

A DIFFERENT APPROACH FOR STUDYING PSI

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Parapsychology is the branch of science which deals with psi phenomena. Historically, the term psi phenomena (or psychical phenomena) has been used to designate a specific class of events or experiences. Instances of the most common forms of psi experiences (telepathy, clairvoyance, precognition) can be found in the literature of all ages. Psi experiences are occurrences in the daily life of people which usually involve a correspondence between the content of a subjective experience (a dream, intuition, hallucination) and an event which impresses people as meaningful, not coincidental, and is experienced as not explicable by ordinary laws of nature and consequently as suggestive of ESP. Because of its inexplicable nature, in the past the explanation of such experiences was often sought in the acts of higher powers or believed to be ascribable to the influence of the deceased. The latter view, expressed in the various forms of the spiritistic movement, dominated Western culture in the last century, the period when, for the first time in history, psi experiences became the subject of scientific investigation.

The scientific study of psi experiences has strongly contributed to a radical change of view about these phenomena. In one of the first major studies in the new field, *Phantasms of the Living*, the founders of the British Society for Psychical Research introduced the idea that psi experiences did not need extra-human agencies to explain them. They tried to demonstrate that the human experiences which were at the time considered as strongly supportive of the spiritistic hypothesis, the occurrence of apparitions of friends or relatives at the time of dying or of undergoing some major crisis in life, together with other types of experiences could be explained by the concept of thought-transference. In addition, they tried to prove that these apparitions happened more frequently at the time of dying than could be expected by chance and consequently could be considered as independent evidence for the existence of thought-transference. In their opinion, thought-transference was an already experimentally proven phenomenon, a "fact of nature." Since they considered thought-transference as a strictly human

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faculty, the novelty of their approach was not only that they tried to reduce a wide variety of human experiences of different appearances to only one phenomenon, but above all that they implicitly introduced the view that all these experiences had to be considered the result of a human faculty.

The authors of *Phantasms of the Living* were not exactly successful in convincing the world that on the basis of these experiences thought-transference should be considered a proven phenomenon. On the other hand, they succeeded very well in promoting the view that psi experiences, whatever their nature, are potentially human and influenced by normal psychological processes. In particular the work of F. W. H. Myers, culminating in his book *Human Personality and its Survival of Bodily Death* contributed strongly to this.

As a consequence, psychical research came to be considered as part of the field of psychology. Indicative of this situation is the fact that at one of the first international conferences on experimental psychology, held in 1889 in Paris, hardly any distinction was made between psychologists and parapsychologists. Participants from both fields presented papers and took part in the discussions. Reading the report (Myers, 1889), few traces can be found of the animosity which unfortunately exists in our day between the two fields.

That the two fields became so much divided is partly the result of another important change which the scientific approach brought about. Gradually the subject of research shifted from psi experiences and the psychological processes associated with them to the effects of psi as an operating principle. In particular, the views and the work of J. B. Rhine were largely responsible for this change. He introduced the approach of large-scale card-guessing experiments with unselected subjects. In these experiments not the psi experience itself, but the effect of psi on the scoring became the object of research. Hence the term "psi" became associated with two meanings. As Eisenbud stated: "The term psi . . . is not limited to the designation merely of a class of events which lends itself to differentiation solely on certain phenomenological grounds from other classes of events; it is frequently used to refer to a quite special and even . . . uniquely operating principle" (Eisenbud, 1956, p. 3). Not the psi experience, but the effect of psi as an operating principle became the object of study of parapsychology. To quote Eisenbud once more: "Somehow, the notion of a psi force has taken deep root, and it is this 'force' that, in one form or another, is held to provide the causal connection between the events that make up those correspondences" (i.e., the psi experiences) (ibid. p. 4).

As a consequence, since the 30's of this century nearly all research in the field became focused on the concept of psi as an operating principle. When studying psi experiences the observations, the experiences themselves of which the existence and occurrence are not in doubt, are the basis for deriving the concept of psi. In experimental research this order seems reversed. When the effect of psi as an operating principle is studied, it is necessary to assume in advance the existence of such an operating principle. Because the existence of the principle has to be presupposed and because the nature of the principle seems to contradict the current world view, this approach of research in parapsychology might be called the "metaphysical" approach.

The shift in emphasis from the psi experience to psi as an operating principle had some important consequences which in my opinion have created many of the problems which nowadays beset the field. It cannot be a coincidence that after Rhine's approach became representative of research in parapsychology, the differences and even animosity between psychology and parapsychology increased so much. This is the more striking because Rhine strongly advocated the application of research methods which belonged to the standard tools of the experimental psychologists. Since one can hardly expect psychologists to take offense because their own methods are used, it is clear that the source of their irritation must have been something else.

This also holds for the present situation. We are all familiar with the main criticisms against our field. To name a few: the lack of repeatability and of control over the phenomena, the emphasis on significance levels instead of on the strength of the effect and the meaningfulness of the result, the lack of sound theories, the distortive effects of publication practices which favor the publication of research that "turned out." But what is often overlooked is the fact that these objections are not at all restricted to parapsychology. They apply to a large extent to all social sciences.

A few quotes from Gordon Westland's book *Current Crises of Psychology* (1978), may serve to illustrate this. As regards the lack of repeatability and of control in psychology it is stated: "Doubt is now unquestionably growing within the ranks of professional psychologists over whether laws, prediction, and control, as conceived for traditional science make sense for psychology" (p. 69). Of the familiar over-emphasis on quantification and the tendency to look for significant results whatever their meaning it is observed: "It is difficult to deny that disturbingly often in many areas of research within psychology the argument for whatever conclusion is being put forward is made to depend

on the quantification as such, which has been carried out in the research, regardless of the logic of what has been done" (p. 37). As regards the doubt over whether psychology should be considered a science it is humbly stated: "Science is a label, never more than an administrative convenience. The only thing that ought to matter for psychology is if its practitioners can justify their conclusions against some rational criterion" (p. 47). When discussing opinions about the value of psychological theories, Westland cites Deese: "Psychological theories, hypotheses, and facts are a jumble of notions. Thus it is difficult to say what single purpose psychological theory serves" (p. 65). The reality of the research practice is viewed as follows: "People will persist in repeating inconclusive experimental designs only until they 'work,' then they move on to other projects with a sigh of relief. Numerous literature surveys have shown that reports of replications of 'successful' research studies are rarely published" (p. 98). These are only a few citations from many which could be used to illustrate that the problems in psychology are to a large extent the same as the problems in our field. Indeed, if Westland had consistently added "para" each time he used the word "psychology," with a few adaptations and written in a more aggressive style the book could have been hailed as another devastating attack on parapsychology. It makes one wonder whether the animosity displayed by social scientists towards parapsychology is not partly generated by the fact that in our field the weaknesses of the social sciences themselves become magnified and can be exposed so clearly.

If it is true that the criticisms directed at parapsychology apply to a large extent also to the other disciplines of social science, then it follows that the real reason for the rejection of our field must lie elsewhere. I believe that the change of research topic from psi experience to the effects of psi as an operating principle has much to do with it. There is no doubt that psi experiences do exist, whatever their explanation, but there is doubt and downright rejection in the case of critics as regards the existence of a psi process. To a certain extent a psi experience can be considered an observable phenomenon; that does not hold for an operating principle. As a consequence, psi experiences can be studied like any other psychological experience or phenomenon, but the study of psi as an operating principle does not make sense unless it is demonstrated first that such an operating principle does exist. Most important, the psi concept as an explanation for ostensibly psi experiences can be falsified or rejected. The assumed operating principle as explanation for an observed effect is much more difficult to falsify or to reject. If an experiment yields a significant result, it is attributed to psi; if the result is not significant, the subject was not able

to use his or her psi ability. But to demonstrate that psi exists seems extremely difficult, if only for the fact that it apparently needs living organisms, primarily human beings, to display its effect. And except for a few phenomena like the constancy effect in perception, humans cannot be considered deterministic systems who act exactly according to prediction every time. This applies especially to the situation where we deal with a non-repeatable phenomenon which occurs rather rarely. It might well be that we will go on finding occasionally high scoring subjects, but nothing indicates that we are achieving the kind of control over this apparently psychologically determined process which amounts to proving its existence. In the debate on parapsychology it appears that psychologists often seem unaware that when they require the existence of ESP to be proven, they require something which cannot even be done for most psychological processes they study. Psi seems to a large extent to be determined by psychological factors and, as indicated by one of the quotes presented above, doubt exists within psychology whether the control over phenomena as conceived by the traditional sciences makes sense for psychology and thus for psychologically determined processes. If psychology doubts whether they are able to control the familiar psychological processes they study, how can we expect to gain control over psi to such an extent that we are able to demonstrate it at will at any time the skeptic requires?

In my opinion, it is unlikely that we will succeed in this in the foreseeable future. But if we fail to gain control, then we seem obliged to present another basis to legitimate the use of the psi concept. At present the concept of psi is mainly based on confirmatory evidence; a specific class of experiences lends itself to application of the concept. Thus, new experiences are reported which allow us to apply the concept to them. But to base the acceptance of such a revolutionary concept on only confirmatory evidence seems to me not sufficient. Therefore, a first requirement for providing such a basis would be to adopt a research strategy which allows the falsification or rejection of the concept. If we adopt such a research strategy and if it turns out that the concept of psi stands up to attempts at falsification and survives the competition with various hypotheses of a non-psi character, then the concept would gain a much stronger position. Therefore, I feel that a different approach in parapsychological research is desirable. This approach, which can be called the "pragmatic" approach because it deals with observables, focuses on the psi experience as the research object for parapsychology. To shift the emphasis to the study of psi experiences has some important consequences and opens up areas of research largely neglected in parapsychology so far.

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An additional advantage of this approach seems to be that more insight is gained in the properties and limitations of psi as it appears in nature. This can be useful for an experimental research aimed at the psi process itself. Although I am in favor of investing more energy in the pragmatic approach, that does not imply that experimental research in this field has to be rejected. After all, there is already a substantial body of experimental evidence suggestive of the existence of psi. In addition, if psychologists do not hesitate to do research on phenomena which suffer from most of the deficiencies the psi hypothesis suffers from, why should parapsychology?

As a first step it seemed necessary to investigate whether psi experiences can be considered as a distinct class of experiences. If these cases are not just random experiences, but are caused by some common process, then we might expect to find some common properties in these experiences other than the attribution of a paranormal character. However, it should be taken into account that psychological variables, such as the effect of time on memory, unreliable testimony, reporting effects, etc., might have influenced the reports of these experiences. Therefore, it was also investigated to what extent such variables could explain the trends in the data.

Contrary to traditional analyses of spontaneous psi experiences, the present study involved mainly quantitative analyses. Each experience, or case, was scored on a number of aspects. Examples of such aspects or categories are: sex of percipient, sex of target person, relationship between percipient and target person, type of experience (dream, intuition, etc.), type of event, number of correct details, time between experience and reporting of the case. The classes within each category were selected in such a way that the scoring excluded as much as possible the need for subjective interpretations. In most analyses chi-square tests were applied to test for the significance of the relationships between variables. The data do not lend themselves easily to application of multivariate analysis techniques, so in the interpretation of the data we had to consider all sorts of possible interaction effects and the fact that the level of significance was not always representative of the strength of the effect. The studies discussed below involve such a mass of data and number of analyses that they would not have been possible without the help of a computer.

It is outside the scope of this presentation to discuss all the results of the analyses carried out. They can be found in the original publications. However, to give an impression of how these studies were carried out an example is given of some of the analyses of the first study. This example focuses on the testing of several hypotheses to

explain the observed relationships and shows how several apparently unrelated observations result in one general hypothesis.

The first collection of spontaneous experiences analyzed was the one published in *Phantasms of the Living*. In this study 562 cases were involved. It appeared that the psi experiences in this collection have a number of characteristic properties and strong significant relationships between variables were observed. Among these are the well known phenomena of a preponderance of female percipients, of cases between near relatives and of cases involving death or other negative events. It was investigated whether psychological factors, which I will call "reporting effects," could explain these findings. As an example I will consider the preponderance of female percipients.

Considering the size of the difference between number of male and female percipients and considering the finding that for the target persons a preponderance of males was observed, it seems highly unlikely that a difference in population figures for females and males could explain the excess of female percipients. Another possible explanation might be that the effect is due to an excess of female contributors of cases to the collection. Since most cases are contributed by percipients, more female contributors would result in a higher frequency of female percipients. However, when considering only contributors who were neither percipient nor target person, it turned out that males contributed cases to the collection relatively more often than females. This is not surprising, because we know that the cases in the *Phantasms* collection were mainly acquired through the (male) officers of the SPR. Although males contributed more cases than females when the contributor was not personally involved in the case, the difference between the percentages of male and female percipients in those cases was found to be no different from those in the cases contributed by the percipients. This implies that we also can exclude the hypothesis, first suggested by the authors of *Phantasms*, that female percipients might be more inclined to report their experiences than males would be. Another possible explanation concerns the frequency of cases in certain types of relationships. Suppose that an exceptionally high number of psi experiences take place in the mother-child relationship, then this could result in a higher frequency of female percipients. It appeared that this hypothesis also had to be rejected. Nearly all types of relationships show a preponderance of female percipients. This includes the spouses, a relationship in which the numbers of males and females are by definition equal. In short, none of the reporting effects investigated provided an explanation for the observed phenomenon of a preponderance of female percipients.

If this phenomenon cannot be explained by a reporting effect, then perhaps we should assume that females compared to males are more sensitive to psi experiences and make better percipients. In order to test this hypothesis I compared female and male percipients on a number of variables which might be indicative of such a difference in sensitivity. For instance, if females are better percipients, then they might be able to cover larger distances or to "see" more details about the circumstances of the target person. However, in none of these analyses was a difference between male and female percipients observed. Thus, all the analyses failed to provide a satisfactory explanation for the higher number of female percipients.

The difference between the number of male and female target persons appeared strongly significant with a higher frequency for males. Because in the majority of cases the event in which the target person was involved had a rather unpleasant character (death or accident) it was assumed that males became target persons more often because of their life style, which might have made them more prone to meet a serious accident or sudden death. However, the data did not confirm this hypothesis. Also, none of the other possible reporting effects investigated yielded a satisfactory explanation for this phenomenon.

It has been hypothesized that the well-known phenomenon of a higher frequency of cases between near relatives might be due to the fact that because of the more frequent contacts between near relatives, such cases are more easily detected. For instance, if the percipient dreams that the target person had a slight accident, the correspondence between the dream and the event has a higher probability of becoming noticed when the target person is seen daily than when it concerns a distant friend or relative who is only seen rarely. If this hypothesis is true, it might show up when, for the different types of relationships, the number of cases involving events with a high probability of becoming known (death cases) are compared to cases with a low probability of becoming known (trivial cases). However, it was found that the data do not support this hypothesis. Other possible reporting effects also failed to provide an explanation. Thus, it seems that it is the nature of the relationship and not some other variable which increases the probability of occurrence of a psi experience.

Another finding was that psi experiences most often involve another person and rarely purely material events. Combining all data discussed above and based on a detailed analysis of the distribution of male and female percipients and target persons over the different types of relationships, the general hypothesis was derived that the nature of the relationship influences the likelihood of occurrence of a psi experience

and of the probability of becoming either a percipient or target person. In each relationship there is usually a difference in the strength with which one partner is emotionally dependent on the other. The more dependent partner will have a higher probability of becoming the percipient, while the more "dominant" and less dependent partner will be more likely to figure as the target person. In addition, it will be mainly those events which pose a threat to the relationship which will become the subject of the psi experience. From this hypothesis it can, among other things, be predicted that if no relationship is involved, as in precognition cases related to the percipient, the difference between the number of male and female percipients will disappear. This prediction could be confirmed in subsequent studies.

A few other findings of relevance will be briefly mentioned here. It appeared that psi experiences are rather poor in detail. Contrary to what might be expected, details turned out to be much more associated with non-serious and trivial events than with death cases. No relationship was observed between identification of the target person and number of details, but a very strong relationship appeared between number of details and identification of the event. From this and other analyses it was concluded that the content of the psi experience is mainly of an all-or-none character, in the sense of "X is dead" or "Y has had an accident." It appears as if details are added in the process by which the content becomes conscious. This conclusion is at variance with Tyrrell's model (Tyrrell, 1946, 1953), adopted by L. E. Rhine, which assumes that basically the psi experience involves complete information about the circumstances of the target person and that details and information are lost and distorted when the content of the experience becomes conscious.

If only random events were being labeled "psi" then we should not expect to find systematic trends in the reports of these experiences. However, the data of this study suggest that a common process might have generated all these experiences. If this conclusion is correct, then we might speculate about the nature of such a process. Two different classes of hypotheses can be considered: one based on the assumption of a psi process and the other on the assumption that some psychological mechanism can account for the characteristics of these experiences. For several reasons it seems preferable to start with the latter class of hypotheses. The pragmatic approach focuses on psi experiences rather than on psi as an operating principle. As stated above, if psi as an operating principle is the subject of research, then it is difficult to adopt the principle of falsification. This can be considered a serious weakness of the metaphysical approach. On the other hand, the concept of the

psi experience generated by a psi process can be falsified. It is possible to construct a number of non-psi hypotheses which might explain the observed characteristics of these experiences. Thus, when studying psi experiences we are able to act as true Popperians. We can construct hypotheses of a non-psi nature and then test these hypotheses on the experiences to see if the concept of the psi experience, and with it the concept of psi, survives these tests.

Other advantages of testing non-psi hypotheses are the following. Since psychological or non-psi factors are likely to play a role in any human activity, it seems of importance, even in the case that a psi process exists, to find out to what extent the findings of this study are due to such psychological factors. In other words, the question is which characteristics of these experiences are associated with the psi process itself, and which ones can be ascribed to other, non-psi causes? Another reason is that while hardly anything is known about the properties of a psi process, we do know or we can find out what the properties are of possible psychological mechanisms which might be responsible for these experiences. From these properties predictions can be made about how they should affect psi experiences and it can be studied whether the characteristics of the psi experiences confirm these predictions.

If we assume a specific psychological process to be responsible for these experiences, then the hypothesis should explain why a large number of people single out one or a few events in their lives which they label "psi" in such a way that together these events show the characteristics observed in this study. The first hypothesis investigated which might explain why all these unrelated experiences show specific characteristics might be called the "social climate" or "cultural" hypothesis. This hypothesis is based on the familiar concept of attribution. It assumes that in a given culture people have "occult" beliefs and expect psi experiences to take place in a certain form. Then, when random events in their lives fit that picture they are labeled "psi." In other words, it is not the experience itself which strikes people as being extraordinary and gives rise to the feeling that the experience must be based on psi. It is the other way around. An image exists of what a psi experience should look like and any experience which resembles that image will be labeled "psi." This hypothesis could explain why, assuming that no psi process exists, the various so-called psi experiences still have specific characteristics.

However, it is likely that in different cultures and epochs the popular "occult" beliefs will differ. Hence, if this hypothesis is correct, one should expect to find different characteristics in collections of psi experiences from different countries and epochs. To put this hypothesis

to the test, I analyzed two other collections of spontaneous experiences, the Sannwald collection and the Rhine collection. The Sannwald collection is made up of cases from Germany, mainly from the 30's and 40's of this century (the Nazi and World War II era). The Rhine collection is somewhat more recent and the cases are mainly from the post-war years and from the USA. The analysis of the Sannwald collection involved 789 cases. For the analysis of the Rhine collection a sample of 1630 cases was randomly selected from all cases of the collection. Although all collections investigated are from Western cultures, it can be demonstrated that the popular view of psi experiences which was prevalent in the period that the cases in the *Phantasms* collection were acquired differed greatly from those of the periods in which the other cases were collected.

It turned out that in the Sannwald and the Rhine collections the experiences showed to a large extent the same characteristics as those observed in the *Phantasms* collection. The main discrepancies between the data of the three collections concern the distribution of the types of experiences (dreams, intuitions, hallucinations). The way the content of the psi experience becomes conscious is clearly influenced by cultural factors. Compared to the data of the *Phantasms* collection, the majority of the outcomes of the analyses of the Sannwald and Rhine collections confirm the results. When failures to replicate were observed, in no instance did the data appear to contradict each other, for instance by showing conflicting trends in the relationships studied. Of the discrepancies observed between the collections, most could be accounted for in a meaningful way. For instance, in the Rhine collection a significantly higher proportion of female percipients is observed compared to the *Phantasms* collection. However, it could be demonstrated that in the Rhine collection the number of female percipients had been increased due to a reporting effect. In this collection many more female percipients had contributed cases than male percipients, probably because the cases for the Rhine collection were mainly solicited from a female dominated surrounding. Other failures to replicate were simply due to the fact that a relationship which was investigated in two collections reached a significant level in one collection, but failed to do so in the other.

The studies of the Sannwald and Rhine collection confirmed the observation that psi experiences are characterized by a number of common trends. Thus, the conclusion from the first study, i.e., that a common process seems likely to be responsible for these experiences, is strengthened by this finding. It can also be concluded that these experiences are not likely to be the result of an attribution process as

described above when discussing the "cultural" hypothesis. Only the type of experience can be considered as being not typical of the psi experience, but culturally dependent.

If the "cultural" hypothesis cannot account for these experiences, then our falsification approach requires that a new hypothesis be put to the test. The hypothesis presently under investigation is based on the assumption that, in general, people often tend to worry about their own affairs, about their future and about people in their environment. It could be that they are inclined to attribute a psi character to their worries if these suddenly become true. For instance, it might well be that parents who are worrying daily that their child might have an accident on the way to or from school attribute a psi character to these feelings when the child becomes the victim of an accident.

Based on this hypothesis, a number of tests can be carried out. For instance, if the hypothesis is correct, one should expect that people who tend to worry often report more psi experiences than people who live relatively carefree lives. The subjects about which people worry should be distributed more or less the same way as the topics of the psi experiences observed in the three collections. Females more often report psi experiences, hence it can be expected that females tend to worry more often than males. Since most psi experiences relate to other persons, according to the hypothesis one should expect people to worry more often about other persons than about themselves. This list can be extended considerably. Unfortunately, not much is known about this subject. Hence the investigation requires first that instruments be developed to measure the degree to which people worry about themselves and about others, to find out which people they worry about and about what subjects. This research is presently being carried out.

I hope that the presentation of our investigations so far has clarified how the pragmatic approach leads to new types of research. To sum up, the main advantages of this approach are that it allows us to study observable phenomena whose existence is not in doubt, that it allows the testing of hypothesis and the falsification of hypothesis and that it gives us a better insight into the psychological factors which affect psi experiences. For these reasons the pragmatic approach could serve well as a basis for justifying research in parapsychology.

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DISCUSSION

MORRIS: I am very sympathetic to your general approach and I think it is of special value to the kind of work that we are doing. We have been viewing psi as being amenable to a top-down approach, where you start with a theoretical concept, versus a bottom-up approach where you start with the anomaly. I know that this is similar to the distinction John will draw later on. I recall being at a convention recently where Ian Stevenson was speaking to a non-parapsychological group. Instead of giving a paper on reincarnation events he gave a paper on anomalous memories of children. It was very well received. One of the problems that arises with your approach is getting an adequate description of enough aspects of the case to do the kind of intense analyses that you would like. I have a possible suggestion for you. When I taught parapsychology at the University of California, Santa Barbara, the first third of the course was devoted to what is not psychic, but looks like it. I would then give the students an assignment to write out in as much detail as they could a potential psychic event that had happened to them. I asked them to focus on getting all the details in and then generate as many alternative hypotheses as possible. This gave me well over a thousand such anecdotes from people who were really trying to come up with alternatives. We found a small group of anecdotes that seemed to be consistently hardest to explain, namely those that were like crisis telepathy cases—time-locked responses to remote crises of loved ones. The other categories they could in general explain away pretty well. You might find something like this a valuable research tool for yourself, to work with people under circumstances in which it is to their advantage to describe events thoroughly and generate alternatives.

SCHOUTEN: Yes, I agree with that. The problem with this type of research so far is that I use existing material. I have just accepted the way these cases were sent in. That is, in general, the philosophy in our department, anyhow. When people come in to report experiences, we just accept them at their face value. You have to take people seriously. But what you said is true because the next step is to interview people and to get the cases on the table, with more details than they normally send in because it really imposes a restriction. Actually Holland is not that big a country, so I hold my fire. I have collected cases, of course, but not on the scale that you probably did. I will do that in a later phase, when I am going to try out specific hypotheses. It is my experience that if you ask people to write down cases and you then discuss

them, you get much more than when they send the case in, because they tend to simplify the case when they write in.

As for the other point you raised, I am not sure whether I agree. It is true that if somebody relates a case you can think of many possible explanations and most often they involve estimated probabilities. Suppose somebody has the feeling it is going to rain. That is such a simple thing that even if someone would feel it is a paranormal experience, he won't relate it to you because it is such a common happening. I think in general if people think of possible explanations they rely on these sorts of estimates. I think that I should not take that into account. When people report the events they experience as paranormal then, regardless of what the probability is, I think we should accept them and include them in the studies.

PALMER: I have great sympathy with the approach you are taking. In fact some of your ideas are very closely related to some of the things I am going to be saying tomorrow. I might just make one translation of terms at this point, which might help people integrate this. What you referred to as a psi experience in your paper is very close to what I will call psi in my paper. And what you are referring to as the psi process in your paper is very close to what I am going to refer to as omega in my paper.

I want to focus my comments on a couple of areas where I think we might have a slight difference of view. It strikes me that the distinction you made between the metaphysical approach and the pragmatic approach is very similar to the distinction the philosophers of science make between inductive and deductive reasoning. In the inductive approach you start with the data and make inductions from that. It may become deductive in the sense that after you do that, you then construct theories and hypotheses. But it is primarily an inductive approach, one that is commonly used in psychology and it is the approach I prefer.

The other approach is really the Popperian view, where you start with the concept and the theory and you do not really worry about where it comes from. You make what I call a bold guess, not really referring so much to the data and then using the data to either falsify or confirm the theory. I think the theory or model that you are trying to falsify can either be of a normal or a paranormal type in principle. I do not see anything inherently unfalsifiable about a paranormal construct provided it is imbedded in a theory from which you can derive testable hypotheses. There is a problem, admittedly, with something like the claim that psi exists. But I do not think it has so much to do

with its inherent unfalsifiability as the way it is sometimes used in parapsychology, particularly with regard to the fact that you tend to reify it and then make an existential claim about it. This is, perhaps, where the notion of the metaphysical comes in.

Also I do not think that it is necessary to demonstrate the existence of psi or some principle first before you study it. Again, I think you can hypothesize some kind of paranormal principle and then see if testable hypotheses can be derived from that. You have to formulate it in a certain way. I think it is the way it is formulated that has been the problem.

Finally, this is a somewhat more general point. There is a tendency, I think, in the philosophy of science in particular, to try to find the one best way to do research. This is very obvious with someone like Lakatos for example. And the implicit assumption is that, once we decide what this is, everybody who is addressing that problem should rush in and use that approach. This is often considered to be an optimal strategy. I think it is rarely, if ever, optimal for everybody to take the same approach, particularly in a fairly new area like parapsychology. So I would strongly argue for pluralism, some people addressing normal hypotheses, other people addressing paranormal hypotheses, and everybody following their own inclination. Let us not try to shepherd everybody into one mold. I would hope that some parapsychologists would become interested in following your example and testing normal hypotheses. I think this is a very valuable approach which has been neglected in our field.

SCHOUTEN: Well, in truth I am not entirely agreeing with you. As you said, there is an inductive and deductive approach. However, it is not usual for you to just think up the concepts and start research. For instance, if I just think up the concept that witches fly on brooms, my next step is to carry out research by putting up a big net on top of an apartment building to see whether I can catch witches. Now, I think that is way out of what you normally do. I think there always has to be a reason for a concept. There is this concept, psi, I think, because there are spontaneous paranormal experiences reported throughout the ages. In this case it is vital, I think, to provide the basis for this field to show that we have reasons to accept this concept. That is why this approach is rather basic to what we are doing in parapsychology. It does not mean that we should not do experimental research, certainly not. There is, of course, at present an ample body of evidence that something like psi is going on. I believe that people have jumped too fast from the spontaneous experience and suddenly found that they were involved in lab testing, without looking closely into what their experiences are.

PALMER: Let me just clarify what I said briefly. First of all, when I was describing the deductive approach, I was obviously describing it in the extreme way. Even a Popperian would not just totally pick a hypothesis out of the blue. Secondly, to reiterate what I said before, my personal preference is for the more inductive kind of approach, which is what you are advocating.

BENOR: In healing there are some interesting anecdotes and there are also laboratory data. It is quite confusing because there are findings which appear to be contradictory. Joyce Goodrich did a very well designed study. She found that the further away, temporally, the healer was from the healee, the better the healing appeared to be; whereas most healers report that the closer the connection, the greater the rapport, and the better it seems to go. It would appear that there are many different factors involved in the relationship. We need to be a little looser than we have been in considering factors that might be involved in a relationship. A relationship can involve rapport, familiarity, degree of actual contact, belief systems, or other connecting links to make the psi occur.

SCHOUTEN: I agree entirely with that. These hypotheses derived from my studies are based on several hundreds of cases. The first time I was in Germany they told me that they had run an experiment with a couple of students who scored fantastically well. The funny part of it is that these students did not like each other at all, but were an amazing couple in the sense of an ESP study, hence other factors must be involved. So that what you say is true. Relationships are very varied and different. We know that more cases occur between near relatives and that it is something which cannot be explained by the more frequent contact between near relatives. From a study of the number of cases between different partners in a near relationship, like mother and child, this hypothesis was devised. But it is a hypothesis which is based on a majority of cases. No doubt in individual cases you will find differences.

GROSSO: I am still just a little puzzled about the way you are using the notion of concepts in psi research. It just seems to me that it is inescapable that we begin with a concept. How are you going to pick out what spontaneous cases to study unless you have some kind of a concept to begin with? For instance, you began in your study with *Phantasms of the Living*; that implied somehow that you have a concept of psi. I am in complete sympathy with you in stressing the value of immersing ourselves in the particulars. But it seems inescapable that we begin with some kind of general, theoretical concept.

SCHOUTEN: That is a nice argument. It is true that a study is not started just out of the blue. That is correct, but the interesting thing

about the *Phantasms* study is that, in fact, the authors became interested for an entirely different reason. They started collecting these cases because they felt that if telepathy and thought transference existed, then one should find instances of it in daily life. So far you are right. But what interested me was that, in those days, the people themselves who had the experiences were not aware of a thing like thought transference, not at all. In general, they considered it as spiritistic. Now, I think what happens throughout the ages is that people have certain experiences which they somehow feel are out of the normal. Of course some can be explained in other terms, but the funny thing is that even in those days certain classes of experience came together, were very pointed and are now considered as ESP. But surely those people did not consider them as such.

GROSSO: Well, they may not have had the word ESP, but as you just said they had a sense that some cases were not normal. So the underlying concept was present, none the less.

SCHOUTEN: That is right. But that could be just the feeling that something unexplainable had happened to them. I still feel that what is needed is to prove that these people were right in studying and considering those experiences as instances of ESP.

HEARNE: I applaud your quest for psi in real life situations. I have come to the same sort of belief myself after a lot of research into the extreme artificiality of the laboratory situation. On your point about the overwhelming preponderance of females in these reports, I would like to report that I have come across the same findings and have recently published a study on that point. I analyzed premonitions reported by 88 persons. These were people who responded to a newspaper appeal. Questionnaires were given to a number of people. Now, I found 90 percent of the percipients were females. It seemed not to be a reporting bias because if you ask who else in the family has premonitions (presumably they are reporting reasonably free of bias) they still report a preponderance of females. So I think we must accept that this is a very important consistent finding. Now, there is also a consistent finding that premonitions concern very unpleasant happenings, even death, to someone close. Having the advantage of modern statistical computer techniques, I came across a very curious negative correlation between the age at the first premonition and the number of offspring produced subsequently. This, of course, was looking at women past child-bearing age. These basic findings in conjunction suggest to me that premonitions are an evolutionary development in the human species, providing functional forewarnings of disasters mediated principally through fe-

males and resulting in the replenishment of group numbers as quickly as possible after losses. This would have been useful, say, in primitive societies. But things are very different nowadays and I hypothesized that before a major disaster young female percipients would tend to psi-miss or exhibit defensive behavior.

Just one other point. In your actual paper, I think you mentioned emotionality as an important factor concerning precognition. I also found a highly significant level of neuroticism in the sample, but this did not apply to extroversion/introversion. But this is a two-edged finding. It might point to unreliability in these people and their reports. That must be taken very seriously, of course. Or it could indicate that emotionality in the percipient is essential in order to perceive emotional distress in others. Now, those are findings which seem very much to fit in with what you are saying.

SCHOUTEN: I think that with two different analyses you arrive at the same conclusion.

WEINER: My question follows very much from Dr. Hearne's. First of all though, let me say I enjoy your approach to psi very much. I have always been interested in the work that you have done along these lines.

My question is related to the specific finding regarding the predominance of female percipients. What occurred to me during this discussion is whether this finding might be related to a difference between males and females in the type of information they obtain psychically. Let us suppose that in a psi experience one can pick up only one or two bits of information. As you said before, more details are required in the identification of an event as opposed to the identification of whom the event has occurred to. Now, if it turned out that the psi information that females tend to concentrate on refers to whom the event occurred to, then their experiences will be more easily identified as paranormal and, therefore, reported. Whereas, if males tend to obtain details of the *events*, they might know only one or two details and therefore might not have enough information to feel that they have a case or to feel like reporting it. I am wondering if you have analyzed for any differences between the sexes in these terms?

SCHOUTEN: I did, but the problem is that if that idea is correct, we would not receive any cases from those males. In the cases in the collections I did not find any differences between males and females which, I think, indicates that it is not so much a difference in capacity. If the hypothesis is true that it is a matter of emotionality, of emotional dependency in the relationship, then if there is no relationship the dif-

ference should disappear. Thus, if you take people reporting precognitive experiences about themselves, I found that in the Rhine and Sannwald collections the difference does disappear. In those cases you find about the same number of male and female percipients, which does not support your suggestions. I think that the connection Dr. Hearne mentioned between number of children and age of first premonition sounds as if it has to do with the personality of the percipient. If a woman has the sort of emotional structure that leads her to want to be married and have lots of children, that might be the sort of person who would be inclined to report a spontaneous case. So I can well imagine that those persons more often report cases than other females.