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## THE VALUE OF SPONTANEOUS CASES

by CAROLINE WATT

### ABSTRACT

The collection and study of spontaneous cases has so far failed to realize the full value of these cases as a source of information about the factors which cause people to have experiences which they interpret as psychic. This may in part be due to an incomplete use of the research tools of observation/description, hypothesis, prediction testing and theory. Studies using 'top-down' and 'bottom-up' research strategies have been informative as far as they go but could go further in exploiting the potential informative value of spontaneous cases. It is argued that an 'integrated' approach, making full use of the research tools, is most likely to advance our understanding of people's spontaneous experiences.

### INTRODUCTION

A woman with a rare blood group, who is a blood donor, woke up at about 5 a.m., woke her husband, and said they must immediately go to London because there had been a train crash in London and her blood would be needed. She eventually felt that she had just had a very vivid dream, but she was shaken to hear later that there had indeed been a serious rail accident that same day, at Clapham Junction.

This report, from the *SPR Newsletter Supplement* of April 1989, is typical of the sort of puzzling experiences which can spontaneously occur to people, and which they may consider to be inexplicable by any normal process of communication or inference. Today, the bulk of parapsychological research is conducted in the laboratory rather than in the 'field', and it seems that the laboratory set-ups are far removed from everyday life. Yet reports of individuals' spontaneous psychic experiences form the bed-rock on which laboratory research is founded. Without such intriguing reports to suggest that our understanding of the world is far from complete, researchers would probably never have begun to attempt to study ostensibly paranormal phenomena in the laboratory.

Opinions as to the value of spontaneous cases have varied. Traditionally, such cases were thought to be capable of providing *proof* of the existence of some possibly non-physical means of information transfer (e.g., Gurney, Myers & Podmore, 1886), which nowadays is called psi. This 'proof-oriented' approach is continued today by researchers such as Ian Stevenson (1970a, 1970b, 1987), and in Britain Andrew MacKenzie (e.g. 1966, 1971, 1974, 1987) also presents collections of spontaneous cases, some of which he feels give proof of the anomalous nature of the reported phenomena.

In contrast, but not necessarily in conflict,<sup>1</sup> with the proof-oriented approach are the assumptions of researchers such as L. E. Rhine (e.g., 1951, 1954, 1981) and Rex Stanford (1974a, 1974b, 1987) that spontaneous cases are not well suited for providing unambiguous proof of psi. For these researchers the value of

<sup>1</sup> The proof/process distinction is rather an oversimplification since studies of spontaneous cases showing consistent patterns of recurrent features which seem not to be attributable to non-psi processes also provide support for the existence of psi itself.

spontaneous cases lies less in proving *that* psi exists, and more in suggesting hypotheses on *how* psi might work. These 'process-oriented' hypotheses might then be of heuristic value to laboratory research, guiding experimental protocols and the choice of questions for study. As Alvarado (1987) points out, however, spontaneous cases are valuable in their own right and this paper aims to suggest ways in which psychological researchers can make optimal use of such cases.

#### 1. A CONCEPTUAL FRAMEWORK

Scientific research in general, and psychological research in particular, uses four main tools: theory, hypothesis, prediction testing, and observation/description. The distinction between theories and hypotheses is unclear. For the purposes of this paper, I regard theories as relevant to a wide range of situations and phenomena whereas hypotheses are much more situation-specific. A *theory* attempts to explain why spontaneous cases have certain features, and to suggest some characteristics of spontaneous cases which had not been identified prior to theory construction. There seem to be few theories of spontaneous cases which have been systematically described and tested, but one example is Rex Stanford's (1974a, 1974b) Psi-Mediated Instrumental Response (PMIR) theory. An *hypothesis* is like a mini-theory, where one suggests a possible explanation for a particular characteristic of spontaneous cases. There is no shortage of hypotheses in the literature of psychological research, and examples are included throughout this paper. Hypotheses are simply statements, but they generate predictions, or suggestions for how a researcher might actively test a part of an hypothesis. This active *prediction testing* is a major part of experimental parapsychology, but unfortunately there are fewer examples within the study of spontaneous cases. Some examples will be described in part 4.

Finally, *observation/description* is the term used to denote the basic gathering of information or data in spontaneous cases. One can distinguish between *systematic* and *spontaneous* observation/description. The former refers to what happens when a prediction is formally tested. The latter occurs when the observation has heuristic value, suggesting promising areas for formal investigation; for instance one may re-evaluate old data or see what has been found in experimental parapsychology to check on some new ideas, or simply notice features of old or unsolicited new cases which might stimulate new ideas. The research approach described in parts 2 and 3 below has generally used observation/description spontaneously, whereas the strategy demonstrated in part 4 uses both systematic and spontaneous observation/description.

Observation can only be done by those who are physically present. Therefore most psychological researchers must rely on careful descriptions by others of what has been observed to provide the basic data for detailed analysis. Thus, description is crucial to any research project. When working with existing case collections, the researcher is limited by the descriptive standards of those who originally investigated and documented the cases. Both Schouten and Gauld, whose work with existing collections is described in more detail below, found difficulty in coding many cases because of the lack of detailed information. If a researcher is actively investigating new cases, he or she is in the privileged position of being able to set very high descriptive standards. The more

information that is available about a case, the more likely it is that patterns will be identified. Ideally, information which is gathered should not simply bear on the evidentiality of the case (for instance, number and quality of witnesses) or be restricted to the details of the ostensibly psychic experience itself (for instance, whether the percipient was asleep or awake at the time he or she received the message which was judged to be psychic). It may also be informative to consider the complex of factors surrounding the events. For instance, what were the percipient and the agent doing just *before* the experience which was felt to be psychic? Were they both dreaming? Was there anything they might have heard or seen earlier that day which might have caused them to be thinking/dreaming along similar lines? Morris's (1986) model of how observers conclude that psi has taken place suggests which aspects of the wider context of a case might be worthy of careful observation and description. Investigation of the network of factors within which the spontaneous psychic experience is embedded might suggest new process-related hypotheses, and might strengthen or weaken the evidentiality of that case.

The role that each of the four tools (theory, hypothesis, prediction testing, observation/description) plays in guiding a particular research effort provides one way of distinguishing different styles of investigation. In this paper I distinguish among top-down research, bottom-up research, and what I call integrated research.

#### 2. TOP-DOWN APPROACHES: FROM HYPOTHESIS TO OBSERVATION/DESCRIPTION

Top-down research is theory-driven. That is, the scientist begins with a theory of how something works. This theory is composed of hypotheses which generate predictions which, in turn, may be tested by conducting experiments which provide data which are observed and described. If the predictions are confirmed the theory is supported, and if predictions are disconfirmed the theory is weakened. Perhaps because there seem to be few precisely formulated theories for spontaneous cases many researchers in this field explicitly use only two of the four factors described: hypothesis and observation/description. It seems that if there is any theory guiding their research, it is often implicitly held rather than explicitly stated. Also, hypotheses tend not to be tested formally, though again the researcher may have implicit expectations of what will be observed. Top-down research into spontaneous cases is therefore characterized by a movement from hypothesis to observation/description, as depicted in Figure 1.

##### 2.1. Example of an informal top-down approach

Tony Cornell conducted several 'house shaking' field experiments in order to explore the hypothesis that poltergeist phenomena were caused by geophysical jolts and vibrations (Gauld & Cornell, 1979, Ch. 7). He began with the hypothesis (derived from what might loosely be called the 'geophysical theory') and then proceeded to see what happened when a (condemned) house was shaken mechanically, by vibrating a steel shaft embedded in a wall of the house and by thumping the side of the house with a 60lb 3oz weight. He did not make any explicit predictions of what the field experiments might test, though some predictions were implicit in his expectation that the experiments might suggest

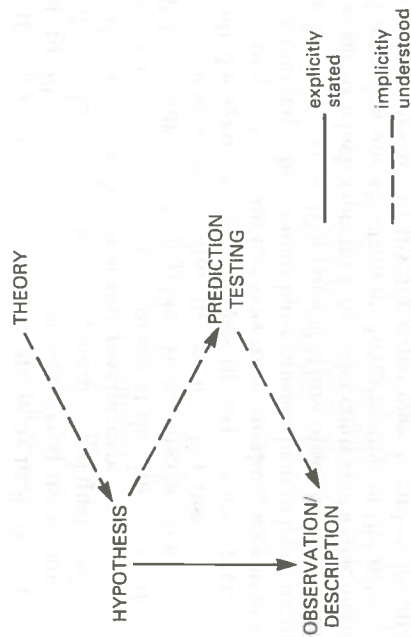


Fig. 1. Top-Down Approach.

whether poltergeist-like phenomena could be produced by mechanical means and if so, whether people within the house could remain unaware of the cause of these phenomena.

Even though the report of these field experiments did not begin with a clearly stated theory or make formal predictions, the descriptions of what was observed during these experiments (the house eventually began to disintegrate) suggest that, if the mechanically-induced stresses were similar to those which might be produced geophysically, naturally occurring vibrations sufficient in magnitude to produce small object movements would also cause quite severe structural damage. Thus, the geophysical hypothesis is unlikely to account for the full range of phenomena often seen in poltergeist cases.

## 2.2. Example of a partially formal top-down approach

Stevenson (1972) considered two hypotheses regarding the causal agent in poltergeist cases: that the phenomena are caused by either living or discarnate agents.<sup>2</sup> Whereas Cornell actually went into the field to gather information bearing on his hypothesis, Stevenson conducts a thought experiment, suggesting the characteristics which *might be* observed in cases with discarnate or living poltergeist agents, were one to look. For example, phenomena produced by a discarnate agent might be unrelated to any particular person, might move heavy objects over a longer range, with a controlled trajectory and few breakages, and might cease after exorcism. According to the living agent hypothesis, on the other hand, only light objects would be moved over short distances and in a rather uncontrolled way so that there were more breakages, phenomena might be localized around a particular person and might cease if that person were given psychotherapy. The main differences between the discarnate agent and the living agent hypotheses are therefore that the former appear to be more purposeful and controlled while the latter are less discriminating. This is clearly a top-down approach, though Stevenson does not actually go so far as to make

<sup>2</sup> For a recent example of a poltergeist case that suggested a discarnate agency see Betty (1984).

observations pertaining to his hypotheses. I consider this to be a more formal approach than Cornell's because fairly specific mention is made of what patterns might be observed for each hypothesis.

## 2.3. Example of a formal top-down approach

Gauld has analysed spontaneous cases quantitatively, using a sample of 500 cases of poltergeists and hauntings selected from printed sources ranging from A.D. 530 to 1975 (Gauld & Cornell, 1979). One aim was to ascertain whether an objective analysis would differentiate the features of poltergeist cases from those of hauntings cases (Gauld had noted that some cases contained both sets of features, so he wondered if the traditional haunting/poltergeist distinction was valid). The hypothesis was informally phrased as an exploratory question: 'whether or not poltergeists and hauntings constitute two distinct categories of phenomena, or whether the field would be better divided along different lines entirely' (p. 231). Each case was scored as to the value of testimony, the amount of detail and the presence or absence of 63 characteristics. Gauld does not explicitly state what theories or hypotheses, if any, guided his choice of these scoring criteria. In order to throw light on the haunting/poltergeist 'hypothesis', the cases were analysed as objectively as possible to see whether they naturally clustered into the two traditional groups.<sup>3</sup> The traditional distinction was supported, and his analysis was informative in that it re-classified as poltergeist cases some cases which had been regarded traditionally as hauntings, and vice versa.

I see Gauld's work as an example of a formal top-down approach because the cases were coded and analysed objectively. However, it seems that no specific theory guided his choice of scoring criteria, and the 'hypothesis' was phrased as an open question rather than as a precise statement. Also, there was no prediction testing regarding the specific characteristics which haunting and poltergeist case clusters would be expected to show. Although the analysis was primarily descriptive or classificatory rather than being concerned with how to understand the processes causing spontaneous cases, Gauld has provided some valuable descriptive data and has found some initial patterning of cases which might allow some other researcher to look more closely at process-oriented questions.

## 2.4. Weaknesses of the top-down approach

The strength of this approach depends in part on how the initial hypothesis was arrived at. If it was derived from a formal and well-specified theory, then the hypothesis in turn could be quite specific, and there would be little ambiguity in interpreting the significance of observations bearing on the hypothesis. There is a danger, however, that if the initial hypothesis is based on less firm foundations, conclusions based on relevant observations may be misleading. Some of the patterns suggested by Stevenson's hypotheses, for example, themselves depend on an implicit assumption — that discarnate agents have more power to influence objects psychically than living agents. While I can appreciate this assumption, it

<sup>3</sup> The curious researcher is provided with a valuable source of data, as each case is listed in the book's appendix together with its source, its testimony, detail and characteristics scores, and the outcome of part of the computer analysis.

could equally well be reversed, to assume that living agents have more psychic power than discarnate entities. This hypothesis might, for instance, be inferred from the arguments made by some mediums that the deceased often have difficulty in communicating with the living because their beings are much more 'weak and insubstantial' than living persons.

It is perhaps more logical for researchers first to identify patterns based on careful observation and objective description, and then to develop hypotheses and models about the causes of the patterns. This bottom-up approach lessens the risk of researchers considering only certain hypotheses or theories and ignoring others, a trap into which it is especially easy to fall where the field of inquiry is not well understood.

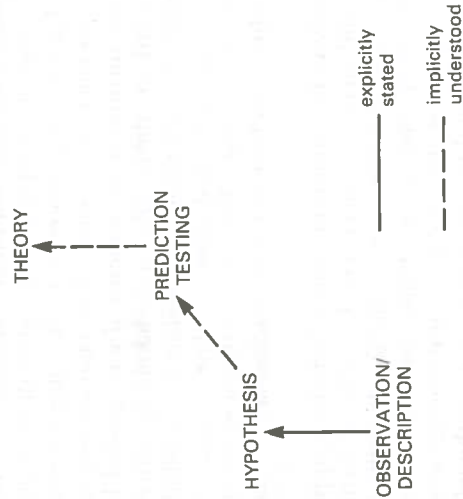


Fig. 2. Bottom-Up Approach.

### 3. BOTTOM-UP APPROACHES: FROM OBSERVATION/DESCRIPTION TO HYPOTHESIS

Just as with top-down research, generally bottom-up research into spontaneous cases does not explicitly use all four factors described in part 1. Rather, there is a tendency for these investigators to begin with the observation and description of the characteristics of spontaneous cases, and to move towards hypothesizing the reasons for these observed characteristics, as shown in Figure 2. Again, theory rarely guides this research except implicitly, and predictions are rarely tested, except implicitly. The degree of formality or objectivity with which the cases are described depends on the use which investigators aim to make of these cases. Andrew MacKenzie, for example, may take an informal approach in part because he regards spontaneous cases as of limited value presently. Sybo Schouten, on the other hand, sees spontaneous cases as a valuable source of process-related information. Thus he has described cases quite formally and subsequently has formulated quite specific hypotheses based on patterns which he has objectively identified within these cases. In many cases he then goes on to test predictions based on these hypotheses, and I regard this as a prime example

of the integrated approach to be described in part 4 below. When he goes no further than proposing hypotheses, this may be seen as bottom-up research.

#### 3.1. Example of an informal bottom-up approach

MacKenzie's most recent (1987) book is a collection of spontaneous cases particularly concerning apparitions and sensed presences. Occasionally, MacKenzie remarks on trends or patterns which he has noticed among groups of cases. For instance, at the close of his chapter on the sense of benevolent presences, he notes that 'it is remarkable how many experiences of the kind described here occur in churches or chapels' (p. 52). Sometimes he comes quite close to hypothesizing the reasons behind the case characteristics which he repeatedly notices. For instance, 'strange happenings tend to occur in the vicinity of people with psychic gifts' (p. 57) implies that psychics cause these strange happenings; and, 'the evidence suggests that people who live in a house affect the atmosphere of it' (p. 63) implies that there is something about people's perception of a house's atmosphere which is affected by its inhabitants. Later in the book, when discussing how some people's apparently paranormal experiences may end with a change of consciousness of the experient, he says 'I have noted time and again how some simple action, such as deciding to look away from a figure, causes it to disappear' (p. 161).

MacKenzie seems to have identified these apparent trends quite informally—as impressions which he has developed while collecting cases rather than as patterns which have emerged from a quantitative analysis of cases. This may be because it is not his main concern to explore the processes behind such trends: 'I feel that in our present state of knowledge the most we can do is to list such experiences . . . and note particular features of them' (MacKenzie, 1987, p. 236). Other researchers take a more optimistic view of the value of spontaneous cases, and for them the informal observations of researchers such as MacKenzie may usefully suggest promising areas in which to look more objectively for patterns.

The strength of the informal bottom-up approach depends very much on observational rigour: any implicit or explicit hypotheses derived from relatively subjective observations must be treated with caution because patterns of case characteristics may be unreliable if they are based on poor observations. Some apparent patterns may be overlooked because they are less noticeable than others, or because they do not fit with a researcher's particular area of interest or expertise.

#### 3.2. Example of a formal bottom-up approach

Schouten is responsible for what is perhaps 'the most detailed, quantitative (statistical), and, in many ways, sophisticated of contemporary case studies' (Stanford, 1987, p. 8). Schouten (1979a, 1979b, 1981, 1982, 1983) has painstakingly analysed 562 of the cases in the *Phantasms of the Living* (Gurney, Myers & Podmore, 1886) collection; 789 cases from the Sannwald collection of spontaneous cases (Sannwald, 1961); and 1630 cases (a representative sample) from Louisa Rhine's huge collection of over 10,000 spontaneous cases (e.g. Rhine, 1969).

Each case was given a series of numerical scores according to 32 previously-defined categories. No explicit theory guided the selection of the coding system,

but Schouten implies that he chose it in order to allow an analysis of both ESP and possible non-ESP factors related to some common trends which had appeared in previous studies of spontaneous cases. In order to make the scoring system as objective as possible Schouten laid down an explicit set of rules (described in detail in Schouten, 1979a) for the assigning of scores within each category. For example, the criteria applied in Category 20 (Experience indoors or outdoors) are as follows: 'Indoors includes all cases where the percipient was in a building, vehicle, etc. Outdoors all cases where the percipient was in the open air. A person having an experience when sitting in the entrance of his tent would be considered being outdoors' (Schouten, 1979a, p. 60).<sup>4</sup> Patterns are identified by using a computer to total the number of cases showing each variable, such as the number of female percipients, and then exploring (through the application of standard statistical tests, usually the chi-square test) all possible relationships between variables, for example whether action was taken by the percipient when the event was serious.

Some very significant and sometimes counter-intuitive relationships between variables were found (for instance, trivial events are more rich in details) and many of these relationships were replicated across case collections (for instance, conviction—inferred if action was taken by the percipient—is generally stronger the fewer the number of reported details).

One pattern which Schouten identified in the 'Phantasms' collection was that there were far more cases involving negative events such as death or serious illness or injury than there were cases involving positive events, material damage or trivial events. He found that serious events were longer remembered and thus more frequently reported than trivial events, and that most of the trivial events were reported by the percipient, suggesting that usually trivial events are not seen as worth reporting, unless one is personally involved in the event. Therefore, reporting bias accounted for some of the observed pattern. However it could not easily explain why only 0.5 per cent of cases concerned *serious material* damage (that is, damage to property), yet 8.7 per cent of cases involved presumably less memorable *slight physical* injury (that is, damage to person).

This pattern suggested to Schouten that information about events concerning a person is probably transmitted because these events are more important to the relationship between the percipient and the target person than more material events (thus, there were more cases involving slight physical injury than serious material damage because the former would be more likely to threaten the relationship between the percipient and his or her partner). This led to him formulating the hypothesis that 'events which pose a greater threat to the relationship will have a higher likelihood of becoming the subject of an ESP experience with the other partner' (Schouten, 1979b, p. 435). Events causing personal injury rather than material damage would be more likely to threaten a relationship.

Schouten did not go on to test predictions based on this particular hypothesis, which is why I regard this case as an example of bottom-up research. In part 4.2,

<sup>4</sup> Alvarado, 1987, points out that despite these guidelines there may remain some subjectivity in Schouten's judgements on how cases should be scored. This problem could in part be overcome by using several judges and excluding characteristics which proved to be ambiguous.

however, I do make some suggestions as to how this hypothesis might be tested actively.

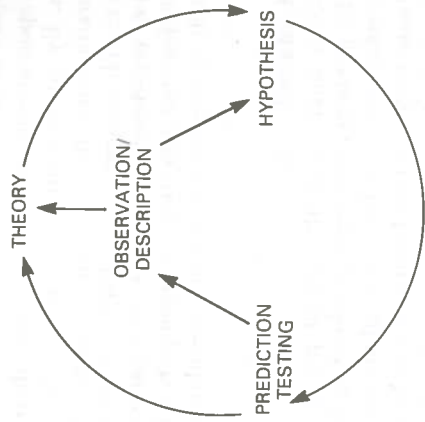


Fig. 3. Integrated Approach.

#### 4. THE INTEGRATED APPROACH

I admit that it is an oversimplification to pigeonhole research as 'top-down' or 'bottom-up'. I have made some hint at the true complexity of research strategies by saying that often prediction testing and theoretical input is implicit rather than explicit. What I really hope to have conveyed so far is that the various research strategies illustrated above are *incomplete*; they do not use all the tools at their disposal to investigate spontaneous cases fruitfully. Figure 3 illustrates what I see as a more complete research strategy into spontaneous cases: a continuous flow from theory to hypothesis to prediction testing to theory . . . and so on. Spontaneous observation/description may be used to check on the validity of theories and hypotheses, and predictions are tested by making systematic observations. A cynical interpretation of Figure 3 would be to suggest that it represents researchers going round and round in circles. The true picture would be more of an upward spiral, suggesting that on progressing through the research stages, knowledge advances and this feedback modifies theories and hypotheses to accommodate new findings or observations.

The integrated approach as depicted in Figure 3 is an ideal, and I can think of no example of systematic research into spontaneous cases which is progressing in this way, though readers who are more familiar with the literature than I may have good candidates to offer. Experimental parapsychology more often approximates the integrated approach. Although individual researchers may not themselves conduct all of the theorizing, hypothesizing, prediction testing and observation/description in a coordinated research program, they are generally aware of the practical and theoretical relevance of their work and usually try to explain the implications of their findings.

If spontaneous case research more explicitly considered the theoretical relevance of findings, stated hypotheses such that they would lead to testable

predictions, and modified theories and hypotheses based on observation/description I feel there would be a greater chance of learning more about such experiences. The examples below do not demonstrate the full potential of the integrated approach to spontaneous case work because there is little theoretical input behind the ideas. By going further than before, however, and actually making and testing predictions, these examples come closer to the more complete, integrated research strategy which I feel is potentially more fruitful than simple bottom-up or top-down approaches. In what follows, I distinguish between research on existing case collections, as exemplified by Sybo Schouten's work, and active research on fresh cases, a more speculative suggestion of my own.

#### 4.1. Working with existing case collections

Schouten's (1982, 1983) analysis of the sex of percipients in the Rhine collection found that 83.3 per cent of percipients were female and only 16.7 per cent of percipients were male. He considered several alternative hypotheses for this pattern, but for the sake of illustration I will describe how he tested only two of these hypotheses. The first proposes a non-psi explanation: reporting biases caused the preponderance of female percipients (hypothesis 1). The second proposes a psi explanation: women are more sensitive to psychic impressions than men (hypothesis 2). Each of these hypotheses generates predictions as to the sorts of patterns which might be found in the case collection. The reporting bias hypothesis gives rise to the prediction (1) that a comparison of the numbers of male and female case contributors who were not directly involved in the case as percipients or target persons (assuming that those directly involved would be more likely to report a case anyway because it had more impact on them personally) would find more females sent cases in than males. Schouten indeed found that in the Rhine collection significantly more females contributed cases than males ( $p < 0.0001$ ) where they were not directly involved in the events and so hypothesis 1 was supported. Schouten made various predictions to test hypothesis 2, including that if females are more sensitive to psychic impressions than males, then they might be expected to 'see' more details of cases than males (prediction 2a) or perhaps they could cover larger distances (prediction 2b). On analysing the relationships between the variables sex of percipient, number of details reported and distance between target person and percipient, no significant difference was found between female and male percipients and so predictions 2a and 2b provided no support for hypothesis 2. Hypothesis 2 was not confirmed by any of Schouten's other analyses, and he noted that this finding was in accordance with the findings of experimental research that males and females do not appear to differ in ESP ability. Therefore, these results suggest that the preponderance of female percipients in the Rhine collection is due at least in part to reporting biases.

These findings are not the last word on sex differences in ESP ability, however, since predictions 2a and 2b assume a particular model of psi where the increased sensitivity of the percipient to psi signals is not accompanied by an increased sensitivity to other 'noise' signals which could mask the psi signal. Different underlying assumptions of the nature of psi might lead to different predictions and, perhaps, different conclusions. Therefore, for Schouten's approach to come

closer to the ideal integrated approach using observation/description, hypothesis, prediction testing *and* theory, he ought to be more explicit about the particular theory of psi communication which is guiding his hypothesis formulation.

#### 4.2. Actively seeking out new cases

Where this research strategy differs from those described above is that instead of testing predictions against data which have already been observed and described, researchers would go out into the field to test their predictions *actively* by attempting to locate as yet unreported spontaneous cases. When considering weaknesses of the top-down and bottom-up approaches, I stressed the need for objective identification of trends or patterns observed in groups of cases. Active researchers might use as their starting point patterns which had been reliably identified by other researchers, as illustrated above by the work of Gauld and Schouten. Hypotheses would be suggested to explain the patterns and these hypotheses would give rise to predictions about where other cases might be found, or what characteristics these cases might have. These predictions would in turn be tested by going into the field and seeking out new cases.

As an example of how an active approach to integrated research might work, consider the hypothesis that 'events which pose a greater threat to the relationship will have a higher likelihood of becoming the subject of an ESP experience with the other partner' which was suggested to Schouten by various objectively-identified patterns (Schouten, 1979b, p. 435) as described in part 3.2. above. Events threatening a relationship are likely to involve physical illness or injury to one of the partners; material damage, such as loss of money or possessions, would be regarded as less threatening.

In order to formulate a prediction, researchers would first try to get a more detailed picture of the facts surrounding the patterns by asking related questions. For instance, exactly what sort of events threatened the percipient-partner relationship? Events causing physical harm to the people involved; or events causing psychological harm, such as divorce or separation? For how long had the pair been involved in the relationship? A matter of weeks, years, a lifetime? Was the relationship a legal one, such as marriage; a genetic one, such as between a mother and son; or an informal one, such as friendship? Is the emotional involvement of the pair with each other an important factor? The more details that are known about the complex of events surrounding the pattern, the more specific and therefore powerful hypotheses and predictions can become.

Let us speculate that the answers to these and other questions have refined the above hypothesis so that it may now be tested in the field: specifically, the detailed pattern predicts that in fact when the percipient and agent are *genetically* related there is a greater chance of events which threaten this relationship becoming the subject of an ESP experience between the pair. To try to control all factors except for the genetic one under consideration, researchers could compare parent-natural child ESP cases with parent-adopted child cases. Thus, the percipient and agent would have shared most of their environmental experiences in both cases, but in the former case they would also share genetic information, while in the latter they would not.

To seek out cases where a serious incident had threatened a parent-natural child vs. parent-adopted child relationship, researchers might place newspaper advertisements; they could consult adoption agencies or legal registers to narrow the search for non-genetically related pairs (bear in mind that these particular suggestions might introduce bias and ethical problems, but they may stimulate better ideas from readers). When no apparent crisis telepathy incident had yet occurred, pairs could be left with instructions on whom to contact if it occurred in the future. Again, such instructions might introduce bias in that pairs might be primed to interpret ambiguous events as psychic in nature. However, both sets of pairs would be biased in the same way, and subsequent investigations of the details of a reported spontaneous psychic experience would presumably be able to support or weaken the original interpretation of the experience. Counselling bureaux which had been set up after major disasters could report in confidence whether any crisis telepathy had been experienced by individuals, and whether they were genetically related. Police involved with accident investigations might be able to report parts of their interviews with accident victims and witnesses, again anonymously.

These are just some ways in which researchers could actively look for new cases to test the prediction that there will be more crisis telepathy cases among genetically-related than genetically unrelated pairs. If the prediction was confirmed then this would suggest the development of a genetic theory or model of psi, and this theory might suggest new hypotheses for investigation. If the prediction was not confirmed, then this too could be informative, suggesting that there might be another, non-genetic, reason for the pattern which originally stimulated the hypothesis. In both cases, researchers may use observation/description of cases which are already at hand, or of work that has been conducted in experimental parapsychology, in order to confirm new findings or to look again for alternative explanations for the original pattern. Thus, actively testing predictions by going out to look for new cases may lead into a productive cycle of integrated research which may tell us more than we presently know about spontaneous psychic experiences.

##### 5. CONCLUDING COMMENTS

This paper has presented different approaches to the study of spontaneous cases of ostensibly psychic experiences. Much of the work which has been conducted in the past has failed to make full use of the factors which can contribute to productive research: observation/description, hypothesis, prediction testing, and theory. An integrated approach to spontaneous case research may lead to a positive cycle of advancing knowledge, with potential spin-offs to other related areas of research, including possibly experimental parapsychology. Two illustrations were given of how one could conduct integrated research into spontaneous cases. The first tested predictions on existing case collection, as exemplified by the work of Sybo Schouten. The second was more speculative, suggesting an active approach to testing predictions by looking for fresh cases.

In practice, just as with laboratory experimentation, ethical concerns must guide decisions on how to test predictions actively. The active approach may be

difficult if not impossible given the sparsity of spontaneous cases that come unsolicited to our attention; Gauld, for instance, is rather pessimistic about the practicability of pursuing such an approach. Such pessimism is justified as long as spontaneous cases fail to be described to the extent which is necessary to begin to identify relatively robust and detailed patterns leading to hypotheses. My aim is to encourage researchers to evaluate what they are learning from the collection of spontaneous cases and to consider the ways in which spontaneous cases could be more fruitfully used to aid in our understanding of people's apparently psychic experiences. The very process of looking for patterns, considering causal explanations for these patterns and thinking up predictions derived from these hypotheses would, I feel, encourage researchers to look at spontaneous cases in a different light—not as a collection of intriguing anecdotes, but as a potentially rich source of information about the factors, both normal and paranormal, which cause people to have experiences which they interpret as paranormal. This is the real value of spontaneous cases.

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## A PSYCHIATRIST EXAMINES FEARS OF HEALING

by DANIEL J. BENOR

## ABSTRACT

The efficacy of psi healing has been reported for many centuries. It is claimed to accelerate recuperation from a wide variety of illnesses and occasionally to produce instantaneous cures. Though 61 of 140 controlled trials of healing have demonstrated significant results, healing continues to be ignored by conventional medicine, or worse, denigrated and denied to sick people who might otherwise enjoy its benefits. This paper examines some of the reasons psi healing has not been accepted by Western scientists. The parallels with other psi phenomena are obvious.

Psi healing (which I shall abbreviate as 'healing') has been recognized as an effective mode of therapy in most cultures, with recorded observations dating to ancient Egyptian hieroglyphics. I define healing as the intentional influence of one or more people upon one or more other living organisms, without intervention through physical or chemical means. Healing may be done through the laying-on-of-hands or through mental influence alone. It appears to be effective from a distance of a few inches to thousands of miles.

I have identified 140 controlled trials of psi healing published in English, of which a dozen are doctoral or masters degree dissertations (Benor, in press [a, b]). Of these, 61 demonstrate effects which could have occurred only once in a hundred trials or less often (See Table 1). William Braud (1989) considers 149 experiments with living targets. (He includes experiments on telepathic influence over movements of humans, mammals and fish, as well as imagery projection, all of which are bio-PK but outside the category of psi healing.) He finds that '53 per cent of these "bio-PK" studies yielded significant outcomes, as judged by the original experimenters'.

Despite these many experiments, dating back to the early 1960's, there has been little recognition accorded to psi healing as an established phenomenon — even among *parapsychologists*. Healing continues to be viewed by most scientists as a 'fringe' phenomenon and most of them avoid having anything to do with it.

In part this derives from the practitioners of healing. Most healers are strong on intuitive faculties and weak on linear functions. They keep few, if any records, while making broad claims for their successes in treating any and all of the ills of man and other living things.

In part this shares the common problems of devaluation of all psi phenomena. I am not saying that healing practice and research should not be criticized. Critical analyses of healers' unsupported claims, of research methodology in healing, and of weaknesses in theories are salutary. As James Lovelock said, 'Good criticism is like bathing in an ice-cold sea. The sudden chill of immersion in what seems at first a hostile medium soon stirs the blood and sharpens the senses'.

I criticize those who reject healing out of hand. I hypothesize that many critics would cloud the evidence with any excuses to support their disbeliefs rather than