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The Place of Spontaneous Cases in Parapsychology

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ABSTRACT: In modern times parapsychology has neglected the study of spontaneous cases for an emphasis on laboratory work. This means-centered approach to psi research has important consequences. One is that we do not know if our results can be generalized outside of the laboratory. In addition, our field is unable to answer many of the questions experiencers ask us, thus limiting the social usefulness of parapsychological research. Increased attention to spontaneous cases may help us solve these problems. Moreover, some case studies allow for more sensitive and relevant study of particular issues and make possible the investigation of phenomena that cannot be studied in any other way. It is suggested that future research programs follow a converging approach where psi is studied with different methodologies and in different contexts of occurrence. It is also important to recognize that both laboratory and case investigations have problems and advantages of their own and that their combined use may help the field more than their separate use. Parapsychology must understand the value and necessity of pluralistic approaches to the study of psi phenomena which, after all, occur both in life and in the lab.

Over a decade ago Rhea White (1985) made a strong plea for studying spontaneous cases in her 1984 presidential address to the Parapsychological Association (PA). More recent presidential addresses presented in 1991 (Braude, 1992), 1992 (Palmer, 1992), 1993 (Radin, 1993), and 1994 (Delanoy, 1994) have discussed the issue from different perspectives and with different emphases, although none were devoted exclusively to the

This is a more complete and revised version of the 1995 presidential address to the Parapsychological Association given at its 38th annual convention held in August in Durham, North Carolina. It was written while I enjoyed the financial support of the Parapsychology Foundation, the Society for Psychical Research, and the Perrott-Warrick Fund. I am grateful to Nancy L. Zingrone for useful editorial suggestions that considerably improved this paper. Useful suggestions were also obtained from William Braud, Gertrude Schmeidler, and Rhea White. Eberhard Bauer, Piero Cassoli, Enrico Marabini, and Alejandro Parra helped me to obtain references published in Germany, Italy, and in Argentina.

topic. Other writings on the subject outside of the context of the PA include the influential defenses of spontaneous cases made by Ian Stevenson (1987a, 1987b), Rhea White (1990, 1992), and Steve Braude (1986) (see also Alvarado, 1987a; Biondi, 1987a, 1987b; Gauld, 1993; Hearne, 1985). I will take up this problem here today in an attempt to clarify the place of spontaneous cases in parapsychology.

Parapsychology today, in the terminology of Abraham Maslow (1954), is to some extent a means-centered science as opposed to a problem-centered one. That is, many parapsychologists base their research on preferences for a particular method—the experiment—instead of focusing their efforts on a particular problem that can then be studied in a variety of ways. Such reliance on what is almost a single approach to the study of psi begs several questions. What are the implications of the creation and/or the acquisition of knowledge from the predominant experimental perspective in the field today? What are the limitations of such an approach? What is the price we pay when we so singlemindedly define the experiment as the royal road to legitimation in parapsychology? Or, stated in a different way, what are the advantages of spontaneous cases in the study of psi phenomena? These questions will guide my thoughts in this paper.

My sympathies are clearly with a more pluralistic approach than the one prevalent in the field at the moment. Briefly stated, I agree with what Rosenthal and Rosnow (1991) say in *Essentials of Behavioral Research*: "Given the limitations of any particular strategy of inquiry, more than one approach to understanding is a logical necessity" (p. 137). This means that I do not take the position of Braude (1986) and White (1990) who seek to dispense with the experimental method. Instead I prefer a more clear balance between approaches to the study of psi phenomena. Like most of you, I believe that there is much to be gained by controlled observations, both experimental and quasi-experimental. Repeatable and carefully measured results obtained through experiments are a basic part of science, and an approach that is of basic importance in parapsychology as well.

The point is not to throw away the experimental legacy of our field. It is essential to continue this work, to extend it, and to improve on it. But at the same time we need to recognize that the current emphasis on the experimental method as the single most important generator of parapsychological knowledge is problematic. Our methodology is in desperate need of expansion, a point argued recently by William Braud (1994a). The royal road to knowledge is not the experiment alone, but rather a combination of approaches designed to converge in meaningful ways. I would like to expand my point by focusing on previous research trends in our field and by delineating some of the limiting consequences for a human science that depends so much on experiments. My discussion will touch on such conventional approaches to spontaneous case studies as case collections, surveys, and field studies. I will not touch in detail but want to acknowledge the variety of methodologies recently listed by White (1992) as well as recent defenses of the value of exploring our own psi experiences (e.g.,

Braud, 1994b). In addition, and as pointed out by J. B. Rhine (1955) 40 years ago, the issue is not limited to a contrast of experimental and spontaneous case approaches. There are gradations between these categories that complicate the distinctions when issues such as induced experiences are considered. Consequently, I am simplifying the debate for the sake of argument.

THE DECLINE OF SPONTANEOUS CASES

With the rise of the Rhinean paradigm there was a clear decline in spontaneous case research in Anglo-American parapsychology. Nancy Zingrone (1988) charted this trend in her quantitative study of American parapsychological journals. I have also found this trend in my own counts of experimental and spontaneous case research papers presented at Parapsychological Association conventions from the first convention in 1958 to this one in 1995 (see Figure 1).¹ For example, reports of cases similar to the famous Cheltenham ghost (Morton, 1892), the Chaffin Will case ("Case," 1927), the Wilmot case (Sidgwick, 1891), and the Karin poltergeist case (Wijk, 1905) are less frequent now. So are case collection studies similar to *Phantasms of the Living* (Gurney, Myers, & Podmore, 1886), the latter work of Camille Flammarion (1900), and the work of Ernesto Bozzano (1923), and Walter Franklin Prince (1928), among others.

Although we have to be careful about generalizations made outside the English-speaking world, it is safe to say that the impact of J. B. Rhine's work was such that by the 1940s and 1950s the methodological emphasis of the field had turned from spontaneous cases and mediumistic studies to that of ESP and PK experiments. This change is clearly articulated in such books as Amadou's *La Parapsychologie* (1954), Pratt, Rhine, Smith, Stuart, and Greenwood's *Extrasensory Perception After Sixty Years* (1940), Carington's *Telepathy* (1945), and Soal and Bateman's *Modern Experiments in Telepathy* (1954). Although the research conducted in Europe maintained its own unique "flavor," there were also several examples of interest in the new experimental quantitative and forced-choice procedures that carried the prestige of America and the stamp of objectivity. Among these were studies conducted by Urban (1949) in Germany, Zorab (1957) in Holland, Perot (1965) in France, and Buscaroli (1954) in Italy. In Latin America, perhaps the best example is the work of Fernández (1953).

The new experimental parapsychology also brought a very critical atti-

¹ The figure also illustrates a slight increase of interest in spontaneous cases. However, the proportion of case studies in the 1990s may be deceiving as only half of a decade is charted as compared to the whole decades of previous years.

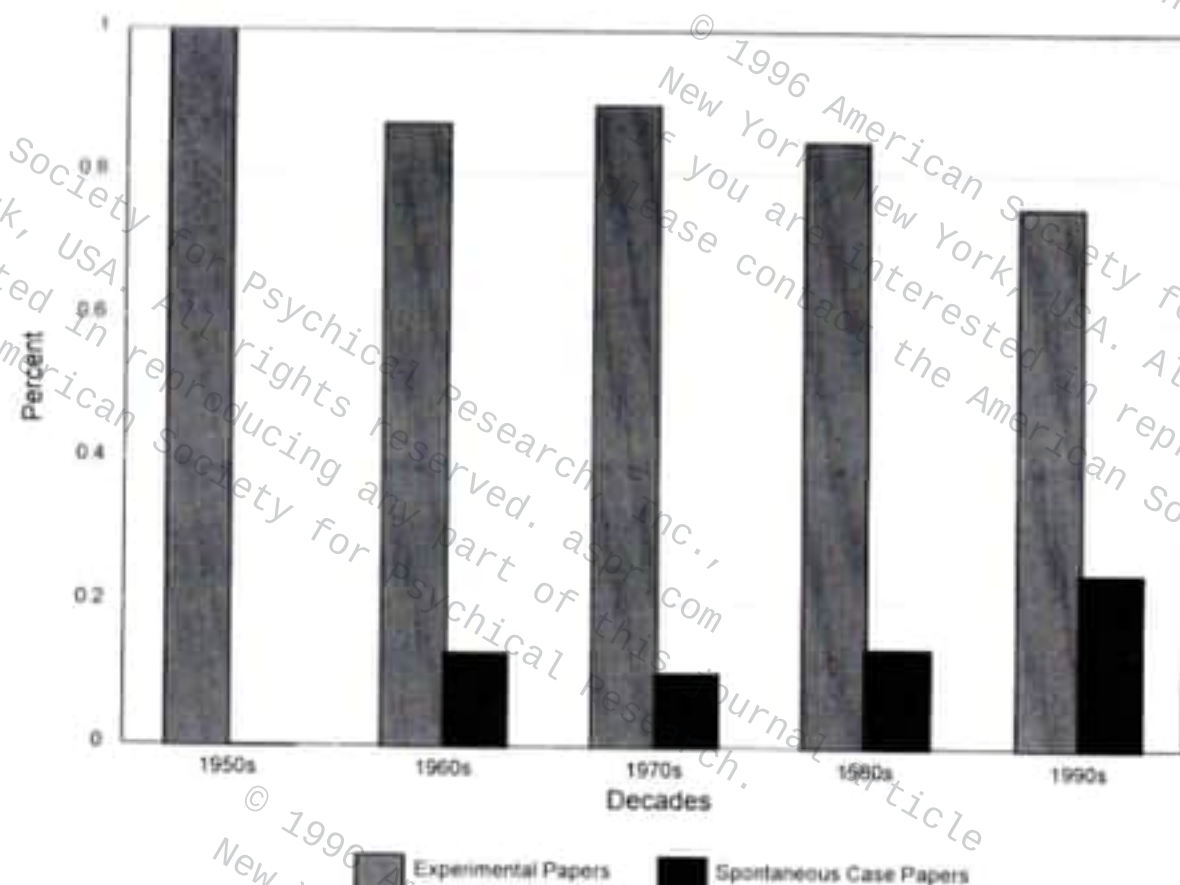


Fig. 1. Experimental and Spontaneous Case Papers in PA Conventions

tude towards spontaneous cases.² Ehrenwald wrote in 1947: "Contemporary psychical research justly prides itself with having outgrown the stage when it relied chiefly on anecdotal accounts" (p. 60). Both J.B. and Louisa Rhine described the change from case studies to experiment as the natural evolution of a scientific discipline from an underdeveloped period to a more sophisticated one (J. B. Rhine, 1937; L. E. Rhine, 1977). There is some merit in this perspective in terms of some of the advantages of laboratory studies, but this view of the history of science in general and of the history of parapsychology in particular is a biased one, which takes as its model the development of the physical sciences while ignoring the particular complexities, needs, and problems of the social and behavioral sciences.

Two influential papers that damaged spontaneous case research as a methodology were those of D. J. West and J. B. Rhine, both published in 1948. In the *Proceedings of the Society for Psychical Research*, West argued that the testimony involved in some reports of spontaneous phenomena was unreliable due to problems of memory and other issues. In the *Journal of Parapsychology*, Rhine contended that cases could never pro-

² Criticism of the value of spontaneous cases are not new. Some older examples include Innes (1887) and Podmore (1898).

vide evidence for psi and that they were important only in terms of providing ideas to guide experimental research. This viewpoint was articulated by both J.B. and Louisa Rhine in many publications (e.g., J. B. Rhine, 1955; L. E. Rhine, 1967) and was the underlying assumption for L. E. Rhine's case collection research (e.g., 1951, 1953). Later criticisms appeared in the books of Flew (1953), Amadou (1954), Rao (1966), and Thouless (1972), as well as in a variety of articles (e.g., Girolini, 1987; Hövelmann & Krippner, 1986). The fact that these viewpoints have been strongly questioned by Stevenson (1971) and Braude (1986), and that there has been much writing from other authors defending spontaneous case research (e.g., Alvarado, 1987a; Hearne, 1985; Murphy, 1953; Tyrrell, 1946) does not change the fact that many writers in the field have labelled spontaneous case research as methodologically inferior to experimental research. The consequences are clear: something labelled as flawed and inferior has low prestige and few followers. As a consequence, little research on spontaneous cases has been conducted in recent decades.

The issue of the value of spontaneous cases as evidence of a psi process is not a black and white one. Certainly there are plenty of problems, but what worries me is the narrow-mindedness of those who argue that the approach is of little value or useless. The utility of all that we do, including gathering experimental evidence, is relative, a matter of degree. Evidence for an ESP or a PK process may have higher validity and reliability in some experimental contexts, but that does not mean that spontaneous cases are never evidential. As Whately Carington pointed out in his book *Telepathy* (1945), the issue is not one of absolute proof. Carington goes on to draw an analogy with ethology and history, in which sources of errors not present in an experiment do exist but which still allow researchers to reach a certain degree of consensus knowledge of some reliability, within certain constraints. We may add to this the work conducted today in some areas of psychiatry as well as in the fields of sociology and anthropology. If other human sciences can use case studies as generators of useful knowledge, why should parapsychology be different?

Some may argue that case studies are too loose and imprecise to bring any significant knowledge to the field. But this is easily turned around—as I will argue later—when we consider the disadvantages and limitations of the tightness and precision of experiments. In fact, it is precisely such characteristics of case studies that increases their value as a research approach.

Others may argue that our phenomena are not credible and that the field is under constant attack and this is reason enough to adhere solely to an experimental methodology. But I would argue that we are too obsessed with one method and with issues of credibility, much more so than the social and behavioral sciences I have mentioned, which gather the bulk of their data from research done using nonexperimental methodologies. To allow political fears to force us to narrow our thinking about our own

subject is to narrow our conception of science, to adopt a position that is limiting in the extreme.³

Many writers have asserted that evidence is a matter of degree and that methodological pluralism is a desirable stance. Such statements, however, do not reflect the actual practice of our field. This is apparent from the low number of spontaneous case research papers included in this conference. In addition, like earlier writings (e.g., J. B. Rhine, 1948; L. E. Rhine, 1967; Thouless, 1972) that characterized spontaneous cases as hypothesis-generators, parts of the revival of recent interest in spontaneous ESP experiences focuses on their value as a correlate of test performance in laboratory experiments such as the Ganzfeld. Charles Honorton (1993) clearly articulated this argument when he said that spontaneous experiences are "likely to provide better predictors of laboratory success than many of the other kind of predictors that we have used up until now" (pp. 330-331). Thus, while important for experimentation, this perspective devalues spontaneous cases by casting them as nothing more than adjuncts to the experimental method.

Another indication of the low status of spontaneous case research is the way in which some of our textbooks describe it. In many of these books the authors do not question the capacity of experiments to create knowledge, whereas they systematically question the case approach. Rhine and Pratt's *Parapsychology: Frontier Science of the Mind* (1957), K. R. Rao's *Experimental Parapsychology* (1966), and Louisa Rhine's *Psi: What Is It?* (1975) are examples of this uncritical attitude towards experimentation coupled with hypercritical attitudes towards case research. Other books such as Edge, Morris, Palmer, and Rush's (1986) *Foundations of Parapsychology* and Broughton's (1991) *Parapsychology: The Controversial Science* similarly devote space to criticize the case approach without bothering to examine the experimental approach in the same way. Overall, this situation says much about the attitudes of some of our researchers towards spontaneous phenomena. Unfortunately, the criticisms they give us are one-sided. Instead, what we need are discussions that qualify all methods equally.

THE LIMITS OF THE EXPERIMENT

It is well known that the investigation of spontaneous cases suffers from a variety of problems such as the difficulty in assessing chance, memory problems of the experiencers, misidentification, fraud, and other issues. But there are many problems discussed in the literature that limit the

³ In addition, spontaneous experiences are valuable even if we do not pay attention to evidential or paranormal explanatory models. They can be studied as human experiences important for reasons other than paranormality and as opportunities to test for conventional hypotheses.

validity of experimentally-produced knowledge as well. Issues such as the context in which the individual is tested, the implicit demands of the experimental procedures and instructions, pressure to perform, the experimenter's personality and interpersonal skills, and the artificiality of the situation are some of the more obvious ones (Rosenthal & Rosnow, 1969; Weber & Cook, 1972). These are seldom discussed in our literature in the same detail that the flaws of spontaneous cases are discussed.

Another indicator of this attitude are differential reactions to criticisms. In his well known paper published in *Advances in Parapsychological Research*, Akers (1984) argued that modern experimental psi studies contain many actual and potential methodological problems as well as suffer from poor reporting standards. However, few researchers in the field seem willing to argue that such problems prove that the experimental method is flawed or that it should be abandoned altogether. But this is exactly how many have argued for years about spontaneous case research.

Not only should we recognize that a number of problems plague the experimental method, but we should also admit that its advantages are relative. Martin Orne (1971) has remarked that "laboratory observations are not of necessity better data—they are only data which are better controlled in a very clear sense but inevitably less controlled in yet another sense" (p. 260). Examples of what is rarely controlled in experimentation are the dynamics between subjects, experimenters and assistants, subject's anxieties at being tested or in reaction to the laboratory environment, and the unspoken motivations and needs of the subject and experimenter. Regarding the latter, Richard Broughton (1993) has written that "experimental parapsychologists have done little more than pay lip service to psi's need-serving character" (p. 23). This is clearly seen in the demands we put on our experimental subjects who are asked to do tasks that, at best, are not relevant to their psychology, needs, or motivations, and at worse, are perceived as meaningless. I do not mean to say that experimentalists in our field have been completely unaware of these issues or that they have not taken steps to improve the situation. There is, in fact, a considerable history in experimental parapsychology of attempts to bring experimental tasks closer to everyday situations.

For example, there is a modern experimental but qualitative tradition that developed during the 1940s and the 1950s partly as a reaction to Rhine's statistically-oriented approach. This approach, involving qualitative psychological and medical analyses, was considered by many to tap into "real-life" aspects of psi. Much of this work has been done outside of English-speaking parapsychology. Among these are Piero Cassoli's and Enrico Marabini's (1954) free-response work as well as that of Hans Bender (1957). In addition, there is Orlando Canavesio's (1947) EEG and medical studies of psychics and René Warcollier's continuation of his life-long free-response work (e.g., Warcollier & Khérumian, 1954).

Other examples of attempts to make the experiment more natural within the quantitative forced-choice research tradition include Fisk's (1951) use

of home testing, Ratte's (1961) use of competition tasks, Anderson's (1966) use of fantasy in ESP testing, Beloff's (1969) use of people in love as ESP subjects, and Johnson's (1973) testing of nonintentional ESP in the context of academic test taking.

Other strategies have focused on providing more naturalistic targets than playing of Zener cards. These efforts include theatrical dramatizations of situations related to the targets (Cassoli & Marabini, 1954), the use of multisensory aids to telepathic transmission (Krippner, Ullman, & Honorton, 1971), the use of pictures (Braud & Braud, 1973), the use of geographical locations in remote-viewing tests (Dunne & Bisaha, 1979), and the use of emotions as targets (Schouten, 1980). The reawakening of interest in free-response tests that started in the 1960s and continues to this day has brought us closer to naturalistic situations. This includes the ESP work done with dreams (Ullman & Krippner, 1970), partial sensory deprivation (Honorton, Berger, Narvoglio, Quant, Derr, Schecter, & Ferrari, 1990), and OBEs (Tart, 1968). An important chapter of this period of experimental research was Rex Stanford's PMIR model, the only research program designed to test assumptions directly relevant to naturally occurring psi in daily life (for a review and recent developments, see Stanford, 1990).

These, and many other methodological innovations, have changed our field. They have created a new experimental parapsychology more sensitive to the participants' psychology. Another kind of sensitivity that has occurred in recent years is reflected in attempts made by experimenters to relate to subjects as human beings and to make them feel welcome and as comfortable as possible within the constraints of the laboratory. The interest of experimenters in the psychodynamics of human interaction is generally not reflected in research reports and to a great extent, has to be experienced in order to be understood. But there are studies that have considered some of these variables explicitly. Bononcini and Rosa (1987) have investigated the group dynamics of clinical contexts, as has Carpenter (1988) as part of the ESP testing situation. In addition, Patric Giesler in his field experiments (1985a, 1985b) took the cultural context into consideration. Giesler's sensitivity to the context, needs, and beliefs of his subjects stands without precedent in the experimental psi literature.

However, it is one thing to talk about ecologically valid experiments in the sense of social and psychological sensitivity, and it is another to talk about the perennial problem of generalizing experimental results outside of the experimental context. No matter how sensitive our experiments are we are still dealing with conditions that mimic the "real world," that are artificial no matter how much we change the laboratory or measure the psychology of our participants. Granted this, what is the effect of these conditions on our findings? How valid is experimentally-acquired knowledge under these considerations? Can experimental findings be generalized to other contexts? Although these issues have long been discussed in psy-

chology by experimental researchers, they have not been resolved by any means.

I do not argue that experimental research is of no value because its evidence cannot be generalized to the outside world in anything but a limited way. But one has a right to ask if experimental findings are ever valid outside of the testing situation in which they were produced. I would ask this same question of those who simulate tornadoes in the laboratory and those who study the behavior of caged animals. This is precisely the crux of the issue when one deals with the knowledge claims that most of experimental psychology and parapsychology present. If psi experimental results cannot be generalized beyond the confines of the experiment, we are certainly dealing with a sad and limited science indeed.

Considerations of this sort are not new in parapsychology. Old discussions of the table levitation studies of W. J. Crawford (1916) by Bozzano (1926/1991) and by Sudre (1956/1960) have argued that the mechanical characteristics of PK "discovered" by Crawford were the creation of his own ideas and methods. Others, such as Pierre Lebedzinski (1924), have argued that the phenomena of spirit controls and communications are shaped by the way they are studied and by the assumptions and beliefs behind those involved in their study. Can we fault the experimental method used in ESP and PK research in this way?

Although there is evidence of experimenter effects in psi testing (White, 1977), we do not know for sure if the actual features of psi in the experimental situation have been affected as well. Nonetheless I would argue that discussions of so-called psi-effects should be viewed cautiously before one announces them as evidence of the nature or characteristics of psi, as has been done in the past (e.g., Rhine & Pratt, 1957). I am referring here to position effects, displacement, psi-missing, consistent missing, differential and preferential effects, and the more recent concept of signatures. Although one may speculate on how some of these "effects" may express themselves in real-life situations (e.g., L. E. Rhine, 1965), I think we should seriously consider the idea that these findings may say more about the context and situation of testing than about the nature of psi. This may be particularly the case for decline, position, and differential effects in which the effects in question may be reactions to the monotony and constraints of the experimental conditions. In summary, to quote Rex Stanford (1990) from a slightly different context, "the deliberateness and self-conscious ego-involvement of typical laboratory tasks might distort research outcomes in various ways" (p. 54).

THE ADVANTAGES OF SPONTANEOUS CASES

A unique advantage of spontaneous case studies is that they allow us to see if experimental findings can also be found outside of the experimental context. This approach may be valuable in helping us decide if what some

take to be intrinsic characteristics of psi are merely findings related to the context of experimental testing. By varying the context of occurrence we may be in a position to approach psi from convergent perspectives. When I propose this I do not mean to use spontaneous cases as an auxiliary to the experimental method but to approach spontaneous case research as a valuable tool in its own right, a tool capable of providing useful knowledge about psi phenomena. For example, both Tyrrell (1946) and Ehrenwald (1947a) criticized Rhine's reliance on card-guessing as a basis from which to speculate on the similarities of ESP to perception. They argued that the variety of forms of expression in spontaneous experiences showed a different picture, one more complex than the one gleaned from Rhine's writings.

A comparison of findings from poltergeist research to those of experimental PK is another illustration of this issue. Studies of spontaneous movement of objects have suggested that in some cases the movements attenuate with distance (e.g., Roll, Burdick, & Joines, 1973). This does not appear to be the case if only PK findings from the laboratory are considered.

Similarly, we may question the validity and generalizability of other findings about psi in the experimental context. Several ESP studies that have focused on such psychological variables as belief in ESP, extraversion and introversion, and other personality variables may not be telling us much about ESP itself; rather, they concern the ability subjects have to perform in an artificial situation. For example, forced-choice tests conducted to determine whether or not psychotic patients exhibit ESP (e.g., Zorab, 1957; for a review, see Rogo, 1982a) do not show much evidence of psychic functioning, a finding that has led some to conclude that ESP is a normal ability, or at least, one unrelated to mental illness (e.g., Rhine & Pratt, 1957). But does it make sense to test schizophrenics using forced-choice tests that require attention and interest in the task that are not characteristic of psychotic patients (Rogo, 1982a)? In contrast, the clinical but evidentially problematic and ambiguous observations of psychiatrists such as Jan Ehrenwald (1947b) suggest that ESP functioning in psychotics may interact with the syndrome in question and with the needs of the individual. To suppose that such phenomena will transfer to an irrelevant and extremely artificial forced-choice testing situation or even to any experimental situation shows a lack of sensitivity to the dynamics of both psychopathology and psi phenomena. Similar problems exist with experimental research that has attempted to study psi performance in relation to neuroticism and to other psychological variables.

Similarly, the same may be said of attempts to make generalizations about the distribution of psi among different cultures, such as Foster's (1943) experiments with Native Americans and Rose's work (1951) with the aboriginal peoples of Australia in which both researchers took forced-choice testing methods to the places where these people live. The lack of

cultural relativity displayed in the use of Western testing approaches has been criticized by many, including Pobers (1956) and Giesler (1984).

These examples are not recent ones, but we may extend the point to Ganzfeld studies and other experimental situations in which personality and cognitive variable questionnaires are administered.

To complement experimental studies of psi and mental health we may follow psychiatry's example. Psychiatry has long been known for its use of nonexperimental methods to complement laboratory and clinical trial findings. For example, the case collection studies of multiple personality patients (e.g., Ross, Miller, Reager, Bjornson, Fraser, & Anderson, 1990) and the survey studies of the distribution of dissociative phenomena among the general public (e.g., Ray, June, Turaj, & Lundy, 1992) have been essential to increasing our understanding of certain psychopathologies as well as assessing the distribution of experiences with possible relationship to psychiatric concerns in nonclinical populations. There are always evidential problems with case collections and survey studies, of course, but these problems can be handled with care and common sense. We could take a similar approach to the relationship between psi and mental health or between psi and adjustment problems. The literature already has examples of case studies that show possibilities for research. William Roll's (1970) poltergeist studies are an example, as are the cases studied by Hyslop (1909) and by McHarg (1973), in which the psi components of apparent hallucinatory experiences are considered. I hope that this approach will develop into the acquisition of a large number of cases as well as extend to more single case studies.

In regard to anthropological issues, spontaneous case research also provides us with opportunities to learn from cases about the cultural and psychological correlates of psi. For example, Stevenson's (1983) comparison of American and Indian reincarnation-type cases provides cultural insight as do McClenon's (1994) more recent studies with Chinese, Japanese, and American students. Piero Cassoli (1955) in Italy has conducted single case studies as has Patric Giesler (1982) in Brazil. The methods used in these studies are more sensitive to cultural and social dynamics than the experiments above mentioned. In addition, Cassoli's study underscores the importance of religious beliefs and the family context in understanding aspects of a case of religious visions, healing, and a crying crucifix (more about this later). Giesler's paper uses symbolic anthropology, which allows him to explore issues of meaning and social context in a poltergeist case that occurred in a ritualistic context. The investigation of deeper issues of this sort are practically impossible to study in the laboratory.

The point here is that experimental results can only draw half of the picture. In some cases nonexperimental approaches constitute a methodology that is superior to experimental ones because we can document the richer context of social and psychological variables in which the phenom-

ena are reported to occur. The deeper insights we gain are well worth the loss of strict precision and control over some of the relevant variables.

We can also discuss the limits of the experimental method in terms of the phenomena it is not suited to investigate, among which are telepathic impressions, apparitions, reincarnation-type cases, deathbed visions, non-recurrent PK, poltergeists, and near-death experiences. As Tyrrell said in 1946: "The truth is that we can apply the orthodox method of science to a corner of the psychical field, but not to the whole" (p. 232). In some cases, such as the Maher and Hansen (1992) study of a haunting, semi-experimental conditions can be achieved. But overemphasis on experiment as the royal road to knowledge means that in practical terms, many of the fundamental phenomena of parapsychology remain unexplored, forgotten, or neglected because they cannot be constrained to the method. It is likely that is why these manifestations have been ignored in recent years. Instead of attempting to study the phenomena that puzzle people and that are so important to their beliefs, we have chosen to develop a parapsychology limited only to that which is easier to handle, that which mirrors the so-called respectability and exactness of physics and experimental psychology. We have labelled such approaches as rigorous and cost-effective. But, in fact, we are paying a high price for our preference for experimentation. The bulk of our research, confined as it is to experiments, cannot tell people much about their experiences. We are utterly ignorant about basic knowledge of the phenomena that we should be able to explain, or at least understand better (on this issue, see White, 1985).

Of course the goal of many who conduct experiments is precisely this general level of understanding. I do not deny the value of this. But perhaps Karlis Osis (1985) described the situation accurately when he wrote: "We must keep our scope small in order to make experiments manageable Unfortunately, habituating to a small scope also tends to narrow our thinking so that, like a spot-light, it covers only a small chunk of reality" (p. 110). A consequence of such narrowing, something which is one of the accomplishments of Rhine and his associates, is a parapsychology defined mainly as the study of ESP and PK, as operationalized in its most limited form to suit the experimental context. But what about our understanding of the rich human experiences people have in their daily life?

Our field pays very little attention to phenomena that have historically been of key importance in the shaping of human affairs. I am talking about the influence or interaction of claims of psychic phenomena on beliefs, practices, religions, and various social and political institutions throughout history. Some of these include beliefs in the healing powers of some European kings (Bloch, 1961/1973), apparitions of the Virgin Mary (Zimdars-Schwartz, 1991), the miracles of saints (Ward, 1982), and "epidemics" of religious revivals and collective possessions reported in previous centuries in Europe (Knox, 1950). Socio-historical analyses of spiritualism show how beliefs about and the practices of mental and physical medium-

ship interacted with the issue of the position of women in society (e.g., Braude, 1989; Zingrone, 1994).

Recent survey studies have shown that a significant proportion of the population claim to have had psychic experiences. Haraldsson and Houtkooper (1991) summarized recent research with nationally representative samples of the populations of 13 Western European countries and the United States. In all these surveys three questions were asked about telepathy, clairvoyance, and contact with the dead. Although ambiguous in their interpretation, the results indicated that about 100 million Western Europeans and about 145 million Americans reported at least one of these experiences.

Unfortunately, most parapsychologists refuse to address these claims directly and even take great efforts to distance themselves from such phenomena and those who experience them. Regardless of the millions of individuals who have experiences in daily life, our studies continue to focus on artificial environments and methodologies that, at best, address the experiential issues only indirectly.

Even when some seemingly psychic experience can be studied experimentally, our studies lose such important dimensions as meaning and intensity. As Stevenson (1981) has argued:

We can observe the effects of strong emotion much better in spontaneous cases than in laboratory experiments. The important events in life that generate strong emotions do not happen in laboratories, or not often. I am referring here to such events as serious illness, accidents, other stresses, and—above all—death." (p. 132)

Compare, for example, the ESP dreams of disasters Barker (1967) and Stevenson (1970) studied to the less emotionally-engaging themes of the dream telepathy experiments (Ullman & Krippner, 1970). In addition, the affective and perceptual qualities of out-of-body experiences studied in the laboratory (e.g., Tart, 1968) pale in comparison to those of spontaneous OBEs collected by such researchers as Celia Green (1968). The issue here is not that we are dealing with different phenomena, but rather that by confining our knowledge to the phenomenologically simpler and/or qualitatively different experiences that occur in experimental contexts, we may not understand those that happen in real life.

It is precisely in the study of the richness and variety of these experiences that spontaneous case research is at its best. As seen in Figure 2, when ESP experiences are examined it is possible to gather information on the theme, form, type, and incidence of the experience, among others (Irwin, 1994). Figure 3 provides a more detailed view of the varieties of one of the variables, the form of the ESP experience. All such variables can be fruitfully studied without the constraints of the experimental method and with attention to complex variable interactions, as Sybo Schouten (1979, 1981, 1982) has done.



Fig. 2. Aspects of Spontaneous ESP Experiences

Our studies can tell us much about the distribution of particular claims. I have compared the results of four survey studies conducted with college student samples using Palmer's (1979) questionnaire (see Figure 4). These studies, conducted in Brazil, India, and in the United States, give us an idea of the frequency of claims of specific phenomena. The figure shows that waking and dream ESP are the most common claims, while auras and movement of objects are reported least often. In addition, other studies have started to address the issue of interrelationships between experiences (e.g., Alvarado & Zingrone, 1994; Palmer, 1979), and I hope that in the future there will be much more to be said about the overall structure of spontaneous psi functioning.

The study of poltergeists is another area that yields much about phenomenological and contextual aspects of the experience not possible to obtain in other ways. Single case studies such as the Karin (Wijk, 1905),

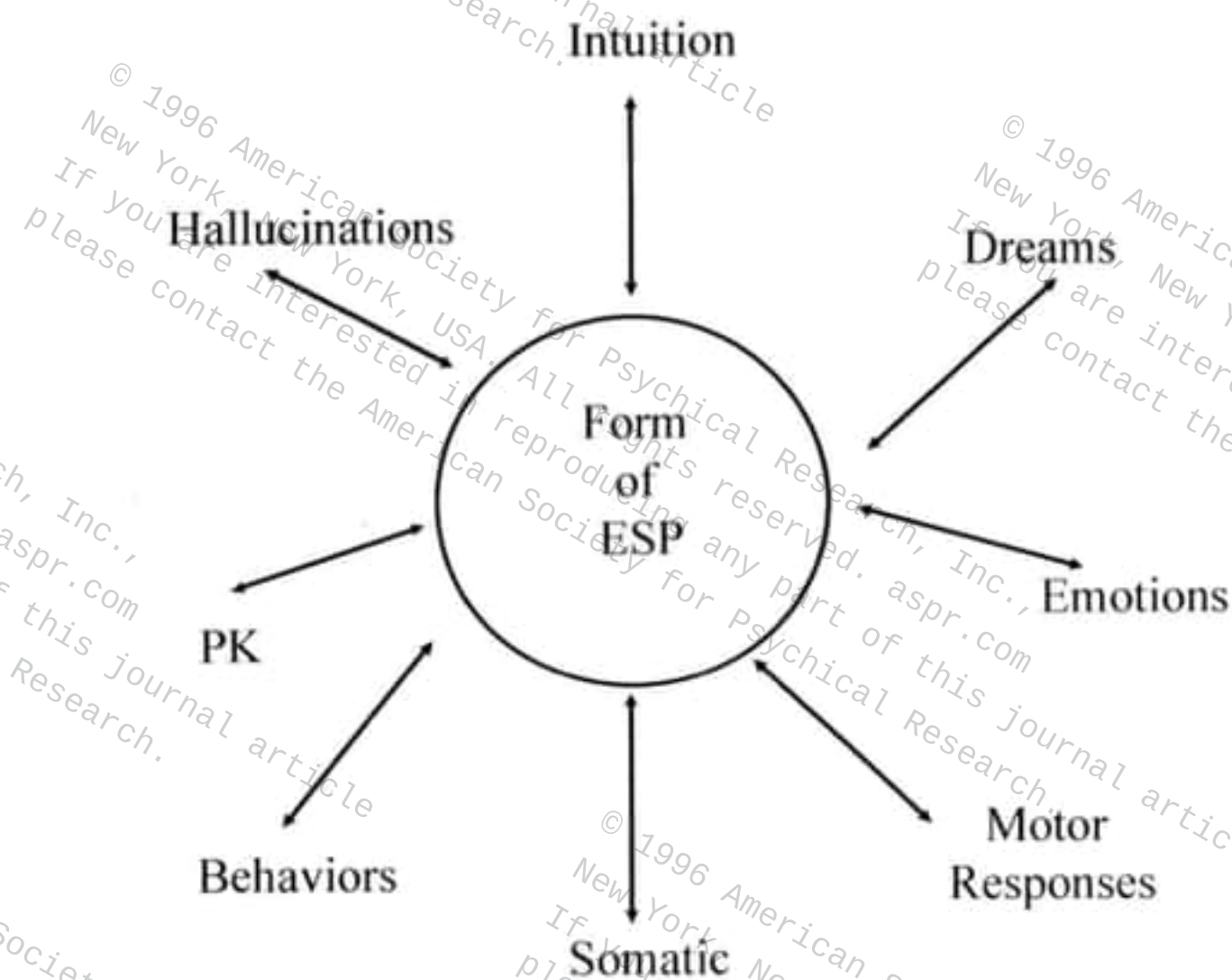


Fig. 3. Varieties of Form of ESP Experiences

Seaford (Pratt & Roll, 1956), Miami (Roll & Pratt, 1971), Rosenheim (Bender, 1968), and Guarulhos (Andrade, 1984) cases are invaluable in their depiction of both the features and the social complexities of such phenomena. Similarly, pattern analyses presented in the case collection studies of Cox (1961), Roll (1977), Gauld (Gauld & Cornell, 1979), and Huesman and Schriever (1989) provide even a more complete perspective of the features of this phenomenon.

Another example is the study of recurrent ESP dreams. Marabini (1956) had the chance to study the recurrent ESP dreams of a single percipient. He focused on their symbol content and similarities with non-ESP dreams. Schriever (1987) and Stevenson (1992) also underscore the phenomenological richness of ESP in recurrent dreams in their studies.

In short, I would like to state the obvious: To understand the spontaneous we need to study the spontaneous. There can be no substitute. Unfortunately, most of the research conducted in recent years has neglected the obvious.

Such neglect is not only unfortunate because of the means-centered approach prevalent in parapsychology today, but because it encourages us to fail to acknowledge or to fail to care about our social responsibilities as

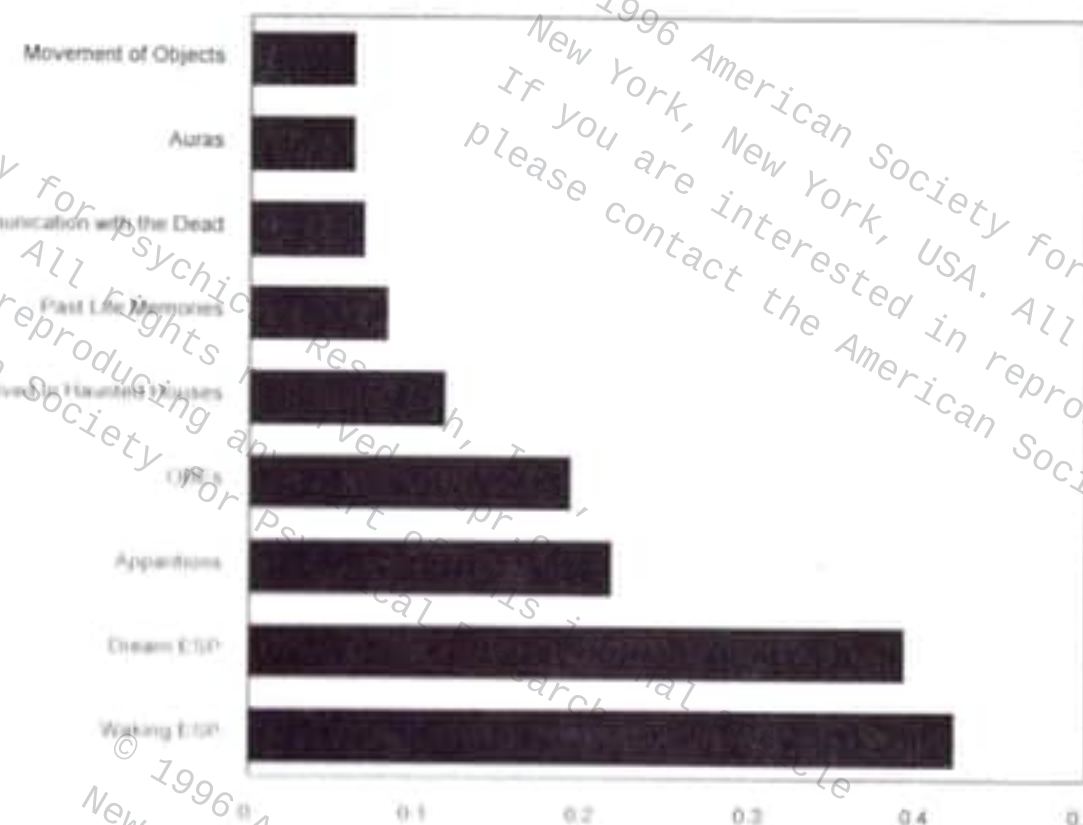


Fig. 4. Median % of Experiences with Student Samples Using Palmer's Questionnaire
 Note: The studies listed in the figure were conducted in Brazil (Zangari & Machado, 1994), India (Chelha, Alvarado, & Sahni, 1987; Usha & Pasricha, 1989) and in the United States (Palmer, 1979).

researchers. People who report experiences of interest to us want to know what such experiences mean to them. How many times have we all been asked to explain experiences and we have not had a clue about what to say, except offering the usual virtually useless qualifications? I have been in this position many times as people have claimed past-life memories, ESP experiences, near-death experiences, auras, and other phenomena. We have all gone through these experiences. We all have our songs and dances to deal with them. First, we say little research has been done about that topic. Or, if we are honest, we just say we do not know. Second, we list normal explanations, emphasizing that neither spirits nor psi are necessarily involved, and on we go. The most frustrating part of all of this is not knowing what to say because we ourselves, as a field, have made the choice to remain ignorant. Why not put ourselves in a better position to counsel people by conducting more research on the subject, including spontaneous case research?⁴

This is not to say that we have not acquired any knowledge about these phenomena nor that we cannot counsel people even though we need to know more about these phenomena. But as parapsychologists, we can answer very few of the basic questions experienced ask us about the nature, meaning, and purpose of their experiences. These topics are discussed in every issue of *Exceptional Human Experience*, edited by Rhea White.

Our neglect of spontaneous cases is intimately connected to our conceptions of the purpose of science in general and of parapsychology in particular. Modern science, including the social and behavioral disciplines, have become so academic, fragmented, and specialized that the purpose of the research enterprise is not always clear. As argued by philosopher Nicholas Maxwell (1984) regarding science in general, and to some extent by Abraham Maslow (1954) in psychology, and by Emily Cook (1991) in our field, there has been a separation between a concept of impersonal and objective knowledge and one in which we seek knowledge related to our human concerns, the things that are of value in life for us. This includes assessing our place in life and in the world through understanding its phenomena and through applying what we learn to solve our problems. As such, this does not presuppose any particular method of inquiry. But it is a particularly useful perspective from which to argue for the use of spontaneous case methods in the study of phenomena generally perceived to be connected to such human issues as spirituality, survival of bodily death, personal transformation, and mental health. Many of these phenomena are best studied outside of the laboratory, although the motivation for some laboratory studies does come from such concerns. A well-known example of the latter is the role that concepts on nonphysicality had for J. B. Rhine (see, for example, *The Reach of the Mind* [1947] and *New World of the Mind* [1953]). Rhine justified the classic card and dice tests on the basis that results with these procedures could say much about the existence of nonphysical aspects of human beings. One may argue—as was done by Ferdinando Cazzamalli (1954)—that Rhine was asking too much from mere card games. I would also argue that if we are going to generalize our results to these issues we should do so by paying more attention to the spontaneous phenomena that originally inspired such concerns in the first place.

There are many areas that may be examined with these purposes in mind. One of them is the study of aspects related to religion and parapsychology as reflected in miracles of the Catholic, Islamic, and other traditions. With a few exceptions, this topic is not part of the agenda of modern parapsychological research. But these phenomena have been reported and have been immensely influential around the world. Such authors as Quedo (1969/1971), Raet (1970), Réginald-Omez (1956/1959), Rogo (1982b), and Thurston (1952) have discussed aspects of these phenomena and the conceptual issues associated with them. Unfortunately these attempts offer little in terms of patterns or process. A common question has been: Is there anything special about religious contexts for the manifestation of psi? There are indications from the observations of Leroy (1928) on levitation, of White (1982) on ESP phenomena, and from my own study of luminous phenomena (Alvarado, 1987b) that the phenomena reported to occur around Catholic saints are of a higher magnitude than the phenomena reported around individuals in secular contexts. But our knowledge is too limited to say more about this issue.

Other phenomena such as Marian apparitions, a topic included in Dean Radin's 1993 Presidential Address to the Parapsychological Association, may allow us to study the religious context more directly through field studies of active cases. One model to follow are the studies conducted in recent years by a group of French and Italian researchers who have investigated the psychophysiology of children who claim to see and hear the Virgin Mary (Gagliardi & Margnelli, 1989; Laurentin & Joyeaux, 1985/1987).

And of course, there is the perennial problem of deciding what is the nature of reported miraculous healings, visions, incorpation of the body, bilocations, and other phenomena. Can they be explained by recourse to such conventional principles as suggestion, hallucinations, and misperception? Do they require parapsychological explanations? An interesting example of possible approaches is Piero Cassoli's (1955) case study. Cassoli studied a 17-year-old Italian girl named Angelina Ronza who claimed to have had visions of the Virgin, seemingly was healed of gastric disturbances, and had a crucifix that seemed to weep. After a careful analysis of the testimony and through personal observations, Cassoli leaned towards a paranormal explanation of the crucifix phenomena. But the most interesting part of his study was the social psychology of the household in question. Cassoli's investigation not only revealed that Angelina lived in a context in which the Virgin's miracles were discussed and accepted but that Angelina's own phenomena started after she was given a relic related to a previous well-known case of a weeping statue of the Madonna. In other words, Cassoli was able to make a good case for the idea that the social context in religious phenomena is important in the induction of the phenomena and in the determination of its phenomenology. Such case studies are both basic to our understanding of such manifestations and also are essential to our ability to assess the cultural and social dynamics of the context of occurrence of these phenomena.

The issue of miracles is related to another matter of concern to the general public, the relationships between spirituality and psychic phenomena. Spirituality is difficult to define, but I would argue that this is one of those topics of great importance that need to be addressed using the life history methods employed by sociologists. Rhea White (1982) has noticed that some reports of psychic phenomena in Catholic saints "may have been associated with the degree to which they had advanced in their mystical lives" (p. 18; see also Grosso, 1985). A clue to this may be gleaned from the study of systems of mysticism and their practice along the life course of individuals for whom unconditional love, charity, and a sense of mission are a form of everyday life. As seen in Melquiades Andrés Martín's (1975) study of Spanish mysticism for the period between 1500 to 1700, this tradition emphasized a life-long commitment to contemplation and involved transformations of the self and the integration of the concepts of Christ and of God within the mystic's essence by overcoming both the mind and the body. As Felice Masi (1994) argued in a discussion of

stigmata: "The Cristic idea of love, passion and redemption is incorporated in the whole individual . . . the psychological aspect is not a normal terrene one, it is not egocentric and worried with day to day concerns" (p. 26, my translation). These transpersonal dimensions cannot be measured by our psychological instruments. But they may be addressed through analyses of the life histories of the mystics and their situational concerns. Instead of neglecting them, we should realize that these problems have been of interest for millennia in many mystical and occult systems and are still important to many people around the world.

Some concerns of experiencers are unrelated to religious issues. For example, some people want to know why they have so many experiences or even a single one and whether they are different from persons who do not have these experiences. Here is where our individual differences research may offer something to experiencers and at the same time allow us to conduct investigations based on particular hypotheses or models. Research on the cognitive correlates of psi and psi-related experiences such as Irwin's (1979) research on coding style, Richard's (1990) research on hypnotic susceptibility, and my own research with Nancy Zingrone on vividness of visual imagery and fantasy proneness (Alvarado & Zingrone, 1994) will eventually help us to assess aspects of the psychology of psi claims in naturalistic contexts that may lead to the development of testable models. This research may also help us to explain why some people have more experiences than others, one of the concerns of the experiencers. In addition, case studies offer the unique opportunity to consider complex interaction models by including contextual variables in the mix in addition to personality and cognitive variables. We need to know how contexts that engender belief and intense emotion such as religious settings, séances, and solitude affect psychic phenomena. Do such contexts interact with other psychological variables? We generally focus on simple models in which we correlate a measure of a psychological variable to psi experiences. But the situation could be more complex than that. The influence of cognitive and personality variables such as absorption and neuroticism may be context dependent in those natural settings in which psi is reported. Kelly and Locke's (1981) socio-cultural model of altered states of consciousness and psi and Irwin's (1992) recent model in which psychic experiences are seen as a function of an interaction of life experiences, beliefs, and cognitive variables are examples of this approach.

Models like this are common in many areas of psychology. They remind us that it is important to get into the life pattern of the individual, into his or her needs, motivations, and interpersonal relationships as Bender (1966), Coleman (1964), Ehrenwald (1955), and Roll (1970) have argued. This can include the meaning the experiences have for the life course and the self-concept of the experiencer. I am still impressed by a statement I taped in an interview over two years ago with a frequent experiencer who talked about how psi was an integral part of herself. She said: "Psychic experiences are a part of me as is my skin, my eyes, my hair, but even more

profoundly. Because my skin, my eyes, are going to disappear, while all these things do not disappear, they continue, they continue." Such statements help us understand that spontaneous case research opens the way to the study of what Rhea White (1993, p. 62) calls the "ripple effect," that is, the impact and importance of psi experiences throughout the life course of the experiencer. Such perspectives as these can directly address the basic questions experiencers ask us that we are completely unable to answer.

Another concern of many experiencers is that their experiences are an indication of mental health problems. Research that addresses these concerns includes McCreery and Claridge's (1995) study of the relation of OBEs to schizotypy, Persinger's (e.g., 1984) studies of psi experiences and temporal lobe symptomatology, and Ross and Joshi's (1992) study of a variety of psi-related claims and their relationship to dissociation and traumatic experiences. But parapsychologists seem reluctant to conduct such studies. Similarly, we seem to be reluctant to mount a research program of great practical importance, which would be designed to develop empirically-derived criteria for differential diagnoses between, say, schizophrenic delusions and ESP experiences. Over a century ago, Edmund Gurney reported some general differences between telepathic and subjective hallucinations in *Phantasms of the Living* (Gurney, Myers, & Podmore, 1886, Vol. 1, pp. 496-502), and the topic has received recent consideration in the writings of West (1960) and Marabini (1992). But there is a dearth of empirical studies on the subject. Such a topic might be better studied by using the case comparison approach, such as that used frequently by psychiatric researchers who have systematically compared different syndromes in terms of antecedent conditions and basic features.

SPONTANEOUS CASES AND EXPERIMENTS IN PERSPECTIVE

Up until now, I have argued for the advantages of spontaneous cases and defended their value as a scientific approach to the study of psi. In particular, I have argued that the study of cases is fundamental to parapsychology because it offers unique insight into a wide range of specific phenomena via the experiences themselves. I have also argued that the study of cases allows us to address issues of social concern embodied in particular claims (such as miracles) as well as to address such wider issues as spirituality, mental health, and the underlying causes of these events in people's lives. Spontaneous case research can work together with the experimental approach to attack the problematic issue of generalizability of laboratory results to other contexts as well as to present the possibility of systematically contrasting findings that may highlight the impact of context on the findings.

The issue as I see it is not one of competition between methodologies, although I realize that my comments may have suggested such a perspective, especially when I have mentioned the ways in which case studies can

be superior to experiments. Rather, my view is one of methodological equality, to the extent that I believe that some aspects of a problem require different approaches and that methodologies should be chosen on the basis of their strengths and weaknesses.

In criticizing experimentation, I have pointed out some problems of generalizability of laboratory results as well as problems with validity of laboratory findings as they are affected by the socio-psychological concerns of the immediate social context of experimentation. But we have to be aware that similar problems exist with case study methodology as well. The context in which cases are published or collected in case collection studies, the situational variables inherent in an interview, or the mode and style of presentation of a questionnaire can affect the validity and generalizability of the information collected. Patric Giesler (1984) has pointed to some problems in anthropological fieldwork that can bias results when the supposedly impartial observer imposes his or her views on ethnographic studies. In addition, there are the widely differing contexts in which spontaneous phenomena are reported to occur—contexts such as séances, moments of solitude, experiences of being near-death, or religious settings. These do not necessarily generalize to all real-life contexts.

Although there is some evidence from different areas of psychology that naturalistic studies may obtain different results from those conducted in more artificial laboratory conditions (e.g., Willems, 1969; Yuille, 1985), some areas show better agreement (e.g., Locke, 1986). The issue is not clear in parapsychology. Here I can only speculate. What would be the appropriate course of action when a survey produces findings at odds with those of an experiment? Perhaps the nonexperimental finding is the problematic one because of a lack of construct validity in the measures used. On the other hand, an experimental study may have missed the point by using a task irrelevant to the issue in question. Such problems deserve more attention and empirical exploration.

We need to proceed along the lines of Campbell's (1986) and Maslow's (1954) assertions that it is more important to think in terms of what is appropriate to a problem rather than preferring a particular method for its own sake. In parapsychology, I would argue that once we have identified and defined a problem, we should develop research programs that are multi-methodological. We could do much more by studying our phenomena from different perspectives, picking and choosing approaches according to their strengths and weaknesses. This is what Tyrrell in 1948 called "convergent synthesis," or the act of "examining, comparing, and reflecting upon all types of evidence which have any bearing on the subject" (p. 41). One way to do this might be to include a combination of spontaneous and experimental approaches in the same study. Haraldsson (1975) did this when, within the same study, he correlated both ESP test scores and spontaneous precognitive dreams to frequency of dream recall. Another way to do this might be to implement a general program of research that includes both experiments and case studies, such as Irwin's (1981)

work on the relationship between absorption and out-of-body experiences and Persinger's study of ESP and its relation to the earth's geomagnetic field (Persinger, 1987; Persinger & Krippner, 1989). Several of the recent Ganzfeld studies could follow a similar approach and investigate both the claims of ESP experiences as recorded on experimental intake forms and ESP test scores in relation to scales of personality variables. Even though the data to do this already exist in a number of laboratories, the experimenters who collected these data seem interested only in the experimental results and their correlations to the psychological measures. Similarly, when the experience claims are used, it is only to predict performance during the experiment. A convergent approach would require correlation of the scales of personality variables both to test performance and to spontaneous experiences.

Another example of a possible convergent approach would consist of a combination of methods that variously emphasizes the experiment or field settings. Giesler's (1984, 1985a) psi-in-process approach does exactly this. He conducted experiments in a culturally sensitive environment, utilizing detailed ethnographic information in his design. He correlated experimental psi scores to information obtained through observational methods to obtain findings more sensitive to the participant's social context. It is also possible to conduct controlled observations and quasiexperiments during field studies, such as that done in poltergeist and haunting cases studied by Roll and Pratt (1971) and Maher and Hansen (1992), as well as in the recent psychophysiological studies of children claiming to see Marian apparitions (e.g., Laurentin & Joyeaux, 1985/1987).

Much more could also be done to test hypotheses derived from case studies in experiments as well as to use case research to confirm experimental findings. For examples, experimental work has a track record of studies that relate ESP to extraversion, belief in ESP, perceptual defensiveness, neuroticism, and altered states of consciousness, among other variables. More could be done in spontaneous case research to study these relationships. Similarly, experimental work could be carried on in which participants are categorized according to their predominant form of ESP, the frequency of their experiences, and the type of experiences they claim. In addition, these experience claims could be correlated against nonpsi aspects of experimental procedures. For example, I would argue that reported alterations in consciousness and body image in the Ganzfeld correlate positively with previous mystical experiences and OBEs, as these phenomena also involve alterations in consciousness and the sense of self. In addition, if ESP scoring in the Ganzfeld is related to the ability to enter internal attention states, perhaps a phenomenon like the OBE would be a better predictor of ESP than would phenomena like waking ESP or the experience of seeing apparitions.

The issue, then, is one of doing our best to understand psi across different contexts of occurrence and using different methodologies, a strategy

also suggested recently by William Braud (1994a) in an article about the conceptual and methodological expansions of our science.

Although it may not be realistic to expect every individual to employ such broadened approaches, I hope that we may collectively be able to increase the number of spontaneous case studies and pursue the integration of methodologies I have suggested. But I would like to stress that much more basic work needs to be done on spontaneous cases. Sometimes I feel we spend too much time talking about the importance of spontaneous cases—as I have done in this address—and too little time doing case research. More of us need to practice what we preach. To start with, we need to improve the level of sophistication of spontaneous case research. We need to move away from proof-oriented case studies, that is away from the presentation of just one more evidential precognitive dream or one more evidential poltergeist case. Although there is always a place for proof considerations, much more needs to be done on process, and many more specific research hypotheses and models need to be tested (Watt, 1994). Without process-oriented hypothesis-testing, case studies will continue to be meaningless anomalies that seem unrelated to other aspects of nature in general and human functioning in particular.

We must also do more to improve and standardize our methodology. In the case of survey studies this means we should try to get a better understanding of the meaning of responses made to our questionnaires. Most of these studies do not collect descriptions, nor are they survey responses followed up by interviews. Instead, we rely on simple yes and no answers that may be deceiving. This is not only because the cases are not evidential, but because the experiences respondents have in mind may not conform to our definitions of specific types of psychic phenomena.

In addition, we need to pay attention to methods used in other disciplines, such as participant observation and experiential sampling, among others, as well as to statistical techniques that will help us better understand the phenomenology of our cases (e.g., cluster analysis) and the relationship of different possible causal factors (e.g., different forms of path analysis).

All of this can help us address systematically what Gardner Murphy (1955) has called the "natural history of the paranormal communication process" (p. 98) and to provide the field with opportunities to bring experimental and case studies closer together.

CONCLUDING REMARKS

To conclude, I would like to emphasize that case studies in parapsychology are important for at least three reasons. First, this line of research forces us to pay attention to people's spontaneous experiences, which are unfortunately neglected in the modern research literature on the subject. As a consequence, we will be more aware than we are now of the human and

social relevance of our field. Second, it confronts us with the fact that there are areas and characteristics of psi that cannot be studied in any other way. In fact, the very characteristics of the phenomena demand approaches other than the experimental studies that predominate in our field today. Third, case studies remind us that psi phenomena are too complex and have too many dimensions to be constrained exclusively by the experimental method or solely by the parameters of case studies. Our science needs both the exactness and rigorous hypothesis testing of experimental work as well as the more expansive and contextually sensitive methodologies of the social and behavioral sciences. Parapsychology must understand the value and necessity of pluralistic approaches to the study of psi phenomena which, after all, occur both in life and in the lab.

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