ARE WE MAKING PROGRESS?

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In research fields which are in an early phase of development, like parapsychology, the phenomena under study are mainly of a spontaneous character. Spontaneous phenomena occur in daily life, are not under control of the investigator and are observable without the use of special equipment. More advanced fields, like physics or biology, deal with properties of the phenomena on a more fundamental level. These properties are usually only observable in the laboratory under specific conditions created by the investigator.

Thus the research methodology changes with the development of a field. Research methods are always based on knowledge, or assumed knowledge about the phenomena to which they are applied. Research starts with systematical observation, which is based on knowledge of how to distinguish the phenomenon under study from other phenomena. Research then develops into experimentation. In experimentation research methods can be considered instruments which enable measurement of properties of the phenomenon. To be able to develop instruments for measuring requires considerable knowledge about elementary aspects of the phenomenon. Therefore in advanced fields the research methods reflect the progress in theoretical insight and the resulting technological achievements. In the beginning of the development of a field, however, such knowledge is limited. As a consequence the research methods can then only be based on assumptions made about the nature of the phenomenon. Such assumptions are strongly influenced by the outwardly observable characteristics of the phenomenon and by social factors. The development of research methodology in parapsychology illustrates this process.

A re-assessment of the research methodology in parapsychology, the topic of this Conference, involves basically the question whether the methodology applied leads to progress in the field. In view of the above two aspects of progress in methodology can be discerned. One concerns progress in the sense that the research methodology becomes better adapted to the knowledge or assumptions we have about the phenomena we study, in our case paranormal phenomena. Associated with this is

that the research methodology applied becomes more and more specific for the field. This aspect will be discussed in the first part of this paper. The second aspect concerns the results of the methodology, the question whether the methodology is successful and leads to progress in knowledge. This question is clearly too extensive to be exhaustively discussed in this paper. However, some important aspects of it will be considered in the second section, especially those concerning the problem of what criteria could be applied to estimate degree of progress in a research area.

I. The Development of the Research Methodology in Parapsychology and Its Suitability for the Study of Paranormal Phenomena

The First Period: Observation and Description. In the history of the development of research methodology in parapsychology roughly three periods can be distinguished. The first period starts in the last century with the beginning of scientific research in this field and lasts till the thirties of this century. In this period research is concentrated on the study of gifted subjects, persons who claim to be able to produce at will mental or physical psi phenomena. The methodology applied was mainly that of observation and description.

The aim of these studies was first to establish the genuineness of the phenomena produced, that is, to establish that the phenomena were not brought about by applying known perceptual or motoric abilities. Therefore much energy was devoted to ensure proper controls, which often lead to rather artificial test conditions in which the subject had to demonstrate his or her ability. Already from this period it becomes clear how strongly the methodology applied is influenced by the concepts the investigators have about the phenomena. Because paranormal phenomena were often *a priori* assumed to be of a non-materialistic, mental nature, the studies carried out were mainly directed at proving the truth of this assumption rather than being aimed at obtaining knowledge about the phenomena.

Only a few studies were carried out in which the effect of variables on the alleged psi abilities were systematically studied. An example is the Heymans, Brugmans, and Weinberg (1921) investigation, but it is noteworthy that even that study was carried out with one gifted subject (Schouten & Kelly, 1978). It might have contributed strongly to the impressive success of the study that the investigators put so much trouble in designing an experiment which created a test situation optimally adapted to the subject.

As a result of the research of the first period a consensus was growing

among parapsychologists about some general conclusions. A negative one: that so many mediums, especially the ones who produced putative physical phenomena, turned out in the end to be frauds that it hardly seemed worthwhile to further invest time and money in them. On the other hand it became quite clear that paranormal abilities were most likely a human capacity and that the spiritistic hypothesis was not needed at all to explain the phenomena observed. It also became clear that the descriptive method mainly applied so far did not contribute much anymore to an increase in knowledge. Most seriously, this method could not produce the kind of evidence many parapsychologists looked for to convince the scientific community that psi phenomena exist.

What probably also contributed to the growing feeling that change was needed was an increasing discrepancy between psychical research and experimental psychology. The two fields started in the last century on a more or less equal footing. Both as regards the way of thinking and theorizing, compare for instance Freud and Myers, and in the research methodology applied, mainly observation and description, there was not much difference. But for various reasons the two fields grew apart. Psychical research developed slowly, partly for lack of resources and because of other social factors, but perhaps also because it remained somewhat fixated on the aim to prove the existence of psi. At the same time psychology developed rapidly into an experimental field with new approaches and research techniques.

The Second Period: The Forced-Choice Technique. The second period is characterized by the large scale application of the forced-choice research methodology. It starts with Rhine's introduction of the card guessing paradigm and its associated methods of statistical evaluation. In itself this procedure was not new. Probably the first card guessing experiment had been already carried out in 1894 by Richet (Richet, 1921). In a first experiment different subjects guessed a total of 433 times playing cards hidden in opaque envelopes, but without success. A second experiment applying the same procedure carried out with a gifted subject, a lady Richet had known a long time, had better results. In 14 days the subject made about 5 guesses a day and obtained 12 hits in 68 trials, a clearly significant result. However, the same experiment repeated with the same subject one year later consisting of 65 trials yielded only chance results. Hence Richet experienced already what we are so familiar with. Research with unselected subjects often fails to produce results and research with selected subjects often turns out to be unrepeatable as far as results are concerned.

Rhine's introduction of the forced-choice method can be partly seen as an attempt to catch up with the developments in experimental psy-

chology. He felt a research method was needed which would be acceptable as regards evidential value to his colleagues in the other sciences and reports based on mere observations and verbal material were clearly not sufficient. Since psychology is involved with faculties which are under normal conditions possessed by all human individuals, Rhine probably reasoned that the likelihood of acceptance of ESP would increase if it could be demonstrated that psi was also a common human faculty. As a consequence, it was necessary to abandon the notion that psi abilities would be limited to gifted subjects. Whatever his reasons have been for defending so strongly the notion that all people have psi, the consequence was that research with gifted subjects became replaced by research with unselected subjects. Still it is noteworthy that some of the most impressive results from that period are based on studies with one or a few subjects. Examples are the Pearce-Pratt study with one subject (Rhine & Pratt, 1954) or the Pratt-Woodruff series, in which the high scoring was attributable to only five of the 32 subjects (Pratt & Woodruff, 1939).

Probably because of the controversial nature of research in parapsychology and the fierce debates raging about Rhine's results, little attention was devoted to the assumptions and rationale behind the forced-choice technique. One striking characteristic of this research method is that it appears so different from what happens in spontaneous ESP. It probably originated from the idea that subjects might get spontaneous impressions from the hidden contents of envelopes, a type of experiment sometimes carried out with gifted subjects or mediums. But in the application of the card-guessing technique the idea of obtaining impressions about the target was soon abandoned. It turned into a technique in which subjects in quick succession called out or pointed to cards. Hence one might ask why this methodology was considered so suitable to measure ESP. A common argument ran as follows. When subjects have to choose between a number of symbols or cards, there is no reason to prefer one symbol over the other. All symbols have an equal value to the subject and hence all symbols have an equal probability of being chosen. But if subjects have some psi ability then the probability of choosing the target increases slightly and this will result in a few extra hits in addition to what can be expected by chance. The total number of correct guesses might then lead to a statistically significant result.

Since the capacity for ESP in subjects has in itself nothing to do with the statistical evaluation, an implication of this justification for the method seems to be that the best strategy to optimize results is to use low probabilities for correct guessing. In that case only a few extra hits

by ESP will increase the total number of hits beyond the significance levels. But reality soon taught that such is not the case and Rhine settled on the one in five probability which became the standard after the introduction of the Zener cards. Clearly something must be wrong with the above reasoning. One explanation for this lack of relationship between success in ESP scoring and the number of alternatives in the forced-choice methodology might be that calling habits exert a stronger influence on the subject's decision than the relatively weak psi signal. In addition, but this is an assumption perhaps the inhibitory effect of such response bias becomes stronger if the subject has to choose from a larger number of alternatives. But this reasoning violates one of the basic assumptions on which the forced-choice technique seems to rest, viz., the assumption that the probability for each alternative is equal and that there is no reason for the subject to prefer one over the other. However, it is by now well known that subjects do display strong response bias when guessing targets and this indicates that subjects are not neutral as regards their choice of the different alternatives. So the logical conclusion would be that reducing response bias would lead to an increase in ESP scoring. This was investigated by me in an extensive study carried out at the end of the sixties (Schouten, 1975). In order to be able to reduce response bias, I first carried out a number of experiments to study the properties of calling habits. Based on these data I developed a theory of why subjects display the response bias they show so abundantly in their calling patterns.

The main finding was that subjects have an incorrect concept of what random is and that by guessing according to that concept they produce the non-random patterns we observe. This theory was to a large extent confirmed in the final study, the aim of the investigation, in which subjects received training to reduce response bias. This was of course not done by teaching them random response sequences, but by teaching them a different concept of randomness than what they had before. In fact, the aim of the training was to teach subjects not to employ any concept or strategy when guessing targets.

The experiment succeeded quite well in the sense that all subjects learned to guess targets with significantly less zero order and sequential bias. Of the 34 subjects, 28 subjects managed after on average 3.5 sessions of training to produce response sequences which fell as regards response-bias within the significance levels. Since each response series involved 400 calls it can be concluded that there has been a real reduction in response bias and not merely a reproduction of specific prelearned sequences which statistically conform to criteria for randomness. This conclusion was supported by the finding that the speed of

calling had increased and was higher for the latter, more random series. The calling speed for the random series turned out to be on average one guess in three seconds. This value does not indicate that subjects were consciously trying to construct random calling sequences. Hence I felt that in this experiment the reduction in response-bias has been real, and that in the last most random series in most guesses the alternatives had had an equal probability of being chosen.

The experiment was carried out with the aim of enhancing ESP scoring, but unfortunately the reduction in response bias did not result in higher ESP scores. To me that seemed to indicate that at least some of the assumptions on which the forced-choice method rested must be wrong. But the forced-choice method has more peculiar characteristics which give rise to doubt of its appropriateness as a research methodology for parapsychology.

After much discussion about Rhine's experimental results it became clear that merely demonstrating a significant excess in hits in an ESP forced-choice experiment did not yield much in terms of increase in knowledge, nor did it help to convince skeptics that ESP should be considered a proven phenomenon. So more and more studies appeared in which scoring in card guessing were compared under different conditions. But when the same subject was tested under different conditions the forced-choice method did not allow comparison with the number of ESP hits of that subject in the two conditions, but only the number of total hits which are made up of both "chance" hits and ESP hits.

Consequently only in the case of strong ESP effects or large numbers of subjects having some ESP ability might it be possible to find a difference between conditions. And even if such an effect is observed the size of the effect will probably not reflect the actual difference in ESP scoring. Hence it is no surprise that these studies often failed to have results and in so far as they did succeed the effects were nearly always small and often inconsistent.

A similar story can be told of the methodology applied in PK research. Here the study of macro-PK phenomena was replaced by the dice-throwing technique. At that time PK was still seen as exerting a mental "force" on the objects. This makes the PK dice-throwing technique the more surprising. There is no doubt that physically and neurologically it is impossible for human subjects to follow exactly the falling of a number of dice, and to predict during the fall how the dice end up when they come to rest. Consequently how could one expect a subject to know what force should be exerted and at what point, assuming that the subject would have been able to apply such a mental force. In fact, the dice-throwing paradigm for PK rested on the as-

sumption that subjects possessed two magical sort of abilities: not only PK in the sense of exerting a mental influence on matter but also an ESP ability to know when, where, and to what extent to apply that mental "force."

Even if the assumptions of the forced-choice technique are correct, and assuming that most people are able to exert psi abilities, then still the forced-choice technique is so insensitive that it would be unrealistic to expect consistent results over experiments. The contribution of "chance" hits to the score, the total number of hits, is simply too high compared to the effect of ESP on the scores. In research in other fields the situation is different. In learning experiments scores reflect largely learning, in perception research scores reflect largely perceptual abilities and so on. With the forced-choice technique the scores reflect mainly randomness. In addition, the statistical techniques we apply are mainly developed for research in which the scores do largely or entirely reflect properties of the phenomenon under study. Thus in an analysis of variance the "noise" is reflected in the variance of the scores, but the scores themselves are representative for the phenomenon under study.

Although this discussion has only touched upon a few aspects of the forced-choice methodology it suffices to illustrate that many questions can be raised about its appropriateness for application in research in parapsychology. Especially in view of the way Rhine and his followers thought about ESP the method seemed not well suited. However, I know well that in the end it is never such considerations which decide whether a research method becomes widely used or not, but that other aspects of a more pragmatic nature are decisive.

Very attractive properties of the card guessing paradigm are its simplicity, the speed with which data can be collected and its cheapness. No complicated equipment or housing facilities are needed, a simple deck of cards suffices. But above all the most important consideration has probably been the fact that, despite the inherent improbability, the method seemed to work and nothing attracts more following than success.

The Third Period: A Variety of Research Methods. The sixties of this century can be viewed as a transition period, not only in a cultural sense for Western societies, but also for experimental parapsychology. It can be considered as the beginning of the third period in the development of research methodology in parapsychology.

Dissatisfaction with the progress in the field, a feeling that the possibilities of the nearly generally applied Rhinean methodology of forced-choice ESP and dice-throwing PK were exhausted and, above all, growing doubts about the success of the forced-choice paradigm as a

research technique for studying ESP, created a strong interest in other, more promising methodologies for research.

The 1968 Parapsychology Foundation Conference, which was also attended by scientists of repute from other fields such as Karl Pribram, Henry Margenau, and W. Grey Walter, reflected this mood. At this conference the discussions, which were limited to ESP, concentrated mainly on three new techniques: free-response studies coupled with ASC induction as exemplified in the dream research, psychophysiological studies, and animal research. To a lesser extent this tendency also showed at the 1968 PA Convention held in Freiburg, Germany. Of the four PK studies reported only one was of the forced-choice type. As regards ESP studies the forced-choice method still dominated, but of the 18 already 7 employed other than forced-choice techniques. Within a few years this trend resulted in the nearly complete disappearance of the dice-throwing studies and in a strong reduction in the application of card-guessing in ESP research. What replaced the old methodology was a much wider variety in research methodologies, of which two techniques became strongly dominant. In ESP research this was the free-response approach, especially in combination with the ASC induction technique of the ganzfeld sensory isolation procedure. In PK research the RNG studies became the standard approach.

Whatever its further merits, it can be argued that, as regards its assumptions, the research methodology of the last 20 years is much better adapted to what we know or assume about ESP. Free-response ESP is more comparable to spontaneous ESP. It can be considered as a method to provoke spontaneous ESP under more or less controlled conditions. Not only does it better reflect ESP as it shows in daily life, also the theoretical background of the free-response technique as it is applied in research appears more plausible. Given the assumption that ESP exists and can be considered an ability, the noise-reduction model as applied by Braud and Honorton seems logical. At least this approach does not carry with it the inherent inconsistencies from which the forced-choice methods suffer.

Another important advantage of the free-response technique over the forced-choice method is that in principle the free-response method yields scores which are more representative of the degree of ESP transmission. That is because free-response scores are based on agreement in various aspects between mentation and target. On the other hand, there are still many problems not really solved at the moment which diminish the value of this method. Because an essential feature of the free-response technique is that the content of the subject's experience matches the target, the evaluation should be based on the degree of

agreement between mentation and target. But we still lack sensitive evaluation techniques to do this and therefore the free-response experiment is often evaluated as if it had been an extremely time-consuming, guessing task.

Another problem which needs more attention is that hardly any studies have been carried out in which a direct comparison has been made between the effectiveness of free-response studies in comparison to other methods. The best we have are indications as provided by Honorton (1978, 1985, 1986, and also at this Conference), based on meta-analyses, that in terms of relative number of significant experiments free-response studies, especially the ganzfeld variety, are superior to forced-choice techniques. However, the most convincing evidence can only come from studies in which the scores from the same subjects obtained with the different techniques are directly compared and when it is found that the free-response condition does yield higher scores.

Two other promising new techniques were also extensively discussed at the 1968 Parapsychology Foundation Conference: psychophysiological research and animal studies. For these two approaches it also holds that the rationale for the techniques makes sense. Psychophysiological studies can either yield indications that certain bodily states are conducive to ESP or that psi effects which are still unconscious to the subject are detectable by reactions in the organism. Animal research rests on the rationale that if ESP is a property or ability associated with humans it is also very likely that it can be found in other biological organisms. In addition there is some evidence that animals sometimes succeed in feats which are difficult to explain even when accepting extreme sensorial sensitivity.

Hence it can be concluded that the present methodology applied in ESP research seems better founded and more appropriate to the phenomena than the forced-choice methods and in that respect we might say that progress has been made. However, this holds only insofar as ESP phenomena are assumed to be the result of a still unknown ESP process. The philosophy of the Parapsychology Laboratory in Utrecht has been that other models should also be considered in our search for the explanation of paranormal experiences. We know that various psychological factors must have an effect on ESP experiences. So it seems worthwhile to explore also models which do not assume an ESP process, but try to explain these experiences by applying psychological concepts such as, for instance, attribution. In such an approach research would for instance, be aimed at studying when and under what circumstances people have ostentative ESP experiences in their lives and what function these experiences have. I feel that this approach has been too much

neglected by parapsychologists and in this respect more progress could be made. It has been more or less left to the critics, but they have made little contribution in this direction.

On the surface, the present-day RNG PK studies look like a modernized version of the dice-throwing technique. The difference, however, lies again in the degree of association between the type of phenomenon we think we are dealing with and the research methods applied. As shown above dice-throwing is a somewhat illogical test procedure when PK is considered as an ability to exert a mental "force" on material systems. The RNG studies, however, are based on an entirely different concept of PK and the methodology applied seems well-suited to that concept. The work of Schmidt and Walker have yielded the different versions of the observational theories. Others have presented novel theories like von Lucadou and Kornwach's "Model of Pragmatic Information." All these theories have in common that only random processes can yield PK effects. Thus as regards modern PK methodology we might even say that in this case theory came more or less first and that the research methodology was derived from it. In that aspect it appears that the research methods we apply now in PK are optimally suited to the phenomenon under study.

There is still a problem, though. Although the modern theoretical views on PK seems to many to reduce strongly the incompatibility between the concept of PK and modern physics, a view I do not share, it has increased the difference between PK in the lab and what can be considered spontaneous PK. Even if we reject all claims of the physical mediums, then we are still left with the problem of explaining the poltergeist phenomena. And, poltergeist phenomena are more suggestive of forces exerted on stable macro-objects than that they suggest OTs as an explanation. Hence as regards spontaneous PK one might argue that the present research methodology is rather a step back instead of an improvement. It appears that OTs are better suited to explain ESP phenomena than spontaneous PK and that therefore the micro-PK studies are rather to be considered as part of the research into ESP. Apart from descriptions of poltergeist cases there is not much research carried out anymore which might increase our knowledge about spontaneous PK phenomena.

There are of course more methods applied at present in research in parapsychology than the ones discussed above. For instance, there is research in the analyses of spontaneous cases where in my opinion also good methodological progress is made. But free-response and micro-RNG studies are by far the most popular, even to the extent that they tend to one-sidedness.

The overall conclusion seems to me that as regards the relationship between the phenomena, or theoretical concepts about the phenomena, and the research methodology good progress has been made in ESP research, with the exception of the "psychological" approach, but little in research is aimed at explaining spontaneous PK.

II. Does the Research Methodology Yield Results?

The fact that the research methodology becomes better adapted to the phenomena under study does not automatically imply that the research will also become more successful. It seems, however, an essential condition which must be fulfilled before success can be expected. The evaluation of success of research depends n many factors, among others on how success is to be measured.

By What Criteria Can We Judge Progress in Science? Success or progress in science is a multi-dimensional concept. Its evaluation depends on the criteria one applies to judge progress by and of the level to which the current position is compared. Many fierce debates in parapsychology have actually been discussions in which implicitly a specific definition of progress was applied. One example is the frequent discussions on the repeatability issue. From these discussions it appears that authors sometimes base their opinion on the often not outspoken assumption that no progress can be made at all unless we have a repeatable experiment. A very specific concept of progress indeed. Another example is the critic's assertion that a hundred years of research in parapsychology have not yielded any results and that therefore the subject can be discarded. That judgment is mainly based on the perceived lack of control and predictability in parapsychology. Consequently, they apparently consider these criteria as conditions which have to be met before any degree of progress can be attributed. In its most simple form the concept of progress involves the following elements: starting point, the present position, the distance to the position one wants to reach and the speed with which the present position was reached. These elements constitute a scale and as with every scale it assumes a dimension along which the scale is to be used and a unit to express distances on the scale.

In science the question of which dimension one should apply to measure progress is rather unclear. When criteria of a practical nature are chosen then progress can be expressed in, for instance, the increase in number of research institutes, or funds allocated to research, or increase in applications from applied research as expressed in the number of patents awarded. With such criteria the problem of what unit to select

to measure progress by is automatically resolved: the number of institutes, or dollars, or patents. However, such criteria are better suited to be applied to more developed sciences than to a science in its infancy such as parapsychology. In the special case of parapsychology another criterion could be acceptance by and integration into the other established sciences, which can be expressed in the number of university affiliated research institutes or professorships. Although all of the above mentioned criteria are important dimensions of progress, most people will first of all associate progress in science with increase in knowledge. But progress in knowledge is rather difficult to define. There is often no evident criterion to apply to the concept of knowledge, and no simple unit exists to express degree of knowledge in.

Hence what often happens is that progress in knowledge is expressed not in terms of increase, but in reaching a certain level of knowledge. Thus Lakatos, for instance, offers us a criterion that progress in science exists when the theoretical growth anticipates the empirical growth, i.e., when theory proves successful in predicting novel facts (Lakatos, 1978, p.112). Another example is the above-mentioned criterion of the repeatable experiment. It is clear that in parapsychology, as well as in many other sciences, considerable progress must have been made before such levels of knowledge are reached. In fact, in my opinion, the repeatable experiment, in the sense of repeatable results of experiments, in parapsychology will only be realized after we have obtained more or less full knowledge about ESP. It will be the result of our research efforts rather than a condition which has to be met before we are allowed to do research.

Criteria for progress as mentioned above, which are based on reaching a certain level of knowledge in the future, seem to me rather useless for assessing the present state of affairs. There is simply no way to predict when or whether a certain level of knowledge in the future will be acquired. The best one can do is to express the noble wish that such levels will be reached. To state that a field can only be considered a science after such levels are reached is in fact denying that field the chance to ever become a science. Because, especially in developing sciences, there is no unit to express degree of knowledge. Such levels, even if it was certain that they would be reached one day, are also useless as a point of reference as compared to the present situation. Even assuming that psychology would one day reach the level Lakatos requires, it is impossible to say, for instance, that psychology has now covered one-third of that way. The same holds for parapsychology. It is possible that one day a consensus will exist that a specific research method yields repeatable demonstrations of ESP, but there is no way

to predict when that day will be and how far we are away from it. It might be around the corner, as some people believe in the case of the ganzfeld technique, or it may be centuries away or perhaps that day will never come.

Thus for various reasons it appears unsuitable to apply future levels of knowledge as a criterion of reference to express the present state of progress. Hence we are forced to evaluate progress by comparing the present situation with the situation some time in the past. However, that does not solve the problem that we lack a clear unit to express increase in knowledge. Suppose we accept as an item of knowledge that sheep score better in ESP tests than goats do. How can we compare this with for instance the finding in psychology, that male infants are more irritable and physically active than female infants (Gleitman, 1983, p.320)? Which of the two statements involves more knowledge or indicates more progress? Because the value of such bits of knowledge partly depends on the meaning individuals attach to them, and because that meaning depends on interest and various other "subjective" criteria, there is no meaningful and objective way to compare the value of different statements of knowledge. For a parapsychologist the first statement is of more importance than the second; for a psychologist working with children the reverse holds.

Another important aspect which makes it nearly impossible to compare the respective value of items of knowledge from different fields is the uncertainty concerning the validity of the findings. How certain can we be of the statement that in an ESP experiment sheep score better than goats? There are hardly any findings in parapsychology, nor in the social sciences, which are unchallenged and can be considered undisputable. In our field it is the rule and not the exception that positive findings become immediately criticized. In fact, it sometimes appears to me that this is one of the favorite pastimes for some people in the field. But to a lesser degree the same can be said of psychology. Therefore it seems not very useful to base a discussion about progress in a field on specific findings from that field and the value which should be attached to these findings. Hence unless we are willing to restrict the assessment of progress to the simple conclusion that we now know more about parapsychological phenomena than people did in the past, an assessment I am willing to endorse, we need some other methods of reference to indicate the degree of progress. To this end I propose to apply the following criteria to assess progress in parapsychology:

1. The extent to which research has been able to reject incorrect ideas about the phenomena.

2. Does the research have an effect on changing opinions in society about its subject matter?
3. How does the progress in the field compare with the progress in comparable fields?
4. Can the field be characterized as a cumulative science?

The Rejection of Incorrect Ideas about Paranormal Phenomena. The first criterion concerns the rejection of incorrect ideas or explanations. When the present situation is compared with the past we cannot say that research started with nothing and that now thanks to research we have obtained certain ideas about the phenomena. Science is often seen as a development in which no knowledge is gradually replaced by knowledge, but that is in general not true. When we are dealing with spontaneous and observable phenomena it is seldom the case that in the pre-scientific stage people did not have ideas or assumptions about the explanation of these phenomena. Such ideas exist prior to and in every stage of the development of research. The development of science is therefore better characterized by a gradual rejection of many incorrect ideas before proper explanations are found. Thus in the beginning of research the elimination of incorrect views on the phenomena plays a dominant role and can be considered a condition which must be fulfilled before real progress can be made. Indeed, there are many examples in the history of science that especially when a field started to develop, erroneous ideas or concepts for long periods of time effectively prevented the development of fruitful research. Therefore it makes sense in the case of a developing science to express progress in the degree to which the field has been successful in correcting and rejecting incorrect views on its subject matter.

In the case of research into paranormal phenomena we can certainly find examples of how research has gradually resulted in the rejection of once commonly held convictions about these phenomena. But one can never say that research alone brought these changes about; other developments in science and society have undoubtedly also contributed to them. For example, when research started, about 100 years ago, the spiritist hypothesis dominated. On this hypothesis ESP phenomena were mainly considered as an act of the deceased. Since experimental evidence does not support that hypothesis it has dropped out of the field. Another common notion about ESP was based on the telegraph model. A sender was supposed to take action as regards transmission of a "message" and only then could it happen that the percipient by telepathic means "received" that message. Despite the fact that we still employ agents and percipients in experimental settings, I think that few researchers support that model anymore. The principal role of the

experient is now generally accepted, as is so for instance, exemplified in Stanford's PMIR model or in the ASC studies. Other examples can be found in changing convictions about the limits of ESP. Although theoretically we still toy with the idea of the omnipotence of psi the limited effects of ESP in reality are now well recognized.

Thus we know that it is not possible to pick the brain of another person by ESP, or to use ESP to find out what's going on inside the White House or the Kremlin. Also we can assure people that it is not possible to use PK to make other people do what we want. A few more examples of which I feel the results of our research will have an impact on at present are still widely held opinions. It is believed by many that mediums or paragnosts are either swindlers or people gifted with ESP who are able at will to obtain paranormal impressions. Research indicates (Boerenkamp, 1988) that both opinions are probably incorrect. Most mediums do believe in their "paranormal" abilities but from our point of view they are not able to demonstrate them. Mediums might have an occasional spontaneous ESP experience, but their success with people seem mainly based on normal psychological abilities. Another example is provided by paranormal healing. Although more research is needed in this area the findings so far suggest strongly that paranormal healing is effective, but not because of a PK influence or other unknown influences exerted by the healers (Attevelt, 1988).

Although parapsychologists might differ in their views and not everyone will endorse the opinions stated above, it can not be denied that parapsychology is a field in movement. Thanks to research, and often the lack of result of research, we learn the limits of the phenomena we study and are forced to adapt our opinions accordingly. The field is not stagnating and in this respect we can be considered to make progress.

Do the Results of Our Research Affect Society? Closely related to the criterion of the rejection of incorrect ideas is the question whether progress in the field is reflected in changing opinions and attitudes in society about these phenomena, especially changes in misconceptions or harmful practices. Often a relationship will appear between progress in a field of science and the extent to which people change their attitudes or behavior towards the phenomena in question. Medicine constitutes a good example in this respect. The progress in that field has gradually changed people's attitudes towards diseases and their opinions about what should be considered healthy and unhealthy ways of life. A field which makes no progress in knowledge will hardly be able to affect people's opinions. Therefore a second criterion to judge progress by

is to consider the changes in attitudes and opinions about paranormal phenomena of people not directly related to research in the field.

In this respect I feel that our progress is less than it could have been, but it is difficult to judge what one could reasonably expect. In general there appears a time lag between the acceptance of new insights within a science and the time that this knowledge penetrates society and becomes commonly known and accepted. For instance, quantum physics dates from the twenties, but started to spread in society in the sixties and seventies. In other areas, however, as in the case of medicine, the dispersion of ideas seem to take place at a faster rate.

There are examples which indicate that changes in opinions within the field affect general opinions outside the field. One is the above mentioned rejection of the spiritistic hypothesis and the growing conviction that ESP is a form of human experience. On the other hand, it is clear from most of the popular and unscientific literature dealing with paranormal phenomena, the "paraporno" as Martin Johnson calls it, that many misconceptions are still very much alive.

As regards the "paraporno" I feel parapsychology has failed to take a more aggressive attitude towards this nonsense. Parapsychologists know best what the possibilities and impossibilities are as regards psi phenomena and hence it is more or less our responsibility to present this knowledge to society and to take action if views are presented which are at variance with what we know. However, I realize well that in some respects we have a more difficult task here than scientists normally have. One is the lack of resources in the field. The few people who do research in parapsychology work in general under rather adverse conditions and simply lack the time. Another negative condition is that parapsychology is one of the few branches in science which is systematically persecuted by organizations who are critical of research in this area and accordingly try to lower the status of its researchers. One consequence of this is that much time is wasted on rather useless debates, time which could have been used in more meaningful ways. Another is that the impact of what we say on parapsychological matters is less than it could be. That is a pity, because this effectively strengthens the position of the "paraporno" producers.

The Progress in Psychology as a Reference for Comparison. The above discussed criteria for progress remain restricted to the field itself and, although informative, do not say much about what value we should attach when, according to these criteria, progress or lack of progress is observed. The statement that a certain car carries a price tag of $10,000 tells something about that car but becomes really meaningful only when that value is compared with the price of comparable cars of

other makes. Thus a meaningful evaluation of progress in parapsychology can only be made based on a comparison with the situation in other fields of science. It stands to reason to select for such a comparison a field of science which in most respects resembles parapsychology and, in my opinion, the best choice for this is the field of psychology. Hence as a third criterion to measure degree of progress I propose to compare the investment in resources and progress in parapsychology with those of psychology.

As explained above, it is difficult to compare two fields by comparing the relative value of the knowledge obtained in these fields. The best one can do in this respect is to form some global impressions. What we can do, however, is to compare more concrete issues which are related to the matter of progress and the validity of the applied research methodology. Such issues are, for instance, the above discussed aspects related to the impact research has on society, which in the case of psychology means mainly its usefulness. Or issues such as whether the fields have reached the stage that repeatable experiments are carried out, or that the foundations of a solid theoretical framework as a basis for the whole field are established. In addition, a fair comparison should also consider the differences in resources between the fields to be compared. In the following, therefore, I will discuss the state of affairs as regards the development of knowledge and some other important issues in the two fields. In addition I will try to give an estimate of how the resources in the two fields compare.

Opinions of Psychologists About Progress in Research in Psychology. It is outside the scope of this paper, and beyond my capacities, to provide a detailed discussion of all claimed findings in psychology, and the pro's and con's of the research arguments which support these findings. What can be said, however, is that in contrast to a science like physics most findings and developments remain disputable. Newton's laws are generally accepted, but psychology hardly knows any laws and few results of psychology go by unchallenged. It is striking that the most solid findings in psychology are those in perception research and psychophysiology, especially in areas which deal with the neurophysiological basis of perception and behavior. Psychology as a science of behavior and cognitive psychology seem to yield less convincing results whereas psychology as the layman views it, the study of inner experiences, seems hardly to have any consistent findings at all. These seem perhaps bold statements, but I think many examples can be found in which psychologists of repute offer explicitly or implicitly similar views.

Take for instance the not insignificant field of attitude-research.

Abelson (1988) discusses the problems concerning the findings in this area after decades of research and mentions as the most important:

1. The measured attitudes turn out to be poor predictors of behavior despite the presumed meaning of attitudes as predispositions toward behavior;
2. Respondents often conform to the demands of the questionnaire by concocting superficial attitudes on the spot. Such attitudes are extremely labile over time and have come to be called "nonattitudes";
3. Procedures which create changes in attitudes in laboratory research fail to do so outside the laboratory.

In fact, in my opinion this amounts to admitting that attitudes can not be reliably measured and that findings in this area have no relevance outside the laboratory.

Another example presents the continuing debate about the validity of clinical psychology and its practical applications. As recently as 1982 several publications were devoted to the question of whether a meta-analysis carried out on 375 studies of psychotherapy and counseling justified the conclusion of the authors that psychotherapy does work. (See among others Landman & Dawes, 1982.) And this concerns one of the oldest and most extensively researched areas in psychology. But similar remarks can be found for other disciplines as well. Pion and Lepsey (1984) state that "Many critics have argued that psychology has a less than impressive record in understanding and explaining human behavior" (p.743). That statement can undoubtedly be more strongly formulated when it concerns human experience. Or take Fishman and Neigher (1982). They state: "our discipline's own admission through writers such as Cronbach . . . Epstein . . . and Wachtel . . . is that the research accomplishments of psychology have been disappointingly small" (p.542). Wachtel (1980): "Nonetheless, the state of our field seems to me to leave much room for discontent" (p.399). Gibbs (1979) wrote,: "In perception and memory, in learning and development, in social influence and attitude change, one hears the same lament of trivial and irrelevant research" (p.127), and adds: "Those voicing laments and pleas include some of the most prominent names in modern psychology" (p.127). He continues these statements by citing many examples.

The generally felt doubt in psychology becomes already apparent in introductory books on psychology, such as, for instance Gleitman's *Basic Psychology* (Gleitman, 1983). Despite the fact that in this book a fairly rosy and optimistic picture is given about psychology's findings, it is striking how often one finds cautionary remarks offered and conflicting opinions presented. Already the style in which the field of psy-

chology is presented is entirely different from for instance an introductory book on physics.

The above cited opinions from psychologists, and many more can be found, suffice to indicate that within the field of psychology serious doubts exist as to the progress it is making and as to the validity of most of its findings. In that respect psychology is not much different from parapsychology. Our field suffers from similar uncertainties as regards the validity of its findings. What is important, though, is to realize that we are not the only ones who suffer from this feeling of uncertainty. Therefore the tendency often seen in our field, to blame this uncertainty on our subject matter, or research methods, or to use it as a starting point to discuss the question of whether parapsychology can be a science at all, seems to me strongly exaggerated.

Physics was not built in a few centuries, and for various reasons physics might turn out to be an "easier" science to develop than psychology or parapsychology, if only for the reason that physics could start with a large number of phenomena which by nature are already consistent and repeatable, and which can be isolated, a type of phenomena the human sciences largely lack. That some psychologists feel the same is for instance voiced by Wachtel (1980): "Psychology is about the hardest discipline to do research in" (p.403) and further on: "To do really good research in psychology, research that really breaks new ground or gives definite answers to important questions (as opposed to research that simply makes it into journals) is exceedingly difficult" (page 403). If that can be said about psychology, it certainly holds for our field.

Important Issues in Psychology and Parapsychology. In addition to the criterion of research findings the two fields can be compared as regards aspects which are in a different way also indicative of the level of progress. Examples are such issues as repeatability or applicability of the findings. It is no coincidence that such aspects coincide largely with important criticisms leveled against parapsychology. In fact, I believe that a main reason for finding so many experimental psychologists among the fiercest critics of parapsychology is that parapsychology functions as a kind of mirror which magnifies strongly the weaknesses of psychology itself. For reasons of space I will only consider a few, especially those who are of relevance for the issue of the methodological approach.

The rather exhausted subject of repeatability, the topic of the 1983 Parapsychology Foundation Conference, is not only of great concern to parapsychologists. Although the opinions differ, I suppose that most parapsychologists agree that as yet we have not found the repeatable

experiment with which to demonstrate psi, or, more accurately, to demonstrate specific effects from conditions on psi. I concur with that position. However, here again we are not such an exception as many seem to believe. Westland (1978) flatly states that "numerous literature studies of surveys (in psychology) have shown that reports of replications of "successful" research studies are rarely published" (p.98).

I have already mentioned Abelson (1988) who implicitly states that in attitude research the degree of uncertainty is so large that most findings must be considered as unreplicable. Also Fishman and Neigher (1982) speak about "single-study experiments with data that are unreplicated, under aggregated, and biased" (p.542). In general it is felt that replicability in the sense that it allows predictions to be made, or that it yields reliable applications, is very poor in psychology.

An area closely related to repeatability is the usefulness or application of research findings. Effective applications can only be based on solid and repeatable findings. Therefore lack of applications or doubts about them tells a lot about the degree of repeatability of the findings on which these applications are based. Here again psychologists themselves are aware of the dubious nature of many of their achievements. Helmreich (1983) complains about the limited influence of psychology on aspects of spaceflight: "One can . . . assign responsibility to the investigators for producing products of dubious utility" (p.447). Bouchard (1976) when discussing laboratory research is of the opinion that "Laboratory experiments . . . lend themselves to unjustified and often erroneous extrapolations" (p.364). This view does not create much confidence in the applications based on that research. In the same vein Chapanis (1976), when writing on Engineering Psychology, signals that: "Most laboratory experiments in psychology have only very limited relevance for the solution of practical problems" (page 730).

Fiske (1979), in an article adapted from a Presidential Address at an APA meeting expresses serious doubts about the whole area of personality research and even believes that it will never develop into a science (p.738). I have already mentioned the doubts which exist about the applicability of research in clinical psychology. This is also reflected in the cautionary statements which are made in the *Report of the President's Commission on Mental Health* in 1978, cited in Parloff (1979) where statements are found like: "Treatment by various types of psychotherapy is as yet of unestablished efficacy" (p.300) or "follow-up studies generally indicate that failure or success appears independent of the type of treatment received", etc. (p.300). As to other important aspects of research, Westland (1978) mentions among others the following crises in psychology: The Usefulness Crisis (is there any reason why

the science of psychology should be considered relevant?); The Laboratory Crisis (is laboratory experimentation capable of producing results which are valid outside its walls?); The Science Crisis (is psychology a science?); The Professional Crisis (who or what is a psychologist?); The Publication Crisis (mainly studies are published which "turn out"). Nearly all of them apply to parapsychology as well, but it seems to me that fortunately we are more aware of it.

So it appears that in many respects the situation in psychology is rather similar to our situation. The difference seems to me more to be found in the differences in the nature of the subject matter of the two fields and the differences in size than in their respective levels of progress. Psychology deals mainly with phenomena which are experienced by all people and hence are taken for granted. People are not inclined to question the existence of phenomena they don't understand provided they experience them daily. The functioning of the brain, the riddle of the mind/body relationship, the miraculous capacities of memory, perception, language, etc., are all taken for granted because everybody experiences them. However, parapsychology is dealing with experiences which are relatively rare and of a spontaneous character, and therefore they are less easily accepted. Nearly all of the problems discussed above which trouble psychology apply to parapsychology as well. Of course, that is little comfort to us. I am certainly not suggesting that our situation looks better because psychology is not the hard science as is implied in the sometimes arrogant attitude towards our research displayed by psychologists. On the contrary, it is a regrettable situation, because in many respects our progress depends on the progress in psychology.

How Do the Resources in the Two Fields Compare? It is clear that both fields have serious problems as regards their striving to become a progressing science. Without further study it is also clear that the two fields must differ considerably in resources. Critics sometimes love to argue that 100 years of research in parapsychology has failed to produce a reliable demonstration of ESP. Since a simple experiment can never constitute a reliable demonstration, they mean in fact that 100 years of research have failed to yield the knowledge to enable us to control the phenomena and to demonstrate ESP at will. This is correct, but as was discussed above this applies to many findings in psychology as well. Moreover, the "100 years" sounds very suggestive, but does not take into account on how much research capacity this supposed "failure" is based.

In order to compare the two fields as regards resources a rough comparison suffices. In this comparison I will restrict myself to the

human resources, thereby assuming that the research facilities for individual researchers would be more or less equal for the two fields. This is of course not true; psychology has in this respect a clear advantage over us. As regards human resources in psychology the latest data I found are from Stapp et al. (1985) who in 1983 and 1984 polled the entire American population of psychologists. The size of the population turned out to exceed 100,000. The investigators managed to obtain an 82% response rate which resulted in 81,500 responses which could be used for the evaluation. Of these psychologists 74,417 were employed and according to table 8 of the publication, 34,022 of these were involved in research activities. Hence we can assume that the human research investment in psychology in the United States for one year can be set at about 34,000.

If we consider research in parapsychology the picture appears somewhat different. I have not taken the trouble to count for each year how many persons in the United States might have been involved in research. But it seems to me that if the last 100 years is considered, for most of this period it have never been more than perhaps 5 to 10 persons. But to stay on the safe side I will put the figure at 50 a year, which is clearly exaggerated because I don't think that any year can be found that so many people were involved in research in this field. In that case the 100 years of research in parapsychology would amount to a total of 5000 research-years. That implies that the entire investment in parapsychological research in the United States is equivalent to less than two months research in psychology in 1983. If we include foreign countries the picture becomes even worse, because perhaps apart from Britain and Holland the situation in the United States can be considered to be rather favorable for research in parapsychology. Thus in terms of resources we can just as well turn the critics' argument around and ask: "What did two months of research in psychology yield to justify further investment of such huge resources?"

I realize that this comparison is over-simplified, since in the 100 years of our research we profit from developments in other sciences which will not be possible to such an extent in a two-month period. Since both psychology and parapsychology are extremely difficult research areas, for reasons I won't discuss here, the critic's opinion about the results of our 100 years of research and the dissatisfaction which is often noticeable within our field seems to me rather a consequence of a wrong estimation of how fast research in these areas can proceed, than a realistic evaluation.

Is Parapsychology a Cumulative Science? A fourth criterion for establishing progress lies in the type of relationship between the different

items of knowledge which are obtained in a field of science. One can roughly discriminate between two types of collections of items of knowledge. One is a set of items of knowledge with little or no relationship among the different elements of the set. The other is a structure of knowledge with strong interrelationships which can be said to be cumulative in nature. That is, new theories or findings encompass facts or theories which were until then unrelated and these new theories lead to new findings which again result in expanding the scope and explanatory power of the field. Thus research fields can be judged and compared as to the nature of their body of knowledge. This criterion seems to me one of the utmost importance for the evaluation of a field of science.

A science which is characterized by the first type of knowledge can be said to progress, but only in the sense that each time items of knowledge are added. I will call this a collecting science. The value of this process of information gathering is unclear because little is and can be done with the increasing amount of information. It is as if one collects rocks of different sizes and colors and keeps them lying around in the backyard. A science which displays progress in a cumulative sense, which will be called a cumulative science, is clearly much more successful. Such a science not only collects the rocks, but puts them together and constructs a house with them. The difference between a collecting and cumulative science becomes in many ways apparent. An important one is the way the direction of research is established. A cumulative science, like for instance physics, is characterized by a steady and logical development of research methodology and research topics. On the other hand a collecting science like the social sciences, which is only able to add more items of knowledge to the already existing collection without integrating them, is characterized by fads and fashions. The application of new technologies, mainly introduced from other sciences, and new subjects which become fashionable follow each other one after the other, but with little consistency from past to present.

The above characterization of two different types of sciences is of course rather abstract and does not do justice to the great variety which exists within the different sciences. Thus although I feel that as a whole the social sciences are characterized by a rather meaningless collection of tidbits of knowledge it is undeniable that certain specializations within the field, especially those who are closely related to the beta sciences, grow more and more into the direction of a cumulative science.

Nevertheless, I will not discuss it further here for the simple reason that I consider both parapsychology as well as psychology still collecting sciences. Both are characterized by changing fashions in research. Each

PA Convention demonstrates the variety in research methods and subjects of investigation. As regards psychology it suffices to compare two volumes 20 years apart of one of the popular journals for publications of research data. The two volumes will yield an impressive amount of publications. In nearly all these publications, which cover a wide variety of subjects, significant effects or correlations are reported. But it is likely that somebody who is not familiar with the development in psychology will have a hard job to tell only from the contents which volume is the older one. And to add some comments from within psychology which support my views in this matter: in Fishman and Neigher (1984) it is observed that the present situation in psychology: "encourages large numbers of . . . irrelevant . . . experiments . . . The result is that our information wheels spin very fast but make little progress toward cumulative scientific knowledge" (p.542). Dr. Wachtel (1980) comments: "it certainly seems that (to put it kindly) our studies in psychology tend to be of . . . enduring interest. A good 1950s study in the area of personality, for example, could, I contend, get published readily today . . . as an interesting new finding. Our rate of obsolescence is rather low" (p.399). Indeed, a very sympathetic way of phrasing.

If we compare the progress in research in parapsychology in its 100 years of existence with a comparable two-month progress in psychology, and if we take into account what psychologists themselves think about their progress obtained in more than 100 years of research in psychology, then I think there is no reason to feel that we are doing worse. From this I conclude that the rate of our progress, however slow it may look to some, is in itself no reason to express doubt about the research methodology we apply.

Conclusions. According to the criteria applied we can conclude that our field is progressing, although slowly. The research methodology appears to become better adapted to the phenomena we study. The field is certainly not stagnating as regards the rejection of incorrect models or the introduction of new theories and approaches. Considering the differences in investment of resources our progress seems at least comparable to those of psychology. Hence the degree of progress in the field appears not a sufficient cause to change dramatically the research methodology currently applied in parapsychology. However, like psychologists I feel that we also have reason to express disappointment with our achievements. But from the situation in psychology and in the other social sciences it follows that there are probably common reasons why in all these fields progress is so slow. Hence it is likely that our possibilities to increase progress and to find better research ap-

proaches by trying to make improvements within our field are rather limited. We will remain a small research field with little resources and hence our possibilities for improvement of the situation will remain largely dependent on progress in other fields of science.

That does not excuse us from trying to strive at better and more efficient research procedures. The history of parapsychology has shown that such improvements are possible. I am of the opinion that both as regards research in spontaneous experiences as well as in experimental research improvements have been made. In general, parapsychological experiments show a constant improvement for instance as regards the elimination of sources of error. We should be glad that so many people outside the field are taking the trouble to criticize our work because that gives us the opportunity for further improvement. And further improvement in research methodology seems to me possible.

Suggestions for Improvement of Research. First I would like to propose to invest more research into the relationship between psychological variables and spontaneous ESP. As I have argued elsewhere (Schouten, 1984) we should replace the metaphysical, proof-oriented approach in parapsychology by a pragmatic approach. The latter involves that: (a) we should strive to explain paranormal phenomena or experiences and (b) that in these attempts we should keep an open mind for the possible effects of both parapsychological and psychological processes. Parapsychological experiences cannot be seen isolated from the rest of the personality of the experient and consequently psychological processes must play a role in ESP experiences. Therefore I feel that more research should be devoted to the study of spontaneous paranormal experiences, what function they have and how they fit into the experient's life.

I have no doubt that some ostentative spontaneous ESP experiences are in reality coincidences to which the experient for psychological reasons attributes a paranormal character. It is necessary that we learn to distinguish such experiences. Then we might be able to better understand under which psychological conditions experiences occur which might be classified as "real" ESP experiences. If such research leads to the conclusion that nearly all spontaneous ESP experiences are not suggestive of ESP, but are satisfactorily explained by a psychological attribution process then we would have made a great step forward. That would imply that we found the explanation for the main phenomenon we study, spontaneous ESP. However, from what I have learned about the subject it is by no means certain that psychological explanations suffice. In that case experimental research will certainly benefit from a better understanding of the relationship between psychological conditions and the occurrence of ESP experiences.

When the experimental research in parapsychology of the last decades is considered it is apparent that relatively much research is either of the RNG-PK or of the ganzfeld type. Together these two research approaches may well take up more than half of the research effort in the field. This dominance might have turned attention away from other approaches which could also be promising. In this respect I can mention that the 1968 PF Conference discussed possibilities of psychophysiological and animal research. There has been research in these areas but very little compared to the possibilities these approaches offer.

The third area from which we might strongly benefit is a more careful analysis of why certain experiments or approaches are successful. Meta-analysis as discussed by Honorton at this Conference is an important step in this direction. Another suggestion is to study more extensively which subjects contributed to these successes. It might well be that many series of successful studies from the same group of investigators are based on the contributions of only a few subjects. It is important to find this out because if that turns out to be the case it might well be that we had better depart from the Rhinean approach of working with unselected subjects.

Another direction which might lead to improved results is the more careful study of variables which play an important and perhaps vital role in the experimental procedures we apply. In free-response studies which constitute the bulk of ESP research there are important variables, as for instance the judging procedure or the statistical assessment, for which we still lack optimal procedures. We can not properly estimate the effect of an independent variable in a free-response study if, for instance, the judging or the statistical evaluation is also influenced by the conditions.

We do know that subjects sometimes differ strongly in both the direction of scoring or type of effect in ESP or PK research. Jahn and Dunne (1988) speak of a sort of personal imprint, a strong correlation between specific patterns of results and individual operators (p.144). If that is the case, we might improve the efficiency of our research by applying a different operationalization and statistical evaluation for each individual subject, instead of lumping the scores of all subjects together. This technique seems especially suited for process-oriented research. The principle of the technique is to include in each experiment one or two calibration conditions. Suppose two conditions are to be compared in an experiment. If the two conditions are not mutually exclusive, the calibration condition is chosen in such a way that it includes both the two conditions. For example, if in an RNG-PK experiment the OT model is compared with the IDS model, the calibration

condition could be made up of a pre-recorded true RNG sequence and the subject can choose the entry point in this sequence. The result is displayed to the subject. Hence both OT and IDS apply to the calibration condition. For each individual subject the calibration condition can be used to establish the most extreme chance expectation deviating operationalization (number of 1 or 0, variance, runs, decline, etc.). This operationalization is then applied to the experimental conditions. The results of the experimental conditions can then be rank-ordered based on that operationalization according to the extent they deviate from chance.

If one of the conditions receive systematically higher rank-order numbers that might indicate the superiority of that condition. With mutually exclusive conditions a similar procedure can be followed. The above is only a rough indication, most experiments will lend themselves to more refined applications of the principle. Another possibility is that with such procedures it also becomes possible to use an external criterion to discard subjects from the database, so that the final evaluation can be based only on subjects who might have had some ESP effect on the data.

No doubt others will have more valuable suggestions for improvement of research methodology. However, considering the amount of time research takes it is to be feared that most of these suggestions won't be followed. It is the lack of research opportunities in the field, the lack of money and positions, which put the greatest constraint on our progress. The development in psychology shows that abundant resources are no guarantee of success, but without resources faster progress can hardly be expected. I hope that in this respect the future will have more promise than the present.

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DISCUSSION

HONORTON: Well, I want to congratulate you, Sybo, I think this is a very important point that you are making. It is one that I have made myself over the years, particularly with regard to the issue of replicability. We have to look at our own accomplishments in relation to what is going on in other areas rather than looking at parapsychology as though it existed in a vacuum. And when we do that we see that, although we certainly are not doing as well as we want to, we are doing much better than some of the more pessimistic assessments have sug-

gested. So I think that is a very important thing for us to keep in mind. I also think that in terms of accumulation, while I agree with you in general, we are still basically in a collecting phase. There is very clear evidence that there is some cumulativeness that is contrary to what some of the critics, like Hyman for example, say. New researchers who come into the field are not constantly reinventing the wheel. Mostly what they are reinventing is the terminology, so that they do not have to be identified with the excess baggage associated with that. But very much so there is the ability to build on previous research and that is the foundation of the idea of cumulativeness.

SCHOUTEN: Well, I agree there is some accumulation. It is not a black and white scheme. But what I meant by accumulative is that a certain field of science reaches a theoretical basis including laws and relationships which are predictable. These then expound new theories which engulf the previous ones. So you know there is progress, in such a science there is no question at all about it. If you look at a journal volume of 30 years ago, they have a different level of knowledge compared to now, a much more restricted level. I think that is at present not so much the case in psychology and parapsychology.

MAY: Sybo, I also want to add my congratulations. I thought it was an excellent talk. But we physicists have done a number on you and I think we parapsychologists can learn from how we have done that number on you. It is actually a scientific myth that science builds upon; we are told in school that it builds upon this pyramid. There was an excellent series on public broadcasting a while back in this country by John Burke. He made the point over and over again that we believe that all of science has been aimed at the moving present but then if you examine the history of science you can't support that. I got out of the field of nuclear physics research in the early 70s and from time to time I wander over to the library, pick up the most current journal, the one I used to publish in, and discover that I have been away a week. They are still doing the same stuff. But I think what that is is a characteristic of how one gains knowledge in general. You go through periods of plateaus. The period from 1895 through the next 35 years has been known world-wide as the 30 years that shook physics, because at that time there was exponential growth. Another but related comment that I wanted to make is that there are laws of numbers that are derived simply from calculus that state that the rate of change of knowledge depends upon the number of people investigating it. And if you look at psychology and parapsychology where that appears not to be true, all that means is that we are still in the flat part of the curve and eventually we are going to take off.

SCHOUTEN: Yes, I agree with that. I think that is true. In fact you might turn it around and say, well, let us not get too many people in parapsychology because it would not help much. That is not true either. But it is surprising to me that, if one looks in the psychological literature and you see those really big fields, how many people are researching clinical psychology and how little comes out of it in the sense of real solid knowledge you can apply. It is very, very disappointing.

MAY: I did not add what I thought parapsychologists can learn from physicists is that we have got good public relations. We have convinced everybody we are making that sort of progress and I think parapsychologists can use a little of that.

SCHOUTEN: A question which I raised for myself was how is it possible that clinical psychology, working a 100 years and with an investment of billions per year and not yielding any solid result, is still supported vitally? And why is it possible that parapsychology, a small field, is not supported? I think that is a real issue. I think parapsychology is looking in entirely the wrong direction when it looks for answers. I think the reason partly is that clinical psychology, although it is not making progress, is dealing with things that are alive in society. If we go into research which deals with people having problems with psychic experiences—and not everybody would like to do that (personally, me neither)—I think that the moment you establish institutes for that and do research, you show that here is a service we present to society, I am convinced you get money. Same about healing.

MAY: In other words, we should be paying more attention to survival before bodily death.

SCHOUTEN: Certainly.

MORRIS: I would like to follow up very specifically on one of your points about the way of measuring progress namely the potential contribution of parapsychology to the real world problems of people. A certain kind of progress in parapsychology may be what we might within some frames of reference define as negative progress i.e., helping people understand more of what is not psychic, but looks like it, ways in which people may be mislead. In the last six months you have had your final two doctoral candidates graduate at Utrecht, both dealing with groups of practitioners in society. They did theses which on the one hand did not find particular evidence for psychic functioning, but on the other hand provided a fair amount of specific information about what else may be going on there. Within your own criteria this would be regarded as a service, as one of the positive contributions of parapsychology and I would agree with that. My question to you though is can you reflect

for us what impact those two studies appear to be having these days in Holland?

SCHOUTEN: Well, that depends on whether we follow it up or not. I think that depends on what is done with it. In itself, carrying out a study would not bring much money. It is service you provide. I mean it is a fact that so many people need clinical psychology or think they need clinical psychology, that is what brings the money in. Now in our case in Holland at least we established an institute for counseling. It takes some time, of course, but it turns out that works rather well; there is quite an interest. This institute was supervised by us at the university. There is a real need. Psychologists and other service providing organizations are sending people over, because where else do they have to go? The usual situation is that they cannot go anywhere, there are only cranks. Now there is this institute. When the situation is that there is a known procedure, that people are referred to this bureau and so on and if the benefits of it are recognized, it is also very easy to send in proposals and get support for it.

STANFORD: Well I certainly concur with most of the comments that have been made about the value of your paper, Sybo. There was one thing in particular that concerned me a bit. I certainly agree that in some respects, perhaps especially in our textbooks, psychology has been vastly over-sold. But at the same time you are advocating that we maybe do a little bit of over-sell or try a little bit harder to sell parapsychology. So I think we probably need in some respects to sell all of these areas. But I do feel that you may have done a bit of an injustice in one area of psychology. I know this is a parapsychology conference, but when I feel that there has been an injustice done I feel that I have to comment on it. I know a fair amount about the attitude research area. There were some very serious problems there up until the late 60s, when it began to be recognized that the problems existed. And many remedies have been found for these problems. We know now very concretely about the kinds of things that moderate the attitude behavior correlation. We also know that many of the problems of studying the relationship of attitudes to behavior were due to several methodological problems in the way attitudes and especially behavior were measured. We really do have some very good progress in that area. In fact, I would say that what comes out of it has some relevance to parapsychology and can encourage us as well. This is that those doing attitude research had really failed to look at and empirically examine some of their fundamental assumptions about what they were doing. Once they did so and started to do research in that framework, they began to make some meaningful progress in that area and attitude-behavior re-

search has come alive again and, in my opinion, very justifiably so. I think that message applies to parapsychology. I think it is one of the reasons we have for enthusiasm and optimism today. We have started as never before to question the underlying assumptions of our methodology. I do not know of any area of science where that has happened that it did not ultimately lead to some degree of progress. I think we can fully expect that here.

SCHOUTEN: I am glad you are optimistic about attitude research and have more or less a wait-and-see attitude. I am impressed by the techniques developed for it, scaling techniques and so on. I think they did a very good job there. Whether it really will work in the sense that you can predict and measure, that remains to be seen, but it is not a black and white thing. I know you can take polls and make fairly accurate predictions in some areas.

BRAUD: Whenever I hear comments about our lack of knowledge or lack of advancing or accumulating knowledge I am reminded of an analogy that I will share with you. Consider the physics of trajectories. A very young child is able to throw and catch a ball or a stone with tremendous accuracy. That child has a knowledge of the physics of trajectories. It has taken physics literally centuries to encapsulate that knowledge in formulas so that this knowledge can be communicated to other people. There is a kind of informal or tacit knowledge that we can acquire through our own experience very early. Then there are the more formal quantitative aspects that take quite a while to develop. I think that in the fields of astronomy—because the heavens were there for our inspection very early—or in psychology or in parapsychology the subject matter is very familiar to us. I think we learned a great deal very early and that knowledge is so familiar to us that we hardly consider it to be knowledge. We consider it too common and we forget it. Perhaps the curve describing knowledge in parapsychology and in psychology is logarithmic rather than exponential. Perhaps it showed an early acceleration and it is now leveling off, and we are learning some of the more subtle things that were inaccessible before. I think that is a much more optimistic way to view the concepts, the things that we possess. In terms of methods and theories, I agree that we are perhaps on an exponential curve or a linear curve.

SCHOUTEN: Well, I certainly do not disagree. I think the knowledge people personally have is different from the knowledge that concerns what we call science. But I am not talking about that. I mean I am not talking about personal knowledge, I am talking about science. If what you say is true, it applies to psychology and parapsychology too, but unfortunately it does not help either of those fields.

BRAUD: The point is we do a lot of predicting of human behavior. We base our lives upon accurate predictions. Those predictions are based on personal knowledge that has not yet been systematized. It is just so familiar and common that we do not consider it worthy of the name of science.

SCHOUTEN: I am not going to fight about words.

PALMER: It seems to me that when we are trying to assess how much progress we have made in parapsychology we need to be very much aware that we have given ourselves a very big challenge. Psi is a very difficult nut to crack, and I think the reason is because it is closely linked to very complex mental or psychological processes. This is the same problem that afflicts the softer areas of psychology, and they are making about the same rate of progress as we are. To tease that all apart boils down to a trial-and-error process, which is what we have been doing. And that, simply by the nature of the beast, takes time. So in a way it is unfair to compare progress in our field and in the soft areas of psychology to a field such as chemistry or certain areas of physics, where the problem is much less complicated to begin with. In some ways maybe we are too hung up on the question of whether we are making progress. Maybe we are making as much progress as we should expect given the great complexity of our subject matter. We are going to need to keep the faith and let the process run its course. I believe this progress will be exponential. We are on the lower part of the curve right now, and that is where we should expect to be.

SCHOUTEN: An important aspect is how hard the task is that you are dealing with. I would like to argue that parapsychology is one of the hardest fields to do research in. If you deal with physical processes at least in the beginning you start with processes which are repeatable in nature. That is a real blessing. The sun rises each day. When you deal with psychology you deal with much more complex phenomena because so many variables are active at the same time, you can't control them all. But if I carry out a study in learning or perception at least I know that my subjects are learning and I know that my subjects are perceiving. All I have to do is to think of a clever experiment and I can do it and publish it. But the bad thing is in parapsychology I do not even know that. I do not know whether there is ESP in what my subjects do. I can design a clever experiment and it ends up in the wastebasket because just nothing happened. So I think parapsychology is one of the most difficult fields to deal with and that should also be taken into account. Also I feel that we are very strongly dependent on the progress in psychology, for instance. And my personal feeling is that we will not make the sort of progress we would like to make unless psychology

itself is progressing faster. It might be that in the end ESP turns out to be a physical process, certainly not the kind of thing I would like to exclude, but for the time being I do not know. For the time being, it is clear that the human factor plays an important role in the whole thing.

MORRIS: This is really addressing itself both to these issues and to a theme that I think has run through much of the conference. We are studying very complex, open systems. Much of what has been said has really been about expanding our definition of what constitutes the system of the experiment to one which takes into account a whole host of variables attending an experimental situation. Part of what we have also touched on is that you can take each such system even though it is open and embed it within a larger system. Each researcher in some sense is his own system. So is each lab, the parapsychological community, society as a whole. These are all dynamic systems which are greatly affected by feedback into the system as a result of its own activities. And I think it is analogous to the problems that economic forecasters have in that if they do their business they must make a statement of some sort, an announcement of some knowledge or guidance. Once that guidance and knowledge is taken into account by individuals active in the system that economic system changes and has different properties than the system upon which the original prediction was based. If we regard ourselves as trying to be socially useful and we interact with institutions of the sort that are likely to acknowledge progress and foster more, we will find ourselves in an extremely complex dynamic situation that is very hard to anticipate.