his/her experiment.

I realize that this notion of experimentation appears to go against the current ethos of the vast majority of scientists, barring a few theoretical physicists, but I sincerely believe it to be a necessary approach for parapsychology to accept in its second century.

The first century of parapsychology can be characterized as the period of search for scientific acceptability. It was a time during which parapsychology tried its best to emulate the standards of accepted scientific endeavor. In other words, parapsychologists did what was required of them. I think we have at last earned the right to place our own stamp on science.

Parapsychologists must remember that their work is revolutionary. We are working to undermine and/or expand the concepts and as-

sumptions of other sciences. To do so, we cannot go on feeling this compulsion to do everything properly. Parapsychology does not need to be proper. It has to be intriguing, exciting, radical, inventive, sometimes even a little crazy if it wants to continue to exist, if it wants to attract new researchers.

We have in our grasp one of the most potentially exciting topics of exploration that man has encountered. Most important, we have the interest of millions of non-academics. People of all classes and ages are entranced by this topic. For the past century we have caged this field in scientific trappings, we have compartmentalized it and dissected and labeled, and yet we have learned little about it because of this push to make it acceptable.

Of course, there was a necessity for such an action. There still is. I have no doubt that parapsychology can be acceptable to science, but only by discovering its variables. And, in some cases, these variables cannot be discovered within the present structures—and strictures—of science. If parapsychology were to turn away from its objectivity just a little, if it were prepared to view experimental events more as situations in which there are no experimenters and subjects performing tasks, but rather various participants acquiring very personal information and understanding, then I sincerely believe that it would be on its way to shattering discoveries.

We have been objective and scientific. We have those credentials. A true scientist cannot fail to be impressed by them. But it is not credentials of respectability we are after, it is understanding. And to understand more, we must break away more from traditions, from limitations, from supposed laws of science.

What my work has taught me most of all is that we are playing with something of which we have no understanding. We are as arrogant in our own way as all the critics of parapsychology put together. We attach labels and play at being scientists and think up acceptable experiment after experiment and come up only with vague conclusions which are as often as not disputed. I think parapsychology should enter a period of observation and participation and forget about trying to prove anything for a while. As parapsychologists we should immerse ourselves in our research, expand it, make it part of our being and essence. We should allow it to encompass us, to take us where it wants, to lead us anywhere.

Let us revolutionize ourselves before we proclaim any general revolution in science. Let us free ourselves from the habits and rituals we've accepted as "truths" all our scientific lives. If we are to question, then let us make the questions worthwhile. Results, I'm certain, will come.

But the risk must be taken. If it is necessary to break away from the other sciences, if it is vital to shun and be shunned by them once again, then let it be so. If we are truly men of knowledge, men who seek knowledge, then there is no question necessary. Succeed or fail, we are now at the point where the step must be taken. I believe that if we do not, parapsychology will fade away like all else of little value. Parapsychology's second century is a crucial one. Perhaps the most crucial. Whatever else, we have an exciting time ahead of us.

## BIBLIOGRAPHY

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## *DISCUSSION*

RONEY-DOUGAL: I have one question which I've always had with child work. You say you get the most dramatic results at the youngest age and, by the age of eight, you get no results at all. Then, how is it that those of us working with adults get positive significant results? If it dies out, does it come back again in the teens? Is there a period where the child's cognition is such that he has to become totally self-orientated and individual and shut it out and then, as he gets older, he can open up again? How does it actually go right through?

SPINELLI: No, I don't think for a minute that it dies out, in any sense at all. What I do think, however, is what I was pointing to at the beginning of the paper. That is, with adult subjects, the best results that we obtain are under the conditions (most recently anyway) where the adult is in what we loosely term as some form of an altered state of consciousness. A great deal of research has been done with adults, inducing in them through Ganzfeld, through hypnosis, etc., any type of consciousness other than their normal cognitive conditions. If you succeed in doing that, then what you are essentially doing is opening the way for paranormal cognition to be more easily observable. What I'm saying really is that children are more likely to be more often in a paranormal cognition state than adults are, simply because they are in the process of acquiring their normal identity—their self—their ego. Does that clarify things at all?

RONEY-DOUGAL: So, by the age of eight, you need to use hypnosis or Ganzfeld, or whatever, to help that child through it.

SPINELLI: Yes, at about the age of eight, within a Piagetian framework. At about the age of eight is when true internalized imagery begins to take place in the child. The child has a strongly-defined sense of identity; he is able to internalize concepts; he is able to create new ones and so forth. It seems to be a crucial age for constructing the sort of mental state that typifies adulthood. And so it is at that point, I think, that there is stronger and stronger opposition towards allowing a paranormal state to manifest itself.

RONEY-DOUGAL: Is it at that point that those who we might call natural psychics, the Matthew Mannings of this world, will begin to emerge as such?

SPINELLI: That, I couldn't tell you. I don't know.

SARGENT: Ernesto, there is an affinity between what you are saying and some recent theoretical writing, by people like Rex Stanford, particularly, on cognitive operations and how they might relate to paranormal functioning. Now, when you are working with children, there are good tests, like Piagetian tests and IQ tests, that you can use to define pretty well what level of formal thinking they are using and so on. The problem is, I don't think that we can actually look through the full range of children and adults with the same kind of measure, to see if there were similarities between the child's level of typical mental functioning when it's very young and the adult's level of functioning when you get him into a psi-conducive state. Now a possibility here would be something as loose as word association.

SPINELLI: Yes.

SARGENT: And Rex Stanford's work with word associations has suggested that the use of Ganzfeld stimulation actually produces a disintegration in the normal mental functioning which is even more extreme than that which you find in schizophrenia. This comes from the Stanford and Roig study presented at the 1981 Parapsychological Association Convention. So, it might be possible that we could actually find measures that look right through the whole span and see whether there really is much greater agreement between the mental functioning of the child typically, and the mental functioning of the adult phasically, when you are getting these kinds of ESP effects.

The second point I want to make is about the experimenter effect. It seems to me that the experimenter effect is actually a label; it isn't an explanation at all; it's simply reified into an explanation. However, cognitive explorations might give up some insight into how such effects might work. The experimenter has his own cognitive state and it is indeed possible to use chemical approaches to alter the experimenter's state of consciousness during the experiment. That can certainly be done.

Another possibility is that it will be interesting to look at the experimenter's influence on the mental operations of his subjects. Perhaps particular experimenters appear to make it particularly easy for their subjects to loosen cognitive constraints.

As an undergraduate, I actually did do a project on the stability of gender identity in children with Piagetian tasks. I found something terribly embarrassing—that our four-year-old children had a more stable sense of gender identity than the seven-year-olds. Now this is rather tricky to explain because it really shouldn't be quite that way. When we went back and looked at the data, we found that the reason was very simple. There was a huge experimenter effect in the data of the children that I had actually tested. When we looked at the male subjects with a girl experimenter, every boy that she had tested had shown gender constancy. He was a little boy, he knew he was a little boy and you couldn't persuade him otherwise by Piagetian bags of tricks. He knew that even if you put a handbag on him he would never be anything else but a little boy. However, all of the male children that *I* had tested showed exactly the reverse picture. They were perfectly prepared to admit that, if you put a dress on them, they would now be little girls. And we had the devil's own time in trying to figure out why they should have reacted in this way. In fact, we spent hours agonizing over a straightforward experimenter effect in child psychology. These effects are by no means unusual. I think there are two lessons to be learned from that. One, is that one would believe, at times, the experimenter effect only exists in parapsychology, from the hysteria that was generated by some people over it. Of course, this effect appears frequently in child psychology. Second, it might be possible to develop tools within developmental psychology to actually figure out how these effects ever get mediated. So, indeed, the rapprochement that you are suggesting is certainly there.

SERVADIO: I am very much in agreement with many things Spinelli has said, but as an old timer, I want to make a few historical recollections. The shift from the interest in telepathy towards the theory of cognition, I think was first put forward by Charles Richet, who didn't believe very much in telepathy as such and created a term which is now long forgotton—crypthestésie—that would encompass the whole of ESP phenomena. That was written in 1922 or 1923. And another French scholar, Dr. Eugene Osty, wrote a book whose title was *La Connaissance Supranormale*, that is *Supernormal Cognition*. That was a few years after Richet's book, but still long ago. I was extremely interested in the experiments with children, because, of course, I think that Dr. Spinelli has gone a long way in studying phenomena that were not properly studied by other

researchers. This includes the famous studies by Berthold Schwarz in the United States, about paranormal phenomena between parents and children—books that you all know very well. But I was particularly interested by the age of these children. As a psychoanalyst, I know what Freud contended regarding the maximum of instinctual drives in children around about three years of age. This also makes clear why, at that age, not only are there more paranormal possibilities up to eight years of age, of course, but above that age, as Dr. Spinelli pointed out, the concepts are not so evident as in intelligent three-year-old children. This is perhaps why, at around three years of age, the phenomena are so prominent. And I think that, also, Dr. Jan Ehrenwald in one of his essays pointed out the scheme of the human personality, in that it is not a closed system, but is an open system that is very much closed in normal adult life, but is still very much open in very young children.

SPINELLI: I take your point as to the earlier researchers; I can only say that this is my ignorance of the early era.

BLACKMORE: Well, as many of you probably know, I am one of the failed replicators who Ernesto is talking about, so I'd like to say a few words about the problem of trying to do experiments with young children. Ernesto has often pointed out that I probably didn't have the right mental state, the right approach with the children and so on. But I don't think we're going to get anywhere with this kind of argument, because who can say what kind of state I was in when I was doing these experiments. All I can say is, that in doing a guessing experiment with three and four-year-old children, they got carried away, as it were. I mean, that they were so involved in it, they hardly noticed me. They were really involved in their task and they carried me along in it. I feel that it was one of the few experiments in which I really got totally involved. Nevertheless, the results were, as in all my experiments, purely at chance. What I really want to talk about is not that. I did those experiments, trying to replicate Ernesto's work, because I have long been interested in the kind of approach he has talked about today—the idea that children are better at psi, the idea of its relationship to cognition and so on. Potentially, I think there are some ideas worth following up in there. However, I do not think that the results that Ernesto has got really justify much hope in this direction. I hope you'll forgive me, Ernesto, for dragging out the old problems. Since you haven't published anywhere the details of this research, they have not been argued out in public, so perhaps this is a chance to talk about some of the problems. Some of your findings were very fascinating. For example, the fact that when you had a pair of children of similar age, they did better than a pair of children of different ages. However, I think I'm right in saying that

those experiments were in the early series, in which the sender was allowed to choose which picture was going to be the target. Now, in that case, you can explain a lot of results perfectly normally, because the younger children will be more prone to simple biases and preferences and so on, so the younger ones should do better. And, also, children of a more similar age are more likely to have similar preferences, so that would also explain that finding. Now I know that, in later experiments, you corrected that problem, but you didn't replicate all the findings that you got in the earlier ones. If we look at the results of later experimenters, there may be experimenter effects, but they haven't been able to replicate them. So, I think that the very hopeful things that you are talking about, which go on interesting me, are perhaps not going to get us as far as we might hope.

SPINELLI: You are only partially right in that. The results that showed that varying the age of the child produced variations in GESP scores were, indeed, in the first three sets of observations. Now, two of those sets of observations were experiments in which the sender did, indeed, have a choice as to the target. In the third set of observations, however, there was no choice on the sender's part—it was a random target, chosen by statistical tables. So, because there was absolutely no significant difference in scoring between that third set of observations and the first two sets and because all the tabulations were taken together, then it seems to me that your criticism doesn't really hold; that, by varying the ages among those children as well, decline in the scoring effect was also attained.

DEAN: As I understand it, a new born baby—the first day old—does rapid eye movements about 20 hours out of the 24. I think in the measurements that have been made even before birth, the fetus is doing rapid eye movements closer to the 24 hours out of 24 hours, than when it is first born, doing 20. But, thereafter, the number of hours of REM per day drops down by the age of puberty to about the two hours per day that we, all of us, do for the rest of our lives—two hours of REM dreaming out of the 24. In the decline of the telepathy scores from the three-year-olds to the eight-year-olds, was there a parallel with the decline in the number of hours of REM going on at the same time in those age periods?

SPINELLI: That, I couldn't tell you at all, I'm afraid. It was not a connection that I thought of. Obviously, one of the implications of my work is that, if three-year-olds are so good, then, if we manage to go even earlier to weeks-old babies then perhaps we ought to be able to obtain very, very good results. The problem is that I have yet to manage to think up an experiment that will allow me to work with children of

that very early age. However, Dr. Bierman informed me this morning that he had done one. So perhaps, later on, he will tell us about it.

BIERMAN: I want to return to the experimenter effects. I think you got off a little too easily by saying that it is just a label. What I want to propose to you is a specific mechanism which takes place in your experiment. Now, let us look closely at what will happen in a guessing experiment. The child has to respond at a given moment by naming one of the possible targets. Now, if we look at the process involved, it must be that some representation of that target comes into consciousness and activates the child to make his call. Now, what I propose is that the representations of those targets are what I called "Demons" and are characterized by some activity level *which has a random component.* The mechanism for ESP might be that someone uses PK to shift the activity level of this Demon in such a way that it becomes more active than the consciousness threshold and results in a call. So the mechanism that I propose for your experiment, which seems to be ESP is, in fact, PK on a brain state. Now, my guess would be that the PK subject in this experiment is you, that you are doing psi on this random system which is the "Demon" in the child's brain.

Now, we can develop two views—an optimistic and a pessimistic view—with regard to the relations you have found between psi scores, age and IQ. I start with the pessimistic view. The pessimistic view is that, as this experiment was just a PK experiment of you on the children, you had some expectations about this psi strength depending on age and you just confirmed your own expectations. This is a pessimistic view because within this view it is nearly impossible to find a real intrinsic process if experimenters are imposing their own expectations on their experiments all the time. The optimistic view on the other hand, is that there is something really going on there—that the age and IQ effects are true intrinsic psi effects—that psi really happens more often with younger children independent of the experimenter's expectation. For this latter view there is also some support in the "random Demon" model. This model supposes that there should be a representation (or Demon) for each target in a psi experiment. We might assume that for younger children, the associational links between those representations are less strong than for adults. However, the associational links between targets are developing during an experiment which obscures the psi effect. (It also "explains" decline effects.) Because, whenever you "PK" one Demon to above the consciousness threshold, and there is a strong associational link, it just pulls all the other possible targets with it and a choice becomes very difficult for the child. So under the assumption that younger children have less strong associational links the decrease of psi

with age can be explained. In a similar fashion the IQ effect could be explained by assuming that children with a high IQ probably have more different activation levels, so that there are very highly activated Demons that they can attend to very easily—they can concentrate better on that—while low-level IQ children probably are dealing with a sea of Demons, more or less simultaneously activated, a low ability to concentrate. This latter situation is an optimal situation to do all your PK in. In a sense the cognitive system for these low-level IQ children is more random (hence, their inability to concentrate). And that's also the situation which is induced e.g., by the Ganzfeld procedure.

SPINELLI: As to your pessimistic and optimistic viewpoints, I have to be honest with you and say that, certainly, when the experiments began, I had indeed begun to think in terms of an age effect. So you can be pessimistic from that point of view. But, what had produced that idea was an earlier set of experiments I carried out in cognitive development, in which this notion hadn't even occurred to me, but which, in themselves, showed this age effect as well. So, I don't know whether that's optimistic or pessimistic, or both. I don't think I have any real objection to what you have been saying, aside from the fact that my own feeling is that the issues that my work raises fall very readily within the framework of cognitive psychology. Rather than work in a new sort of Demon-chaotic framework, I suppose I'm more willing to work in one that, at the moment, allows the same type of theoretical work, without upsetting too many other people, and allowing other people in a field which is allied to mine to contribute to it. As to the point of PK, all I can say is that, at the moment, this is the research that I'm conducting with children. So now I have shifted my attention away from ESP—from telepathy, specifically—and I'm now beginning to carry out PK experiments with young children.

BIERMAN: You suggested that Sue Blackmore shows the experimenter effect in terms of psychological "treatment" of the children, and what I have been trying to suggest is that the difference between you two is your PK ability. What I suggest is that you do some PK matches or PK tests, or both, and compare your PK ability.

SPINELLI: I certainly have no objection to that. My own personal experience with PK is that I myself don't seem to be a particularly good subject. On the other hand, I'm willing to go along with your point of view that I only become a good subject when I think that it is other people who are doing it. That may be the case.

SARGENT: We're getting into this very long and garbled and incredibly complicated argument from what I shall call the gnome theory coming from Dick Bierman. It's all totally unnecessary. You state in your

paper, Ernesto, that there are, in fact, some highly significant intra-group differences in your data. Under these conditions the effects are due to the subjects. It is utterly gratuitous, when you have expanded variance with a group, to say that anybody other than the subjects is responsible for the effects, to postulate an experimenter psi effect, when you actually have prima facie evidence of highly individual differences.

BIERMAN: What I say is that there are two aspects to the psi process. There is the randomness in whatever system you try to influence (RNG or brain state) and there is the PK source. I won't say that there is no difference between the subjects and that there cannot be a subject effect. Certainly there can, because the amount of random brain function differs. What I suggest is that the source of the PK is coming from the experimenter.

SARGENT: Certainly, but I understood the argument, and I think Ernesto understood, that what you are saying is that he (Ernesto) is the PK source, and there is *clear* evidence that there is an input from the children as well, *at least.*

HAYNES: May I point out the innumerable and often unrecognized difficulties of communication in parapsychology. Perhaps they could be illustrated—or paralleled—by what occasionally happens at European Economic Community debates where every speech is simultaneously translated into the language of every member state of the Common Market. Here is an example. A forceful Englishman, anxious for a firm and speedy decision said, "Now we must shoot the rapids," which came back to his Francophone listeners in clear French as "Maintenant il faut fusiller les lapins" ("now we must shoot the rabbits"). It seems to me that the perils of instantaneous translation in all our languages, and especially in the language of parapsychology, must be looked at, allowed for and, if possible, obviated. I would suggest that we should try very hard to translate into a common tongue the words we use, since they are all too often technical terms, which don't mean the same things to everybody.

There is also the matter of reductionism by way of semantics. This has two main aspects. One is the use of "causation" to mean physical causation alone, which has only, I think, happened over the last hundred years. Before that, there was assumed to be a first cause, the exercise of the will, even in Aristotle's time. And the second, and it has just recently come up, is the habit of evading consciousness and preconsciousness by attributing almost all ESP (which is an affair of consciousness) to the effects of PK (which is an affair of mechanism.)

RONEY-DOUGAL: I happen to have in my possession something called a Magic Doll's House, which the late Colin Brookes-Smith built for my

daughter some years ago. It can actually be used by any child who can sit up and pick something up and put it into something. So it could be used by a child from the age of at least one year onwards. My daughter, at the age of six, still enjoys playing with it. So, with a little bit of technical renovation or repair, if anybody wants to borrow it and wants to put it right and get it working properly again, we've got a nice bit of equipment there.

BLACKMORE: Perhaps I don't have equipment to offer, but I have a baby to offer. I've trained my five-month old baby to pull a string in order to turn a tape recorder on, to find out what sort of music she likes. And I wonder whether, with those sorts of simple responses, one might be able to design ESP experiments. If you can use a baby with those kind of simple responses, to design an ESP experiment, I'd love to try it!

SPINELLI: Sure, I think so. The main problem, as I see it—and perhaps its just because I see it as a problem—is that the experiment won't work. It's because, if what I've found so far is correct, I'm going to have to have two babies.