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The Substantiality of Spontaneous Cases[1]

IAN STEVENSON

INTRODUCTION

In considering a topic for this Presidential Address I was sorely tempted to discuss what might be called the political situation of parapsychology in relation to the field of science as a whole. Yet I have set aside this inclination because I believe that our political difficulties result from the general scientific immaturity of our field—yes, one can still say this after ninety years—and they will be removed or at least diminish only after further advances in our scientific work. I decided therefore to talk with you about my favorite topic within parapsychology, which is also the subject about which I can claim to know most from personal experience, the study of spontaneous cases. Other authors have already said so much

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on this subject (3, 5, 9, 26, 35, 38, 40, 49, 52, 64, 65) that I am not confident of being able to say anything new. I may, however, agree with the statement that although everything has been said before, it is sometimes necessary to say it again!

My theme is that we need to continue and enlarge the study of spontaneous cases[2] if we are to advance the whole field of parapsychology and not just parts of it. I believe this statement justified now for three reasons. First, in my opinion, spontaneous cases provide some of the best evidence we have for paranormal processes. Secondly, most spontaneous cases are so much richer in apparently communicated content than are the products of our laboratory experiments that as things stand now we can surely learn as much from them about paranormal processes as we can from our *present* experimental methods and available subjects. We can never learn much about the processes of paranormal communication from the results of conventional statistical experiments because we have no way of knowing when an instance of ESP has occurred and when a correct result is due to chance. We can learn very little about process unless we know what is being communicated and what omitted, distorted or added. Qualitative material often enables us to know these things and therefore to study processes in a manner quite impossible with most quantitative experiments. Interpretations and theories based exclusively on the data from past and current quantitative experiments are bound to be limited in their scope and value since they do not take into consideration the much richer qualitative material available. We can too easily forget also that we only need statistical methods of assessment in situations where chance is a likely alternative explanation and when we wish a method of identifying the probabilities that chance is not the correct

[2] To avoid surprising the reader later, I shall say here that in discussing spontaneous cases I propose to consider (not necessarily always at the same time) all the naturally occurring phenomena studied by parapsychologists. Thus I shall consider the traditional spontaneous cases such as telepathic and precognitive dreams, visions and impressions; apparitional cases; and out of the body cases. I shall also, however, include in my discussion some other types of cases such as reincarnation cases and poltergeist cases, although these types of cases have not usually been grouped among spontaneous cases.

explanation. In many of the more evidential[3] spontaneous cases and other qualitative material chance is not a reasonable hypothesis at all. We should even take warning that whenever we need statistical methods of assessment we are probably dealing with weak paranormal effects and certainly with paranormal effects usually weaker than those found in most spontaneous cases.[4] Tyrrell made these points very well twenty years ago and I cannot hope to improve on what he said (59, 60,61).

Here I feel that I may be misunderstood and wish to include an important disclaimer. I am not belittling experimental work. Laboratory experiments certainly have other important values apart from permitting more refined measures of probability; they permit a control over other variables rarely achieved in studying spontaneous cases; and they allow investigations of some questions, e.g., the effects of changes in the subject's mood or state of consciousness, that spontaneous cases cannot illuminate at all or only very roughly. When all other factors are held uniform, a variation in one factor during an experiment may permit a clear-cut answer to some question. Spontaneous cases rarely give definite answers. We need both experiment and a renewed and improved study of spontaneous

[3] It will be helpful if I here define two adjectives commonly applied to spontaneous cases. By "authentic" I mean a high reliability of the witnessing and reporting so that one can justifiably believe the perception and other related events to have happened as reported. By "evidential" I mean authenticity plus a justified interpretation that the case has paranormal features. A case may be authentic and yet if the percipient might have gained information normally about the events related to his perceptions the case would not be evidential. But an evidential case must be authentic to be so considered.

[4] Parapsychological experiments that are loosely called "quantitative" have somewhat unfairly picked up some of the prestige attached to measurement in 19th century science which emphasized quantification. Some of its exponents even asserted: "No counting, no science." Quantitative methods applied to parapsychology do not measure any *amount* of extrasensory perception. They simply assess the probability that the results might have come about by chance. (As I shall show later, such assessment is not necessary in the better spontaneous cases.) To anyone who considers the question deeply it must be obvious that a larger "amount" of ESP is communicated in many spontaneous cases than in most "quantitative" experiments, although I would not say the same thing of the results shown by certain rare "high scoring" subjects.

cases. Proper procedure in science requires adapting the method to the problem, and I do not mean to exalt the study of spontaneous cases to the neglect of experimental work. I merely wish to correct what has seemed to me a tendency towards the reverse situation, namely derogating of spontaneous cases with exclusive fixation on laboratory investigations. One can see also that laboratory methods in parapsychology are improving. I fully expect that in the future better subjects and improved, more imaginative methods of working in the laboratory will provide us with opportunities to learn more about processes (and to have more control over communication) than we can have now either with spontaneous cases or with our present experimental methods.

Even if, however, we achieve this happy situation of subjects who can "turn it on" voluntarily under controlled conditions, we will not be able to dispense with the spontaneous cases for obtaining a full picture of paranormal phenomena. And this brings me to my third reason for urging a renewed and improved study of spontaneous cases. This is that in the laboratory we can never study the full play and significance of paranormal phenomena in everyday life. For spontaneous experiences sometimes influence both the conduct of the percipients and the belief systems of those who come in contact with them. Let me here give a small example of the influence of paranormal events in everyday life. In an analysis of 160 telepathic impression cases I found that in 83 instances (52%), the percipients took some action as a result of the experience, apart from simply telling another person about it (55). They often interrupted their plans drastically (usually to go to the help of the agent) in response to the impact of the experience. And percipients make similar behavioral responses to other kinds of paranormal experiences such as apparitions, precognitive dreams, and poltergeists. Although we have few reliable figures about the number of persons having ostensibly paranormal experiences, such indications as we do have suggest that they are numerous and that therefore they enter into a great deal of human behavior.[5]

[5] In the S.P.R. Census of Hallucinations (51) 9.9 % of the respondents reported having had one or more hallucinations, although by no means all of these were claimed to be veridical. In the Mass-Observation Questionnaire (1948) 14.3 % of the respondents reported having had hallucinations (63). Among a "captive" group of (nearly) 2500 schoolchildren of India, all of whom were required to answer a questionnaire on the subject, 36 % reported having had ostensibly paranormal experiences (28).

It seems to me also that spontaneous cases have a profound influence on the beliefs of the persons who come in contact with them. (I do not think this point will be grasped by investigators who do not conduct personal interviews with informants.) This is particularly true of cases which bear on the possibility of survival, such as out-of-the-body experiences, apparitions and reincarnation cases. Conclusions naturally vary with the type of experience. And some percipients draw conclusions that we would not ourselves consider justified by the evidence. But nearly all assert that the experiences have convinced them of the reality of extrasensory perception and others believe they have found in them evidence of the independence of mind from body and of its survival after death. My intention here is not to endorse all beliefs based on spontaneous cases, but to emphasize that they bring or change convictions about life's most important problems for many people. In reaching such convictions, the percipients often defy the scientific orthodoxy of our day, if they know about it. They may have heard that some professors think telepathy is impossible, but they know better. For them it does exist.

If only a few spontaneous cases demonstrate paranormal processes (and I believe the number is far greater) then any picture of human personality which does not take account of them must be ridiculously impoverished and is bound to be improved upon by one that does. One could make the same complaint if the data of the better laboratory experiments in parapsychology are neglected.

I can imagine an objector interposing at this point to say: "Surely this is regression. Spontaneous cases have been attacked, derided, and pretty well disposed of not only by our outside critics, but also by some of the most distinguished and capable parapsychologists." I am undeterred. Critics of spontaneous cases have had their biases too, which can be shown in the sweeping generalizations sometimes applied

against all spontaneous cases because of weaknesses found in some of them. Errors are made, of course, but we do not abandon history because some historians blunder, nor do we abandon court trials because the innocent are occasionally convicted and the guilty more often set free. In order, however, that I may not myself seem to assert an opposite generalization, as if rejecting all criticisms of spontaneous cases, I wish to devote some time to an examination of these criticisms and also indicate ways in which we may improve the study of spontaneous cases.

Although I shall soon come to particulars, let me begin here with a general observation about exactitude in laboratory experiments as compared with spontaneous cases. Spontaneous cases depend on human testimony (usually recorded some time after the events) and this has been thought one of their main weaknesses and indeed their only important one. In contrast, events in the laboratory occur where everything happens under our very eyes and reports of such events should therefore be completely reliable. I used to believe in this distinction, but in recent years have come to have serious doubts. More can go wrong in the laboratory than popular information acknowledges. Much also can occur which is unnoticed. If one relaxes vigilance during an experiment one may simply fail to see and observe some crucial development. The event may be over in a second and if one misses it, one can never know just what did happen.

Recently I myself had an experience of this kind. At the time I was the notetaker for some experiments in "psychic photography." Two other experimenters were in charge of other features of our planned experiment in which we thought we had every contingency allowed for and available for immediate capture in my notes. This I believe was the case until the very end of the experiment. I thought we had finished, but then the subject made one more trial. At that point I had put down my notebook and was putting away the equipment. The subject, however, made this last trial and I was drawn away from my usual task of observer and notetaker when I was asked to place my hands in a particular position on the camera. Immediately afterwards I made notes of what I

thought happened when we obtained, much to our surprise, a print which we all considered definitely paranormal. The next day I was chatting with one of my colleagues about the details of this trial and a little later with the subject himself. To my astonishment I found that my notes did not agree in details with what my colleague remembered of the conditions of this trial. One of us was clearly wrong. The subject's testimony coincided with mine, but later the statement of my second colleague supported my first colleague's opinion. In this case it is probable, I think, that having thought the experiments over for the day, I had relaxed my vigilance and had not paid as much attention to the notetaking as I ought. But it is also possible that in the excitement of obtaining what we all agreed was a paranormal print my colleagues had blurred their attention to what the conditions were. My point is that we shall never know who was right and I feel that my information about some details of this particular trial is actually less dependable than that which I have for *some* spontaneous cases.[6] This and other similar experiences have taught me that we may readily deceive ourselves into thinking that we have more control over conditions and observations than we have.

This question, however, goes far beyond my own deficiencies as an observer. I believe that the withdrawal of many parapsychologists from spontaneous cases has cost us part of our supportive following in persons who, from personal experiences, believe in the paranormal interpretation of spontaneous cases. The loss of public support would be a small price to pay if we had gained equal or more ground by the conversion to our point of view of our colleagues in other branches of science. Unfortunately, the general criticisms of spontaneous cases and withdrawal of interest in them among many parapsychologists have not in fact strengthened our

[6] This sort of lapse does not need to happen in experiments, although it sometimes does. My colleague J. G. Pratt recommends (and practices) the habit of a close routine of conduct in experiments. The routine can be written down in advance. So long as it is followed, the experimenters are free to record only the data as they accumulate. And since they are not otherwise taking notes during the experiment, they are freer to observe and to record any deviation from the planned routine which does occur.

position vis-à-vis our critics. Some parapsychologists hoped by abandoning spontaneous cases to retreat to the higher, more defensible ground of a small number of tightly controlled experiments. But now we find these attacked for several weaknesses. Even apparently excellent experiments have been shown to include some defects of design or avoidable errors of reporting. Although I myself believe that much of our best evidence for paranormal cognition comes from experiments, I think it is both incorrect and foolish to say that all of it comes from them. If this is all you have to show, say the critics, it amounts to nothing since there are so many glaring defects in your experiments. I myself think the criticized experiments are better than many experiments reported by our colleagues in conventional psychology. That is not the point. It is that *no* experiment will be found perfect and unassailable. Having been told some twenty years ago by parapsychologists that spontaneous cases could never furnish proof of anything (35, 36), we are now told by our critics that experiments also furnish no proof (18, 29). It would seem that we have made little progress, at least in convincing our colleagues in other branches of science that we have produced anything which they concede as amounting to proof. I think the error here lies not in the experiments or in the case studies, but in the concept of proof. It would seem wiser to abandon this concept, which after all is virtually unknown anyway in other branches of science except mathematics. Let us return instead to a weighing of probabilities in dealing with all the material at our disposal.

Those parapsychologists who deny the validity of at least some spontaneous cases as evidence of paranormal processes have maneuvered themselves into a very difficult position. If spontaneous cases provide no sound evidence for paranormal processes, what is the justification for any experiments in parapsychology? Such parapsychologists remind me of a geologist who would deny the existence of earthquakes but thinks it worthwhile to study the crust of the earth just in case it should be found weak at some point. I do not think we can have it both ways. Either we believe that some spontaneous cases do provide good evidence of paranormal processes or most of our experiments hang in the air without relation to anything else in life. Surely our position would be stronger both with the lay public and with our scientific colleagues if we accepted the probability that some spontaneous cases provide good evidence of paranormal processes and then used experiments to study whatever aspects of these they may shed light on. It would follow from this that we should expand and improve the study of spontaneous cases. The effort to build the whole case for parapsychology on experiments of a limited scope and success has clearly failed.

As I have said already, I do not mean in any of the foregoing to support spontaneous cases by devaluing experimental work. I am criticizing instead a narrow dependence on experimental work and a tendency to constrict the whole field to such work. I am not calling for a totally new approach to parapsychology, but simply for a deployment of our meagre forces over a wider territory.

Let me turn, however, to the spontaneous cases themselves to see if I can justify a renewed study of them by examining some of the common criticisms which are directed at them.

WEAKNESSES OF SPONTANEOUS CASES

INADEQUATE RECORDING AND WEAKNESS OF MEMORY

As I have already indicated, the soft place in most spontaneous cases is that the experience was not recorded in writing before the related events became normally known to the percipient. Certain types of spontaneous cases, especially apparitions and poltergeist cases, suffer from the additional weakness of unobserved or uncontrolled conditions which might lead to overlooking a normal explanation of the reported experience.

In those rare cases in which the percipient does make a written record of his experience immediately after it has occurred, we may have a document more valuable as evidence than poorly made records of laboratory experiments.[7] Un-

[7] In fact there are many (but not enough) spontaneous cases in which we *do* have a written record of the experience made before the subject

fortunately, this rarely happens and the investigator of spontaneous cases has usually to make or stimulate the first written record of the percipient's experience and of whatever corroborations he can obtain. This means then that the memories of what I call the primary experience[8] have been eroded, modified or supplemented since the original experience and we often cannot easily tell just what modifications have occurred. That some experiences are lost totally by the loss of memories with time was first clearly brought out in the S.P.R.'s Census of Hallucinations, which showed that there was a marked falling off of memories for hallucinatory experiences within the first few months of their occurrence (51). This survey also showed that there was a greater tendency to remember apparitions of the dead (or dying) than of the living.

Experiments in remembering have shown that with the passage of time details of experiences tend to be forgotten. But experiments in the loss of memories with time are not necessarily relevant to the study of all spontaneous cases. Many of the experiments exploring the fading of memories with time (or other inhibitory causes) present the experimental subjects with commonplace and uninteresting material, sometimes even nonsense syllables or rather dull stories (2, 24). Retention of detail in memory depends on at least three factors, namely, (a) emotional intensity of the original experience; (b) repetition; and (c) motivation to remember. We can often find one or more of these conditions favoring memory in the circumstances of spontaneous cases. For example, the experiences are often unusual, as when an apparition suddenly fades, and such features increase emotional intensity and promote memory. Or there is repetition of the content which often occurs, for example, when the child subject of a reincarnation case talks repeatedly to his parents about a previous life. Or there is

had normal knowledge of the related events. Although the argument for spontaneous cases as evidence of paranormal processes does not by any means rest completely on the memories of percipients, it is regrettably true that the accuracy of the witnesses' memories has to be assessed in evaluating the majority of them.

[8] By "primary experience" I mean the original imagery or feelings of the subject so far as they can be remembered and distinguished from secondary interpretations or elaborations when these occur.

motivation to remember from an interest in paranormal experiences which seems to be a common characteristic of those who have them.

Still another important point about memory in spontaneous cases is the frequent occurrence among ostensibly paranormal dreams of the characteristic of vividness or realism which is missing in most ordinary dreams. I think parapsychologists have paid insufficient attention to the relevance of vividness in dreams to paranormal processes. Certainly many vivid dreams are not paranormal and many paranormal dreams are not vivid, but the following figures suggest that there is a connection here worth studying. The report of vividness occurred in 56 (45%) of 125 verified precognitive dreams which I studied (56). An analysis of 449 vivid dreams concerning the death of a person known to the dreamer about whom the dreamer was not then anxious showed that 35 (nearly 8%) were (according to the testimony) coincident with the death of the person dreamed about (32, 33). Now the point here is that vividness in a dream is closely linked with the subjective awareness of prolonged detailed recall of the dream. A percipient may say, for example, "I usually forget my dreams, but I can remember this one just as clearly today as the night it happened fifteen years ago." The distinction has been drawn so often by so many percipients that I think it deserves at least provisional acceptance.

You may say, however, that these are general assertions. What specific instances can be cited of prolonged, accurate memory relevant to spontaneous cases? It happens that a few examples are available to prove that *some* persons at least do have excellent memories for details over many years. Let me cite a few of these.

Prince published an account of an apparently precognitive dream which he had and told to his wife in 1902. Seven years later (in 1909) both he and his wife wrote out accounts of the experience which included a good many details and sent them in to the A.S.P.R. And then eight years after that, Mrs. Prince (whose first account had been lost) again wrote out her account without consulting any memoranda or discussing the case with her husband. This account, written fifteen years after the

original events, contained only one small discrepant detail when compared to her husband's account (31). Prince published another case of a percipient who wrote down an account of his experience—of an auditory, death-coincident hallucination—ten years after it happened and again thirty years later, without on the second occasion consulting his first report or other memoranda. There was only one discrepant detail between the first and second accounts (30). Parsons showed that a description of a building written down six years after it was seen was correct in 18 out of 21 details mentioned (27). And finally I may cite an experience of Heywood, who once kept a report of a case unopened for ten years and then rewrote it, discovering in the second version only two minor errors (20).[9]

Our confidence that some memories at least are to be trusted does not rest solely on the study of individual cases. Hart made a systematic comparison of apparitional cases of low evidentiality with those having high evidentiality, according to his criteria, which included the lapse of time between the original event and the writing out of a report (19). He found that the characteristics of the reported cases of low evidentiality did not differ significantly from those of cases of high evidentiality.[10] I think we can safely conclude that although the passage of time does erode memories of some paranormal experiences, some others are excellently preserved in the memories of the percipient and his corroborators. It may be a weakness of many cases that such erosion of memories does occur, but it is certainly wrong to reject all spontaneous cases as necessarily inaccurate because not immediately recorded in writing.

[9] The numerous well-established instances of accurate oral transmission of information over centuries and among many different peoples are also relevant here. Evans-Wentz described this among the Tibetans (12) and Drucker among the Indians of northwestern North America (10). Hall described evidence of the accurate oral transmission by Eskimos over three centuries (nine generations) of details about the expeditions of Sir Martin Frobisher to the area of Baffin Island (17).

[10] In Hart's analysis of the evidentiality of cases the time elapsed before making a written record was only one of five criteria he used to assess evidentiality. Since he did not publish a separate analysis of this factor, it is possible that its effect was hidden or exaggerated in his data by the other factors.

In the four examples I cited above there was not only little loss of detail, but also little importation of new detail elaborated by the percipient after the original events. I think it important to emphasize this since some critics have argued that spontaneous cases tend to become elaborated with the passage of years and that such embellishments make it impossible later to get to the primary experience of the percipient. Now there is no doubt that embellishment does sometimes occur and it is often difficult to know when a witness has supplemented a particular case and when not. The informants in the Census of Hallucinations tended to remember a disproportionate number of apparitions from remote periods (51). West attributed this difference to secondary elaboration by the informants, the detail of death coincidence having been added later (64). But surely this difference is at least open to the alternative explanation that an experience which had in fact been veridical (and coinciding with a death) would be remembered better than one which had not. The memory of the apparently paranormal experience could be fixed along with the emotionally intense memories of the death and also by the fact of verification. It seems to me that embellishment has to be shown and studied carefully in particular cases, not assumed in every case from our knowledge that it occurs in some. Sometimes only careful comparison of different accounts given at different times will permit a decision on this matter. Salter has described the evidence for secondary embellishment in the (originally weak) Versailles case (41). In my own experience embellishment of the *main* features of an account occurs very rarely. I have had much experience in checking with interviews the testimony of one witness against that of another (interviewed independently) and in comparing what a witness says on one occasion with what he told me (or a colleague) on some other occasion one or several years earlier. The first sort of comparison is perhaps not so useful since, although we may interview witnesses separately, they may have consolidated their accounts by talking together before the interviews, although this has not always happened. But in the second situation, the witness often has no knowledge that I or a colleague will come again to question him all over. And

even if he knew we were coming, he would usually have no written notes with which to refresh the memory of his account and improve its consistency with what he had said earlier. Our sudden arrival on the second occasion also gives him no opportunity for support from the other witnesses in the case.

Just recently I have had opportunities to study one case in which embellishment occurred and two cases in which there was written evidence that none had occurred. The first instance was a case of the reincarnation type which I studied in Ceylon in 1966 and again in 1968. On the first occasion, we took the child subject from his village to the nearby city of Kandy where he claimed he had lived, and in the presumably correct area of the city he seemed to fail to make any satisfactory recognitions of people or places. Two years later I visited the family again with a different interpreter. In going over the case with the boy's father, he told us quite gratuitously that his son had in fact recognized and commented on places in Kandy at the time of our earlier visit. I was astonished at this claim and as soon as possible rechecked the points with the two interpreters of the first investigation of the case. Both denied that the boy had made any definite recognitions during the visit to Kandy when they had been close to him throughout. It is extremely doubtful that the boy said anything to his father which the interpreters did not hear and so here we seem to have clear evidence of an embellishment on the part of the father. His motives for this remain unclear. I think the alteration of testimony was guileless and probably unconscious, for if he had stopped to think that I would recheck his testimony with the previous interpreters he would never have tried to deceive me. Possibly he added the embellishment in his own mind as a happier outcome of the visit to Kandy than we had obtained.

In the second case, an American one, I received (in 1961) a written report from a correspondent about a veridical precognitive impression which she had had. In 1968 I got in touch with this correspondent to ask some additional questions about the experience and we went over it again in further letters exchanged almost seven years after the first report. I could find no trace of embellishment in what the percipient said about the experience on the later occasion as compared to the first one.

In the third instance (also American) I received a written report of an impression case from a witness, the percipient's wife. About three and a half years later, I wrote again in an effort to obtain a report from the percipient himself. In due course he sent this and on comparing it with his wife's earlier version, I found that it contained some additional details of circumstances, e.g., time relationships, but no embellishment whatever of the main features of the experience and related event. Impression cases seem to invite embellishment because they are so lacking in detail compared to, say, dream cases. Yet neither of these cases was embellished by the percipients. Perhaps the knowledge that I had the earlier reports restrained them, but this suggests that embellishment, when it occurs, is conscious and may be voluntarily restrained, whereas it seems to me nearly always an unconscious process. My point, however, is that although embellishment may sometimes occur, it does not *always* occur and in my experience it is exceedingly rare in accounts of the principal features of the experience and related events. When it does occur in the accounts of firsthand witnesses, it is usually (but not always) confined to peripheral details. The witness, for example, may forget the exact sum of a debt owed to the subject and so he interpolates a larger amount. He was correct that a debt had existed, but wrong about its amount. Errors of the time relationships between events often get into later accounts also.

Embellishment is more apt to occur in accounts given by the secondhand reporters of a case. This tendency was noted very early by Gurney and his co-authors of *Phantasms of the Living* who were impressed by the greater tendency for secondhand witnesses, as compared to firsthand ones, to furnish accounts of cases which were more dramatic or implausible than the features of the average case constructed out of the firsthand accounts (16). Secondhand informants, however, do not always embellish the case. Quite as often, if not more so, they drop important details and thus diminish its evidentiality (34).

Quite recently I encountered the following lesson in

distrusting secondhand reports of cases. In this case I heard the report first from a thirdhand reporter who did, I think, accurately report the erroneous statements of the secondhand informant he quoted. This secondhand witness was the presumptive agent for an experience in which (as reported) another man, a physician, felt first impelled, and then ordered by a hallucinated voice, to go immediately to the agent. He did so and, finding that the agent was ill at the time, he was able to arrange for proper medical care of the sick man. But a tape recording of the percipient's account of this episode throws quite another light on it. According to him, he had often thought of calling on the agent, whom he had heard of, but had never met. One day he was in a city not far from that in which the agent lived and instead of going home he changed his plans and went to call on the agent. He arrived at a time when the agent was in fact ill and was able to help him obtain necessary medical care. But there was no hallucinated imperative voice and no strong impulse to call on the agent whatever. There may have been some element of extrasensory perception in the percipient's change of plans and decision to visit the agent at that time, but the case, reduced to the bare bones of the percipient's report, showed little evidence of this. A point of suspicion in the secondhand report of this case was that the agent and percipient were total strangers and had never met, although one had heard of the other. Agent and percipient are very rarely strangers to each other in impression type cases (55).

MALOBSERVATION

Even if you allow that the memories of some percipients are reliable, you may still say that the percipient may have poorly observed his own primary experience and its surrounding circumstances. The inability of witnesses adequately to notice what has happened before their eyes (and of which I have already given a personal example) has been a subject of much comment and considerable experimentation by both parapsychologists and lawyers. The experiments of Davey (22) and Besterman (4) leave no doubt that under certain circumstances observations of simple events can be extremely inaccurate. Such malobservations can particularly affect the reporting of experiences which include manifestations apparently external to the subject such as apparitions, poltergeists and, most notoriously, the phenomena of physical mediumship. Here again, however, it is important not to use examples of malobservation to devalue all spontaneous cases, because the experimental demonstrations of malobservations are not relevant to the conditions of all spontaneous cases.

E. M. Sidgwick properly pointed out that malobservation was particularly likely to occur in the study of physical phenomena because the circumstances required prolonged attention (48). One can usually sustain attention over a fairly short period of time; but a fraudulent physical medium can exploit the lapses of attention which inevitably occur after a time. This extremely important observation concerning conditions in studying many physical phenomena does not necessarily apply to other kinds of spontaneous (or mediumistic) material. For many of these the time of observation is often brief, or the material presented varies, so that attention can shift from one item to another and does not need to remain fixed on some object or area from which it will sooner or later wander off.

Critics of spontaneous cases sometimes cite some of the staged scenes studied by lawyers in investigating the reliability of witnesses (14, 66). Such experiments certainly have some relevance to our field, but again I resist their use to reject all human testimony in spontaneous cases. A short review of several of these staged scenes may help to show the grounds for my reservations.

In the first place, in many such experiments the scene enacted to test the accuracy of witnessing includes some unexpected and startling event such as a commotion or the firing of a pistol (46, 57). Such commotions or loud noises can arouse anxiety in the audience and diminish attention to peripheral, although important details (6, 7). Experiments of this kind are relevant to the assessment of testimony in some criminal cases and in the reporting of highway accidents. There we often find circumstances which arouse intense emotion for a brief period of time and the strong emotion may interfere

with attention to essential details of the event witnessed.[11] This came out in the experiment at the London School of Economics reported by Selbst (46). A quarrel was staged between two persons each of whom drew weapons at the same time. The audience, asked afterwards to describe the scene, missed the fact that each of the quarrelers had simultaneously drawn a weapon. They tended to see one of them (who was rushing at the other) as having drawn his weapon first. Clearly all eyes were on this man and the other man was (for the moment) ignored. The detail is crucial in a question of self-defense in cases of criminal assault. But of relevance for our work in the spontaneous cases are the facts that: a) the witnesses' descriptions of general actions of the scene were in the main accurate and b) their descriptions of details before the commotion of the staged quarrel were much more accurate than those of the period when weapons were flourished.

I think it will be agreed that such experiments are not relevant to the evaluation of testimony in all spontaneous cases. Intense anxiety which would diminish attention is not often a feature of these. The percipient may be distressed by the content of his experience or by his later interpretation of it, e.g., if he thinks it relates to a death, but percipients do not often record being frightened before or during the experience itself. For example, I have been impressed, in reading and listening to accounts of apparitions, that often the percipient at first simply believed he was actually seeing the person whose apparition he visualized, and reacted to this with pleasure.

A second relevant point is that observers, even in the experiments to test malobservation, do attend to events which are unusual in their experience. This was well brought out in the little-known experiment of Crocker, published by Prince (8). The experimenter was a lawyer who wished to study

[11] Since I earlier (page 100) stated that intense emotion tended to fix memories I may seem to be inconsistent here. Intense emotion does fix memories for a total experience, but if the emotion is intense at the time of the perception, attention will be narrowed and peripheral details not attended to. Often the intensity of emotion in a spontaneous case occurs *after* the perception, e.g., when the apparition vanishes or when the percipient believes (or normally learns) that his vision or dream relates to the death of a person perceived in the experience.

malobservation as it related to legal testimony. Seventeen witnesses wrote out accounts immediately after observing a scene in which one actor surreptitiously stole the wallet of another. None of the witnesses reported seeing the actual act of stealing, which was carried out stealthily. But 16 of the 17 witnesses correctly reported that the man whose wallet was taken later returned and said his wallet had been stolen and ten of them reported seeing an unusual collision between two of the actors. The collision of two people in an office and the claim to have had a wallet stolen are unusual events and were observed and reported when the covert theft was not. I think the results of Crocker's experiment relevant to the study of spontaneous cases because these nearly always include experiences which are unusual for the percipients. The Census of Hallucinations (late 19th century) showed that only 34% of those who gave firsthand accounts of hallucinatory experiences had had more than one such experience (51). The number of such persons was very much less (5.5%) in the (1948) Mass-Observation Questionnaire (63). For most percipients of apparitions then the experience would be unusual and would correspondingly attract attention. The affect aroused by the content and related events such as death would later tend to enhance recall. In other spontaneous cases (taking my broader definition of them), such as poltergeists, some precognitive dreams and many reincarnation cases, repetition of the same or similar events probably forces attention and enhances recall.

Rollo recently summarized the contribution of such staged experiments to the study of spontaneous cases. He wrote: "... the events so utterly mis-reported really did occur, and really were witnessed by those who so completely mis-reported them; hence the probability of a genuine report containing palpably false details is quite high; finally, hence the presence of false details will not tell heavily against the genuineness of a report (39, p. 55)."

Inaccuracy about peripheral details does not demolish a case. If a percipient (or other informant) is inaccurate about details he may also have mixed up the main events of the experience and he is more likely to have done so than one who

gets the small details straight. But he may well have some details wrong and the main story correct.

I am so far from believing that faults of observation and memory invalidate all spontaneous cases that I think it can be said that in some spontaneous cases the subject's condition and the circumstances are such as actually to improve attention and memory rather than the reverse. But the most important point is that some percipients are better than others and some are capable of giving accurate accounts of events they have experienced. We can only judge who they are by careful examination of particular instances both with regard to the circumstances of the incident and the condition and qualifications of the percipient.

MOTIVATED ERRORS

A third common objection to human testimony regarding spontaneous cases suggests that such errors as occur are nearly all in the direction of reinforcing previously held favorable beliefs about paranormal events on the part of the percipient and his corroborators. According to this view, motivated errors amplify some simple normal experience into one which suggests a paranormal phenomenon. My own experience in interviewing some hundreds of informants does not confirm this criticism as a generalization, although I have certainly encountered some instances of motivated amplification of experiences. This is particularly likely to happen with persons who seek or find some reward of fame or money for whatever experiences or gifts they may originally have had.[12] But motivated amplification does not seem to occur often with the average percipient who reports one or a few spontaneous cases happening, so to speak, in the course of ordinary domestic life. If their own statements and my observations of their behavior are to be credited, many persons report these experiences to others with the greatest reluctance from a fear

[12] I am inclined to be suspicious of persons who claim to have had *many* paranormal experiences, although a few of these people may be the specially gifted subjects we are seeking! We must be careful to reject no one without careful consideration of his claims.

of ridicule or suggestions of insanity. About half my Western informants request the use of pseudonyms in any publication; many cases put forward in a letter seem to be withdrawn afterwards under pressure of spouses or other relatives who fear that the stigma of abnormality will spread to the whole family.

Certainly a reluctance to be laughed at is not incompatible with a motivated amplification of an experience which seems to support previous beliefs, especially of a religious kind. But many subjects also insist that prior to their experiences they had no settled convictions or knowledge about the experiences which parapsychology studies. In some cases also, we can see the influence of motives definitely antagonistic to the development and investigation of the case. A percipient may gain only the vindication of a friendly parapsychologist to exchange for the teasing and condemnation of his friends and family. One sometimes reads or hears that apparitional cases seem no longer to occur so abundantly as they did in the late 19th century. They may occur, however, and not be heard about. In the past few years five percipients have told me about seeing apparitions; their experiences coincided closely with the "standard" apparition studied by Gurney (16) and Tyrrell (62). In every case the percipients had told one or two intimate friends or relatives and then, perhaps for many years, had told no one until they decided they could trust me with the report. If these percipients had not told other parapsychologists, could it be that they sensed a lack of interest in these matters on the part of many of us? Since Eisenbud (11) has discussed so well the possibility that parapsychologists themselves are sometimes frightened by the magnitude and importance of the phenomena which they study, I have no need to elaborate on this further. But this fear could at least partly account for the withdrawal of many parapsychologists from the spontaneous cases, which withdrawal they then rationalize by pointing to the evidential weaknesses of the cases.

Some instances have occurred of a motivated denial of perceptions or first interpretations of a case. A man is certainly entitled to revise his interpretation later of what he thought he saw or heard, but he ought not to deny later that he had

previously said he had seen or heard something which seemed paranormal to him at the time. The case of Sir David Brewster's later denial of his remarks at a sitting with D. D. Home became notorious because Brewster was publicly exposed in his attempt to deny the remarks favorable to a paranormal interpretation of the phenomena which he had made at the sitting (13). It seems that Brewster later became afraid that he would be judged too gullible. I have myself encountered or heard of several particularly sordid examples of similar denials of previously reported perceptions, enough at any rate to convince me that motivated errors of reporting do not *all* occur in the direction of enhancing paranormal interpretations. Some go towards suppressing the evidence of paranormality.

The foregoing remarks take account only of conscious suppression of evidence and dishonest denial of it. Far more common must be unconscious influences on actual perceptions. Here again some critics write as if motivations only influence percipients in the direction of perceiving an apparently paranormal event and never in the direction of *not* perceiving one. Yet negative hallucinations occur no less than positive ones. Experiments have clearly shown that subjects under group pressure will deny the evidence of their own senses in judging lengths of visual images, for example (1). Other experiments have shown that suggestions and other influences can rather easily influence actual visual perceptions (45, 47). And still other experiments have shown that some persons tend to diminish differences between two perceptions ("levelers"), although others tend to increase them ("sharpeners") (23). All perception occurs with interpretive hypotheses and with tendencies to assimilate the perception to previous experiences (25). In a society as hostile as ours to paranormal events we must expect considerable influence on percipients towards not perceiving and not reporting when they do perceive ostensibly paranormal events.

Again I do not mean to say that motivated errors do not sometimes falsely inflate the value of spontaneous cases. I merely emphasize here as before that we ought not to make generalizations about such matters. Let us study the influence of motivation on the events and their reporting in the details of particular cases, and not make sweeping assertions of what we ought to find.[13]

CHANCE COINCIDENCE

I have left until the last the argument about chance coincidence because I consider it by far the weakest of all the criticisms of spontaneous cases. The argument is usually put in the following form: "Supposing that there is some correspondence between a perception, say a dream, and another event, dreams and other events occur all the time and it is to be expected that every so often they will correspond and this will be noted." There are two ways of countering this interpretation.

First, the proper comparison to be made is not that between all dreams and all events, but that between dreams (or other experiences) thought by the subject to be significant (at the time they occured) which are veridical and those which are not. A great many veridical perceptions are accompanied by deep impressions or (as mentioned earlier) impulses towards action. This counter-argument was well put by Tyrrell in comments on a series of precognitive experiences. "If we accepted this argument, all the cases... are a fair sample of *ordinary* dreams and impressions, and the insistent, compelling character which the percipients describe in most cases (about 80%) must be characteristic of ordinary non-veridical dreams and impressions in about the same proportion. Are they?" (58, p. 38).

Secondly, the claim of a non-chance relationship between the perception and the related events lies in the correspondence of details between the two, which details show that the perception and the related event are both unique. An event is identified as unique by the accumulation of details (or other features) which distinguish it from other superficially similar events. In the

[13] Few parapsychologists have studied in detail just what the influence of motives may be in observations related to our field. Besterman found in his experiments on malobservation that Spiritualists (who might be regarded as credulous) were no worse (or better) as observers of events in a mediumistic seance than were non-Spiritualists (4).

words of Bergson who first, I believe, advanced the argument I am developing: ". . . a scene in which definite persons take definite attitudes is a thing unique of its kind (3)." Death has special importance in spontaneous case reports because it is obviously unique for each person, but a number of other events can qualify almost as well, e.g., birthdays, births of a first son, or (for most people) the date of marriage. I believe that a coincident perception (provided it can be shown also to be unique) related to a death (not normally expected) occurring at a distance suffices to establish the occurrence of a paranormal process. Unfortunately, many spontaneous cases include events which are not so easily identified as unique. These then gain their identification as unique from their details and when these details correspond to an event at a distance, and when the percipient had no normal access to information or grounds for inference about the events, we have evidence of paranormal cognition. Such details might include a particular person driving a car off a bridge into a particular river. The details set off the uniqueness of the event, at least for one driver. Although we have no quantitative method for measuring the nearness to uniqueness, we can agree that as the number and the unusualness of the matching details increases, the identification of the uniqueness of the event becomes more definite. Then if the details of the perception correspond to those of the related event there can be no reasonable doubt that the perception itself is of a unique event and therefore related to that event alone. We have these conditions of matching details in many of the best spontaneous cases and it is because of the abundance of cases of this type that I agree with Salter, who said that arguments on this point with regard to the richer cases require "a ludicrous straining of probability" (42).[14]

[14] As in so many *general* criticisms which fail to focus on the details of a particular case, Harding [quoted by West (64)] is unfair to the role of corroborators when she says their evidence "merely proves that the percipient *purported* to have such and such a supernormal experience on a given date." In fact, the corroborator can often testify to the *details* of the experience reported by the percipient, including the exact time of its occurrence. Unfortunately, corroborators rarely remember as much detail of the experience as the percipient himself. But when they do remember details, they can assist greatly in confirming not only that the percipient had his experience before he received normal knowledge of the related events, but that the perception was of an identified unique event, thus helping to exclude chance coincidence.

THE IMPORTANCE OF INVESTIGATING AND ANALYZING EACH CASE INDEPENDENTLY

I do not mean to gloss over the difficulties of investigating spontaneous cases or minimize the cogent objections which have been put forward against uncritical reliance on human testimony in them. (I hope that my own work shows sufficiently the importance I attach to a complete probing of cases with corroboration and verification and with the interviewing[15] of as many witnesses as possible together with the assembling of any other relevant documentary evidence.) I merely wish to oppose an equally uncritical application of such general criticisms to all spontaneous cases.[16] Weaknesses and hazards which we may find in one type of case, such as apparitions, may not occur at all in another type, such as telepathic dreams. Each type of case requires a different approach to its study, but I contend that with the use of methods appropriate to particular cases, reliance can be placed on human testimony in individual cases shown to be worthy of such confidence.

[15] I attach special importance to interviews whenever feasible. I find they frequently bring out more details about the primary experience and they also permit a much better assessment (than do letters) of the memory, motivations, attitude towards the experience, and integrity of the percipient or other informants. It is not enough simply to gather corroborations and verifications when we have opportunities to go further. West has pointed out that a few "authenticated" cases have nevertheless turned out to be frauds (64). Ideally each case demands a thorough probing as to the plausibility of all details. And adequate investigation should further include study of psychological aspects of the subject as well as analysis of the evidence pointing to paranormal processes in the experience.

[16] We need criticisms of spontaneous cases which encounter the actual details of specific cases. Although I disagree with some of the more sweeping generalizations about spontaneous cases made by two stern critics of them, Dingwall (9) and West (64, 65), it is very much to their credit that they have also pointed out the weaknesses of *particular* cases. With the help of such specific criticisms we can improve our methods.

THE STRENGTH OF SPONTANEOUS CASES IN GROUPS

The substantiality of spontaneous cases does not, however, depend only on the authentication of individual instances. They are additionally strengthened by their numbers and their characteristics.

THE STRENGTH OF NUMBERS

Let us consider first the strengthening force of large numbers of spontaneous cases. At least five thousand reasonably well-attested spontaneous cases have been published in the English literature alone. [*Phantasms of the Living* itself includes just over 700 cases (16).] Many other excellent cases have been published in French, German, Dutch and other languages. We should remember further that some of these published cases have been supported by the corroborating and verifying testimony of at least one other witness and probed with the expert knowledge of one or several investigators and editors. The argument from numbers simply states that although it is very likely that some of these cases are seriously defective in ways which have been indicated, it is much less likely that they are *all* defective at critical points which collapse them (5). Now this argument from mere numbers has some power, but it is far from compelling. There are numerous ways in which a given case can be defective and the different cases might have various, but sufficient defects so that even a large collection might be eroded away to nothing by setting aside this case for that defect, another case for a second defect, and in the end all the others for one defect or another. I myself believe that for most categories of spontaneous cases we have some cases which satisfy the highest criteria, but it is certainly true that we do not have many of this type. (To increase this number is one reason for my exhortation to the renewed study of spontaneous cases.)

THE IMPORTANCE OF PATTERNS IN SPONTANEOUS CASES

The argument for authenticity from the characteristics or patterns of cases of one type seems to me much stronger. So far as I know, it was first put forward by Gurney who drew attention to the commonly occurring features in firsthand reports of apparitions. He pointed out that the firsthand reports of apparitions did *not* have certain features narrated in the popular fiction about ghosts of the time. Gurney asked:

> Why should not such apparitions hold prolonged converse with the waking friend? Why should they not produce *physical* effects—shed tears on the pillow and make it wet, open the door and leave it open, or leave some tangible token of their presence? It is surely noteworthy that we have not had to reject, on grounds like these, a single narrative which on other grounds would have been admitted. Have all our informants drawn an arbitrary line between the mistakes and exaggerations of which they *will* be guilty, and the mistakes and exaggerations of which they will *not*? (16, Vol. 1, p. 165)

Gurney further drew attention to the high incidence of death coincidences among reports of apparitions, an observation unexpected by the investigators and not found in the popular beliefs and fiction about ghosts and apparitions of the late 19th century. (I am aware that the documentation of the death coincidences in the Census of Hallucinations, which followed *Phantasms*, leaves much to be desired, but this is not relevant to the question of why so many apparitions were *reported* to be death coincidences.) In considering this matter of type, West correctly pointed out that fictional apparitions and ghosts show a pattern, but are not on that account genuine and that perhaps some widely diffused notion of what a ghost or apparition should do had influenced the pattern of the late 19th century apparitions (64). And similar cultural trends might influence the occurrence of patterns in other types of spontaneous cases or influence shifts in the patterns of cases from one period of time to another. Now there is no doubt

that cultural forces influence the communication of spontaneous cases to other persons. (In times and places where the percipients are persecuted they naturally go underground. I have already hinted that we may just now be emerging from such an underground period.) It is equally probable that cultural forces have some influence on the patterns of the primary experiences in spontaneous cases.

Here, however, we need to proceed cautiously before we concede too much to cultural influences. The types we see in nature have different origins. In medicine, for example, we have the type of hysteria and we know that many of the manifestations of hysterical behavior are communicated from one person to another so that sometimes it is legitimate to speak of an epidemic of hysteria in which a pattern of very similar behavior emerges in several or even in many persons at about the same time. But we also have in medicine the types of diseases such as myxedema and glomerulonephritis, disorders in which the symptoms in one patient resemble those in another not through imitation, but because of similar dispositions and reactivities within the physical organisms of all human beings. May it not be that some types in apparently paranormal cases are of the communicable variety, but others are of the second or natural reactive variety? The 19th century fictional ghost, for example, could have been spread among readers and writers by the printed word very easily. And the conventional witch of several centuries ago could also have arisen through communication (by mouth and print) of the behavior of identified or alleged witches. But how could the apparitional type which emerged in *Phantasms* have developed through communication and imitation? In the first place, this type, as Gurney pointed out, differed from the fictional ghost of the period. The investigators themselves were surprised at some features of the type which emerged from the different reports. And in the second place, how could the various percipients have possibly known each other and imitated the same model? There are no grounds for believing that the percipients of actual cases had known each other or had heard of each other's experiences except very rarely. The S.P.R. Census of Hallucinations did not specifically go into this matter, but I have studied it, at least informally, in connection with reincarnation cases. The vast majority of informants in these cases have not heard of other cases. A few have heard of only one or two, or rarely of several other cases. And most informants in one culture have no information at all about cases in another culture. Notwithstanding the isolation of the informants from each other, quite marked patterns emerge in the reincarnation cases. The cases in one culture often show both marked similarities and differences when compared with cases in other quite separated cultures. Tlingit and Turkish reincarnation cases resemble each other closely in three important features; yet these two groups are totally without contact (53, 54).

Similar patterns have been observed in spontaneous cases of other types. Series of spontaneous cases studied in Great Britain (15), Germany (43), India (28), and the United States (37) have all shown remarkably similar frequencies of the types of paranormal experiences reported. We find also in different series a marked similarity in two features suggestive of the role of emotions in the cases, namely, close familial or marital relationships between agent and percipient and a high incidence of negatively toned and important events, such as death, in the contents of the experiences.[17] For the following reasons it seems to me unlikely that these patterns result from errors in sampling, i.e., from selective reporting. First, one is only a little less likely to verify and remember a paranormal experience which had to do with a friend, colleague, employee, servant, or other person of one's acquaintance than to verify and report one that concerns a close relative. Problems of identification would be no greater with friends and well-known acquaintances than with relatives. Yet near relatives predominate by far in the agent-percipient relations of all four of the above mentioned series. Secondly, the large number of negatively toned events which form the most frequent content of these experiences cannot result only from our tendency to remember intense experiences, since there would on this basis be just as much likelihood of remembering and reporting positively toned

[17] Prasad and I give some comparative data from surveys of spontaneous cases in different countries and cultures (28).

important events such as births, marriages, etc., associated with apparently paranormal processes. Sannwald's survey provided the best data we have on this difference. In his series nearly 85% of the experiences were accompanied by negative affects and only 15% by positive affects (44).

I should emphasize perhaps that the patterns in collections of cases indicate authenticity, by which I mean that they actually happened as reported. These patterns do not, however, indicate directly anything concerning a paranormal interpretation of the cases within the group. As West pointed out, popular fictional ghosts and cases of alleged witchcraft[18] showed recurrent types in the cases, but were not thereby paranormal (64). Yet it is a very considerable advance if the patterns found in cases do support authenticity which has been all along our first requirement before we undertake further interpretations of any case. And I think we can say further that if a particular spontaneous case is authentic then a paranormal interpretation is often justified. Paranormal explanations are usually set aside on the grounds of insufficient authenticity and rarely for other reasons.

Moreover, the study of types may have some relevance to interpretations of paranormal processes in the cases. I think Hart showed the way here with his ingenious comparison of the characteristics of apparitions of the dead and of the living (19). Eventually also as our confidence grows in the delineated types of authenticated cases, we may come to include in the tests of the paranormality of a case its conformity to the type. Hart gave us another lead here with his comparison of the patterns found in apparitional cases of low and high evidentiality.

[18] I do not think the "witchcraft type" provides an exception to my interpretation of the significance of type. In the several centuries of the persecution of witches in Europe, a vast written and oral communication developed amounting to a kind of mass psychosis. Information about the type was spread orally then, just as later "knowledge" of the fictional ghost type was spread in written form. In contrast, the percipient in the street today, has less ready access to knowledge of the parapsychologist's types which are mostly published in specialty journals and books. Knowledge of these types is now, however, becoming more widespread among the lay public.

I believe that the further collection of spontaneous cases and the study of their patterns holds promise for advancing our understanding of such cases and what they can tell us of paranormal processes. Here the computer can help much by enabling us to handle rapidly data from large numbers of cases and to make comparisons of different subgroups.

THE IMPORTANCE OF ADEQUATE SAMPLING OR AUTHENTICATION

I should, however, stress that I consider worthwhile for such analyses only cases collected under one of three conditions. Either they should comprise a genuinely representative group selected by the techniques of systematic sampling: or they should come from a captive group such as whole classes of students, as in our Indian study (28); or they should be carefully investigated and classified with regard to authenticity. I believe that we can go seriously astray by trying to discern patterns in series which are *both* unauthenticated *and* uncontrolled with regard to the sample population. Particularly unreliable would be most conclusions drawn from cases sent in to a laboratory and accepted into a series at face value.[19] The series depends in the first place on the initiative of the informants who write to the laboratory about the experiences. If such cases are not further probed one can never know how many cases of such a series are worthless and thereby contaminating the genuine cases and leading to false conclusions. My own experiences with such cases is approximately the following. Of 100 cases reported to me in a first letter perhaps more than half can be seen at once to have no grounds for a paranormal interpretation. Of the remaining 50%, about two-

[19] Since L. E. Rhine (36, 37) has been the chief student of uninvestigated spontaneous cases, this remark may seem to show a lack of respect for her enormous contribution to the study of spontaneous cases. She has, however, disclaimed giving any credit to spontaneous cases as providing evidence by themselves of paranormal processes; for her they merely provide leads to experiments. My position is that some spontaneous cases do provide substantial evidence of paranormal processes, although many do not. I further believe that a mixed, uninvestigated group of spontaneous cases may give false leads both for experimental work and for conclusions, however tentative, about paranormal processes.

thirds of the informants will fade away and fail to answer a letter requesting corroboration or further details of the experience. If, however, the informant does follow through by furnishing corroboration and additional details, then I have found very few indeed of the cases to disintegrate entirely afterwards. This is not to say that all the other cases provide strong evidence of paranormal processes, since some of them certainly could be explained otherwise. But once a case has withstood the first probes, it has been rare for me to find it unauthentic in the sense that the main testimony is unsupported or discredited. Obviously if a case fades away when one tries to probe it one can only speculate about the reasons for its withdrawal, but it seems likely that the following reasons for withdrawal are the most important. First, the first informant (who is sometimes a secondhand reporter), when asked for a corroboration, checks his story with another person and learns that the other person he had counted on does not support it. In short, the informant becomes immediately aware of some important error of malobservation, memory, or interpretation and so quickly drops the whole subject. Secondly, many cases seem to be withdrawn out of a fear of criticism, derision and publicity which will prove adverse to the subject. The informant puts out a feeler to an investigator, then becomes frightened again or perhaps dissuaded by sensitive relatives and so withdraws. Thirdly, many informants do not have the time, the interest or the patience to answer all the questions about details which I, at least. usually have to ask them; and some consider the request for corroboration and verification a slur on their honesty, which it usually is not, or a test of their memories, which it usually is. Since, however, we cannot know which of the withdrawn cases are genuine and which not, a series of voluntarily reported and unprobed cases is bound to be seriously defective at least if my experience is shared by others.

THE VALUE OF SOME UNAUTHENTICATED CASES

I do not mean to recommend, however, that all cases of low evidentiality should be discarded as useless. As I have

indicated earlier, we are indebted to Hart for showing that we can do a great deal with them. His comparison of the characteristics of apparitional cases of low evidentiality with cases of high evidentiality showed that the characteristics of the two groups of cases were not significantly different. This justified him, correctly in my opinion, in combining the two groups of cases in order to see what further information could emerge from the analysis of the larger group. This approach seems to me to have much merit and I am planning to use it in the further study of reincarnation cases. I heartily commend also the efforts of persons like Heywood to study the subjective processes in cases of low evidentiality for what light such introspections can shed on psychological factors promoting extrasensory processes (21).

Drawing towards my conclusion, let me say that I am quite well aware that counter instances and criticisms of criticisms do not strengthen any single case. I have deliberately not put forward any specific case as an example of the evidence and information about paranormal processes which a good spontaneous case can provide. My aim has been rather to redirect attention to the study of spontaneous cases in general since I believe that some parapsychologists have been unreasonably frightened away from them. Science is concerned with what *is*, not with what *ought to be*. In the spontaneous cases we have an abundant source of experiences which have been affirmed by many credible witnesses for centuries. If, as I believe, these cases are part of "what is" then we should not abandon their study, but should improve it.

Those who hold back from this endeavor may find encouragement in the revolution accomplished within our generation in the study of animal behavior by modern ethologists. Scorned thirty years ago as mere "bird watchers," they have shaken the dogmas of men who were content to study animals only in laboratories. Those of us who study spontaneous cases are the ethologists of parapsychology and our reward may be to enrich parapsychology, including its experimental side, just as the ethologists have enriched biology.

My plea then is for a renewed, enlarged, and yet improved study of spontaneous cases. I think that in the main the methods

of the early S.P.R. investigators were sound. Gurney very early, for example, emphasized the value of personal interviews in probing cases and he knew well the importance of adequate written documentation for them. Limited resources sometimes prevented these early workers from attaining the ideal they strove for.

With better facilities and understanding we can hope for better results. I myself foresee a time when certain spontaneous cases may be investigated under partially controlled conditions with better documentation and even, in some instances, with objective measurements. One can see this already in the investigation of some of the recent poltergeist cases. And in my own special field of reincarnation cases, I have now at least ten cases in which a written record of the subject's statements was made before they were verified. If we can bring to bear on the rich material of spontaneous cases the best techniques and devotion of which we are capable they may contribute as much as they once did—or more—to the advance of parapsychology.

In conclusion, since I have covered so much ground, I shall summarize my main points.

SUMMARY AND CONCLUSIONS

1. Spontaneous cases of established authenticity and high evidentiality provide strong evidence of paranormal processes.

2. Some spontaneous cases demonstrate a larger "amount" and a wider range of paranormally communicated information than do most laboratory experiments.

3. In many spontaneous cases one can know positively what information is communicated and what distorted or omitted, facts not usually known from data of many laboratory experiments.

4. Spontaneous cases occur in ordinary life and show the practical effects of paranormal processes on conduct and belief much more than do the results of most laboratory experiments. Any view of paranormal phenomena which omits spontaneous cases must necessarily be defective.

5. The emphasis in parapsychology on experimental investigations during the years (approximately) between 1930 and 1960 did lead to some additional knowledge and the well controlled experiments brought conviction about the importance of parapsychological phenomena to some previously resistant persons. On the other hand, the neglect of spontaneous cases which accompanied the emphasis on experimental work may have led to some loss of support for parapsychology among persons who saw it becoming less and less concerned with paranormal events in everyday life which have far more meaning for most persons than the often meager results of laboratory experiments.

6. The neglect of spontaneous cases in theory building by some parapsychologists has limited the usefulness of such theories (based exclusively on experimental results) in devising better experiments or field observations.

7. Criticisms of malobservation, deficient memory, motivated distortions and chance coincidence apply to some spontaneous cases, but not necessarily to all. Some spontaneous cases occur in circumstances which may actually enhance accurate observation and memory. In spontaneous cases rich in detail chance coincidence is a most improbable explanation.

8. The authenticity of spontaneous cases is supported by the observations of similar patterns in collections of cases found in different times and places; these patterns do not necessarily support a paranormal interpretation of the cases, but the study of types may assist in judgments about paranormality.

9. Further useful information should emerge from the analysis of large collections of cases in which at least some of the cases have been investigated and authenticated.

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