CASE STUDIES, FOLKLORE AND PERSONAL EXPERIENCES OF INVESTIGATORS: THEIR ROLES IN EXPERIMENTAL RESEARCH

REX G. STANFORD

The central concern in this paper is how studies of ostensible spontaneous ESP and PK (anomalous interaction), of the paranormal in folklore, and of unexplained interaction in the laboratory can be mutually enriching.

Alternative Conceptions of the Role and Methodology of Case Studies

Historically, there have been several schools of thought about the role of case studies of anomalous interaction.

Authentication of Case Reports (Proof Orientation). Early psychical researchers often wished to use case reports to provide direct evidence of anomalous interaction. This approach is characterized by discussion in *Phantasms of the Living* (Gurney, Myers, & Podmore, 1886). The method here was to carefully investigate accounts of apparitions of the living in order to ascertain the validity and reliability of the facts as recounted. The hope was that by eliminating normal explanations of the reported events the paranormal explanation could be more clearly indicated. Among the normal explanations that Gurney, in particular, wished to rule out in this publication was that of chance or coincidence, and he, accordingly, supplied some census data on the base rate of hallucinations (including hallucinations of persons known to oneself, such as appear in most crisis apparitions). These data he used in combination with a death-rate statistic in order to establish the chance likelihood of a hallucination occurring either 12 hours before or after a death. He could thus estimate the likelihood of the actual, observed veridical apparition rate by comparing it with that expected by chance. Gurney's argumentation here was criticized by the philosopher C. S. Peirce (as discussed in Gauld, 1968, pp. 173–174), in part on two grounds: (1) that the likelihood of a hallucination of a person known to oneself was probably underestimated because persons tend to forget

hallucinations not coincidental with deaths, but to remember those that are coincidental and (2) that the size of the sample actually studied might have been considerably larger than Gurney had estimated (on grounds we need not consider here) and that this would have resulted in the computation of a misleadingly small probability value.

Following Gurney's untimely death (1888), and partly in response to such criticisms, leading members of the Society for Psychical Research conducted a much larger census of hallucinations than had Gurney—a project he had long favored. The report (H. Sidgwick, Johnson, Myers, Podmore, & E. M. Sidgwick, 1894) included a correction for the selective memory problem just discussed. The investigators also carefully screened the data for factors that might introduce other forms of artifact. (My discussion of this early work follows Gauld, 1968.)

The above examples reflect the belief among many of the early psychical researchers that spontaneous cases, properly studied and evaluated, could constitute important evidence for anomalous interaction. That belief accounted, in large part, for their rigor in documenting and evaluating such cases.

The opinion that spontaneous cases can provide evidence of anomalous interaction continues to have advocates. Ian Stevenson (1970a, 1970b) is one investigator who feels that such cases, properly screened and evaluated, provide evidence on the psi-reality issue, over and above that contained in experimental reports. He, for example, chides parapsychologists for denying such cases the status of evidentiality and thereby making the evidence rest entirely upon the experimental reports (1970a, p. 143). He also appears to feel that such cases have potentially important things to tell us about process-related questions (1970a, p. 143; 1970b, Chapter V). I do not gain the impression that Stevenson wishes to use terms such as "conclusive" or "proof" in connection with case-related evidence, but neither do I gain the impression that he would use them with regard to experimental evidence. He seems to see great importance in such cases as providing evidence on conceptual or process issues that cannot be addressed in the parapsychology laboratory (1970a, p. 148) or that can only be addressed there with great difficulty. He insists, however, that each individual case report must be carefully studied to ascertain the facts before it can be considered as meaningful evidence of paranormal communication or of processes related thereto. He has, therefore, emphasized the importance of ascertaining the authenticity of each report (e.g., Stevenson, Palmer, & Stanford, 1977). By authenticity Stevenson means correspondence between the written report and the events it is claimed to

describe. (Authenticity is, then, necessary, but not sufficient, for a claim that a case report represents a plausible case of anomalous interaction.)

I strongly concur with this emphasis on case studies as providing evidence on process-related questions, but I insist that such evidence always has considerable ambiguity. It cannot, therefore, constitute the basis of final process-related conclusions. They must be developed on the basis of experimentation. I would also note that the very process of filtering out cases that have low evidentiality might bias the data in a way that could conceivably mislead in relation to process-oriented conclusions. For example, it might bias the investigator toward the conclusion that anomalous interaction always favors the development of conscious perceptions and cognitions in the "percipient" (whom I had rather call the "respondent"), that something less than this constitutes degraded or "blocked" information (see below). Such filtering helps to select cases that have the appearance of paranormality, but it may also help guarantee that the cases are atypical indicators of what usually occurs during anomalous interaction.

Spontaneous Cases as Providing Process-Related Hypotheses that Require Experimental Validation (Heuristic Orientation). The interest in spontaneous cases as providing evidence on process was shared by the late Louisa E. Rhine (1981), though she felt that the role of spontaneous case studies was to provide a unique source of hypotheses that could broaden the base of conceptualization and research. She insisted, however, that the suggestions or hypotheses derived from case studies require experimental work if final conclusions are to be drawn. Only experimental work could definitively confirm, disconfirm or correct the ideas developed from case studies (1970, pp. 150–151). She saw spontaneous cases as an important, even essential, supplement to experimental work, but the relationship between the two was seen as serial, not parallel, in terms of providing the basis of final conclusions. Findings in the case-study domain could guide laboratory investigations and even strengthen their conclusions, but they could not stand in their stead or on an equal footing in terms of evidentiality.

Perhaps because Rhine differed from Stevenson on these accounts, she also differed from him on choice of methods. Because Stevenson wishes to support relatively strong conclusions on the basis of case studies, he understandably elects to use the stringent approach to case-study evaluation that his predecessors in early psychical research had employed. Rhine, on the other hand, placed less of a burden upon her evidence because it was regarded as supplying information for the development of hypotheses for experimental testing or as providing supplementary, rather than primary, evidence. It, therefore, could be

gathered without subjecting each individual case to the rigorous validation and analysis process preferred by Stevenson. The pooled cases seeming *prima facie* to represent psi processes could be examined for types of events and patterns of relationships with the intention of developing suggestions about the processes of paranormal communication. Indeed, from this perspective it seemed to Rhine that her method was superior to that of Stevenson because it was less selective. It cast its net wider in order to insure that no type of anomalous interaction was excluded. Rhine did not subscribe to the idea that meaningful cases involving anomalous interaction had, individually, to provide solid evidence of such interaction, so long as they seemed to carry a suggestion of its presence. They could still contribute to pattern analyses of process in anomalous interaction. The requirement of strong authenticity for each case that was advocated in the proof orientation would in Rhine's view serve only to exclude some types of cases that involve psi, but that are not convincing when taken alone. The proof orientation was seen as potentially blinding the investigator to many of the manifestations and facets of psi function—an important point to which discussion will turn later. It was seen as too narrowly selective (Rhine, 1970) and as predicated upon the fatuous belief that such case studies can independently demonstrate the reality of anomalous communication.

Stevenson, however, faulted the Rhinean approach to case studies on the grounds that it had been used for purposes for which it was not originally intended and for which it is unsuited (1970a, p. 145), namely, to draw conclusions about process in spontaneous psi (and, presumably, in psi function generally). For example, Rhine on the basis of her casework reached some very negative and forcefully stated conclusions about the lack of evidence for any active role of the agent in telepathy (e.g., 1981). Stevenson believes that the case selection in this instance was likely biased by the very fact that Rhine did not investigate her cases in the traditional, proof-oriented way—an interesting turning of the tables on Rhine's arguments about selection bias in traditionally evaluated cases. Specifically, he noted that because her cases were probably almost all submitted by percipients, as is typical, it is not surprising that she would know little about the experiences of agents in the cases. Obviously, she would have no contact with agents in the investigative process and no access to their unique perspective. It seems to me that Rhine never adequately addressed this criticism in her response (1970), though Stevenson's specific point here should not be construed as undermining Rhine's potentially important, broader point concerning the great selectivity of the traditional screening process

that is intended to insure the anomalistic or paranormal character of cases. There is no *a priori* reason to think that the underlying process(es) in anomalous interaction function only in situations wherein they would produce clear-cut evidence of their anomalistic character.

On the other hand, the Rhinean approach has the potential liability—broached but not discussed at length by Stevenson—that it might produce definite, but spurious, patterns in the data precisely because the data themselves provide little assurance that unexplained processes are likely to be involved. Rhine thought that even if some cases did not involve genuine psi events, the patterns of interest that she hoped to extract from the case data should emerge unharmed by the pseudo-psi cases. This hope was built around the assumption that, given the large sample studied, the spurious elements introduced by the non-psi cases would cancel each other out (i.e., that they were non-systematic in character), but that the elements introduced by the psi cases would reinforce each other. Thus, real information about psi function could be extracted or, at least, tentative suggestions concerning it could be developed.

The soundness of this assumption has been questioned (e.g., West, 1970) because the use of a large sample guarantees only that any consistencies in the data will be detected, but guarantees nothing about the nature of those consistencies. Data patterns might, for example, reflect what subjects are inclined to believe about anomalous interaction, rather than factors intrinsically related to such interaction. Cross-temporal or cross-cultural studies might help to resolve some of these problems because beliefs are often culturally founded or historically conditioned (see, e.g., Schouten, 1983).

It might be argued in reply that if case-study researchers really use their findings simply as raw material for the development of hypotheses that are subsequently subjected to experimental test, then any effects of cultural-historical conditioning upon the research findings could be minimized or actually eliminated (because we would assuredly be studying psi function). This argument has some validity but, on the other hand, reality might not be quite this simple. Culturally-historically conditioned beliefs might themselves influence actual psi performance or function and this might occur both inside and outside the laboratory. What can be said is that if case-study findings are culturally-historically conditioned but, nonetheless, lead to hypotheses that are confirmed in laboratory psi results, we can, in any event, be sure that these are valid patterns in that they relate to psi function. However, the boundary conditions for the findings would still be unclear. We would still not know whether the psi-related findings are historically or culturally

bound. Once again, cross-cultural work might have value in eliminating such ambiguities.

Let us return, however, to Stevenson's point that important case material—such as that related to the role of the agent—can be missed in the Rhineian approach to case studies because each case is not individually investigated in the tradition of the proof orientation, but is taken at face value, provided it meets very minimal criteria. There is a point here, but, in my judgment, it is a bit misguided. What may be needed is not the authentication- or proof-oriented approach to following up initial cases, but follow-ups designed to answer specific questions in which the investigator has a process-related interest (e.g., the mental activity of the agent as a factor in the percipient's experience or behavior). This process-oriented follow-up can help to eliminate ambiguities in case details that are important to process. For example, Rhine assumed that action being taken by the percipient in a spontaneous case of ostensible ESP automatically means that conviction was present, and she combined action taking and stated conviction as equivalent criteria of conviction. However, some persons may take action precisely in order to find out whether their impression is valid. They may be quite puzzled by their experience, but, also, quite unsure that something paranormal is happening or what is involved. Special queries carefully worded to obviate response biases might help to elucidate these and many other process-relevant matters that arise in case studies. Case investigators need to subject their own assumptions to empirical examination and sometimes follow-up queries can aid in this. [Sometimes, too, answers regarding the validity of those assumptions are available in the data already at hand, but investigators do not always make full use of those data. Haight (1979), by separating out the issues of "taking action" and of "conviction" in her own collection of cases, showed that taking action relates differently to her various case forms—intuitions, realistic dreams and unrealistic dreams—than does the sense of conviction, despite Rhine's having unquestioningly combined these two criteria for analyses of "conviction."]

Quantitative Testing of Competing Hypotheses Using Spontaneous-Case Collections with No A Priori Assumptions that "Psi" is Present (The Pragmatic Approach). What may be the most detailed, quantitative (statistical) and, in many ways, sophisticated of the contemporary case studies are those conducted by Sybo A. Schouten of the Parapsychology Laboratory, University of Utrecht, The Netherlands (1979a, 1979b, 1981, 1982, 1983). Schouten analyzes case patterns to learn whether they better match the implications of one or another hypothesis, whether that hypothesis uses nonparapsychological or parapsychological constructs.

One of his major concerns is whether the patterns in his data support or tend to falsify a specific hypothesis. Ruling out alternative interpretations is an important feature of his methodological approach, which is highly falsificationist in philosophy.

Unlike Rhine, he does not begin by simply assuming that genuinely anomalous interaction is reflected in the data. (In this sense, his approach has a kinship with the proof-oriented one, but it leads to a very different methodology because of its different philosophical underpinnings and objectives.) He considers competing hypotheses (including psi and non-psi hypotheses) for their implications for the data at hand and then examines the data themselves to see how well their patterns match those implications.

Schouten scores the data of each case according to values assigned within 32 categories (e.g., categories concerned with age of target person, percipient action, percipient conviction, etc.), and he attempts to reduce the subjectivity (and inter-judge error) involved in case-material categorizations through adherence to an explicit set of rules for assigning values within categories to each case. This is a significant improvement over Rhine's work wherein scoring criteria were, in some instances, not made clear (Weiner & Haight, 1983, pp. 315–316).

Schouten statistically examines all possible interrelationships in his data, but emphasis is placed upon replication across case collections. He has examined the cases of *Phantasms of the Living* (1979b), the Sannwald collection (1981) and the Rhine collection (1982) in an effort to discover patterns in each and to learn which patterns are and are not robust across such changes in time, sampling methods and culture as they represent. One of the advantages of this approach is that it allows examination of the cultural hypothesis, which asserts that what appears in case collections is a function of culturally conditioned expectations, beliefs, etc. Schouten appropriately decries the tendency of parapsychologists to leave the investigation of psychological, social and cultural influences upon cases (or case reports) in the hands of nonparapsychologists. His approach examines a wide array of hypotheses that might have implications for the data at hand.

Schouten has clearly and eloquently explained the rationale and presuppositions of his approach (e.g., 1983). He recognizes that the only assumption needed to justify his methodology is that the data are not random, that consistent, meaningful patterns of some kind—parapsychological or not—are present. That assumption can be tested by examining for consistent patterns. The approach is intended to assay the applicability of specific *a priori* hypotheses and to provide new hypotheses for later testing. Schouten has found some remarkable and

potentially very important patterns of findings, even across the different samples, and many of his major findings are summarized in a recent paper (1983). The consistency of his findings across the three samples mentioned above caused him to reject the cultural hypothesis, and he reports that the only meaningful variation across samples is in terms of the patterns of ways in which the target-related information emerges in consciousness. A potentially important finding here is that Rhine's unauthenticated cases exhibit the same patterns as the carefully authenticated *Phantasms* cases. This finding, taken in combination with other data discussed by Schouten, tends to validate Rhine's belief that the examination of unauthenticated cases is a meaningful way to learn about ostensible anomalous interaction. It tends to bring into question the Stevensonian claim that the lack of authentication by the investigator in the Rhineian approach is very dangerous and can lead to serious error. One possibility for explaining the convergence of the Rhineian and the proof-oriented outcomes is that Rhine's data involved a type of filtering, not by the investigator, but by the persons reporting the cases. People may wish to be precise and cautious in what they report because they do not wish to appear foolish or be revealed as unreliable if anyone should scrutinize their claims. Indeed, Haight, Kanthamani and Kennedy (1979) found that when persons reporting spontaneous cases on a questionnaire were interviewed by telephone and their cases were on that basis reclassified according to the likelihood that they involved psi, equal proportions "improved" and "deteriorated" in terms of evidentiality. Here, then, is evidence that the quality of a spontaneous case report that has not received first-hand investigation need not always deteriorate upon closer scrutiny.

In any event, the generalizability of Schouten's conclusion that investigator filtering does not produce different patterns in the data than were found by Rhine is further strengthened by the results of a small-sample study on high-school students conducted by Haight (1979). She did preliminary screening for quality of cases beyond that done by Rhine, but, as in Schouten's analyses, the patterns in the data were generally very similar to those in Rhine's essentially unscreened cases. Unfortunately, Haight, unlike Schouten, did not report the use of statistical inference in contrasting patterns across studies. Nevertheless, the above findings and additional ones discussed by Weiner and Haight (1983) support Schouten's conclusion about no real difference being made by stringent case filtering and point toward a possible reason why unscreened reports are not highly misleading in the patterns they reveal to investigators. There is probably no reason to think that there are consistent biases in unscreened cases due to factors such as a desire

on the part of the reporter to make a case look better than it actually is, at least if the findings of Haight, Kanthamani and Kennedy (1979, discussed above) can be generalized beyond the high-school students studied. Conclusions about a lack of effect of case screening or a lack of consistent reporter bias toward initial overstatements of their case should not, however, be generalized unquestioningly beyond the sampling methods used in these studies, the populations of individuals studied, or the types of cases involved.

Schouten's application of his method has proven very productive and it is the best exemplar of researching case collections that we have at present. On the basis of that work, he has also provided alternative conceptual interpretations to those proposed by Rhine for some of the central findings from such cases. He acknowledges that his interpretations go beyond the data at hand—they are intended to have heuristic value—but they seem to me to be simpler and more elegant than those involved in the Rhineian model.

What Constitutes an Acceptable Spontaneous Case of Possible Anomalous Interaction? A New Look at Old Assumptions (The Psi-Mediated Instrumental Response or PMIR Model). Despite Rhine's laudable desire that her case collection be broad enough to encompass all forms of ESP and PK cases and her consequent choice of a methodology that eliminated all screening except a very cursory one, it is obvious that she, like previous case researchers, did not include an entire range of cases that might involve the receptive form of anomalous interaction (traditionally called ESP). These are the cases that involve what might be considered the subtle, nonintentional or "unconscious" use of extrasensory information in the service of needs (Stanford, 1974a). An example from my own case files (Stanford, 1974a) is of an individual forgetting to exit a subway train at the proper station, exiting, instead, at the next one, and thereby being able to meet, entirely unexpectedly, the very persons he had been traveling to see. These cases often have features of the following kinds: someone forgets something or makes a mistake that turns out to have unanticipatedly fortunate consequences; one takes an action at a time—when there were options available as to timing—that results in unexpectedly favorable consequences; one decides to do something that one does not usually do, but this results in some wonderful consequence that could not have been anticipated in advance. All of the above kinds of precipitating behaviors can, alternatively, eventuate in the avoidance of some unfavorable event rather than in the encountering of a favorable one.

Louisa Rhine's lack of consideration of such cases apparently derived from (a) her assumption, based upon the prior history of such inves-

tigation and which she certainly communicated to potential contributors of cases, that receptive anomalous communication involves *consciously* knowing about something without the aid of the known senses (or the ability to have rationally anticipated it); and (b) her requirement that cases to be considered must seem *prima facie* to be outside the realm of coincidence. (The latter requirement is understandable, but we should recognize that there is no intrinsic reason why actual psi function should always result in highly improbable circumstances.)

The traditionally excluded adaptive coincidence cases certainly do not reveal conscious knowledge of any target circumstance to which the individual is responding. Instead, they might involve what I would prefer to call implicit knowledge of it that occasions appropriate (instrumental) response. The excluded cases also do not generally provide convincing evidence that something extraordinary occurred, precisely because they look like odd, favorable coincidences that do not involve the detailed, conscious knowledge of an external circumstance that is usually the basis of ruling out coincidence. However, that is not to say that a substantial number of these cases do not involve very improbable circumstances, for many surely do. It is to say that researchers have wanted to base their intuitive likelihood estimates upon conscious knowledge of a circumstance by a so-called percipient rather than upon the improbability of the respondent's behavior that results in the unanticipated adaptive consequences.

Behind every major case study—and almost all parapsychological discussion anywhere—there has been the assumption that the intrinsic nature of extrasensory response is that of information acquired through non-sensory means that struggles for conscious expression as a perception-like image or cognition concerning the target circumstances. I shall refer to this as the perceptual-cognitive view of extrasensory response. It has, indeed, long been assumed that if extrasensory response exists it must take this form as, really, the only conceivable one. Explicit or conscious knowledge of circumstances has been seen as the objective of extrasensory function—or "extrasensory perception," as the traditional conceptually biased term would have it. This assumption is nowhere more clear than in the writings of Louisa Rhine on spontaneous "ESP" (e.g., 1981). Intuitive cases of ESP are, for example, seen as instances in which the extrasensory information is obviously "blocked" from arriving at its conscious destination, such that an individual gets only an urge to act and/or a feeling about something, often not even knowing who it involves and rarely knowing anything about the circumstances involved. She writes at length about the struggle of extrasensory information to enter consciousness and how it is

often blocked from full access to consciousness while persons are in the waking state.

What is implied in all this is that extrasensory functioning is organized such that the implicit objective is *always* to communicate to consciousness information about the target situation. The reason I have long been interested in the neglected cases discussed above is that the aforesaid implication of the traditional perceptual-cognitive assumption is an unreasonable and arbitrary one from the perspective of evolution, adaptation and the survival of the organism. Presumably, the advocates of the traditional position would admit that anomalous receptive communication subserves the adaptation of the organism. What seems unreasonable in the traditional position is the assumption that psi-mediated access to sensorially unknown information will have maximally adaptive effects only when information about the target circumstance to which one must respond enters full conscious awareness. (It also seems, generally, to be assumed that that target circumstance tends to enter consciousness in rather full, rich detail that, however, may be degraded by the vagaries of the percipient's psychology at the time.) Contrary to that assumption, it seems obvious that maximally adaptive response to an extrasensorially apprehended circumstance will often consist simply of a particular instrumental response, one that allows one to do what is important in relation to that circumstance. Sometimes that response is one of simple avoidance of it because of its threat or of contact with it because that would have positive consequences. If, as discussed in detail elsewhere (Stanford, 1974a), appropriate ready responses within the organism can be primed and released (through the psi function), the organism can make adaptive response without the target information having to be communicated to consciousness and being processed there.

Indeed, for many adaptive purposes the route through conscious awareness would be terribly inefficient, absolutely unnecessary and disruptive. It could even block adaptive response, as when the individual does not know what to make of the information in consciousness or what to do about it. I will not elaborate on these matters here, for they have been discussed in earlier publications (Stanford, 1974a, 1977a, 1982). I will, however, note that adaptive response to target circumstances may require conscious knowledge of even the basics of those circumstances only on special occasions and detailed knowledge of them more rarely yet. [Interestingly, in this respect, Schouten (1983) concludes—even on the basis of work with traditional cases—that the actual extrasensory information consciously processed is very minimal and considerably less than has often been believed to be the case.] One of

the circumstances that actually requires conscious knowledge may be the blockage of simpler forms of adaptive psi-mediated instrumental response. When simpler, more efficient adaptation to important extrasensorially accessed circumstances is blocked by circumstances such as the preoccupations of the organism (see Stanford, 1974a, 1977a, and 1982 for detailed discussion), then adaptation may require more conscious processing of target relevant information. Under those circumstances conscious processing of the psi-mediated information may be more likely to occur. (There may also be types of target circumstances for which adaptive response requires some conscious processing of the target information; for example, the death of a loved one may favor conscious access to that information precisely because only in this way can there be adaptive emotional preparation for a real-world encounter with that fact.)

In summary, the traditional perceptual-cognitive view of anomalous interaction is not a highly reasonable one from the perspective of such interaction fulfilling a minimally disruptive, adaptive function for the organism, and it certainly does not accord with the possibility that this function developed in organisms at an evolutionary stage in which consciousness was presumably rudimentary.

In this light, ignoring cases of interesting, adaptationally relevant "coincidences" is short-sighted and reflects a conceptual bias that was probably inevitable, given the history of the field. That is, however, not a good reason for perpetuating what may have been a very serious mistake that impeded scientific understanding of anomalous interaction. The major mistake, of course, consisted in unquestioned allegiance to what was an implicit assumption. Minimally, what is required now is that individuals advocating the traditional perceptual-cognitive view of psi function and those who advocate another be fully aware of the assumptions they are making and that they subject them to both empirical and conceptual examination. Empirical examination of such assumptions can be pursued both through case studies and experimentation, though the latter is definitely required for final conclusions.

The PMIR model that I developed on the basis of (a) the "renegade" cases earlier rejected, (b) findings in traditional psi research and (c) considerations from ordinary psychology consists of a series of very explicit, experimentally testable assumptions about circumstances both favoring and deterring adaptive psi-mediated response. I will not detail the assumptions of the PMIR model here (see, e.g., Stanford, 1982), but I would note that any theory proposing an adaptive and subtle, but potentially very powerful, role for psi (ESP and PK) function must also propose specific circumstances that limit the effectiveness of such

response or that block it altogether. This is necessary because, otherwise, the theory would affront the fact that we have misfortunes in life. In the PMIR model there are a number of specific such assumptions about boundary conditions for psi function. There are even assumptions about circumstances that can cause psi to function in ways that are normatively regarded as maladaptive. The model has also been extended to psychokinetic function (Stanford, 1974b).

While the PMIR model is an intriguing departure from the traditional perceptual-cognitive view, it should be obvious that its credibility must depend upon more than the fact that it consists of a plausible theoretical alternative and a highly specific, testable set of assumptions. Experimental testing of its assumptions is required to know (a) whether the heretofore excluded cases of adaptive coincidence that are subsumed by the theory can realistically be regarded as sometimes reflecting anomalous interaction and (b) whether the specific assumptions of the theory represent valid assessments of functional characteristics of anomalous interaction generally.

The PMIR model has generated considerable experimental research and, additionally, there are numerous studies in the literature that might not have been inspired by the model, but that have relevance to its specific assumptions. Stanford (1977a) reviewed the available relevant research, but there has been considerable subsequent work that is relevant. The latter is still awaiting systematic review, which I hope to undertake in an upcoming chapter for Volume 6, *Advances in Parapsychological Research* (S. Krippner, Editor). Experimental research in the PMIR domain has already provided considerable support for the underlying assumption of the model that adaptive psi-mediated function can occur without the subject having to develop target-relevant cognitions. It thus suggests that the odd, favorable coincidences of daily life are sometimes mediated by anomalous communication. Very important, there is already experimental support for some of the assumptions of the PMIR model concerning how and under what circumstances PMIR occurs in life situations. However, much more experimental work is needed on the assumptions of the model, and some have not been directly addressed by experimentation. The experimental support for the PMIR model to date is exciting because it has much relevance to understanding psi-mediated response outside the laboratory as well as within it. Each of the assumptions of the PMIR model has clear relevance to understanding anomalous interaction in life settings, and each can be tested experimentally because the model is highly specific and has concrete implications. This is probably why the model has attracted much interest and has received considerable experimental

attention. The combination of explicit, testable hypotheses and obvious relevance to life situations is something new in parapsychology, but it is something that may be required for case studies to influence experimental work and for experimental work to have relevance for case studies.

One hopes that when specific assumptions of the PMIR model have been subjected to experimental test and have been supported or modified, these insights can, in turn, guide further conceptual and empirical analyses of cases or even the types of cases collected for study and the questions asked of persons who contribute them. In turn, data gathered from these experimentally enriched case studies can suggest new hypotheses for experimental testing, and the process can go forward. Here is the possibility for a genuinely synergistic relationship between case studies and experimentation, each enriching the other and both contributing meaningfully to the understanding of the relevant processes. In truth, the only way to gain a sound understanding of spontaneous-case events is to undertake experimentation that examines concepts derived from them. This is precisely what Louisa Rhine originally had in mind and it is what has already occurred in the PMIR domain. Nonetheless, the success of the PMIR effort in generating substantial, systematic experimental research is unique. This has not happened in any appreciable way on the basis of analyses of traditional cases by Rhine or anyone else. This raises an important question to which we now turn.

How Can Case Studies Be Made More Useful for Experimentalists' Purposes?

The systematic study of spontaneous cases has so far failed to generate systematic experimentation. It has, indeed, rarely spawned any experimentation at all. The PMIR model has inspired considerable systematic work, but the model did not derive from the systematic study of traditional spontaneous cases—or even from the *systematic* study of nontraditional ones. It occurred to me that both traditional and nontraditional cases could be understood by considering psi function in light of considerations from biological evolution and behavioral science. I would suggest that one reason for the failure of traditional case studies to have eventuated in hypotheses that inspired systematic research is that case investigators have not usually examined their cases in light of any form of broader conceptual integration—from within or from outside of parapsychology. In short, one of the problems might have been a continuation of the rather heuristically sterile bare-bones em-

piricism that tended to characterize the thinking of both J. B. and Louisa Rhine. It is rarely just findings that inspire research, but the ideas within which they can be framed!

Be that as it may, the authors of case-study reports have not discussed their findings in ways that would seem to point clearly toward any experimental work. Since many experimentalists seem disinclined toward examining case-study reports to aid them in developing experimentally testable hypotheses, case-study authors might do well to spell out in their reports the experimentally testable propositions that they feel are inherent in their data. Part of the problem might be in the difficulty experimentalists must have in translating case-study findings into concepts, hypotheses and, ultimately, predictions that invite laboratory investigation. How, for example, do case-study findings on taking action by the percipient or the reports of a sense of conviction in spontaneous cases translate into concerns of the laboratory researcher? These are not easy questions and it now seems important that case-report workers who wish to inspire experimental research come to the aid of experimentalists in these regards. Not all experimentalists are theoreticians and translating findings from a nonexperimental area into concepts and hypotheses with implications for experimentation cannot be expected to come easily. The problem is compounded by the fact that case-study researchers are also not always theoreticians. Theoretical constructs are, however, precisely what is needed to bridge the gap from one area of empirical investigation to another (see below).

If the laboratory is to be a testing ground for "hypotheses" developed from case studies, then considerable care must be exercised to actually develop true hypotheses. The hypothesis to be thus tested must, if it is to be serviceable, consist of or be based upon a proposed tentative explanation of a case-study finding or of a set of such findings. (A hypothesis is, after all, a tentative explanation of an empirical finding.) This hypothesis must be much more than just the attempt to generalize an empirical finding to a laboratory situation, for in the absence of a tentative explanation for the case-study finding, it is impossible to know whether or not the finding will generalize to the laboratory or in what form or circumstance. In short, the investigator must not simply try to generalize a finding from case studies to the laboratory, but must create a true tentative explanation of that finding and then deduce predictions from it that can be tested in the laboratory. Only a "hypothesis" that is a tentative explanation of a case-study finding can indicate under what circumstances an effect should be observed in the laboratory. Only such a hypothesis can indicate what laboratory cir-

cumstances will be comparable to the nonlaboratory settings in which the finding was originally observed. In the absence of a tentative explanation of a case-study finding, efforts to "generalize" that finding to the laboratory are naive. Indeed, once a true hypothesis of this kind is created, the laboratory researcher can move beyond merely trying to generalize the finding to the laboratory and can explore its implications there. Those implications derive from the tentative explanation as applied to particular laboratory situations. It might well be that the absence of such attempted hypotheses on the part of case-study authors (or experimentalists interested in their work) has been what has deterred case-study-based laboratory work.

To make this discussion more concrete, let us consider the case of the researcher who wishes to do an experiment based upon the finding that *persons* are more likely to be the targets of ostensible spontaneous cases than are *material objects* (Schouten, 1983, p. 331). Let us assume that the case-study finding represents a clue to process in actual spontaneous-case ESP, that it is not merely an artifact unrelated to anomalous communication. What kind of experiment should be used to follow up this finding would depend upon how the case-study finding is interpreted. If one's hypothesis (tentative explanation) for the case-study finding is that persons are simply built by evolution to be particularly sensitive to cues about other persons (perhaps because other persons are active sources of both rewards and threats), whereas sensitivity to cues about material objects is less important (perhaps because such objects are more passive in character), then the finding can be pretty directly generalized to any free-response setting with the expectation of better success with human targets than with those representing material objects. This is because the organism is presumed to be built by evolution to respond more sensitively to information concerning others. If, on the other hand, one merely assumes that the case-study finding is a manifestation of the greater importance for the percipient of particular persons and their fates, the hypothesis is very different. Its test would likewise be very different. In the case of this last hypothesis there are various predictions that could be generated, and a number of them would be divergent from those deriving from the hypothesis of greater evolutionary sensitivity. A good study of either hypothesis should involve more than just an attempt to obtain confirmatory findings. It should ideally include circumstances that would lead to divergent predictions from the two competing hypotheses, so that the powerful strategy of falsification and the weaker strategy of confirmation could stand the chance of working together to reduce conceptual uncertainty and to suggest conceptual resolution.

A final caveat is in order. Case study researchers sometimes rather strongly espouse interpretations of their data that are minimally, if at all, supported by the data. (Of course, this happens with experimentalists, too, but I think the temptations may be even greater in case work because of the ambiguities intrinsic to it.) Sometimes the favored hypotheses are claimed to have support simply because certain rather feeble alternative hypotheses have been dismissed (and sometimes with less than adequate testing). What would appear to be equally plausible alternative hypotheses to those favored by the investigator are simply dismissed or ignored. If there is a single general shortcoming in the theoretical thinking underlying most case studies, it is to pay too little attention to viable alternative parapsychological hypotheses than those preferred by the author(s) even while considerable discussion is sometimes justifiably given to non-parapsychological hypotheses. (This is not to say that no attention is given to alternative psi-related hypotheses; it is only to say that they are often given too little consideration.) The moral of this story is that experimentalists wishing to test hypotheses built around case studies already in the literature would do well to consider, in searching for possibilities, not only the conclusions and interpretations favored by the case-study authors, but to go back to the authors' data and methods in order to examine the soundness of their conclusions and to unearth alternative hypotheses that might have been prematurely dismissed. In an area that supplies as little in the way of unambiguous data as do case studies it is tempting to find ways to dismiss alternative interpretations even when they should not be dismissed. The very uncertainty present in the data invites premature closure. Readers of case-study reports need to be continually aware of this frequent problem. In my opinion, two examples in which case-study authors have frequently opted for an unsupported interpretation and prematurely dismissed very viable alternatives are in trying to understand the finding that, across several different studies, women far more frequently report spontaneous cases than do men and in tending to dismiss the hypothesis of active-agent telepathy on inadequate grounds by imagining that an active-percipient model can comfortably account for almost everything.

Inputs from case studies might become more valuable to experimentalists than they have in the past if the basis of such studies were broadened to include the traditionally excluded adaptive coincidence cases subsumed by the PMIR model. Such cases would have to be solicited because few, if any, persons spontaneously report them to laboratories. This is probably because of at least two factors: (1) The public may have come to share investigators' widespread assumption

that extrasensory response is intrinsically perceptual-cognitive in nature, and they may therefore believe that these cases do not represent extrasensory response. (2) They may not share that assumption, but feel that investigators will dismiss such cases as mere coincidences. The best way to gather useful, relevant case material of this kind might be to solicit volunteers and then to circulate questionnaires to them that cover a wide range of events of interest to parapsychologists, including both adaptive coincidence events and more traditional events. Opportunity should be provided for giving detailed descriptions of events. It would be useful to learn about the range and frequency of various experiences within individuals. This could aid the development of process-relevant hypotheses and might aid in understanding whether personal styles influence experience types and, if so, in what manner this occurs. Such surveys might also profit by the inclusion of various psychological inventories or scales. Exactly what would be involved would depend upon the specific purpose of the survey.

What is being suggested here is that case studies might become more serviceable to experimental researchers if surveys were tailored to addressing particular questions of interest to experimentalists and to creators of experimentally testable models or theories.

The Role of Other Nonlaboratory Inputs in Laboratory Work

The directions of laboratory experimentation have been affected by nonlaboratory inputs of other kinds than systematic case studies and this has happened in other areas than just research inspired by the concept of PMIR. Examples are found in much of the work on internal attention states and ESP performance, including studies involving ganzfeld, relaxation procedures, hypnosis and nocturnal dreaming as a setting for telepathy. Folklore or personal experiences of investigators, rather than the results of systematic case studies, have often served as the impetus for the work in such areas. It should be noted, though, that here the work has been largely that of exploring possible settings for successful ESP performance rather than that of testing hypotheses concerning the reasons for the success in such settings (Stanford, 1987).

As a specific example, consider the dramatic and important work on telepathy during stage-REM sleep spearheaded by Montague Ullman, a psychiatrist, working in collaboration with Stanley Krippner. (For a review of dream-ESP work see Child, 1985.) This work seems to have been inspired largely by clinicians' observations of apparent patient-therapist telepathy occurring in dreams discussed during therapy. Ullman's own experiences as a therapist apparently played a role here

(Ullman & Krippner, 1970, Chapter 1). [See, also, the nontechnical book on this work (Ullman, Krippner, & Vaughan, 1973) wherein there is only a brief mention of Louisa Rhine's systematic case studies and no mention of her discovery that dream case reports tend to contain exceptional detail of the target circumstances as contrasted with most waking experiences (Rhine, 1962); indeed, Ullman's dream-telepathy work was underway in a very preliminary way before the Rhine publication just cited.] This is not to deny that the interest in telepathic dreams that derived from clinical experience was not supported in some degree by knowledge of the case studies involving ostensible dream ESP. Nevertheless, when the case material that emerged from therapy sessions was subjected to clinical analysis, it provided exciting suggestions of dream telepathy driven by dynamics that were often interpersonal and involved the therapist. Here was ostensible extrasensory communication that became evident during therapy, that was expressed in patients' (or, sometimes, therapists') dreams, that had apparent relevance to the events of therapy (including interpersonal ones) and that left vivid impressions upon the therapists because of its conceptual and pragmatic implications for both the individual case and therapy in general.

Systematic studies of large bodies of traditional spontaneous case data could hardly have provided this kind of incentive for the difficult systematic experimentation concerning dream telepathy. I must respectfully disagree with Haight's suggestion (1979, p. 180) that traditional case collections provided major impetus for the experimental study of dream *telepathy*. A reading of the original monograph in this area (Ullman & Krippner, 1970) gives a different impression. There is little question that as the dream-telepathy program developed historically it was inspired predominantly by the many observations of ostensible patient-therapist dream telepathy and by the intriguing and potentially important patterns of its apparent interpersonal dynamics. These were undoubtedly what grabbed and held the interest and motivated this difficult work, even if that interest received some support from historical case collections. Note, however, that even had the systematic case studies been the primary inspiration for the dream-telepathy work, this would have been a case of their having inspired the setting of laboratory research, not of their having provided specific, process-oriented hypotheses to be tested.

It seems clear, however, that Louisa Rhine's systematic case studies did play a significant role in inspiring the laboratory study of dream *precognition* (Krippner, Ullman, & Honorton, 1971; Krippner, Honorton, & Ullman, 1972), because her finding (1954) that the dream

cases predominantly involved precognition, rather than telepathy or clairvoyance, is mentioned by these authors in discussing the rationale for that work. It is also clear that the finding of a seemingly talented ESP subject, Malcolm Bessent, who had had many ostensibly precognitive spontaneous experiences, played a major role in the decision to do a precognition study.

As regards ESP testing in the hypnosis and ganzfeld settings, it appears that systematic case work played, essentially, no role. Neither circumstance is one that we usually think of as a naturally occurring one and little has been made in case-collection studies of borderline cases between wakefulness and sleep, a state sometimes occurring during ganzfeld. Instead, ESP work with hypnosis and ganzfeld was undoubtedly inspired by folklore and by ethnological observations of shamans, seers and prophets who often work in apparently altered states of consciousness. In the case of hypnosis, influence came also from accounts of the experiences of Mesmerists and early hypnotists (Dingwall, 1967–1968).

One important example of the influence of folklore on parapsychological research planning is found in the work of Robert Morris and his students. They have examined popular psychic development literature for testable hints about circumstances and regimens favoring successful psi-task performance (Morris, 1977). Popular writings about conditions and regimens believed to favor ESP or PK allow relatively easy translation into experimentally testable propositions and they therefore foster actual laboratory work. Later I will briefly discuss an example of laboratory PK research that came out of this search of the popular literature (Morris, Nanko, & Phillips, 1982).

As a further example of the influence of folklore, Honorton, who is a central figure in the ganzfeld-ESP work, notes that his initial ganzfeld work was influenced in part by the ancient yoga aphorisms of Patanjali, as was his conceptualization of the role of psychophysical noise reduction in psi-conducive states (Honorton, 1981). Patanjali's ancient, but remarkably psychologistic and conceptually conservative, treatise on yoga contains ideas that can quite straightforwardly be translated into hypotheses about psi-conducive internal states. Honorton was able to make use of the yoga aphorisms (and related commentary) in building a research program precisely because Patanjali had written about these matters in a way that could easily translate into scientifically tractable concepts and testable hypotheses.

The influence of the folklore of yoga and the related writings of Patanjali upon Honorton's conceptualization and research on psi-conducive internal attention states again illustrates the principle discussed

earlier that sources of information that influence the experimentalist (or the theoretician wishing to inspire research) must provide easy translation into some fairly straightforward propositions that can be put to experimental test.

It is not surprising on still another account that personal experiences of potential investigators, vivid anecdotes and colorful folklore would influence even experimentalists more strongly than would outcomes from systematic case studies that are left in a relatively abstract form (see above) that does not immediately suggest testable hypotheses. The former are presumably more dramatic and memorable than abstractions. We are unlikely to think about or act upon things not remembered!

Broadening the Bases of Hypothesis Building and Conceptualization

Earlier discussion showed that researchers' personal experiences, folklore and, occasionally, systematic case studies have influenced experimental research. They have sometimes influenced the process-oriented hypotheses that have been tested, but more often they have influenced the settings that laboratory investigators have provided in the hope of eliciting psi performance from their subjects. Stated in more general terms, they have mainly influenced ideas about the psychological circumstances in which anomalous interaction is likely to occur. Researchers might also profit by allowing them to influence in a tentative, testable, way their thinking about the underlying nature of the events studied. Of course, such ideas would require experimental testing.

In accord with the idea of letting nonlaboratory inputs influence the underlying concepts that we plan to test, let us turn now to some folklore of the allegedly paranormal that might have relevance for parapsychological theorization. We shall see that it suggests concepts that, if valid, would have considerable importance for understanding the events that interest parapsychologists.

Faith. Mainstream parapsychologists sometimes seem reluctant to give serious attention to concepts that appear in traditional religions. It is almost as if such concepts are beneath notice because they are seen as reeking with superstition and supernaturalism.

Closer examination of ideas from religious sources might enrich the theoretical armamentarium of parapsychology by providing very useful, testable ideas even if those ideas may sometimes require translation into a more scientific framework. Indeed, such nonlaboratory sources might provide unique suggestions that would not arise from laboratory

investigations because of the limited scope of the latter. Some of these ideas from folklore are neither empirically intractable nor wedded indissolubly to a supernaturalistic world view. One such construct from religious tradition and folklore that may challenge contemporary theorization is that of faith.

This term has several meanings in religious and theological discussions. I shall focus here upon a meaning that has supposed relevance to the occurrence of paranormal events. In New Testament literature faith is defined and discussed at length in the eleventh chapter of the letter to the Hebrews. I will paraphrase, rather than quote, the defining statements and will do so in a way that I hope will provide clarification of the concept in light of examples supplied later in that letter. The author of the letter says, in effect, that faith is the internal sense of realness of things that are not evident through the senses; it is what makes real things for which one has no sensory (or logical) evidence. It is the kind of realness or sense of reality of things unseen or unsensed that causes or allows the individual to take action with full conviction that the assumed basis of the action is valid. This definition eschews theological concepts and might be applied outside strictly religious contexts—even though the author of the letter is obviously using it to refer to a sense of reality of things said or promised by God that allows or causes persons to take decisive action and even to sacrifice their lives.

The central concept here is clearly one of conviction, an inner certainty in the absence of any concrete evidence, and a readiness to act upon that conviction, despite appearances. The letter's author writes of faith as wholly within the context of action, a very Jewish idea that is restated and reiterated in the New Testament letter of James, which has been regarded by certain scholars as too Jewish to be Christian, if such irony is possible (see introductory remarks to this letter in the Oxford Study Edition of the *New English Bible with the Apocrypha*, Oxford University Press, 1976). In the letter of James, action is said to be the test of faith (Chapter 2, verses 17–18).

Faith, as it is viewed in the New Testament, is evident in action upon an inner conviction in the absence of objective evidence supporting that conviction (and, sometimes, in the presence of seemingly contradictory evidence). In a sense, faith is said both to make that action possible and to be demonstrated by such action. Faith in the absence of the action is, however, seen as a meaningless abstraction. Faith is, then, shown in the commitment one makes through action. In Biblical illustrations of faith, action is the very way in which the outer evidence of the inner conviction is realized or, sometimes, encountered, as in

examples given in the eleventh chapter of the letter to the Hebrews. Faith in Biblical terms is decidedly not just a means of bringing about miracles; it is seen as a condition for realizing anything at all in a relationship with God (Hebrews 11:6). The scriptural reference just cited also shows that faith is seen as intimately linked with trust—a belief in the goodness of character or dependability of someone or something—a combination that helps inspire action. (In Biblical settings this trust is always trust in the nature of God. Believers must both believe in God's reality and trust in his character.) A fuller discussion of this sense of faith and trust as it relates more broadly to parapsychology and to outcomes of PK research can be found in earlier papers (Stanford, 1974b, 1977b).

In circumstances of alleged miracles described in scripture (both Old and New Testaments) faith is often seen as a bold, action-based assertion of the inner sense of reality of something desired or believed to be accessible through God. An example is found in Acts 9:36–42. Peter, as described in the account of raising Tabitha from the dead, first prays and then turns toward the body and just simply tells her to get up. Likewise, Jesus in raising Lazarus (John 11:1–44) boldly ordered people to remove the stone in front of the tomb and shouted for Lazarus to come forth. The account gives no sense at all of a "Let's pray and see what happens" mentality. There is, instead, a feeling of authoritative action. There is a sense in such accounts that the very boldness involved in acting upon faith is, somehow, itself very central to the realization of miraculous outcomes. It is as though the boldness of faith, which involves some form of action, is important in bringing about events that would not otherwise occur, including paranormal events. It seems a bit like leaping into an abyss with the assurance that a friend will see to it that there is a net below! There is, indeed, a sense that the very act of doing so is what assures that the reality will be there, that the act of leaping in belief and trust insures that the net will be there.

A New Testament story that seems to exemplify this perspective is reported in Luke 17:11–19. Jesus while on a trip encountered ten lepers who asked him to have pity on them—in other words, to heal them. His reply was very interesting, for he told them to go and show themselves to the priests (perhaps so that they could be certified or officially recognized as cleansed of leprosy?). Then the account is very explicit that while they were going on their way in obedience to his authoritative command, they were healed. Healing is thus intimately linked to action on faith in the face of contrary evidence.

Even if we do not place any credence in the historical truth or accuracy of such accounts, we can perhaps accept that they reflect genuine

folk beliefs about paranormal events and that they might, therefore, also reflect conditions favoring paranormal events. Folklore often contains considerable wisdom, even if it is sometimes wrapped up in mythological ideas, and it has even pointed us toward drugs commonly used in modern medicine.

What might be the connection between acting on faith and the psi-mediated realization of some objective, assuming there is some validity in such folklore? Two hypotheses immediately suggest themselves.

If we assume that conviction is at the heart of the matter—as is suggested in a remark attributed to Jesus (Mark 11:22–24) to the effect that when one gives a command with no inward doubts about its being efficacious, but believes that it is happening, it will in truth happen—then whatever will increase that belief or conviction should aid the process. Social psychologists have demonstrated repeatedly that if persons act contrary to what they believe to be true, they will tend to adjust their beliefs or attitudes to accord with their action, provided that they are acting with a sense of responsibility for their actions and do not feel too externally forced (for theory, see Festinger, 1957; for an early study, Festinger & Carlsmith, 1959). If an undesirable circumstance exists, but someone prays and believes that there is the possibility of its being changed, action in accord with the idea of its being changed (even in the face of contrary evidence) may actually strengthen the faith that it has changed. The strengthened belief might, in turn, favor paranormal change in the direction of the belief. I have read statements by Christians who believe in the efficacy of prayer who urge those who pray not only to believe that they have received the object of their request, but to act as though they have received it (e.g., to stop spending time worrying and even to lay plans in accord with a belief that the prayer has been answered). But if there is paranormal efficacy in faith-related action, the explanation of such efficacy in terms of faith enhancement is not the only possible explanation.

Another possible explanation is based upon the concept that the incentive value of the paranormally-mediated event can be enhanced through faith-related action. (The term *incentive value* refers to the degree to which a given event can be satisfying to the organism who stands to benefit by it. The incentive value of an event is therefore a function both of the strength of the need and of the capacity of that event to satisfy that need. An event has the greatest incentive value when a need is great that can be satisfied by that particular event.) If the probability of occurrence of a paranormal event is positively related to the incentive value of that event, as the PMIR model suggests (Stanford, 1974a), then the paranormally mediated event should be more

likely as a result of the commitment created by faith-related action. This is because the incentive value of that event is presumably enhanced by faith-related action commitments. When one has engaged in actions that assume the event has occurred or is occurring, the event itself becomes more important or meaningful. This is my own preferred explanation for the folkloric belief that faith-related action is efficacious, but it is not antithetical to the faith-enhancement explanation discussed earlier. Both could work together. In any event, action that is not backed up by some degree of conviction should not be efficacious, according to the folklore of faith.

If any credence is given to the folklore of the paranormal, the faith concept is worthy of serious attention on several accounts: (1) It is very widespread and central in folklore of the paranormal; (2) there are laboratory PK findings that suggest that the concept has usefulness (see below); and (3) the concept itself hints at inadequacy in at least some contemporary theorization because several theories, in my judgment, do not seem easily able to encompass effects that might be related to faith (although there is not the opportunity here to explain why that is the case). Fortunately, the so-called observational theories are at last being expanded such that psychologistic considerations play a role (e.g., Millar, 1986; von Lucadou, 1987), but I am not yet sure that they are ready to encompass the seeming implications of the faith construct.

The discussion above concerns the possible role of faith in what is traditionally called PK. Discussion of the possible relevance of the faith concept to extrasensory response must await another occasion.

There is a considerable literature on laboratory PK that has relevance to the topic of faith, that suggests that the construct has potential value within parapsychology, and that may suggest the need for incorporating related constructs into parapsychological theorization. (For a comprehensive, albeit now somewhat dated, review of PK research, including work with relevance to the faith concept, see Stanford, 1977b.) More recently, the research of Morris and colleagues on the relative efficacy of non-striving (as contrasted with striving) sets and goal-directed (as contrasted with process-oriented) imagery in PK tasks may have relevance here (Debes & Morris, 1982; Morris, Nanko, & Phillips, 1982). Faith is certainly a mind-set that would favor both of the factors indicated as being effective in the studies just cited, namely a non-striving attitude and a clear mental picture of the expected end result. (Faith would certainly not favor a sense of striving or a desire to effect the end result through process-oriented means!) If the folklore of faith is correct, it would not be the goal-relevant imagery *per se* that is important, but the sense of realness of the desired goal that is favored by

such imagery. That is a matter that has not, to my knowledge, been properly explored in the experimental PK literature.

In sum, there are sufficient indications from both folklore and from laboratory PK research to suggest that the concept of faith, pretty much as it is conceived in religious folklore, should be given more serious consideration by parapsychological theorists and should be the topic of further laboratory investigation.

Expressiveness in Psi Function. There are allegedly paranormal events reported from outside the laboratory context that would seem to challenge in yet another way the contemporary conceptualizations of psi function. Some investigators of poltergeists (Roll, 1972, Chapter 13) regard the allegedly paranormal events of such cases as expressing the inner state of the personality of the poltergeist agent, for example, his or her anger and frustration. Poltergeist events are, however, not the only allegedly parapsychological events of an unplanned, unsolicited, nonintentional nature that might exhibit this characteristic of expressiveness. (I say "might" because this interpretation of the psychology of poltergeist agents is controversial within parapsychology; see, e.g., Alvarado, 1984).

The reader of literature on the physical phenomena reported to be associated, at times, with mystics can hardly fail to notice that many of the puzzling events appear to have a highly expressive character. [For an excellent introduction to this topic one can do no better than to examine *The Physical Phenomena of Mysticism* by Herbert Thurston, S.J. (1951).] This expressive character is true both of supposed events presently lacking a cogent scientific explanation (e.g., levitations) and of those potentially having one (e.g., stigmata and "tokens of espousal" that include a flesh-like ring appearing on a finger of a person who has experienced mystical union with Christ). Here I will consider only the case of alleged levitation. There is no opportunity here to discuss the evidential status of such cases, though Thurston (1951) considers it at length. The evidence seems at least sufficiently interesting to warrant preliminary consideration of its potential theoretical meaning for parapsychology. (In my view, preliminary theorization is meant to suggest hypotheses for future empirical examination. It can, therefore, legitimately examine facets of alleged psi experience that are not demonstrated with great rigor.)

It is worth elaborating here in what sense levitation among mystics generally seems to be expressive in character.

First, expressiveness is often essentially an involuntary or automatic function, as when facial pallor reflects inner fear. It is clear that levitation among mystics is generally not deliberately initiated by the lev-

itator. (This is true of the best documented cases, even if there are undocumented claims that persons have sometimes apparently levitated for practical purposes, e.g., levitation by a yogi in order to cross a river.) An example of the involuntary—indeed, sometimes, countervolitional—character of levitation is found in the life of St. Theresa. She sometimes resisted both the physical event of the levitation and the spiritual ecstasy (rapture) that seemed to induce it because of fear of too much adulation being heaped upon her or because of a desire not to distract others, etc. She even reported that she prayed for public manifestations of this sort to cease (and they did, in fact, allegedly occur with less frequency). She sometimes grasped onto objects to try to prevent the levitation, generally to no avail according to her statements. (See Thurston, 1951, Chapter I, which also includes discussion of similar attempts at resistance by other Christian mystics.) A reading of the mystics' own accounts of their levitations creates the impression that they were ambivalent about them. From a worldly perspective they wished to resist them in public settings and often tried to do that, but they also reported a burning desire and an ecstasy that seemed to draw them upward, first in "spirit" and then in body. It is, however, quite clear that the alleged physical levitation occurred involuntarily and, sometimes, contrary to mundane concerns of the levitator.

Second, such levitations are expressive not only in that they are automatic or involuntary, but because they reflect the mystic's emotional or spiritual impulse, much as bodily gestures reveal one's inner feelings. Any reading of the accounts of typical instances of levitation should convince the reader that these events generally occur in a state of spiritual "rapture" (ecstasy) in which the levitator's whole being seems overcome and lifted beyond earthly confines in an overwhelming sense of love and heavenly joy. The spontaneous movement of the body seems to reflect this joy. [Obviously, such emotional states would sometimes favor hallucinations of being levitated and Thurston (1951) notes cases in which such hallucinations definitely occurred.]

Third, the movement is upward, contrary to the effect of earth's gravity. Christian mystics, among whom levitation during a state of rapture seems to me to be especially common (relative to mystics of other religions), believe in some sense that heaven is "above" and earth, "below" and that Christ, toward whom the rapture is felt, is "up in heaven." The desire to commune with Christ and the feeling of its consummation might, therefore, find expression in a specifically upward movement of the body. It should be understood that these mystics do not report any effort to get the physical body to ascend into heaven. It would seem that the effect, if real, might be an expression of the

mystic's intense experience of self in relationship to Christ (or God) at that time, which is an experience of the self being lifted up, figuratively, if not literally.

Although a literal interpretation of such experiences is surely a difficult one for most parapsychologists, myself included, to accept, we should perhaps not dismiss such evidence out of hand or give it no consideration. To do so would mean that we would not consider its potentially profound implications for our conceptualizations of both the paranormal and of the world. Those are worthy of consideration, for we should not want to miss anything that might provide fundamental and important clues, even though we should not accept undocumented, fanciful tales. The ideas that would seem to flow out of acceptance of these levitation accounts are radical and perhaps frightening, but we must not close our minds to such possibilities just because they are not dictated by our present understanding of the events we study in the laboratory or might be unpopular in this day of a seemingly boundless fear of what certain skeptics might think. While no conclusions may be warranted by the levitation data presently available, perhaps they can help us to avoid theoretical complacency by remaining alert for cracks in the hulls of our conceptual vessels.

There are also suggestions of expressiveness (or perhaps of the effect of expectations) in some laboratory PK studies. These are studies whose findings can be interpreted as suggesting that when psychological conditions favor PK (as when the subject is not engaged in egocentric effort with regard to the desired objective), the direction of the deviation from mean chance expectation will accord with the feelings of the subject about the likelihood of psi-mediated hits occurring under the circumstances at hand (see several studies by Cox, 1951, and one by Stanford, 1981). The specific suggestion here is that when conditions are otherwise favorable to PK, conditions that make subjects doubt the likelihood of success will eventuate in psi missing. While this interpretation (discussed in Stanford, 1981) of the studies cited just above is an *ad hoc* one and while direct experimentation is required for definitive interpretation of such findings, these data, like those from the nonlaboratory settings discussed above, suggest the potential importance of expressiveness in psi function.

To what degree any "expressiveness" effects are mediated by expectation is a theoretically important, but unexplored, question. The expressiveness topic may have been given too little consideration by contemporary researchers, myself included. Certain interpretations of the expressiveness phenomenon would give it great theoretical import, and it is for that reason that it is broached here. The first step would

be to see whether substantial evidence of it can be found in laboratory work.

Vicarious Suffering. To say that something is *vicarious* means that it is done, endured, or suffered by one person in the place of another. Many of us are familiar with the concept, held by many Christians, of the vicarious atonement. According to that doctrine, Christ's agonies that ended his earthly life were suffered in order to spare us the agonies associated with what would otherwise be the consequences of our sins. What is probably much less well known is that the concept of vicariousness—and of vicarious suffering, in particular—is widespread in religious quarters other than Christianity (even if it has been conceived in the latter on a grander scale). It is common to read in books on major yogic teachers of the belief by their disciples that these teachers have deliberately taken upon themselves the suffering (karmic consequences) that their disciples would otherwise have encountered because of their selfish or sinful actions. It is often believed by disciples that this vicariousness is the cause of their teacher's severe illness or death. This vicarious suffering is believed to spare the disciple much suffering and to aid him or her in spiritual progress due to the removal of impediments (since improper reaction to our own karmas or action consequences is supposed itself to create dire consequences). One of many examples that might be cited is the belief of Sri Ramakrishna's disciples that his prolonged suffering prior to his death served just such purposes (Isherwood, 1965). (Sri Ramakrishna was a widely esteemed teacher within the Vedantic yoga tradition.) This is more than just an abstract belief, for the disciples typically report what they believe to be direct, concrete consequences of such suffering for themselves individually. The idea in all such cases is that the unselfish love of these spiritual preceptors is manifest in their willingness to suffer in order to spare others suffering.

The surprisingly widespread concept of vicarious suffering has found its way even into musical theater. The dramatic and successful modern opera *Dialogues of the Carmelites* by Francois Poulenc is the story of ill-fated Carmelite nuns who were the victims of political paranoia occasioned by the French Revolution and who were consequently guillotined. The story is built upon a historical incident, though details are fictionalized. The central figure, Blanche, is a withdrawn, rather fearful soul who nonetheless, in the end, comes willingly to die a martyr's death—an uncharacteristic act of boldness and heroism. The dialogue of the opera suggests that she was able to do so because a mother superior, who had earlier died, had suffered a hard, very agonizing death in order that someone else in the order (Blanche) would be able

to die in simplicity and dignity. Blanche's heroism is deeply touching, not just because it would be admired under any circumstances, but because in order for it to happen her habitual character had to be transcended. But it would appear that she had paranormal help in terms of the vicarious suffering of the earlier mother superior. The story is, in this respect, apparently fiction, a fascinating device for allowing dramatically satisfying self-transcendence by an otherwise unheroic individual. Fiction or not, it reflects a concern about vicarious suffering that exists in both Christian and non-Christian religious quarters throughout the contemporary world.

One example of what might be interpreted as a form of vicarious suffering is the spontaneously developed agonizing prayer of "travail" that unexpectedly falls upon some "Spirit-filled" Christians at times during "prayer in the Spirit." At that time, it is believed, intercession for particular others is made whereby they are spared awful fates of various kinds or relieved of burdens (see, e.g., Hagin, 1980). Some have compared such travail to that of childbirth with its pains and labor, although this seems usually to be intended as a metaphor. There is often much "groaning in the Spirit" and crying by the intercessor during these periods. The person who is the alleged target of such ostensible intercession is almost invariably not present and is often far away. Sometimes the target person is wholly unknown to the person praying, and it is claimed that only later, if at all, does the intercessor learn of the predicament of the target person that was miraculously alleviated at the time the "spirit of travail" lifted from the intercessor. The reports of such events are, taken at face value, sometimes of potential parapsychological interest, but careful scientific investigation is needed for any conclusions.

Although vicarious suffering is a widespread idea that appears in a number of cross-cultural settings, it is easy to think of a very conventional explanation for many of its most common manifestations. In cases wherein an esteemed spiritual teacher is seen to take on the suffering or karmas of others through his or her own suffering and/or death, it is conceivable that such beliefs develop in order to add meaning to perplexing events and to protect against the feeling that the world is a senseless or unjust place. The follower of a spiritual teacher often regards that teacher as worthy of veneration and of wonderful character. For such a righteous person to suffer greatly through disease or other harm is very unsettling. It potentially creates the feeling that the world is an unjust place, one that can at best be described as uncaring and cruel, a place of reckless fate.

Melvin Lerner (1980), a social psychologist, has along with others

investigated the "just world hypothesis," the hypothesis that we all want to believe that the world is intrinsically just despite obvious appearances to the contrary. The world is just only if people get what they deserve or, at least, what they allow to happen. Lerner asserts that if we do not believe in a just world in this sense, our fates seem potentially very much out of control. That would be a threatening circumstance, indeed. Thus, as has been shown in numerous experimental studies, we will do things like disparage victims of misfortune in order to make the world seem a more just, comfortable place in which to live—they must, after all, have deserved it or stupidly allowed it to happen. Lerner alleges that religious concepts of justice after this lifetime, as in the ideas of reincarnation and karma, derive from this protective inclination to believe in a just world.

Let us consider in more detail how the just world hypothesis might be used to explain the situation in which vicarious suffering is attributed to spiritual teachers. If terrible things happen to such an upright, esteemed individual, there must be an explanation for it that perpetuates a sense of justice in the world. If not, it would seem horribly unjust and threatening. A simple, comfortable way to reinterpret such suffering by beloved innocents is to believe that they must have been willing to take on suffering in order to spare others affliction who were less able to live with it in spiritual equanimity. Therefore, the suffering of these esteemed persons is seen as both volitional and meaningful. Thus, belief in vicarious suffering under these circumstances might represent only a way of maintaining our belief in a just world.

That is one way of understanding at least a portion of the claims related to vicarious suffering, but it does not comfortably account for other claims such as the prayer of intercessory travail. Nor need it be the whole story in the case of alleged intercession by suffering spiritual teachers—though it is hard to escape the feeling that it does play some role in that. The fact that we can find a way to understand persons' desires for belief in vicarious suffering by spiritual teachers does not necessarily mean that there is no paranormal phenomenon involved. By analogy, it is easy to think of credible reasons why persons might wish to believe in extrasensory communication, reasons why it would make them feel more comfortable. That, however, tells us nothing about whether such communication actually exists. That is a question for direct scientific examination. Likewise, only scientific investigation could in principle resolve the issue of whether paranormal vicarious suffering actually exists.

Perhaps the widespread belief in vicarious suffering is based upon something that is genuinely paranormal. There are hints in some re-

ligious accounts that genuinely paranormal vicarious suffering might actually occur, but at present those stories are more tantalizing than satisfying to the scientific mind. If such events are real, they seemingly have importance for understanding the wider meaning of scientifically unexplained interactions. All that is clear at present is the need for research on this matter.

The study of nonlaboratory events that may be of this kind should be relatively easy since many events with possible relevance here are said to emerge during the travailing, intercessory type of "prayer in the Spirit" that sometimes occurs in Pentecostal, neo-Pentecostal and Roman Catholic charismatic Christianity. I have myself read some extremely interesting—that is not to say well documented—reports from such quarters and have personally interviewed participants in one such case. Claims in this area should be investigated in the same ways that investigators have examined spontaneous cases of various kinds for authenticity and validity. If evidence should be found that such intercessory-prayer cases contain an anomalous element, there would still be the possibility that they do not truly represent vicarious suffering. Those reporting such experiences do not always claim that their travail is truly a burden-sharing phenomenon. Nor do they claim that their experiences during such intercession always resemble those of the person on whose behalf the intercession is occurring, though that not infrequently appears to be the case. They see such intercession as one of intense, agonizing struggle with something that is harming or threatening another person. The struggle is ended when the burden or threat is lifted off of or moved away from the other individual.

I wonder, however, if paranormal vicarious suffering is a real phenomenon, whether its occurrence might be more widespread outside traditionally religious contexts than one might at first imagine. It is possible that it occurs under various circumstances in which individuals are bonded by love and affection.

If that is the case, the first step toward providing evidence of it might be to learn whether one individual in a close and loving relationship with another sometimes experiences, through paranormal mediation, the suffering of the other. That would not demonstrate that such experience represents a paranormal sharing of the actual burden, but it would be a first step in the direction of investigating vicarious suffering. Fortunately, research is already available that addresses this preliminary objective.

Louisa Rhine (1967) amassed a sizeable collection of cases in which the ostensible anomalous communication between individuals—almost always persons emotionally very close—consisted of pain and physical

suffering occurring to one person that were in some important ways shared experientially by another person at, apparently, the same time. In the vast majority of such cases there was, in Rhine's view, *prima facie* reason to believe that the information thus shared would have had to be paranormal in character. She concluded that the specificity of shared symptoms as to location, type, etc. was very suggestive of genuinely paranormal communication. Although her interpretation of such events was that of extrasensory communication that somehow went awry and did not emerge as an intuition of what was really happening and to whom, she remarks that such cases appear, in a way, to be sympathetic responses, as evidenced by similarity of experiences by the target person and the experiencing individual. She notes that a considerable majority of such cases did not successfully serve the communicative function that occurs in intuition cases, and for this reason she compares them to hallucination cases, which rarely convey the information that is carried by intuitions.

I wonder whether the obviously sympathetic form of the experience might not indicate a willingness of the person experiencing the paranormally communicated symptoms to share the burden of the experience, a kind of nurturant reaction. We have no definitive information in Rhine's report on whether such a willingness and the development of related symptoms led to a lessening of the pain or suffering of the target person. However, in some of her cases there are tantalizing hints of this that are not discussed by her with regard to this possibility. Specifically, in five of her childbirth cases the person who seemed to paranormally experience vicarious pain felt it as very severe, whereas the person actually giving birth experienced an easy delivery (Rhine, 1967, p. 118). Rhine indicated that the former individual had herself previously given birth to one or more children and suggested that she must have, following paranormal prompting, experienced the pain as she thought it would be, rather than as it actually was. That is one possibility, but it is not the only one. The possibility of vicarious, paranormal burden sharing should in such instances also be considered. There was no cogent reason at this stage to ignore it as a possibility, although it is easy to understand why Rhine did so, given her unswerving commitment to a perceptual-cognitive view of anomalous interaction. It is interesting that the persons in such cases who seemingly suffered vicariously were individuals who were well equipped for doing so because they had themselves gone through childbirth. If paranormal vicarious suffering exists, it might be more effective when the individual doing the vicarious suffering understands the type of experience the other would normally have.

Such cases prove nothing at this stage, but it is important to allow them to foster investigations that might bring into question our most cherished assumptions about anomalous interaction, if for no other reasons than that the perceptual-cognitive view is showing serious shortcomings (see above) and that the consistent failures to convincingly explain the phenomena we study might derive from wearing conceptual blinders.

It is interesting to note that Rhine indicates a very great preponderance of women as the experiencing person in such cases, a feature that is entirely in line with the emerging evidence (Hall, 1984) that females may be more empathic than males (at least by certain indices that are used). At any rate, if women are more likely—due to biology, culture, or both—to provide nurturant response than men, this, too, would fit with Rhine's findings. If there is paranormality in her "psychosomatic psi" cases, its meaning is presently unclear. They do encourage further inquiry into the question of whether such experiences are, at least in some instances, a reflection of vicarious suffering, an interpersonal adaptive function by which one person lessens the burden of another.

Though there is presently no way of knowing whether paranormal vicarious suffering is a reality, I wonder whether our instinctive reaction of empathic response to the pain of others—especially those we love—might play such a role on at least a small scale. (I also recognize that it probably plays a number of other, non-paranormal roles!) If paranormal vicarious suffering exists in any of the settings discussed above, it would have fundamental implications for understanding anomalous interaction. As remarkable or even radical as the suggestion of paranormal vicarious suffering may seem, it attracts our attention because of its perennial presence within the folklore of the paranormal. It deserves empirical investigation because of its potential theoretical and pragmatic implications.

Perspective

The earlier speculations and suggestions about the roles of faith, expressiveness, and vicarious suffering in anomalous interaction are intended mainly to show that a closer linkage of empirical research with some prevalent ideas in the folklore of the paranormal could potentially yield insights that would challenge our present rather laboratory-bound and constricted theories. Such challenges are important because we have so few indications at present that contemporary theories in parapsychology have taken us to the heart of the phenomena

we wish to understand. At the same time, there is nothing in the discussion of these three topics above that refutes or even decisively challenges contemporary theorization. This is because constructs derived from folklore that are supported, at best, by minimal laboratory research are not equivalent to constructs derived from controlled research. They are simply wedges that, if used, might open a door to shed a clearer, fuller light upon the events parapsychologists study or upon the fallacies that can evolve within folklore. Either way, knowledge would have moved ahead. And the specific topics discussed here are only three of many possible thrusts into the darkness of our ignorance that might be suggested on the basis of the folklore of the paranormal.

REFERENCES

Alvarado, C. S. (1984). Psychological evaluations of poltergeist agents: A critique. In R. A. White & R. S. Broughton (Eds.), *Research in parapsychology 1983* (pp. 117–118). Metuchen, NJ: Scarecrow Press.

Child, I. L. (1985). Psychology and anomalous observations: The question of ESP in dreams. *American Psychologist, 40,* 1219–1230.

Cox, W. E. (1951). The effect of PK on the placement of falling objects. *Journal of Parapsychology, 15,* 40–48.

Debes, J., & Morris, R. (1982). Comparison of striving and nonstriving instructional sets in a PK study. *Journal of Parapsychology, 46,* 297–312.

Dingwall, E. J. (1967–1968). *Abnormal hypnotic phenomena* (4 vols). New York: Barnes and Noble.

Festinger, L. (1957). *A theory of cognitive dissonance.* Stanford, CA: Stanford University Press.

Festinger, L., & Carlsmith, J. M. (1959). Cognitive consequences of forced compliance. *Journal of Abnormal and Social Psychology, 58,* 203–210.

Gauld, A. (1968). *The founders of psychical research.* New York: Schocken Books.

Gurney, E., Myers, F. W. H., & Podmore, F. (1886). *Phantasms of the living* (2 vols.). London: Trubner.

Hagin, K. E. (1980). *The art of intercession.* Tulsa, OK: Kenneth Hagin Ministries, Inc.

Haight, J. (1979). Spontaneous psi cases: A survey and preliminary study of ESP, attitude, and personality relationships. *Journal of Parapsychology, 43,* 179–204.

Haight, J., Kanthamani, H., & Kennedy, J. E. (1979). Further work on spontaneous cases from high-school students. *Journal of Parapsychology, 43,* 53–54.

Hall, J. A. (1984). *Nonverbal sex differences: Communication accuracy and expressive style.* Baltimore: The Johns Hopkins University Press.

Honorton, C. (1981). Psi, internal attention states and the Yoga Sutras of Patanjali. In B. Shapin & L. Coly (Eds.), *Concepts and theories of parapsychology* (pp. 55–68). New York: Parapsychology Foundation, Inc.

Isherwood, C. (1965). *Ramakrishna and his disciples.* New York: Simon and Schuster.

Krippner, S., Ullman, M., & Honorton, C. (1971). A precognitive dream study with a single subject. *Journal of the American Society for Psychical Research, 65,* 192–203.

Krippner, S., Honorton, C., & Ullman, M. (1972). A second precognitive dream study with Malcolm Bessent. *Journal of the American Society for Psychical Research, 66,* 269–279.

Lerner, M. J. (1980). *The belief in a just world: A fundamental delusion.* New York: Plenum.

Millar, B. (1986). Psychology of the OTs: The observational quasi motor model (OQM)—Part 1. In R. D. Nelson (Ed.), *The Parapsychological Association 29th Annual Con-*

vention: Proceedings of presented papers (pp. 243–256). Durham, NC: Parapsychological Association.

Morris, R. L. (1977). The Airport Project: A survey of the techniques for psychic development advocated by popular books. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in parapsychology 1976* (pp. 54–56). Metuchen, NJ: Scarecrow Press.

Morris, R. L., Nanko, M., & Phillips, D. (1982). A comparison of two popularly advocated visual imagery strategies in a psychokinesis task. *Journal of Parapsychology, 46,* 1–16.

Rhine, L. E. (1954). Frequency of types of experience in spontaneous precognition. *Journal of Parapsychology, 18,* 93–123.

Rhine, L. E. (1962). Psychological processes in ESP experiences. Part II. Dreams. *Journal of Parapsychology, 26,* 172–199.

Rhine, L. E. (1967). Hallucinatory experiences and psychosomatic psi. *Journal of Parapsychology, 31,* 111–134.

Rhine, L. E. (1970). Dr. L. E. Rhine's reply to Dr. Stevenson. *Journal of Parapsychology, 34,* 149–163.

Rhine, L. E. (1981). *The invisible picture: A study of psychic experiences.* Jefferson, NC: McFarland.

Roll, W. G. (1972). *The poltergeist.* New York: New American Library.

Schouten, S. A. (1979a). Analysis of spontaneous cases. *Research Letter,* No. 9, 55–62.

Schouten, S. A. (1979b). Analysis of spontaneous cases as reported in 'Phantasms of the Living.' *European Journal of Parapsychology, 2,* 408–455.

Schouten, S. A. (1981). Analysing spontaneous cases: A replication based on the Sannwald collection. *European Journal of Parapsychology, 4,* 9–48.

Schouten, S. A. (1982). Analysing spontaneous cases: A replication based on the Rhine collection. *European Journal of Parapsychology, 4,* 113–158.

Schouten, S. A. (1983). A different approach for analyzing spontaneous cases: With particular reference to the study of Louisa E. Rhine's case collection. *Journal of Parapsychology, 47,* 323–340.

Sidgwick, H., Johnson, A., Myers, F. W. H., Podmore, F., & Sidgwick, E. M. (1894). Report of the Census of Hallucinations. *Proceedings of the Society for Psychical Research, 10,* 25–422.

Stanford, R. G. (1974a). An experimentally testable model for spontaneous psi events. I. Extrasensory events. *Journal of the American Society for Psychical Research, 68,* 34–57.

Stanford, R. G. (1974b). An experimentally testable model for spontaneous psi events. II. Psychokinetic events. *Journal of the American Society for Psychical Research, 68,* 321–356.

Stanford, R. G. (1977a). Conceptual frameworks of contemporary psi research. In B. B. Wolman (Ed.), *Handbook of parapsychology* (pp. 823–858). New York: Van Nostrand Reinhold.

Stanford, R. G. (1977b). Experimental psychokinesis: A review from diverse perspectives (pp. 324–381). New York: Van Nostrand Reinhold.

Stanford, R. G. (1981). "Associative activation of the unconscious" and "visualization" as methods for influencing the PK target: A second study. *Journal of the American Society for Psychical Research, 75,* 229–240.

Stanford, R. G. (1982). An experimentally testable model for spontaneous extrasensory events. In I. Grattan-Guiness (Ed.), *Psychical research: A guide to its history, principles, and practices* (pp. 195–205). Wellingborough, Northamptonshire, UK: Aquarian Press (for the Society for Psychical Research).

Stanford, R. G. (1987). Ganzfeld and hypnotic-induction procedures in ESP research: Toward understanding their success. In S. Krippner (Ed.), *Advances in parapsychological research: Vol. 5* (pp. 39–76). Jefferson, NC: McFarland.

Stevenson, I. (1970a). (Letter to the Editor.) *Journal of Parapsychology, 34,* 143–149.

Stevenson, I. (1970b). Telepathic impressions: A review and report of thirty-five new

cases. *Proceedings of the American Society for Psychical Research, 29,* whole issue (198 pages).

Stevenson, I., Palmer, J., & Stanford, R. G. (1977). An authenticity rating scale for reports of spontaneous cases. *Journal of The American Society for Psychical Research, 71,* 273–288.

Thurston, H. (1951). *The physical phenomena of mysticism.* London: Burns Oates.

Ullman, M., & Krippner, S. (1970). *Dream studies and telepathy: An experimental approach* (Parapsychological Monographs No. 12). New York: Parapsychology Foundation, Inc.

Ullman, M., Krippner, S., with Vaughan, A. (1973). *Dream telepathy: Experiments in nocturnal ESP.* New York: Macmillan.

von Lucadou, W. (1987). The model of pragmatic information (MPI). In R. L. Morris (Ed.), *The Parapsychological Association 30th Annual Convention: Proceedings of presented papers* (pp. 237–254). Durham, NC: Parapsychological Association, Inc.

Weiner, D. H., & Haight, J. (1983). Charting hidden channels: A review and analysis of Louisa E. Rhine's case collection project. *Journal of Parapsychology, 47,* 303–322.

West, D. J. (1970). ESP in life and lab [Book review]. *Journal of The Society for Psychical Research, 45,* 305–308.

DISCUSSION

NEPPE: Rex, I must admire somebody who is capable of being so punctilious within himself as to keep to his time when he has got a wonderful 36 page manuscript. I thank you for that. What I would like to ask you about are the three areas you were alluding to and implying you really wanted to mention a little bit more about.

STANFORD: Well, I just mentioned the areas, but I did not get into them. If you will invite me to get into some of the other ramifications . . .

NEPPE: This is what I am inviting you to do.

STANFORD: Let's take faith for example. Here is one instance. There is a fascinating story in the scripture in which Jesus was touring around the countryside and ran across ten lepers. They knew his reputation and asked him to heal them and he said, "Well I'll tell you what you do. I want you to go to the chief priest and tell him that you have been healed." They took off. They still had leprosy. The story says that they were cured as they went. Now we do not have to believe anything about the historicity of these types of accounts. We can properly be very skeptical about these things. But this and many other stories in the folklore suggest that action can be an integral part of faith. We get stories from ministers and from Christian books, for example that tell us that once we pray for something we should act as though it were true. How do you account for that? I would suggest that one possibility is that psi or whatever is involved is responsive to needs. When an

individual goes out on a limb on faith something has happened. He has stuck his neck out. He, made a commitment. Action is a commitment, isn't it? Once a commitment is made it is hard to go back on it. So the need that could be subserved by the event actually happening is enhanced under those circumstances. This may be one possible explanation. I like that because it fits in with my own theory that an incentive value is an important factor in terms of the probability of psi actually happening. It also fits in with a lot of psychic folklore that says psychics give better readings when people have real needs. My brother used to say that if someone had a real need he would give him or her a bang-up reading. I do not know if that is true or not, but there is a lot of folklore here about this kind of thing. That is about faith.

I want to say a little more about expressiveness. It is interesting. Let us take levitation claims first. Certainly there was not conscious motivation involved but St. Teresa hated levitation to happen because she feared greatly that there would be too much adulation. She knew within the church how people could be worshipped and mobbed when miracles started happening. If you do not believe that happens, by the way, read a good biography of Padre Pio. It could get to be a very bad scene, and she did not want to be a part of it. So, she would sometimes hold herself down. Such levitation seems expressive in the sense that it is automatic, much in the sense that you would express emotion without trying to. Maybe even though you try to resist it, you may express it. It seems expressive in the sense that the body lifts up or the person's consciousness in a phenomenological sense moves upward. They believe Christ is on high, in heaven. There are a number of other elements you could get into. This all assumes that levitation happens. Maybe it does not. But I think we also have some PK literature that tends to support the notion that at least the form that our results take in terms of affecting random event generators may depend upon things like fears, that our sense of reality gets expressed out there in what the random event generator is doing. Again there is nothing in our contemporary theories that can possibly cope with that. This is my point. I am not trying to say we have any proof of levitation. I am trying to say there are a lot of things hanging around out there that, if they were true, would really rattle the windows of our theoretical buildings and maybe the walls. The one about vicarious suffering to me is extremely interesting. There is a great deal of folklore on this. I think it comes into Shamanism. I think it comes into yoga. It certainly comes into Christianity. It even gets into opera. My favorite modern opera is *Dialogues of The Carmelites.* The mother superior has a very difficult and uncharacteristic death and it turns out that that was apparently in order

to help the heroine, who is not a heroic figure at all, to die a very heroic martyr's death in the end. But that is of course just fiction. But sometimes fiction derives from folklore which upon occasion might derive from some element of truth. Vicarious suffering is interesting because it almost suggests an economy or an ecology in the psychic world. If one person loves and is concerned about another, the vicarious suffering effect can be expressed by them. It is as though the suffering can be transferred or shared. It may be a way in which conservation of energy in some sense enters into the total picture. Well, it is very challenging. It is also very farfetched, I admit. We do not have any kinds of empirical research on vicarious suffering. I do not know who will ever be heroic enough to start this kind of research. Charley Tart once did an experiment where he suffered a lot on behalf of science. I will not get into the that right now, but he was being shocked to see if his subject could react to it physiologically. But most of us are not quite that heroic. I wanted to say something more about vicarious suffering, though. There is a possible analog to this that we could study in the field. Among so called spirit-filled Christians there is a strong belief in what they call intercessory prayer in which individuals sometimes totally spontaneously believe that they have laid on them a burden for other people. They suffer, they groan, they cry, they agonize, they sometimes experience physical pain. Some say it is almost like childbirth. This is supposed to remove some burdens—sickness, imminent danger, and so forth—from various people. Now, I have heard some very interesting folklore about these things—in books and I have heard people talk about them. I even interviewed both of the participants in one such case. That is one possible area where there might be something like vicarious suffering going on. I would suggest that if this is true, what underlies it in a psychological sense may be a kind of love, a willingness to share the "beingness" of another person. If vicarious suffering really happens, it is a first class mind boggler in terms of the way we have traditionally thought about psi events. I hope that if what I have said today does anything, it helps us to recognize that one thing we have got to be doing all along is to start questioning our own constructs. They are so limited by specialized experience that sometimes we box ourselves in. Theories are largely for throwing open our vision of things and after that for the purpose of finding out whether we can falsify them or not. So take what I am saying as something we need to check into to see whether it will hold up in the light of any kind of reasonable investigation. There are a lot of other areas that could challenge our theorization.

ISAACS: Thanks for a really wonderful paper which I think will stim-

ulate a lot of research. I agree with you that certainly there is evidence for psi being expressive. Water has appeared mysteriously in poltergeist cases where there might have been tears. We have got fire where there might have been anger. We have rock throwing where there might have been anger also. I wanted to challenge your notion that case studies have not provided any hypotheses for testing. I agree with you that there are no formulators of those studies, or clearly put together hypotheses for testing in the way that you have been so careful to provide in your PMIR theory. But I would suggest that the case studies have stimulated areas of research. First of all there is survival research. I think that the case studies carried out by the SPR did encourage further interest in survival which led to mediumistic communication studies. In addition, I think that those surveys also implicated the role of altered states and particularly of dream telepathy and dream precognition as being vehicles of psi. They pointed out the relationship between the emotional significance of the situation surrounding the psi event and by implication motivation as an important aspect of psi. I think that a parallel to that was the emphasis on the emotional closeness of the agent and the percipient. This was tested in some of the "sweethearts" work. Also it could be argued that viewing the OBE as a veridical ESP experience rather than as just an interesting dream may have been bolstered by the case studies.

STANFORD: First, there has been a vast tendency to exaggerate. Let me make a distinction first. I am talking about specific conceptual hypotheses that derive from the research that has been attempted in the lab. I am not talking about settings in which we studied whether ESP might occur. I think even their role has been exaggerated vis à vis the influence of systematic case studies. Now, folklore and individual experiences of investigators are cases in which there have been influences on experimental work. Let's take dream telepathy. Read the monograph *Dream studies and telepathy. An experimental approach* that the Parapsychology Foundation published on this and you will find out that evidently the strong interest in that came predominantly from clinical interactions, therapy in which telepathy was occurring as reported in dreams. That was the big emphasis. There was not even a mention, as I recall of the spontaneous case literature. But in the case of precognitive dream studies it is quite different. There is no question at all that Mrs. Rhine's findings that psychic dreams are predominantly precognitive did influence the research. It is mentioned in the introduction to the Malclom Bessent study on precognition. I do not suggest—and I make it very clear in the paper—that there was support from all the historical case studies with regard to dream telepathy as

well. I am really talking about specific hypotheses that come out of it. Now the "sweethearts" work certainly comes from what I would call the folklore cases. But I do not know of systematic investigations, looking at that in the case literature, that led to a hypothesis. I think that comes out of spontaneous cases for sure, but I do not think it comes that much out of the systematic case studies. By the way I would not want anyone to go out and think that I am saying systematic spontaneous case studies are worthless. Far from it. I think they are potentially extremely valuable. The whole purpose in my saying what I have said is that I hope the people who do that kind of research will be more ready to talk about specific testable hypotheses. I think it was Schouten who found that living targets seemed to be better in spontaneous cases than material things. The specifics do not matter that much. Well, what do you do with that in the lab? Well, you can do many different experiments, but the problem is that you do not know which experiment to do until you create a hypothesis to explain what happens. Why should that be? Is it, for instance, that living targets are more important to use simply because of the fact that they pose potential threats or sources of reward and therefore have survival value so we are simply more vigilant about them? That is one interpretation, one hypothesis. Another interpretation might be that through evolution we have developed structures that more readily receive and process that type of information simply because historically survival depended on it. That would lead you to a very different type of experiment. This is part of the problem. Suppose we just wanted to generalize a case study finding to the laboratory. That does not work too well either. What does generalization mean? When you start to change methodology you've got to have constructs to guide your generalizations, and this is exactly the crux of the problem. I do not mean to suggest that *Phantasms of the Living* has had no impact upon experimental research. What I am talking about is a specific kind of impact. I am sorry I left that impression in my oral presentation.

HARARY: There is a lot of spontaneous material out there that we are not hearing about. I often wondered in investigating alleged poltergeist cases if the really good ones were the ones where people were doing quite well. Where all sorts of interesting things were happening but they were so well adjusted to it that they were not having problems and didn't feel like calling for an investigation. We get called into the ones where the people are having all sorts of psychological disturbances. In the Soviet Union they claim that they are doing quite a lot on empathetic suffering where they are shocking rats or starving mice and then seeing what happens to their fellows some distance away. They

say that they are doing it and that they are getting results. I have been there but I haven't seen the experiments.

STANFORD: That is interesting. I hope my presentation does not inspire any such research.

HARARY: But I think one of the reasons why people are resistant to that approach is because of the potential implications that this stuff might be . . .

STANFORD: I think you are right. First, there may be a lot of dramatic events going on that you never hear about because people are not alarmed and they are not running out seeking help. For example, I walked into a physician's office, the very first time I ever visited this doctor and when he asked me what I did and I said parapsychological research, he opened up and got a load off of his mind! He had done his residency in an old city with lots of wonderful big, very old, elegant houses that might look as though they would be haunted. But this seemed to be a very level-headed man. He swore that they had physical phenomena or at least audible phenomena, very mysterious things going on all over the house. All I am suggesting here is that I think there is a lot more of this material around. I found out that people would come into the lab and open up and tell me all kinds of things, not just about their love for animals and things like that, but also about psychic experiences. It has got to be a safe environment. If we can create a safe environment, we can learn quite a bit.