SPONTANEOUS PSI AND EXPERIMENTAL PARAPSYCHOLOGY

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> "Research is never free of assumptions, whether they are explicitly stated or only implied. Either way, they strongly influence the investigator at every stage of his work. The scientist's assumptions are like the imaginary features used to fill in unexplored territory on a map. But just as the explorer venturing into unknown territory checks his charts and corrects them to show the realities discovered, so also scientific explorers need to examine their assumptions and be alert to make changes as they may be required."
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> (Pratt, p. 127)

This is the opening paragraph published in 1978. Gaither Pratt discussed "some assumptions relevant to research in parapsychology" and at some point speculated that all psi events may be spontaneous. This speculation provides an important background to this paper. Initially it may also be important to focus on certain hidden assumptions in connection with experimental laboratory based research, field work investigations and spontaneous cases.

In the hard sciences laboratory experiments have led to the most successful explanations of a large variety of events and relationships as well as to many applications on which we can rely with considerable confidence. Consequently it is not surprising when it is generally assumed that laboratory experiments are the most appropriate means by which progress in any scientific field may be achieved, particularly when it is also assumed that the best controls can be obtained in laboratories.

If it is kept in mind, though, that research is supposed to lead to explanations which in turn improve our conceptual framework and understanding and which may also provide us with opportunities for applications, it is not clear that our confidence in the superiority of laboratory work is always justified. Particularly when questions need to be answered which deal with complex behavior patterns of living organisms, laboratory research may be of little assistance if experimental work can only be carried out in truncated sections. It is often assumed that in comparison with laboratory research, field investigations suffer from control limitations in general as well as from particular control problems due to improvisations. These assumptions must also be questioned.

No laboratory experiment provides absolute control over all variables that may have a bearing on the data to be extracted. Nevertheless, it is assumed that all relevant variables are sufficiently controlled for the results to be accepted with some confidence. Although some field work investigations can probably be controlled to a similar degree, it could still be argued that completely unexpected events may exert an uncontrolled influence in a field setting. But as long as this can be readily appreciated, such events are not really different from equipment failures in a laboratory. On the other hand, one difficulty which is often ignored in the laboratory is the question as to how far the experimentation really deals with the issues which gave rise to the research in the first place. In other words, in the laboratory it may be impossible to control factors which invalidate the findings. Finally, it is often assumed that in the field setting it may be more difficult to manipulate independent variables. However, after a thoughtful analysis of a particular situation it is often possible to introduce manipulations after all or at least to select different states of the independent variable from those that occur in the natural setting anyway.

It may be of interest to pursue some of these questions in a somewhat more orthodox area of science, namely ethology. The study of animal behavior, that is, the study of real life, species-specific behavior and of the evolution of behavior, provides good examples of successful field research. It is difficult to see how our present understanding of orientation, migration and courtship behavior of various species could have been achieved through laboratory experimentation alone. (Chauvin, 1975; Eibl-Eibesfeldt, 1970; McGill, 1965).

Pigeon homing research was at some stage thought to include a possible parapsychological component (Pratt, 1953). In the fifties two well-known parapsychologists, Gaither Pratt and Robert Thouless, carried out research in this field (1955). Robert Van de Castle should not be forgotten either; in his student years he sometimes participated as an assistant. Through careful field investigations, Pratt and Thouless as well as other ethologists who were not particularly interested in the possibility of a parapsychological component, made valuable contributions to this interesting branch of animal behavior (Pratt, Kramer & St. Paul, 1956; 1957; 1958; Pratt & Graue, 1959). The latest and, I understand, still not universally accepted explanation of the initial directional orientation of released homing pigeons is also based on careful field investigations with good examples of controlled manipulations of independent variables. It is now assumed that the original orientation—shortly after the pigeons have been released at some considerable distance from their home loft—is based on a non-random, relatively static distribution of particles in the air. There is now considerable evidence that these particles themselves, or their distributions or some other aspects of them, vary in a systematic way over large areas of the earth's surface and that pigeons can detect these variations through a sense of smell and are consequently able to orientate themselves shortly after release. It is not assumed that pigeons can smell their home loft from substantial distances, but it seems they are able to smell the air in which they are released in such a way that they can judge in which direction they have to fly in order to reach the kind of air that agrees with that experienced in the general vicinity around their home loft. Manipulations to test this hypothesis included temporary elimination of olfactory sensations as well as replacing the natural air from one releasing position A with the air from another releasing position B some substantial distance away. This was achieved by maintaining the pigeons at A in containers with air from B and by reducing their sense of smell through a spray for something like 30 seconds just prior to release from A. Significant orientation towards the home loft occurred within this time frame, but in agreement with release from position B. (Wallraff, 1983; Benvenuti & Wallraff, 1985). I hope that parapsychologists who are interested in field work, but who are reluctant to be involved because of the negative assumptions outlined earlier, will gain some encouragement from this particular example as well as from other research conducted by ethologists.

I have discussed some assumed as well as real advantages and disadvantages of laboratory and fieldwork research without specifically referring to spontaneous cases. It is reasonable to suggest that spontaneous cases can be more readily accommodated in field research, but I shall also attempt to point out how laboratory experiments may perhaps cope more successfully with spontaneous psi events.

At this stage it may be appropriate to ask (1) are all psi events spontaneous and, irrespective of the answer to this first question, (2) are those psi events which we can presently only record as spontaneous phenomena, spontaneous *per se* or is the inability to produce them as an act of volition perhaps due to the conditions under which we attempt to obtain psi results? Obviously it is difficult to answer the first question with certainty. The fact that we ask it points to some doubt about the volitional control that human beings can exercise. As a strategy it would be of considerable interest to contemplate a world in which only spontaneous psi events occur and to work out what are the consequences and how to incorporate them into our research designs.

When I refer to spontaneous cases I refer to psi events associated with a person or persons who experience these events over which they have no volitional control. It is generally assumed that additional psi events occur over which volitional control is possible. On account of the speculation that all psi events are spontaneous I may also call laboratory-based events spontaneous even though it is usually assumed that events generated in the laboratory are based on volition. I hope that in the following discussion it will not be too difficult to infer from the context which of the two categories is under consideration.

I agree with Stevenson (1987) that spontaneous cases need to be carefully investigated and in this discussion I am particularly interested in cases which still generate psi events and which may provide opportunities for direct investigations. As we know from RSPK cases (Roll & Pratt, 1971) spontaneous phenomena are not random events even when it is difficult to specify the exact conditions under which they occur. ESP card tests or micro-PK experiments are not usually considered to be investigations of spontaneous events; yet it would be possible to think of them as such and it would not be in disagreement with theoretical considerations derived from the observational theories (Millar, 1978) or from theories developed by Rex Stanford (1974, 1978). All we require are disposed systems, random event generators which have a bearing on the desired outcome as well as feedback opportunities and/or needs which are reduced or satisfied. All this can and may indeed occur without volition—whether consciously applied or not—having a bearing on the results. If laboratory experimentation which produced evidence for psi is really based on spontaneous events, then we can once more conclude that these events are not simply random, but follow certain regularities. If we accept that some regularities exist in connection with spontaneous psi phenoma, it should be possible to develop appropriate procedures and controls. That means that a working hypothesis which states that all psi events are spontaneous, does not suggest that we should abandon hope of ever controlling psi events to a desirable degree. It rather suggests that we adopt a different perspective which encourages such questions as (1) what are the regularities which can be discovered in spontaneous events and (2) what are the conditions under which human beings might experience psi events (over which they have no volitional control) more often? We may also find that a greater emphasis on field investigations is more appropriate.

In considering spontaneous cases we may be reluctant to take too much notice of them because, for each specific case, problems may be raised which do not allow a clear verdict as to whether all the events associated with that case could not have occurred without psi. I would suggest that if we are confident at all that we have evidence for psi from highly controlled, but rather artificial laboratory experiments, we must also expect that psi will operate in real life situations. Consequently, in at least some of the more doubtful cases, those that we would more often disregard, psi must also be involved. It is more difficult to know whether useful information can be extracted from a collection of cases which includes relatively weak ones as well as pseudo-cases. On the other hand the more severe the criteria are which we employ in order to obtain a case collection involving psi, the less representative of psi in real life situations is this collection going to be. Useful work with existing collections has been carried out by Sybo Schouten (1979a; 1979b; 1981) and perhaps some of his findings can be of help in obtaining samples which are more representative yet not unduly diluted by the inclusion of too many pseudo-psi events. Another consideration which should not be forgotten is that those spontaneous cases which include phenomena which can be measured and assessed in some way—that is, particularly PK phenomena—often display substantial strength. This is in stark contrast to most laboratory experimentation.

I became aware of the rather strange unrelatedness between the rate of apparent success in a PK experiment and the equivalent physical forces required to achieve success. Although to West (1983) this unrelatedness suggested further justifications for his skeptical views of parapsychology, my own experience was more positive. More than 30 years ago in my student years I worked in my capacity as a qualified fitter and turner for the head of the Physics Department of the University of Tasmania. We had constructed a micro-balance with a very thin quartz fiber of less than one-thousandth of a millimeter in diameter. In a controlled environment subjects were asked to influence the turning movements of this fiber, according to a random sequence, in such a way that it would turn more slowly during some trials and faster during others. The sequence of slow and fast target trials was determined through a random number distribution. With these laboratory experiments we obtained results which are statistically significant. The assumed PK involvement was not directly observable and it is not possible to point out particular trials with a definite PK component. Consequently the percentage of PK influenced trials cannot be clearly estimated.

The torque required to produce a change in the turning movement of the fiber could be fairly accurately calculated and, by further reducing the diameter of the fiber, we could accurately predict that if the same physical force acted upon the fiber, this would change the movements of the fiber substantially. However, the PK results which we apparently recorded, did not seem to be affected when the physical requirements were reduced. Under strict laboratory controls we had tried to reduce the required forces in order to obtain more substantial results. For this purpose we had reduced the order of magnitude by a factor of about 10, that is, one-tenth of the equivalent physical force would achieve the same measurable changes that we had obtained previously. Yet we continued to obtain similar significant, but relatively weak results. In a somewhat less controlled setting—however under conditions where controls were less critical because I had scaled up the micro-balance to something close to a macro-balance—I still obtained similar significant results when the equivalent physical force requirements were more than 100 times larger than in the micro-setting. Without going into the full details of the experimental setup, I must acknowledge that, although unlikely, it is not impossible that ESP rather than PK was involved. In that case the physical changes would not have been relevant. Nevertheless, my first introduction to psi indicated that parapsychological results do not readily conform to what we expect from changes in the physical setup.

Perhaps the results from the micro-balance as well as from the larger balance can be more readily understood if we consider them to be spontaneous events which only occurred intermittently and which, if necessary, could be strong enough to cope with the macro-balance. The frequency of the spontaneous occurrences could then be regarded as being relatively independent of the magnitude of the equivalent physical forces involved. The strength of the reaction of the micro-balance during a spontaneous occurrence might have been determined by the assumed need to succeed as experienced by the subject—probably below the level of awareness—or by the feedback which the subject obtained. For instance, if the subject who viewed the micro-balance through a telemicroscope did not perceive the reduced fiber as easier to influence—in fact, the subject could not see the thickness of the fiber itself, but only an indicator (which did not change) attached to the fiber—then there is no reason to expect that the subject's PK influence should increase when the diameter of the fiber was reduced. Similarly, when subjects were confronted with a much larger balance which could be viewed directly this may not have appeared as a more difficult physical system, because in actual size it was about equivalent to the image of the micro-balance as seen through the microscope.

With the micro-balance I also experienced a dramatic increase in the strength of what apparently were more haphazard PK activities. However, most likely, this increase was based on psychological factors which are much more difficult to specify than the physical changes which we had introduced.

The significant results to which I referred earlier had been mainly obtained with a subject who was employed as a secretary and who was quite happy to be of assistance, but who was not inspired by and probably not even fully aware of the wider implications of these experiments. The changes which we recorded and which were presumably due to her PK, were relatively small for single trials, and within the range of variations which occurred during control trials without a subject. Nevertheless, during her participation as a subject these changes occurred fairly consistently in agreement with the target requirements over a sufficient number of trials. Consequently a significant difference between the slow and fast target trials could be demonstrated.

When a senior student participated as a subject the results were quite different. In one way they did not support our hypothesis that the micro-balance movements would change in agreement with the slow and fast target sequence. On the other hand, the subject achieved movements of the balance which I had never observed before and which could not be explained through any external factors. I had built this balance, I had adjusted it and I had tested and worked with it under a variety of conditions and I knew to what extent variations could occur due to control limitations. For instance, temperature variations had an effect on the balance and we had controlled this by encapsulating the balance in such a way that temperature variations could be kept within one-tenth of 1 degree centigrade. For several months I had observed variations which occurred for various known and unknown reasons and I knew the range within which these changes had occurred. During trials with the student subject, the observed movements substantially exceeded any of the previously observed variations. Although I did not use such descriptive terms at that time, it seems very likely that I had witnessed directly observable PK, yet what occurred was not sufficiently in agreement with the requirements of the experimental hypotheses to manifest itself as a statistically significant result. An experimenter, only concerned with the experimental hypotheses, could have ignored the dramatic movements of the micro-balance altogether.

I should add at this point that the student apparently thought of the experiment as one that gave him an opportunity to demonstrate his will power and he became quite emotionally involved about it. This involvement seemed to be based on needs and expectations below the level of awareness and I doubt that direct instructions could have produced anything close to that psychological state which apparently had something to do with the dramatic PK results. In his final years, in a small department, the student may also have reacted to the interest in these experiments expressed by the head of the department. He also knew that the secretary had apparently been successful in changing the movements of the balance. In other words, the psychological interactions with other persons may also have played an important part.

If we assume that these psychological factors were largely responsible for the strong but haphazard PK effects, some suggestions can be made in order to take advantage of such PK occurrences. On the one hand we need to be prepared to investigate and if possible to record a larger range of variables which might indicate psi activities. On the other hand in general and on an individual basis we need to pay much more attention to psychological factors. Methodological purists (Timm, 1980; 1983; Hyman & Honorton, 1986) may be alarmed because the investigation of a larger number of variables increases the risk that a significant result for one may occur by chance and, consequently, that positive claims which are not really justified, could be encouraged. Although these risks exist they can be overcome in a satisfactory manner if either allowances are made for testing a number of hypotheses (where each one is regarded as a confirmation of the psi hypothesis, in which case an appropriately higher significance level must be demanded) or if only one hypothesis is selected (and confirmation of the psi hypothesis is limited to this one hypothesis) but additional hypotheses are tested in order to obtain better predictions for future investigations.

To pay proper attention to psychological factors which probably mainly operate below the level of awareness is a much more difficult task. Anthropological investigations should be useful and provide us with some insights. Reichbart's publication in 1978 was of considerable assistance, particularly when he pointed out that in many non-technological societies the occurrence of psi events was initially encouraged by various manipulations which, however, were not regarded and experienced as tricks by those who participated in these rituals.

The most significant practical contribution has, I believe, been made by Batcheldor (1979; 1984) and I shall return to him somewhat later. In the meantime it seems appropriate to point out that the experimental results obtained with the micro-balance when the student participated as a subject perhaps fit better into the framework of a spontaneous case study. I certainly do not wish to discourage laboratory research, but the micro-balance example suggests that it is unwise necessarily to exclude the possibility of a case study approach even when the initial investigations indicated a rather restricted laboratory experimentation. In appropriate circumstances and where it is suggested by the initial developments, I would also like to encourage the introduction of laboratory research as an extension of spontaneous case investigations or of field studies in general.

It may be of some interest to look at a second and, in a way, more typical example of spontaneous events. The case is more difficult to evaluate in overall terms, that is, whether any psi was involved at all, as well as with respect to particular aspects that have a bearing on our discussion. I prefer once again to consider events which I experienced myself because I can at least try to take into account a range of aspects including relatively intangible ones which cannot be readily extracted from published reports.

In January 1979, I was a member of a trekking party in Nepal (Keil, 1981) consisting of three Europeans and ten Nepalese including one English speaking guide. In Marpha our guide was bitten by a dog and, although the wound was only superficial, there was a real risk that he might have been infected by rabies. The owner of the dog was absent and according to medical advice in Jomoson the only way to find out for sure would have been to wait for ten days. If the dog had died, treatment was required in Pokhara not later than 14 days after the incident. Our guide who had only been promoted to this position recently, was anxious to continue as originally planned and after long discussions we were persuaded to walk as planned to the village of Chame. We expected to arrive there in nine or ten days. Chame had a post office with a transceiver and there was another one in Marpha. We intended to get in touch with Marpha and if necessary our guide could have walked ahead of us from Chame to reach Pokhara just in time.

A few days before we reached Chame we had started to wonder whether we had made the right decision. We were walking in a remote region which until 1978 had not been open to tourism at all. We had crossed a pass of nearly 18,000 feet. Chame and Marpha are separated by the Annapurna range with peaks above 2600 feet. At that time the communication equipment in Nepal was ancient and we wondered whether it would be possible to establish contact at all. We had also started to realize that virtually no other trekkers had come into this region. The pass was closed because of snow for several weeks only a few days after we had managed to go across it. The prospect of continuing the last section without our guide started to look decidedly more difficult. At that stage and including previous visits, I had been trekking in Nepal for something like seven weeks. After leaving the towns I had never met any Nepalese who could speak English. (I should briefly add here that with an enormous increase in tourism since 1979 the situation has now changed considerably.) It was, therefore, a considerable surprise to us when on the ninth day and about two hours from Chame a Nepalese stopped and talked to us in English. We had stopped for lunch and because it had started to snow we had stopped on the trail and we had not tried to find a rest spot some distance away from the trail as was our usual custom. As it turned out the Nepalese was from Marpha and was in fact the owner of the dog. He told us that she had been vaccinated against rabies, but was inclined to bite because she had pups nearby. Obviously this solved our problem, satisfied our conscious and perhaps unconscious needs and also provided feedback about the outcome of events which, perhaps with the help of psi, we had unconsciously initiated some time earlier. There were certainly a number of possibilities how psi might have operated in order to achieve the desired outcome, even though it is impossible to say whether, e.g., the timing of the lunch stop, the decision to stay on the trail for lunch or other potentially variable factors were involved.

As I indicated earlier there is no certainty that psi was involved at all. Beyond the factual details of this experience, which I tried to convey as briefly as possible, there were, however, also less tangible aspects which impressed me. The psychological distance between Marpha and Chame is certainly much more substantial than is suggested by the trekking distance. Marpha in the Kali Gandaki valley and Chame in the Marsyandi valley are separated by the Annapurna range. I doubt whether in 1979 one in 300 from either village had ever visited the other. The owner of the dog traveled on government business and had been in this region before, but he did not visit Chame more than once every three or four years. Whatever the events were which unconsciously with the help of psi we might have set in motion, it is quite certain that it did not occur to us at all that we might meet the owner of the dog.

If it is assumed that this case contained some degree of psi involvement, the question again arises how such an example might be of help in our research endeavors. The unconscious nature of the processes is perhap the most outstanding feature of this case. It would be reasonable to suggest that concern about the problems as we saw them was an important ingredient, but we had no idea how the solutions could be achieved. If we had any wishful thoughts, they were more concerned with establishing communications with Marpha and getting the news that the dog was alive and well. This suggests that within a complex system psi operates rather independently of our limited understanding of this system. That means, if we attempt to prescribe psi solutions to complex problems we may have less success than if we allow psi to find its own path, so to speak. To some extent this is recognized in laboratory research when subjects are asked to obtain so-called hits in micro-PK tasks rather than to influence particular sections of the set up on account of which the hits may occur. Nevertheless, from the laboratory based experimental point of view, this is often seen as regrettable—unfortunately it is not certain where psi might be effective in the system—rather than as a desirable procedure which is likely to be of assistance. But it is even questionable whether it is necessary and desirable to specify the experimental procedure as a psi task at all. The Klintman research (1983; 1984) which suggested that in certain reaction time experiments evidence for precognition may be found, is a recent example of an indirect psi investigation. Whether his results clearly established the involvement of psi is still open to questions. Some psychological aspects in the Klintman research were obviously less conducive to psi manifestations. If precognition changes the reaction times to certain stimuli, it is difficult to associate this with conscious or unconscious need fulfillment or satisfaction. It is also difficult to see how feedback could have played a major role.

Games of luck have sometimes been used in more or less indirect psi investigations. The competition between players may indeed increase the needs felt by some or all of the individuals, but the opposing needs of several participants probably do not produce the best basis for psi results. While it is impossible to suggest whether the assumed psi involvement in Nepal was due to one or more participants or whether a possible combined psi involvement may have been more effective than the sum of isolated single contributions, individuals in the Nepal group at least were in general agreement and expressed similar concerns and needs. The Nepal case included some further elements which we can perhaps recognize in other psi research areas. Before we stopped for lunch on our way to Chame it had started to snow. The snow created a fairly uniform perceptual field, muffled sounds and may have led to something like a ganzfeld situation. Most of the time we were walking. We were certainly not relaxing in comfortable chairs. However, the uniformity of the muscular movements may also have had a positive effect, perhaps facilitating group cohesion at an unconscious level. Similar activities can be detected in the Batcheldor groups as well as in various rituals.

This may be an appropriate point to look briefly at an area of what is regarded as relatively successful psi research in the laboratory and to ask how far the success on account of the ganzfeld (Honorton, 1978; 1985), that is, on account of an altered state of consciousness, may also be associated with aspects of the procedure. It is likely that this experimental procedure can better accommodate and record spontaneous psi events which occur in a variety of ways and to some extent in unpredictable ways than those used in traditional ESP experiments. I am not suggesting that the ganzfeld environment is irrelevant. The ganzfeld may indeed help subjects to generate spontaneous psi events more frequently. Nevertheless, the relatively passive, free response procedure may also be important, because it can cope with a wider range of spontaneous psi events than, for instance, the procedures used in ESP experiments in which direct hits are recorded.

Remote viewing experiments (Puthoff & Targ, 1975; Targ & Puthoff, 1977) have also been presented as relatively successful procedures with which evidence for psi may be obtained. On account of some methodological questions the case for psi through remote viewing experiments is probably not as strong as through ganzfeld research (Edge, Morris, Palmer & Rush, 1986). Nevertheless, if it is assumed that fairly strong psi results may be obtained, the free response procedures—which again allow for the inclusion of a wide range of perhaps spontaneous events—may have something to do with this.

If the magnitude in terms of the equivalent physical forces of psi events is considered and if some of the above speculations about the spontaneous nature of psi are relevant, the Batcheldor sitter groups can be recommended. Batcheldor developed procedures with which potentially large scale and relatively frequent psi events can be generated and investigated. The sitter groups are in many ways a modern version of the earlier groups that met for seances. But there are also important differences. There is no suggestion that any external agency is responsible for whatever may happen and the participants are gradually encouraged to assume that they themselves can create various psi phenomena. Batcheldor paid particular attention to two psychological ingredients which seem to assist in generating psi events. On the one hand his procedures help to reduce unconscious fears through a process which has some similarity with desensitization. The group setting reduces the personal responsibility of the individual and the meetings create an atmosphere somewhat removed from normal daily existence, which allows the participants to expect and to accept events which, under different circumstances, they would probably reject. A good deal more about this aspect of his procedures could be said (Corwin, Tart, Isaacs, Ehrenwald & Auerbach, 1986). However, it is sufficient to point out that this ingredient of his psychological mix is well suited to gradually eliminate fear and rejection of spontaneous psi events and increasingly encourages such phenomena. It also caters for unconscious processes. His second ingredient involves procedures which, to some extent below the level of awareness, are likely to raise expectations and increase belief that extraordinary psi events may occur. This is achieved by providing a setting in which unconscious muscular activities are bound to occur and are likely to be co-ordinated initially by some and gradually by most or all members of the group. On account of these unconscious activities physical movements of tables or other objects may be achieved by normal means which are nevertheless surprising to the participants and increase further expectations. To date this seems to be one of the best ways of creating, partly at a level below awareness, something like the needs and expectations that seemed to have been involved in the two examples which I discussed earlier.

Batcheldor himself has been decidedly cautious in his reporting and his work has only slowly made an impact on research in parapsychology. Partly this is probably due to the fact that such investigations can be easily misunderstood, ridiculed and distorted. I shall briefly return to this point somewhat later. It should also be acknowledged that not all the sitter groups obtained results with which the participants were satisfied. Among the apparently satisfactory ones only some can be regarded as psi events with a degree of confidence. Although the environment in which sitter groups operate can be controlled to some extent, it is not equivalent to a laboratory setting. Questions which arise are: At what point can we be sure that events are not just unconscious muscular activities? Given that events beyond this level occur, can they be accounted for by deliberate and/or other normal manipulations or is psi necessarily involved? The first question is likely to be evaluated differently by different observers and participants. As indicated earlier the mistaken assumption which most participants have, that unconscious muscular activities cannot be responsible for co-ordinated movements of objects, actually helps to build up expectations.

In my own assessment of these events, which I have observed in a number of sitter groups, I was initially inclined to be quite severe. I regarded all movements of objects which could be remotely based on unconscious muscular activities as outside the realm of psi. Nevertheless, for a period of time lasting for some months, one of the groups obtained results which I take seriously. When the participants gradually got bored with the events I found that not only those phenomena which I had regarded as psi events faded away, but also other movements which previously had occurred apparently more easily (I had thought they were based entirely on unconscious muscular activities) became more and more difficult. On the basis of this experience I am now inclined to think that psi involvement may have been more extensive. The realization that psi was probably involved when the movements could perhaps still be accounted for by unconscious muscular activities is not really surprising and is in agreement with more general considerations which I have outlined at the beginning of this paper. That is, if psi occurs under extreme circumstances—in an artificial laboratory environment or in the "one in a million" spontaneous case—then at least to some extent we must expect psi to be also present when alternative explanations cannot be ruled out. Perhaps I even have now an indication of how far unconscious muscular activities are responsible for the movements of objects, because presumably only these activities are still continuing without much change.

For the record I will briefly mention one movement of a table which I observed under adequate illumination. I was situated at the end of a fairly long and narrow table (48 × 18 inches or 122 × 46 cm). One participant was standing at the opposite end and two were standing opposite each other near the middle of the long side of the table. All participants had their hands clearly visible on the table. If I had used considerable hand pressure I was the only one who could have raised the table end opposite to me. This was the end that lifted up about 40 cm. While it was in this tilted position I took my hands off the table on my side. The table remained in the same tilted position. This indicated objectively what subjectively I knew, that I had not pressed down my end of the table in order to raise the opposite one. I then extended my hands as far across the table as possible and pushed against the other end. From this position I also carefully observed the other three participants and I could not discover any explanation for the position of the table. I could see the other three participants sufficiently to rule out any simple trickery. Pushing against the other end I definitely experienced a resistance maintaining the table in the tilted position. After about five or six seconds the resistance suddenly seemed to disappear and consequently the table dropped back to a horizontal position. I referred to one particular event, because on this occasion alternative explanations were ruled out to a considerable degree. Many similar events had occurred, but because of my own position in the room in relation to others and because of additional participants, I could not observe all of the relevant variables with the same degree of clarity.

I have no doubt that Randi would claim that these phenomena were due to deliberate or unconscious trickery and I would not be surprised if some parapsychologists had considerable doubts about any involvement of psi even if the sequence of events had been recorded by a video camera. After all, stage magicians perform tricks which can be recorded. However, we all know that a laboratory setting does not guarantee the absence of manipulations. If you have met a small group of people once a week for more than a year, this should also count for something as far as controls are concerned. I am not suggesting that people whom we know well and do not suspect of any wrongdoing should be regarded as being above suspicion. But observations over months provide some indications about involvement, motivation and, in this context, about dexterities and skills which might be consciously or unconsciously applied.

There are three further aspects which may have a bearing on the reception of such claims among parapsychologists. One is simply the magnitude of the alleged phenomena. When it is only intermittently possible to obtain statistical evidence for micro-PK in the laboratory by examining a large number of trials, any claim of such substantial happenings must appear to be somewhat suspect. However, it is precisely the order of magnitude and the opportunity to make direct observations which should encourage us to pursue this line of investigation. Any remaining doubts that a particular event was due to something else but psi, are not really different from the doubts which are involved in statistical evaluations of laboratory experiments, i.e., type I errors and/or experimenter effects which suggest relationships which perhaps do not exist. The second aspect is that large scale phenomena do not readily fit into the emerging theoretical frameworks to which I referred earlier. Von Lucadou and Kornwachs must be mentioned though, because their extension of the observational theories, which is based on a system theoretical approach, is less restrictive as far as large scale phenomena are concerned (Lucadou, 1983). Stanford's theoretical contributions (1978) were rightly applauded by Braud (1980). But in his evaluation he did not discuss the problem of large scale phenomena. Yet if intentional or unintentional manipulations can be ruled out with some confidence, some large scale directly observable phenomena probably provide the least distorted and contaminated indications about psi. I would suggest that such indications might be of considerable assistance in constructing or modifying theories. The third negative aspect is the parapsychologist's perception of how his research is perceived by his colleagues as well as by critics and skeptics who like to debunk this field. It is obvious that skeptics have a much better chance to distort and ridicule sitter group investigations as compared to micro-

PK or similar psi laboratory-based research. I do not suggest that such misgivings about the Batcheldor work are groundless or that they should be ignored, but there is some risk that through a process of rationalization we find arguments why laboratory work is preferable and perhaps superior, when our rationalizations are really motivated by a public relations concern. This concern is justified and in a certain place and at a certain time it may also justify the rejection of certain reasonable research procedures, as long as we realize why these rejections were made and as long as we do not elevate these public relations concerns to the status of superior research strategies for which there is no empirical basis (Keil, 1983; Mitchell, 1983).

For the record it may also be of interest to point out that in the development of the successful sitter group to which I just referred, at some stage progress was considerably slowed down. After some initial "success" probably entirely due to unconscious muscular movements, we had tried to generate much more difficult movements which, probably at an unconscious level, we still regarded as too difficult or impossible to achieve. When we tried to increase our movements by smaller more "believable" steps, progress was more clearly noticeable. Most of the time the group worked under adequate illumination. Once movements occurred it did not seem to be as important to maintain the same group structure as was suggested by Batcheldor. However, changes among the participants mainly occurred within a somewhat larger and compatible group of adults who had previously attended evening classes in parapsychology and who knew each other quite well. After the previously successful group had ceased obtaining movements which suggested a psi involvement, the addition of a new and still very enthusiastic participant from another recent evening class seemed to regenerate some aspects of the previous success. On the other hand, the introduction of a Philip type background story (Owen & Sparrow, 1976) did not seem to have such a positive regenerative effect. At the time when psi presumably was involved it was difficult to judge how far success depended on the presence of particular persons. The way unconscious muscular movements developed certainly suggests individual differences. It is quite likely that similar differences exist with respect to psi involvement, but this is more difficult to assess.

Returning to wider aspects of the Batcheldor approach, it seems to provide an opportunity to generate recurrent, large scale, directly observable spontaneous psi phenomena with relatively unselected people. Although the group setting and other aspects of the approach make controls somewhat more difficult than in the laboratory, once the scale of the events is sufficiently large, control opportunities are probably comparable to more traditional experiments. Spontaneous events are not only encouraged to occur, they are also controlled to some extent in the way they manifest themselves. This is not so much achieved by individuals, but by what might be called the collective unconsciousness of the group. Although the sitter groups probably provide the best opportunity of generating large scale psi events, different groups are bound to have different problems which are not easily solved by one set of rules. Although events may occur for a period of weeks and months and even years, detailed investigations may be neglected when events on an even larger scale are expected, but do not develop. Whatever the psychological factors are which allow a group to generate psi events, at some stage these factors are likely to diminish over time to a point when psi events will no longer occur.

The sitter group to which I referred earlier had one experience which perhaps suggests that additional manipulations of the procedures may have beneficial effects. The group met on the day Australia won the America's Cup in 1983. I was not present during this meeting, but the participants agreed that the general enthusiasm in Australia which was felt about this yacht race noticeably increased the strength of the phenomena which the sitter group experienced. This was at a time when in my estimation psi events may well have occurred. Consequently this externally introduced enthusiasm and elation probably increased the strength of psi events. This in turn suggests the possibility of deliberately introducing experiences which create additional enthusiasm and elation. Obviously it would be rather difficult to find enough races or similar happenings with a desirable outcome which inspire the general public. Nevertheless, there are now many films available on video cassettes which generate quite powerful emotional states and psychological experiences for a fairly wide range of viewers. Such additional experiences may well be beneficial for sitter groups who have reached a stage when they can co-ordinate unconscious muscular activities and who are starting to generate psi events.

Suitable videos may also create experiences which, at least for a limited period of time, suggest strong needs and goal-directed desires which might be of help in laboratory research. Unfortunately, it is more difficult to suggest how such artificially induced needs can be incorporated into more traditional experimental designs. It might be possible to select a video which succeeds in involving most viewers quite strongly in, say, one person, but where the commercially produced film finishes with a relatively open ending. Based on discussion with viewers a variety of different statements about the continuing existence of the main character could be prepared as video or simply as audio tapes. Under experimental conditions at the end of the video film a random event generator open to PK influences could be used to select one out of a number of the prepared additions. Under the impact of powerful emotional experiences induced by videos it may also be appropriate to record the changes which occur in a variety of labile targets situated in the vicinity of the subject, which may be affected by PK. These may not be the most appropriate ways to utilize artificially induced involvement, but considerations along these lines seem worthwhile to pursue.

Exploring the possibility that all psi events are spontaneous may encourage us to look for relatively non-volitional normal processes which in some way can be linked with paranormal ones. During the 50s and 60s creative processes were sometimes regarded as suitable human activities which might provide a useful linkage. To date such complex creative processes have not thrown much light on psi. Stanford's theoretical suggestions (1978) that relatively simple and, in a sense, primitive systems might be associated with psi activities are in some agreement with what participants experience in sitter groups. Unconscious non-volitional muscular activities as well as other more or less spontaneous activities may be worthy of more detailed attention in connection with psi investigations.

My emphasis on large scale phenomena which so far have mainly occurred in connection with spontaneous cases, but which, as the sitter groups indicate, may be generated repeatedly and under controlled conditions, is based on my assessment that an overemphasis on process research is likely to isolate parapsychologists from the rest of the scientific community. In the same publication from which I quoted at the beginning of this paper, Pratt came to a similar conclusion:

> "In my judgment, the acceptance of parapsychology as a legitimate and urgent field of research will come about through new and more compelling evidence of the reality of psi phenomena. It will not be achieved through the slower policy of confining research to process alone, not even if that course should lead eventually to an explanatory theory that is satisfactory to those who are already working in the field" (1978, p. 133).

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DISCUSSION

HARARY: I have a couple of questions. First of all, I am a little confused about your definition of spontaneous psi events and how what is happening in the laboratory can be classified as spontaneous psi. The second question is this: in your paper you say that the ganzfeld situation helps to generate spontaneous psi perhaps or can at least cope with psi better than other procedures. You also seem to suggest that ganzfeld makes a stronger case for psi than the remote viewing or other experiments. That is a very broad suggestion, and I'm not convinced that it is on target.

KEIL: To start with the question of what is meant by spontaneous psi events, my additional restriction here was that I assume that in what I call a spontaneous psi event, the person associated with the event has no volitional control over it. Now I agree that in most laboratory experiments we assume that people have some control over psi. I am not saying that this is not perhaps the case, but I am saying that perhaps we should contemplate, for arguments sake or for our strategies, a situation where there is no volitional control even in the laboratory.

Now you might ask how do we get results in the laboratory at all if people have no volitional control? Well, this simply means that if you have people in the laboratory who wait long enough you may get spontaneous psi events and you may of course also create conditions where these events may more frequently and readily occur than under other circumstances. But even in the laboratory you cannot really say that at this particular trial you influence psi and at the next one you don't. I don't think that we can make that sort of claim about psi in the laboratory. Consequently I would think that, at least as an argument, it is reasonable to suggest that what we get in the laboratory are spontaneous happenings over which people have no volitional control. The next question was I think the ganzfeld one. Can you just rephrase this a little bit?

HARARY: I was fascinated when you said that the ganzfeld shows more evidence for psychic functioning than, say, remote viewing.

KEIL: Now I didn't really say this or, if I did say it I didn't quite mean it that way. The amount of criticism that is leveled against remote viewing is somewhat stronger. Not being involved in this field, I take it on the basis of the literature that the general evidence for the ganzfeld is somewhat stronger. It may very well be that if one accepts the remote viewing experiments as actual evidence of psi, perhaps the rate of psi occurrences is even higher. I cannot judge this.

HARARY: I find your question about volitional control really a key to the way we do research. If we feel that the participants in psi experiments are incapable of actually determining when they will exhibit psi functioning, then we do a certain kind of research. We try to create conditions which bring out psi functioning or make it at least happen spontaneously. If, however, we feel that people are capable of actually determining when they will express psi functioning, then we do a completely different kind of experiment.

KEIL: In the past we have assumed, perhaps too much, that people can determine when their psi is functioning and perhaps we have built our experiments a little bit too much around this. I would suggest as a thought exercise that we consider what would happen if we assume that there is no volitional control at all. Is it possible to assume that at least some of our laboratory experiments can still fit into this picture? If then there is not all that much left, is it perhaps worthwhile to pursue a little bit further what we can do experimentally if, in fact, volitional control to that degree is not possible?

HARARY: In my experience it definitely is possible to have dramatic volitional control and to determine exactly what you are going to do, and when, by working with the specific psychological skills involved in psi functioning. A spontaneous event is not an abstract that we can't understand, but a definite set of psychological processes which are involved in deliberately deciding that you are going to express and experience what we're calling psi.

KEIL: In terms of your initial questions, did we come to an end or was there some other bit left?

HARARY: What determines an event? In my own opinion the psi event is determined by your experience of observing something which you consider unusual. Now whether something unusual has actually happened is another question. What could be happening is that you simply notice something happening and then you determine to call that a psi event because it seems unusual to you. Yet, it may not be at all unusual within the broader perspective of nature as a whole.

KEIL: I agree with that problem that we have. I think that in itself it could be presented as a complete paper, so I think it is best not to pursue it too far here unless someone wants to do it.

MORRIS: At the start of your paper you noted that in laboratory research maybe certain experimental work can only be carried out in truncated sections. Can you elaborate a little on your concept of the truncated section in experimental research?

KEIL: I mentioned in the paper an example from pigeon homing research. That was originally also of interest to parapsychologists. This is an example of the study of long complex behavior sequences. If this study had been pursued in very small units, as we tend to do in the laboratory, it would not have been meaningful research. In contrast to the hard sciences, where investigating small units usually has been successful, I think in the life sciences one has to admit that, at least in some cases, the piecemeal investigation of minor behaviors in psychology did not lead to the build up of meaningful theories. I am simply saying that, from our experience in the hard sciences, we should not necessarily assume that this is always the most successful approach. Nevertheless, in the life sciences it may at times also be a suitable approach.

MORRIS: Yes, I think that sounds very reasonable. I suspect that in both the hard and soft sciences truncated research strategies have more success. The more you study a simple closed system the more you eventually find yourself needing to examine it in context, thus invoking systems. When studying systems of considerable complexity such as ostensibly psychic events, that strategy no longer works.

STANFORD: I certainly concur with your inferences, Dr. Keil. There can be and often are what at least look like spontaneous psi events in the laboratory. Those are going to happen and the lab is not a place that keeps them out. Any scientist who does not keep his eyes open for the unexpected, the unplanned in an experiment is losing what are historically some of the most important sources in scientific research—what we call serendipity. Some of the greatest discoveries have been based upon this. Would you agree with me that when we observe these events in the lab, even though the conditions are well controlled for the original purposes of the experiment, the events that we observe may be somewhat ambiguous precisely because we did not plan to observe those events ahead of time and did not have the conditions set up to resolve uncertainties about their nature? Would you agree that the spontaneous events that we observe in the lab must serve as a basis of an hypothesis about which we must do further research? I have got just one more remark. I think we already have a paradigm, a kind of methodological paradigm, that has been used a fair amount in the literature which bridges the concerns that you have and the more traditional interest. That is the PMIR research paradigm; not the theory itself, but the opportunity to let subjects manifest what are really, from their standpoint, spontaneous events in the laboratory, but under conditions where we plan to observe those things ahead of time. I just bring that up because you did not mention that there is already a paradigm which we bridge across the two concerns.

KEIL: I certainly agree that further investigation is possible. Certainly my suggestions here are in no way directed against a scientific approach, that is, against further test and verification. In a short paragraph which I did not read I also pointed out that if you test the large number of hypotheses where each one could be taken as indication of a psi phenomenon then there is the risk that one of these hypotheses may come up by chance. You may have to resolve this by either specifying in advance the one on which you relied or by demanding a higher significance level for all those that you test. You can simply try to ascertain that what you got in the first place can be repeated in a follow-up experiment. I certainly agree with that kind of verification approach.

HASTINGS: You mentioned the problem that investigators feel they cannot control variables in the field as they can in a laboratory experiment. But in your description one possible equivalent is the concept of intervention. Sometimes intervention can be tailored to address a particular variable, so while it is not quite the same thing there is some level of investigation of variables that can be carried out in that way.

KEIL: Perhaps I did not express it sufficiently clearly, but I certainly tried to make the point that in my mind the field setting provides equal opportunities, but they are quite often not seen by the traditional laboratory scientist as being equal.

TART: We have used the word "spontaneous" a lot and it is interesting that nobody defined it. I thought I might mention a few ways in which that word is used otherwise they are going to get mixed together during the conference and we will get a little confused not knowing which one is which. You may want to respond to these.

One use of "spontaneous" is to mean that the event is perfectly appropriate to the current situation. I don't think we have used it in that way yet. Another use of "spontaneous" is unpremeditated—there wasn't a long term plan beforehand, but something happened just on the spur of the moment. A third use of "spontaneous" which I think is going to come in frequently is that "spontaneous" means "unreliable." We just cannot *make* things happen because spontaneous things happen.

Now, there is going to be a lot of difference in how unreliable psi is in that sense. I would argue that we certainly do not have reliability where the probability of psi manifesting is one. Nobody, I think, would claim that someone under certain conditions could produce perfectly accurate psi 100% of the time, no matter what. Some people would certainly argue that the probability can get pretty high and so we can always shift it a bit, that is what psi experiments are about. You set up a situation in the laboratory where normally nothing happens but chance. People try to use psi and they are far from perfect, but you do begin to get some results. We have some degree of reliability.

Now underneath that issue of reliability, though, is a deeper question. I think that is the model we have of what psi abilities are. For example, sometimes in the parapsychological literature I have seen the word "spontaneous" used to bring out an underlying model that psi is somehow *inherently* unlawful, that it is never going to be really predictable and controlled. That is an interesting model, but if you use that model it means your research is ultimately doomed. Your research can be a kind of artistic appreciation of the complexity and non-understandability of the universe, but if you use that model it does really inhibit the kind of research you are going to do.

There is another model that says psi is lawful, but it clearly is not lawful in the sense of a few easily understood variables that we have under control. So to explain our lack of reliability we have the unconscious model of spontaneous psi, which says it is lawful, but there are psychologically unconscious factors involved. This unconscious dynamics model usually has the addendum that those factors are often "perverse." They are going to make something happen that you are not set up to measure. Look more closely at this model. Do you see the unconscious as just something that interferes with our attempts to get rational understanding and control, so that the unconscious factor will always make psi perverse and keep results coming and going and being strange? Or do we have a model of possible growth, of getting into possibilities of altered states of consciousness, understanding your life? But do we have a model where the unconscious can be seen as *friendly* in a sense and ultimately much more able to produce reliable psi results? I would like to hear your comments on that. Just keep us sensitized to the issue of what's the model behind the spontaneous.

KEIL: I agree largely with what was said at the end, the unconscious problem which I equated with non-volition. But at the same time I think we should not assume too much reliability in the first place. I did suggest that psi events are not chaotic, are not entirely random and that lawful relationships can be found. But, as I think you indicated, these relationships are not as clearcut as we are normally accustomed to finding in the hard sciences or, for that matter, even in some of the life sciences. So my picture is not one of utter gloom. Nevertheless, it is one where, perhaps, the reality of the non-volitional aspects or the unconscious interference should be clearly kept in mind and perhaps explored a little bit more. It is not enough simply to assume that if you have a subject in the laboratory and tell him what to do and if what he is supposed to do is of a psychic nature, that he can do it. I think that is perhaps the other extreme from which I want to get away. I am not suggesting that the reality is one of complete lack of volitional control. But perhaps we should try to contemplate the lack of volitional control a little bit more in order to see whether by doing this we might come up with better research techniques.

MISHLOVE: I enjoyed your presentation very much, particularly your emphasis on an experimental approach which combines rigorous measures of psi, such as the model that you presented, with a field study measurement at the same time. It seems to me that there are two different kinds of measurements of psi that we try to make or two different types of hypotheses about psi. One kind refers to hypotheses relating to the actual measurement. Is it detectable? What kind of a system can be used to detect psi? What magnitude of psi is detected with that system and what are the time/space constraints of that system? Those types of hypotheses seem to me to be most ideally suited to the conventional experimental approach. Then we have process-oriented hypotheses about psi, the psychological variables, and in my view they are inevitably always contaminated by the experimenter effect. In order to get at the underlying mechanisms, as Dr. Harary pointed out, there do seem to be psychological principles. Yet they always seem to be, in my experience of looking at the field, unique to the particular experimenters involved. This kind of a multi-method approach offers perhaps our only possibility of getting at that. My final comment is that some of the ideal studies that I have seen in this area are those of Patric Giesler, with his multi-method research in Brazil, where he took random number generators into a field setting. There is also some of the work that Julian Isaacs has done, training PK, where he had anthropology researchers observing that training system. Could you comment on these notions?

KEIL: I have talked to Patric Giesler for quite a time and I certainly appreciate his approach. Of course, it is not often possible for ordinary parapsychologists, whoever they might be, to do that kind of field research. Perhaps for that reason I did not particularly address myself to it.

HARARY: I was very interested in what Charley Tart said about psi being lawful. I published a paper called "Psi as Nature" years ago involving the observer's relationship to apparent randomness and whether psi is lawful or whether we are dealing with completely capricious phenomena. I think we are dealing with something lawful here, otherwise it is not worth studying, as Charley pointed out. Even in your study 30 years ago you were comparing the influence of observable behavior, of something you could look at with a random sequence. What is the relationship between the random sequence itself and the behavior of that device? We cannot be sure that the effects were not coming out of an underlying structure in the randomness. What we see as an unusual effect may, in fact, be something in the inherent properties of the events themselves and not something that we ourselves are influencing. Just one response to Dr. Mishlove. I appreciate what he said, but I think that there are psychological variables that are unique to the experimenters involved at a particular time. But there are also some that must be continuous, wouldn't you agree, across psi functioning in general? Otherwise we will never get anywhere. We will be looking at completely different situations every time we do an experiment.

KEIL: Well, I certainly agree with that. I also refer to the Batcheldor groups. With a similar technique, relatively independent groups in various countries and places get similar results. I certainly agree that there is some lawfulness behind it. Coming back to the randomness of the events, it seems to me the general question in any PK experiment is whether what you get really has anything to do with the definition of psi, or with the random event generator? I suppose the only answer to that is to run the machine by itself when no one tries to influence it and then to compare these dry runs with those that a subject tried to influence. If the subject gets something that we do not expect from the machine, we suggest on the basis of such comparisons that the subject had something to do with it, but we cannot claim a causal relationship in absolute terms. I think we always settled with this kind of difficulty, but, if the relationship is strong enough, after a while we feel confident enough to say that something statistically significant is happening. Now, I think, when it came to the haphazard movements of the micro-balance it was interesting. Then of course you can only judge this by knowing something about the physical set-up and about its behavior over a long period of time, by checking earthquakes and that sort of thing. They even had this thing built on a concrete block, so the building itself could not influence the results. It was quite nicely controlled and on that basis I had a degree of subjective confidence that something was happening. But at the same time you cannot have absolute certainty. Something might have been externally happening which coincided by chance with the presence of the students, but it seems to be too much of a coincidence to reject. But to that extent I think that we are always talking in terms of the probabilities involved and not the certainties.