THE DISTRIBUTION OF PSI

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APOLOGIA

This paper critically discusses the common assumption that psi ability is widely distributed. The primary task here is the preliminary one of deciding what questions should be asked of nature and how she may be queried to best effect. There is no pretence of providing a conclusive solution to the points raised: on the contrary, only further work, both in detailed and extended analysis of the extant literature and in new experiments can be expected to provide final answers.

In one respect, however, this paper should perhaps be nailed to the door of the old Duke laboratory at dead of night. It is the author's view that the results of most positive experiments do not depend on the subjects at all, but on the experimenter. While strong evidence exists that experimenter-psi is sometimes involved, the task of this discussion is not that of reviewing the literature with respect to the strength or ubiquity of the effect, but instead it explores the consequences of such a view for the distribution of psi and thus for parapsychology in general.

INTRODUCTION

There are two opposed views on the distribution of psi. The first is that psi ability is rare but when it is present it is quite strong. This will be referred to as the psi-star hypothesis. Historically this idea has been expressed in the West in terms of Saints and witches. Furthermore much of the early work of psychical research was performed with specially gifted persons, generally mediums.

The second opinion is that most people have some psi which can be coaxed out under appropriate conditions, but which is usually quite weak. This will be called the democratic psi hypothesis. Some support for this comes from spontaneous cases. It became the prevailing view with the Rhine revolution of the 1930s: after an initial period in which the traditional policy of experimenting with high-scoring subjects was pursued, interest shifted rather abruptly to testing groups of unselected subjects (Pratt, 1977, p 74).

A belief in some generally distributed psi ability in addition to the strong powers of special individuals exists in many societies. Particularly in unspecialised societies in which each individual must be his own butcher or tailor he must also act as his own psychic when the need arises. It is perhaps of some sociological interest that the hypothesis of widely distributed psi found favour in the America of the 1930s with its aggressively democratic outlook.

In terms of experimental work the idea of widely-distributed psi is an optimistic one. On the psi-star hypothesis it is necessary first of all to find a psychic: on the democratic view of psi, however, any unselected group of people can be used to answer questions about paranormal effects.

One of the turning points in the history of parapsychology is the sheep/goat work of Gertrude Schmeidler. The major experiment reported together with McConnell (1958) used unselected classes of students. A total of 1157 subjects were tested with ESP cards giving 10,035 runs. The sheep (believers) scored above chance expectation while the goats scored below. The absolute size of the deviation was small (+0.10 and -0.07 of a guess respectively per run of 25 trials), but because of the number of trials the difference between sheep and goats was unquestionably significant, with a probability of 3 x 10-5. This seemed to set the seal upon the democratic psi hypothesis. Almost all parapsychological experiments since have tacitly assumed this model of psi distribution.

In addition to this practical work Schmeidler and McConnell devoted a chapter (How Common is ESP?) to discussion of the subject matter of this paper. At that time (1958) they stated that the evidence for which model was correct was inconclusive. Unhappily no substantial advance has been made on the issue since then. The empirical distribution presented by Schmeidler and McConnell is one of the few available in the literature. It seems almost inconceivable that such a vitally important practical question should be almost ignored by parapsychologists in favour of a theoretical predilection.

Schmeidler and McConnell's contribution, however, was by no means limited to the attempt to make an unequivocal deduction. On the contrary, on finding this attempt in vain they went the other mile and asked what the extant data suggested. Their conclusion was that "The evidence, from this research and from other sources, is .... consistent with the hypothesis that psi ability is widespread in the populations that have been tested.". They go on to comment on the gifted subjects found by other experimenters " .. there is evidently a higher level of such ability possessed by only a few .... a level which is so much higher than the normal one that .... it is convenient to think of it as a different function.[11]. In short they suggest that there are two parts to the distribution curve, a small scoring rate component due to a substantial portion of the population and a special subjects component, very much rarer, but scoring at extremely high rates. This compromise seems a very reasonable one on a descriptive level. On the conceptual plane, however, the ascription of the lower rate component to subject psi will be shown to be, at best, questionable.

THE DESCRIPTION OF PSI DISTRIBUTION

Frequently the view is expressed that psi is normally distributed. We may perhaps ignore the fact that there is probably no psychological variable known which is precisely normal (Dorfman, 1978, p 1180). In this context presumably what is meant is that the distribution curve rises smoothly to a peak and then drops away again approximately symmetrically. For this to be a testable statement we need to have some way of measuring psi. Should we perhaps use the psi-coefficient or Schmidt's theta, or would Walker's omega be best? (Timm, 1973; Schmidt, 1975; Walker, 1975). In practice we do not know which, if any, stays the same with changing p-values of the targets. It therefore seems best to avoid making an arbitrary decision and to work directly (at least initially) with the empirically observed distributions of scoring.

Consider recruiting several hundred experimenters who go out and test everybody they can with 9 runs of ESP cards under GESP conditions. Each subject tested then has a mean run score: the frequency distribution (or corresponding empirical probabilities) of these can be graphed. Figure 1 a, b and c show respectively the (psi-hitting) distributions that might be obtained if the population consisted solely of a homogeneous group of subjects with zero, a little and a lot of psi. While it is improbable that the real situation is composed of a mixture of a few homogeneous groups, as will be illustrated here, this clearly reveals the basic principles. It seems likely that any real population will generally contain a majority of subjects who show no psi under the conditions of the experiment (curve a). This may be mixed with b to produce figure 2 and with c to give figure 3, or indeed with both resulting in the distribution of figure 4. The dashed line indicates binomial (chance) expectation. In both figures 2 and 3 the curve is drawn as clearly bimodal: in practice however in figure 2, where the peaks of the curves are close together a bimodality will likely not be visible and the effect of psi-scoring will simply be to shift the curve somewhat to the right. The curve (fig. 2) represents the democratic psi distribution: many of the people tested show psi but at a rather low level. Figure 3 represents the psi-star distribution: for clarity the height of the scoring peak is exaggerated. Here few people display psi but when they do it is at a rather high level. Figure 4 is the composite "Schmeidler-McConnell distribution". The empirical curves would specify the psi-scoring distribution under the particular conditions of the experiment and could be compared with theoretical ideas.

IS THERE A SINGLE PSI DISTRIBUTION?

If such empirical distributions were available, a large number of interesting modifications could be tried. For example, the experiment could be repeated with some allegedly psi-enhancing technique. Enhanced scoring means that either the non-chance peaks get bigger (more people score) and/or they shift to the right (the same number of people score but they do so at a higher rate). If the curve were of the Schmeidler-McConnell type (fig. 4) the possibility exists that the two scoring peaks might react in disparate ways. As another possible modification suppose we simply perform a different number of runs. A common simplifying assumption is that scoring rate is constant for a given subject. This is not generally true: declines are ubiquitous. Perhaps, for example, some subjects have only a certain amount of information to spend; thus they obtain the same significance level regardless of how many runs they do: the scoring rate will however decline with the number of runs (cf. Walker, 1976, p 49-50). Another simple comparison would be between ESP and PK.

To talk of such experiments is however, at the moment, somewhat Utopian. Hardly any empirical data exist: some of these will be examined later. One point which it is important to make is that these curves may contain useful information about the psi process, information which is usually thrown away when attention is paid only to the mean and variance. It is only fair to add, however, that such work can normally be done only with a much larger data-base than is usually the case in parapsychological work: this has certainly contributed to the general experimental malaise.

It might be that the curves obtained under different conditions are quite chaotic and there is no relationship between them. Under these circumstances the distribution of scoring is restricted to a highly specific set of experimental conditions.

If, however, some degree of cross-condition predictability exists it might arise in two different ways. Firstly, subjects who score high under one set of conditions might also tend to score high under other conditions etc. This common factor could be regarded as a measure of ability. Most parapsychologists hold such a view. It is to be expected, however, that a rank correlation between different

Figure 1. (Psi-hitting) Scoring distributions for (a) no psi,

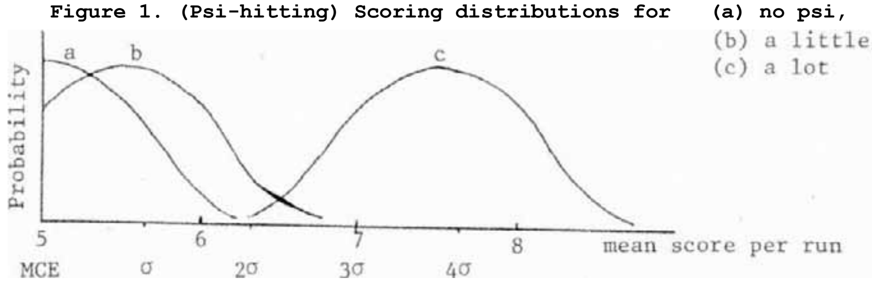


Figure 2. The democratic psi distribution

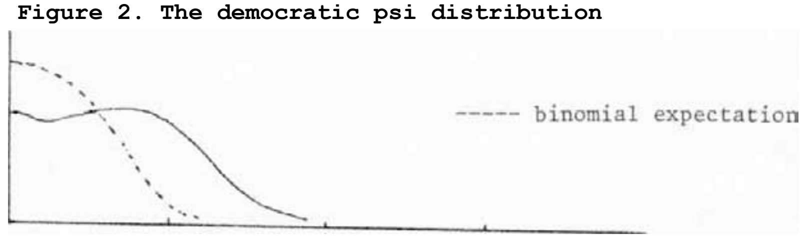


Figure 3. The psi-star distribution

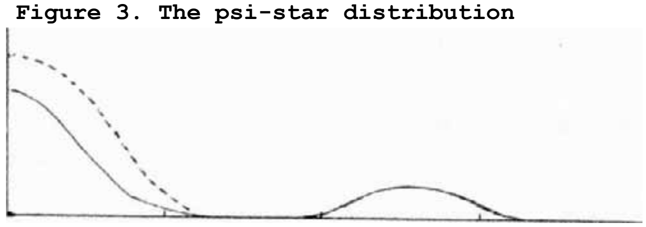
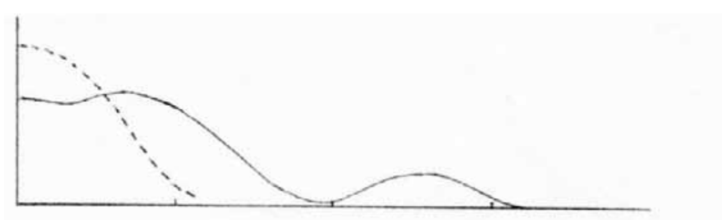


Figure 4. The Schmeidler-McConnell distribution



conditions would not be perfect but would be attenuated by individual differences. Secondly, it is possible that such intrasubject differences are predominant and although the features of one distribution might be predicted from another, the scoring would be due to different subjects in the two cases. In such circumstances no simple measure of psi ability can be defined.

POTENTIAL AND ATTAINMENT

It is general in psychology to draw a distinction between potential and attainment. Roughly, potential is the maximum possible attainment for a given person. There may be certain conditions in which a subject can score at a much higher rate than under other circumstances. How important this is in practice must be determined by experiment. In principle one could test a given population in some standard way and then in such a way that each subject works under his optimum conditions. This operational definition of potential, however, is a rather fuzzy one because we currently have little knowledge of how to set up the required optimum conditions. In practice the experiment would reduce to comparing scoring distributions under a number of different conditions. The question of interest is how and to what extent such manipulations would affect the distribution.

The idea of potential, as used here, requires some stability in scoring under the specified near-optimum conditions. A one-off situation can allow one at best to say "His potential was ....". Similarly, if for instance it were to transpire that all subjects very infrequently give exceptionally high scores, but at random with respect to any specifiable conditions then such a measurement of potential would have little, if any, predictive value.

WHAT POPULATION?

Sampling theory specifies that each subject unit used in an experiment should be sampled at random from the population of interest so that inferences may be made about that population. In practice, however, this is usually impracticable. The exigencies of life dictate the selection of the individuals tested and the population to which the results can be generalised is therefore to some degree fuzzy. Major characteristics are almost always clear, for example one might test schoolchildren, gerbils or Australian Aborigines. The most commonly used subjects in parapsychology, however, are university undergraduates. It has been said that experimental psychology is largely the study of psychology students.

Perhaps the closest approach to the requirements of sampling theory in common use is the employment of classes of students. In much work, however, subjects are semi-selected by, for example, replying to newspaper advertisements. Even greater selection is made in some cases where successful subjects are re-cycled in experiment after experiment. Some of the altered states work, for example, uses this kind of subject selection. In such cases the attempt to generalise to a population is rather dubious.

For the purpose of constructing population distributions greater attention should be paid to subject selection than is usually necessary in parapsychological work.

WALKER'S WORK ON ESP DISTRIBUTION

Walker (1975, p 16-20) has tried to calculate from his theory the distribution of ESP scoring to be expected in the population. His theory is one of the observational type (Millar, 1978). In these theories all psi is basically time displacement PK. The psi source is triggered by the feedback of results at a later time and this triggering affects the random generator at the earlier time the target was generated. In the case of ESP the RNG is something in the subject's head at the time he made his guesses. The idea underlying Walker's treatment is that the brain is not a terribly good random generator. The observational theories in general assert that only pure chance events, ultimately derived from Heisenberg uncertainty on a quantum level, can be affected by PK (Millar, 197 8). In most real systems, however, the bulk of the output is deterministic, while only a little is pure chance. The presence of the deterministic component, which cannot be affected by PK, would be expected to result in a lower PK-scoring rate than would a pure-chance RNG. Walker argues that the brain similarly contains a small proportion of noise upon which PK can operate and considers that this dilution is the major limiting factor in ESP ability. Further, he considers that chance variations between different people, in the way the counter-dilution strategy is applied, are the determiners of the ESP distribution.

In Walker's neurophysiological theory both the part that can be affected by PK (W) and the part that can not (C) are dependent on postulated quantum mechanical effects in the brain. Walker has calculated these data rates and it turns out that something like one bit in 10,000 (W/C) can be determined by PK influence.

Walker has to assume some model by which the brain constructs its guess from the two different kinds of noise. Unfortunately he does not spell this out in detail, but it appears to be something like the following. Before he starts to guess, a subject selects some method of classifying a subset of the bits which pass through his consciousness in the inter-trial interval. The selection of a classification procedure is the step which results in individual differences, as will be seen later. The bits are used to add counts to a number of "bins", each of which represents one of the equiprobable targets.

For simplicity the principle will be illustrated with only two target alternatives: in this case each bit adds a count to one or other of the two bins. One might, for example, increment the bins in terms of the states of some progression of brain areas. It is assumed that the classification procedure, once decided, is adhered to for all trials. The origin of each such count would not be known to the subject; however it might for example turn out that bin one contains on each trial one count from data representing the itchiness of his big toe, a second count relevant to his breakfast that morning etc. Similarly a count might be derived from the W-channel and this correctly represents the target: such a count would be classified into the correct bin. Since, in the absence of learning, the subject has no knowledge of which counts are due to PK and which to non-psychic factors, the best he can do is to perform a majority vote. (It is tacitly assumed that MV here follows the normal statistics of signal enhancement.) On average, in the example, the correct bin (one) has one more count than the other (all the contents of which are randomly related to the target) and one will therefore become the slightly more probable guess alternative.

the assumption of consistency means that if a subject has happened to select a classification scheme in which, say three W-derived counts are classified into bin one (correctly) on the first trial, then he will continue doing this on subsequent trials. Consequently, the whole population will consist of people who regularly obtain 0, 1, 2, 3 .... correct counts per trial, in addition to irrelevant counts.

The relative frequencies of successive levels of ability is determined simply by the binomial expansion which gives the probabilities of choosing by chance, in the initial selection of classification procedure, successively 0, 1, 2, 3, ... PK-determined counts. The total number of bits (n) from which these are derived is determined not from within the theory but from the empirical proportion of the population showing no ESP. For convenience of computation the number n is assumed constant for all subjects. The scoring rates corresponding to the successive levels of ability are determined by the excess of 0, 1, 2, 3 .... counts in the correct bin, in the presence of all the other spurious counts distributed at random.

Using this treatment it is found that using ESP cards 9.5% of the population has one W-count per trial and scores around 5.6 hits per run; those with 2 counts (0.5%) score at 6.1; 3 counts (0.01%) at

6.6 etc. There are two interesting points to note here (1) according to Walker's empirical data 90% of the population does not score at all (though see later discussion) (2) the scorers come in a discrete series, rather than a continuum. Mathematically the discreteness arises from the numerical values of W/C and n. With other parameters the distribution may become (virtually) continuous.

One can criticise Walker's derivation on grounds of the arbitrariness of his model; however it is clearly necessary to assume something in order to obtain any result. More seriously, however, there is one certain and one probable error in the derivation.

Firstly, from the integral for genuine recognition probability (his equation 46) it seems that Walker assumes that one or another bin is taken as the guess if it exceeds MCE: in fact the bin taken as guess will be that which contains the most counts. The correct solution can be obtained from signal detection theory. In the multiple alternative forced choice guessing procedure of this theory, as in the present situation, one bin contains signal plus noise and the others only noise (McNicol, 1972, p 73). The distribution of the <u>maximum</u> noise is required: this is given by Tippett (1925). Hence the difference distribution between the signal bin and the maximum noise bin may be obtained and the genuine recognition probability can be calculated. Walker's result over-estimates this.

The second problem is with the extension of the model (illustrated above for binary choice) to the multiple choice situation. Walker uses every bit to increment one bin. This is perfectly admissible for the noise bits: it does not matter that each is constrained by the initially chosen classification scheme to increment only one of two rather than any one of the possible alternative bins. It does matter, however, for the signal bits. One possibility is that some W-bits will increment the wrong bins, since the right one is not one of the two allowed alternatives on that trial. The mathematics becomes complicated in this case. The other possibility is that a W-bit will always find some way to increment the correct bin; but in this case it would seem that a bit from the W-channel can be used to convey more than a single bit of information. This is the alternative implied by Walker's equations, though it may be meant simply as an approximation to avoid the mathematical problems of a more exact treatment.

PROPOSED MODIFICATION AND EXTENSION OF WALKER'S BASIC IDEA

The above technical criticisms are relatively unimportant in comparison with Walker's basic idea. Is ESP scoring primarily limited by the inability of the brain to function as a good random generator? In that case what about PK? If a pure chance RNG were used for a PK test with a successful ESP subject, then the PK scoring rate on Walker's above model alone ought to be some orders of magnitude higher than f or ESP, furthermore the rate should be the same for everybody. This is not true in practice: indeed, scoring is likely the same regardless of whether ESP or PK is involved, if the conditions are psychologically identical (e.g. Schmidt and Pantas, 1972). Walker has not explicitly extended his treatment to PK but his mention of ESP "decoding" and PK "encoding" (Walker, 1975, p 36) clearly suggests that he has in mind some similar treatment for PK.

If the scoring rates for ESP and PK under psychologically identical conditions are the same, however, this suggests that there are not two different processes involved but one, common to both kinds of psi. According to the observational theories, the step common to all kinds of psi is that of feedback. While it is, of course, possible to assert that psi sources are by their very nature of different strengths, this is not very illuminating. Another, and more interesting possibility, is that the psi source is only slightly affected by the feedback and to different extents for different subjects.

The important modification introduced here is consonant with the general picture of the observational theories: the things of importance happen at feedback and not, at the time of guessing for ESP, or target generation for PK.

Host work on ESP has centered upon the time the subject makes his guesses and has neglected the later feedback. There is empirical evidence that the state of the subject at the time of guessing has some influence on the result, however Stanford (1978, p 207) has suggested explicitly that the effect of many of the apparently important psychological factors is simply to render the subject's brain at the time of guessing a better or worse random generator. He suggests, for example, that rational, sequential and contextual constraints make the brain a worse RNG and thus less responsive to PK influence, whereas in free-floating states like that of hypnagogic reverie it becomes a better random generator and hence can be more readily influenced by PK.

At first sight there seems to be a contradiction between the above emphasis on the pivotal role of the events at feedback and the empirically determined importance of the state of the subject at the time, of guessing. However, these are not incompatible: it might be that the primary limitations on psi scoring are set at feedback, but the result of the psi source firing would be limited (perhaps in an all-or-nothing way) by the quality of the RNG on which it was working, in this case, the observed equality of scoring rates in psychologically equivalent ESP and PK tasks suggests that in the cases tested the subjects's brains were acting as rather good random generators. This might, however, not be true in general: PK ability would therefore be expected to be more widely distributed than ESP. J.B. Rhine (1946, p 98) has indeed expressed just this view, judging by his own experience. However, there is a lack of experimental evidence bearing on the question.

In summary, the model proposed is that the major limitation on psi ability lies in how much effect the feedback information has in triggering the psi source. This may be described in information theory terms: the feedback signal is heavily perturbed inside the organism by extraneous noise on the channel leading to the psi source. The feedback has a different degree of effect on the psi source for different people. Thus, it is possible that the distribution of psi ability may ultimately be understood in terms of the normal neuronal circuitry between the sense receptor used by the feedback and the psi source. One complication arises when the RNG operated upon contains deterministic noise in addition to pure-chance noise. In ESP the brain of the subject may be such an imperfect RNG, the quality of which depends on his state at the time. Therefore ESP scoring may be less frequently observed (and/or less in magnitude) than PK scoring.

The main event in psi is characterized above as a sensory input signal causing firing in some normally unrelated brain area. The analogy which immediately springs to mind is that of synaesthesia in which the input of one sense is experienced in another as if via crossed lines; for example the sound of middle C might be accompanied by a green flash. This and the accompanying information theory concept suggest a number of novel possibilities.

Walker's suggestion that high-scoring subjects might come in a discrete series of scoring rates seems also to be worth at least some attention. If evidence for such an effect were to be found it could be of considerable theoretical value. A plot was made of the rates of the high-scoring subjects reported by Rhine (1964) but it revealed no evidence of scoring clusters. However, such a crude procedure is unlikely to reveal banding. It would be interesting to examine the run-score distributions within the data of high-scoring subjects. In particular, such a subject in decline might be expected to give a distribution consisting of the series of successively lower bands.

WHO IS THE SUBJECT?

In order to determine the scoring distribution of a population of subjects it is necessary to know who the subjects are. There is usually no problem in psychology: for example, in a sensory perception experiment the subject is the person to whom the stimulus is presented and who makes the response. Making the response is rather less important if the stimulus is known to someone else, such as the experimenter. In such a case the possibility exists that the experimenter may subtly cue the subject via sensory means so that his responses may not be due to the sensory manipulation which the experiment is designed to explore. In parapsychology just who makes the responses is even less important, since these may be affected not. merely by sensory means but also via psi. Consequently, the only way one could be sure in a psi experiment that the result was due to the subject would be to present the stimulus *only* to him. Unfortunately, this is just what *cannot* be done in parapsychology, since there is currently no way known of screening anyone from being affected by, say, a given pack of ESP cards.

From the very earliest days of research it has been clear that the very existence of, for example, telepathy, would make it impossible to know for certain whether the result in a psi experiment was due to the mechanics of the effect itself, or whether the subject merely found out what the experimenter wanted to happen by telepathy and acted to confirm his desire. An anarchic phenomenon such as psi, with no known limitations, would by its own nature render improbable any exposure of its inner secrets.

The observational theories have taken seriously the empirical suggestions that psi is independent of space and time and thus act to dampen any hope still springing in the parapsychologist[1]s breast, that these variables can be used for the purpose of screening off unwanted influence. At the same time these theories have as yet yielded no other certain physical limitation which can be used to curb the unruly nature of psi. They do, however, suggest the possibility that ultimately suitable feedback manipulation may provide our Alladin's lamp. For the moment, though, they have made the darkness denser by replacing the ungainly and improbable early conceptualizations of experimenter influence by an elegant scheme in which the experimenter is merely another subject affecting a random generator. The RNG in this case is *the whole experiment*. No longer do we make laborious schemes "S gets E's expectations by telepathy and reacts so as to honour them" but instead "Psi is goal directed: if E has psi and he wants the experiment to produce a particular result, then he will use his PK to bring that about".

We are therefore forced to use other, relatively uncertain, means for deciding who the subject is. The primary one of these is that psi effects appear in association with certain individuals. Criteria secondary to "psi source by association" can be formulated, such as that effects are strongly correlated with the psychological state of one person. The point which should be emphasised here is that, at present, only on these weak correlational grounds can we say that psi effect X is produced by person P.

If psi effects appear around one person we may suspect that he is a psychic. On this basis it seems fairly certain that the effects produced in association with high-scoring subjects, such as Stepanek (Pratt, 1973) are exactly what they seem. His results are (virtually) independent of the identity of the experimenter but on the contrary are sufficiently associated with Stepanek that a number of parapsychologists have flown half way around the world for the privilege of working with him. It seems likely that it is a characteristic of high-scoring subjects in general that the results are singularly associated with them and rather independent of thes experimenter,

When we examine the typical successful group experiment using unselected subjects, however, a totally different picture emerges. Scoring is not associated with individual subjects: indeed such experiments typically have no individual high scorers, though the result, as a whole, is significant. On the other hand, results are conspicuously associated with particular experimenters. In the last few years Honorton, Schmidt, Braud and Stanford have been among the most prolific of these.

On the basis of the major criterion we have for judging ownership of a psi effect, there is the suggestion that these special experimenters are actually psychics in disguise. However we conceptualise the fact of experimenter differences, though, it has important implications for the distribution of psi. Most workers using group experiments either find no evidence of psi at all, or alternatively marginal effects which do not recur on replication. Experimenters of this type (e.g. Pratt, 1977, p 75) do, however, sometimes find high-scoring individuals. In strong contrast, the results of the special experimenters, using groups, give statistically strong evidence of democratic psi which is fairly robust on replication: they too find occasional special subjects. In other words the literature suggests that special experimenters find the Schmeidler-McConnell distribution, while the rest observe only the psi-star component. It is clear that any empirical attempt to determine psi distribution must take account, and rather powerful account, of the identity of the experimenter.

It is of the utmost importance to determine whether the difference between experimenters is due to experimenter-psi or the normal sensory-mediated experimenter effect: we know in principle how to control for sensory-mediated effects: if experimenter psi is involved on the other hand, parapsychology is in deep trouble (Broughton, 1979).

The first thing to determine is whether there really are only a few special experimenters: those who are in the minority should be most carefully inspected. McConnell (1977, p 203) has commented how dependent parapsychology is on a very few experimenters " .... in the years 1970-1975 more than 50% of all the statistically evaluated experimental papers in the Journal of the American Society for Psychical Research were authored or co-authored by one of five parapsychologists." Rhine (ATV, 1976) has given his opinion on the frequency of experimenters who can obtain evidence of psi "... we have found that very few people can succeed as investigators, very few people can go out and get it ....". There seems to be a consensus that a handful of special experimenters bring home most of the parapsychological gravy.

It has been known for a long time that experimenters working with the same subjects get different results (White, 1976; Kennedy and Taddonio, 1976) but this has usually been attributed to the psychological skills of the experimenter in coaxing psi from subjects. Rhine (ATV, 1976) continues ".... no matter how many degrees they have, how much education, how smart they are in many ways, there is a kind of smartness, there is a kind of intelligence, a social intelligence that they've got to have before they can get people to perform right ....".Pratt joined with Rhine in an extended treatment of the importance of social interactions (Rhine and Pratt, 1976, p 131-138). It is rather chastening to find that one so capable of these human insights should yet himself fail so dismally to elicit psi in unselected subjects (Pratt, 1977, p 75).

The hypothesis that experimenter differentials are due to psychological variables has been widely accepted by parapsychologists. In the early days there was, of course, no alternative explanation available: it just had to be psychological. However, there is surprisingly little hard evidence. A detailed discussion of the rival merits of the sensory-mediated versus the psi-based models is outside the scope of this paper. However a brief resume may be in order.

(1) Parker (1977) found that successful experimenters scored in a psi task disguised as a personality inventory, while the unsuccessfuls did not. Sargent (1979b,p 177) also claims confirmatory unpublished data. This does not, however, absolutely distinguish the two hypotheses since it is conceivable that an experimenter psychologically able to elicit psi in others may do so for himself, perhaps even when he is not trying to.

(2) Parker (1977) also looked for personality differences with the 16 PF, but failed to find any. Sargent (1979a), restricting himself to ganzfeld experimenters, did report differences indicating that successfuls are significantly more sociable (A), extravert (QI) and enthusiastic (F) but less neurotic. Parker (in press) has, however, described Sargent's work as "little more than a dressed up case

study in which results are obtained by fitting the data to the hypotheses.

Even if personality differences were to be established this, per se, would have little bearing on whether the experimenter effect is sensory or psi-based. Personality differences might be relevant only in so far as they were correlated with experimenter psi differences. It would be necessary to employ multivariate (here bivariate) methods to decide which came first.

However, an explanation of the extraordinary success of our few special experimenters in terms of the kind of psychological variables measured, seems somewhat implausible. Unless the personality characteristics associated with them were similarly extremely rare, they could not possibly account for a large part of the variance.

(3) Experimenter differentials are obtained even when subjects and experimenter never meet (e.g. West and Fisk, 1953). Since contact is necessary for a sensory-mediated effect, these results at least, cannot be due to a sensory influence.

(4) If E-effects are due to social interaction then one might expect experimenters to become more skilled at eliciting psi as time goes on. In fact special experimenters, like special subjects, generally decline. Schmidt, for example, cannot now "elicit" psi from his subjects. It might be counter-argued that experimenters are likely to become progressively less enthusiastic and thus counter any learning incline. It seems difficult, however, to reconcile this with the long-term failure of centers such as the FRNM to train enthusiastic youngsters to elicit psi.

(5) The data of different subjects collected by the same experimenter tend to show the same "signs of psi", for example particular displacements (West, 1953; Sargent, 1978). It appears somewhat strained to suggest that E subtly cued subjects to make, say, -1 displacements.

(6) A few workers have tried directly to manipulate treatment of the subjects. Perhaps the best-known of these is Honorton, Ramsey and Cabibbo (1975). Briefly, one group of subjects was treated nicely and the other badly. The nicely treated group scored significantly above chance, as expected, and there was a significant difference between the scoring rates of the two groups. However, the badly treated group scored significantly below chance to about the same degree as the positively - toned group scored above.

If we take this result to heart the best thing we could do would perhaps be to treat subjects as abominably as possible, so that significant, if negative, scoring could be secured. Further, the effect was restricted to only one of the two experimenters. Thus, while the result was evidently due to an experimenter influence, the effect of the subject treatment, was rather less important than the identity of the experimenter.

The experiment, moreover, made no attempt to determine whether the result was due to sensory interaction or E-psi. To be sure, at present there is no certain way to do this, but at least a start could be made. A simple addition would have been a condition in which there was no sensory contact between S and E. In general, these studies treat the problem of E-psi or E-psychology by the debatable expedient of ignoring the psi possibility.

PROPOSED SCHEME FOR PSI DISTRIBUTION

On empirical grounds the view of sensory-mediated experimenter influence has no striking advantage. On the contrary, the experimenter-psi idea seems to be a nose ahead. The sensory picture does, however, have one advantage: it seems so much more intuitively likely. On the other hand the E-psi view has hardly ever been discussed and is relatively unfamiliar. The bulk of this section will therefore be devoted to outlining a scheme of psi-distribution involving E-psi and developing a few of its ramifications to the point at which testable consequences can be drawn.

The scheme proposed is that there is no generally distributed low level psi ability in the population: the real distribution is a psi-star one. The few experimenters who themselves have psi use their subjects simply as random generators: their process-oriented studies are therefore likely to reflect more about their own hopes and fears than to reveal anything about psi. The remaining experimenters see only the psi-star component.

A host of questions spring to mind.
<u>If</u> it is true that in successful experiments the experimenter is actually contributing the psi might one not expect all his subjects to score at about the same level?
It has often been noted that this is actually observed. Schmeidler and McConnell (1958, p 96), for example, comment "There is little inhomogeneity within or among our subjects.". It is current practice to perform single-mean t-tests (Stanford and Palmer, 1972) to ensure that results are homogeneous over subjects and they almost always are. If the scoring rate were higher this would be a strong indication that something other than subjects was causing the results: as it is, this finding has been tacitly attributed to the low power of the tests to detect differences round about the typically low scoring level. However, some differences are to be expected between subjects in ESP tests in their responsiveness to experimenter PK. If we invoke the brain RNG picture discussed earlier the subject differences would be partly independent of whether the subject himself, or the experimenter, was providing the

PK input. It could be expected that for experimenters with psi, any manipulation of subjects which made them better random generators would result in improved scoring. For an experimenter without psi, whether his subjects were good RNGs or not would be irrelevant, since there is no PK influence to work upon them. In other words appropriate social interaction might be of the essence for a special experimenter, yet be of little or no importance to experimenters without psi.

Even if the experimenter is the psi source, why should one be suspicious of the answers he obtains in process-oriented experiments? After all, it is genuine psi regardless of its origin. Maher and Schmeidler (1978), p 391-392) have put this with some eloquence "Much concern among parapsychologists revolves around the issue of psychokinetic 'experimenter effect[1]. However, ... experimenter PK presumably can also be used in an effort to derive nonegotistical answers to legitimate theoretical questions ....". This is possible but seems somewhat implausible in that it ascribes moral rectitude to actions which are not under direct conscious control. For experimenters with psi their dream world becomes objectified, by PK, to a very tiny extent, in external reality. It is then, as if we were to put our faith in the validity of an experiment done in a dream. Palladino was, presumably a psi-source too.

If the experimenter is the origin of the psi, why does he not always get the results he wants? In practice our special experimenters do usually obtain results rather close to their expectations (e.g. Honorton's muscle tension experiments, 1976, p 219). That they do not always succeed entirely, but obtain something related, or indeed the reverse, is no more remarkable than that other special subjects should sometimes make displacements or psi-miss.

Why should there be a relatively higher concentration of special subjects masquerading as experimenters than there are special subjects in the general population? The proportion of special experimenters is actually a rather small fraction of the number of serious parapsychologists: there are, at the outside, ten special experimenters in the Parapsychological Association, which has something like 200 members and associates. On an absolute basis, then, that is 5%. This is still probably many times the frequency of psi-star subjects in the general population. It would be surprising indeed if professional parapsychologists were not a very special self-selected breed. One of the factors involved in the process is clearly the ability to get results. Rhine and Pratt (1967, p 132) make no bones about it "A psi experimenter is one who, under conditions that insure he is not fooling himself, can get results. All others should do something they can do well.".

If the special subjects who are our special experimenters obtain scoring from their subjects, then should it not be the case that undisguised special subjects are able to elicit psi from an unselected group?

There are many claims that special subjects are able to transfer their powers, for example with Bill Delmore (Kelly and Kanthamani, 1973), but there has been little experimental investigation. A recent study (Palmer and Tart, 1979) looked for psi transfer with Matthew Manning, but failed to find any convincing evidence of it. The authors, however, present no evidence that Manning was able, at that time, to produce the psi effects which were required from the hopeful recipient of his powers. One of Rhine's early talented subjects, Stuart, did produce significant scores using unselected people: further, it is interesting to discover that while Pratt was himself scoring successfully as a subject, he also "elicited" scoring from students (Rhine, 1964). However, the experimental conditions employed, would not pass muster by current standards.

Is it reasonable to assume that experimenter-psi remains about constant?
On the contrary subject psi sometimes erupts suddenly in someone for a few weeks and then vanishes forever (e.g. Mitchell, 1953). Similarly there are suggestions that a number of experimenters have success in their first experiments and then go into rapid decline. It is not improbable, however, that this is a reporting artifact. The psi ability of special experimenters seems to last for longer than that of special subjects. One might speculate on possible psychological reasons for this. Schmidt is now well into decline and it might be argued that Stanford's results are no longer what they were. Braud is at his peak. In terms of longevity, though, on this picture of psi distribution, Honorton must surely be the Stepanek of the experimenter set, and he still shows no signs of decline.

In addition to the long-term comings and goings of psi ability, there are short term variations. At the 1976 Parapsychological Association convention in Utrecht Schmidt talked about his "dry times" when he could "elicit" psi from no-one, presumably including himself.

If the psi we see in groups comes from experimenters but that of special subjects comes from themselves, then would one not expect to see evidence of two kinds of psi?
In terms of personality research Schmeidler (1974, p 101) has put it beautifully "We need two strategies: full personality studies of the best, authenticated psychics to see what commonality they show and comparable studies of others for whom we have ESP or PK scores. If the two sets of findings mesh, we accept the second possibility above, a continuous range in psi ability. If they are discrepant, we accept the first (a qualitative difference).". Much more research is needed but I cannot help quoting Koestler (1974, p 167)
".... ironically, Rao's composite portrait of the potentially high scoring ESP subject .... is in almost every respect the exact opposite of Pratt's description .... of the highest scoring person known at present - Pavel Stepanek.". If the ESP process is indeed a two stage one, as suggested, then perhaps the characteristics

associated with having good connection to one's psi source are quite different from those associated with having a good brain RNG. At least this suggests some quite exciting psychological prospects for investigation.

The proposal has been written in the form of one particular person having the psi: is it not more reasonable to suggest that people combine, as Kennedy and Taddonio (1976, p 22-23) imply? This seems highly probable and somehow, it sounds much less radical, but in fact it is only a rather minor modification. If we once conceive of the experimenter having a "psychic finger in the pie", it ceases to matter much how tiny a finger it is: he is still going to pull out the plum. More explicitly, this modification does not, increase our confidence that our special experimenters' process-oriented results are meaningful. Secondly, to produce interpretable data we must either learn to detect where the psi is coming from, or be able to screen the experimenter portion off, just as with the unmodified proposal.

Will the psi-star component be the same regardless of the psi ability of the experimenter looking? The answer to this is dubious. In the absence of a theory of psi nothing can be said. If we wish to adopt the observational theory view then the answer is probably, no.

According to the observational theories, the function of PK is to change probabilities, in other words psi is an alternative name for luck, the kind of luck that sticks with a given person. Consequently, an experimenter will not only be lucky in having his experiment turn out according to his hopes (using his own PK) but will also be lucky in finding the kind of subjects he needs. In other words, an experimenter with psi will find (if he wants that) many more subjects of the psi-star type than would an experimenter without psi. At first sight this seems more hopeful for process-related investigation than a group experiment with a special experimenter; but this may not be true. If the experimenter's desire is to show, say, that standing on the head improves psi, then his goal-directed PK will act so as to satisfy that need, selecting only special subjects who will show just that behaviour. There is some empirical justification for the suggestion that special experimenters find more psi-star subjects, for example compare Fisk to West (West, 1973; 1954). Is there any real evidence that special experimenters have psi? If the scheme proposed above is correct then experimenters with psi will get their experiments to work, while those experimenters with no psi ability will obtain only chance results. The micro-PK review by Honorton (1978) provides a convenient base on which to test the prediction. Most of the experimenters involved have considerable experience in the field, thus data about them are available.

How can an experimenter's psi ability be determined from the literature. The only practicable method is to scan for work in which these researchers have scored significantly as subjects. It is probable that this is a rather poor criterion for psi ability. It will be heavily contaminated, particularly by reporting bias: it has generally been regarded as somewhat improper for an experimenter himself to have psi: however the probable result of this will be to attenuate any relationship which exists, rather than to inflate it. If any of the experimenters in a multi-author paper qualify as psychic then it is classed as "psychic experimenter". For literature references see Millar (1978). (Note: in that paper one experiment was incorrectly classified.) An experiment is classified as successful if the result is significant at or better than the 5% level. The data are presented as Table 1.

TABLE 1

Relationship between psi ability of E and the success of his micro-PK experiments

	Success	Failure	Total
Psychic E	33	9	42
Residue	2	10	12
Total	35	19	54

Chi-square (corr) = 13.1 df=1 $p<3x10^{-4}$

Psychic experimenters have significantly ($p<3x10^{-4}$) more experiments succeed (79%) than the others (17%), as predicted. It is, moreover, rather surprising to find that psychic experimenters account for over 3/4 of all the studies. The scoring rate for the residual (non-psychic) experimenters is likely to be considerably inflated by the usual bias towards publishing only positive studies. Honorton (1978, p 9) takes an outside estimate that 10 insignificant and unreported studies occur for every one reported. More plausible would be a ratio of 2 to 1. In this case, for the non-psychic group, the two successful studies would actually be selected from 36, rather than the 12 reported: this is 5.6%, whereas chance expectancy is 5%. It is unfortunate that the last step involves a guess, however reasonable it may be. It is interesting, therefore, to calculate the probability of the observed number of successful experiments for the non-psychic group occurring by chance alone. The exact binomial probability of two or more such studies from the reported 12 tries is 0.12, not significantly different from chance expectancy. The data strongly suggest, that at least in micro-PK, if an experimenter does not have publishable psi himself, then he is not likely to see any in his experiments.

In summary, a scheme has been proposed relevant to the distribution of psi in the population, one which contrasts rather strongly with the usual view. In this scheme, only a few have psi ability and of these some are parapsychology's special experimenters. The picture is coherent and in accord with much experimental evidence. Further, it seems to have some predictive value. Only experiment can, in the last resort, determine how much or how little truth it contains.

EMPIRICAL DISTRIBUTIONS

Schmeidler and McConnell (1958) present the distributions of total scores for subjects each performing 9 runs with ESP cards. These are reported separately for sheep (889) and goats (363), the data have been summed to reconstitute the parent population (889 subjects), giving Table 2 and figure 5.

TABLE 2

Observed and expected distributions of Schmeidler and McConnell's subject-total scores, for the 889 subjects (526 sheep, 363 goats), each performing 9 runs

run total	0-34	35,36	37,38	39,40	41,42	43,44	45,46
number observed	27	46	62	79	98	119	96
number expected	32.6	34.5	56.0	80.3	102.1	115.4	116.7
chi-square (1 df)		3.99x					4.23x

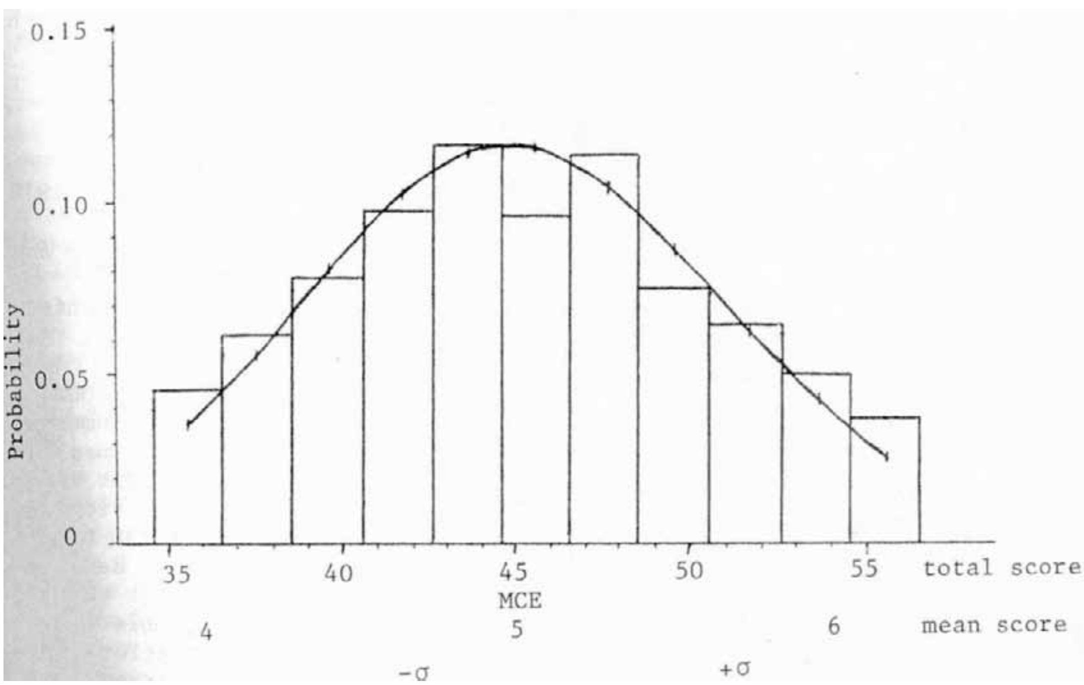
	47,48	49,50	51,52	53,54	55,56	57 and over
	115	77	65	50	37	28
	105.9	86.5	63.7	42.6	25.8	26.9
					5.01x	

x $p<0.05$

chi-square = 17.5 df=12 ns

Overall, the curve conforms to binomial expectation. However, 3 of the 11 histogram bins are independently significant. The two extremes

Figure 5. Scoring distribution for sheep plus goats



are over-represented. On the low-score side this is due to the goats while on the high it is the result of the sheep. Furthermore chance scores are under-represented. This is due almost entirely to the goats.

The last would hardly be worth mention save that a similar effect has been noted elsewhere, but in a run-score distribution with a single subject. Bierman (1978b) observed under-population of chance scores in computerised precognition self-tests, in both of the conditions used, as well as in old data. Bierman comments that such an effect is to be expected if the subject has a fixed amount of information which he may add to the system and he employs it in targetting for run-score instead of for hits on a trial-by-trial basis.

A micro-PK experiment (Broughton, Millar, Beloff and Wilson, 1977 and in preparation) used pre-recorded targets on cassette tape. The results for all experimenters were homogeneous and the data for the 256 subjects were pooled. However, the experiment gave no evidence of psi and thus, not surprisingly the distribution conforms to binomial expectation.

Walker (1975) enterprisingly assembled a distribution for 1002 subjects from 14 different experiments reported in early volumes of the Journal of Parapsychology. Unfortunately, he simply assumes that these studies can be meaningfully pooled. There is no attempt to determine whether the experiments have homogeneous distributions: this is quite improbable since they varied in such important factors as experimenter, type of test, number of trials and p-hit. He attempts to correct for the p-hit differences by using, not the scoring rate, but the genuine hit probability; however, this is a shot in the dark. Furthermore Walker's plot is "corrected for those expected in the original data from chance alone". To subtract the binomial expectation for those subjects who scored only at chance requires their number. Walker does not explain how this estimate was made.

At this stage meaningful pooling of disparate experiments is not in general possible: the unit of analysis, for the present, must be the individual experiment (or indeed only the part of an experiment which can be considered homogeneous). In order to obtain a distribution of subject scores with some statistical stability it is necessary (a) for each subject to perform a reasonable number of trials and (b) a large number of subjects should be tested. The number of experiments in which, say, more than 100 subjects are tested under standard conditions is rather small, thus partly accounting for the rarity of empirical distributions in the literature. While there is still little knowledge of how psi may affect the distribution these large-scale experiments are primarily important in providing suggestions to visual inspection, of the kinds of thing that may be going on.

However, Walker's approach of using much smaller experiments to find out about distributions, while premature, holds out more promise in the long term. If parapsychological workers routinely reported subject distribution data (as was done much more frequently in the early days) then, even though each one was extremely contaminated by noise, tests could be made on the set of curves. For example, suppose we take the suggestion from Schmeidler's data that chance-scores are under-represented. If we have 20 curves from small-scale experiments the Z-scores of the middle bins can be compared with a single mean t-test against the expected mean of zero.

In addition to the above large-scale curves reported, two recent experiments have collected sufficient data to yield distributions, though in neither case has this actually been done as yet. The first is a large-scale classroom screening exercise reported by Palmer, Tart and Redington (1976). This employed about 2000 subjects in all. There were 10 cells in the experimental design, thus giving about 100 subjects per cell. While there were differences between cells the overall mean and variance did not differ from chance expectation.

The chance values of the first two moments are consistent with the possibility that if there are enough conditions, subjects, experimenters in an experiment, the overall distribution curve will approach chance expectation. The few psi-star subjects out on the tails will be diluted into overall insignificance by the residuum. This would be consistent with the large-scale trials carried out each day by the gambling industry. It is, furthermore, entirely compatible with Braud's "spreading thin" idea (Wood, Kirk and Braud, 1977; Braud and Braud, 1979), which in its turn follows immediately from Walker's theory (analogous to run-score targeting: see Millar, 1978), if it is assumed that the experimenter is the major source of psi in his experiments.

The other set of data which have not yet been published in full is the Amsterdam fast RNG experiments of Bierman and Houtkooper (1978). A standardized micro-PK task was attached to each of ten different experiments: in total 92 subjects were employed. The strategy of combining standard screening trials with on-going experiments might be considered by other experimenters interested in populations.

Thus of the only two curves currently available to the writer, one of them (Schmeidler and McConnell) shows only the democratic psi distribution (bi-directional here) and the other (Broughton et al.) shows no psi at all.

In addition to distribution curves it is of considerable interest to determine the incidence of psi-star subjects. A thorough survey of the literature, while desirable, is outside the scope of this paper. However, some estimate of the frequency should be made here. Some selected examples of screening experiments are Fisk, 1951 and follow-up Mitchell, 1953; West, 1947; Randall, 1974a and follow-up, 1974b; Tart, Palmer and Redington, 1979. Estimates of the prevalence of psi ability range all the way from 6% to 0%. The last study may perhaps be selected on account of the large number of subjects employed. From an initial batch of 1835 subjects, 3 out of the 10 finalists scored significantly in the training study, 2 at the 5% level and one at 0.4%. Because of the large number of drop-outs the 0.16% of psychics may be conservative. From comparison Schmeidler and McConnell (1958) found no consistently high-scoring subjects among their 1308 subject sample. As a very rough estimate perhaps one in a thousand may be a psi-star subject.

DISCUSSION AND RECOMMENDATIONS

The above has dealt with psi distribution in a narrow sense; however, the ideas outlined, particularly with respect to parapsychology's special experimenters, have more far-reaching consequences.

The most objectionable feature of the experimenter-psi scheme is that almost the entire literature of the past 50 years must be discarded. Some possibilities might be suggested to retain the good features and discard the unpalatable ones. For example, could it not be that special experimenters are quite ordinary and that those who do not see evidence of psi are actively inhibitory? This does not seem likely: high-scoring subjects are not often extinguished by even the most psi-null experimenters (e.g. West and Fisk, 1953). In any case there is direct evidence that the psi-plus group has psi and in terms of frequency they are certainly the odd men out.

What about the possibility that psi-plus experimenters simply release their subjects' psi, presumably via the experimenter's PK. At first sight this seems an excellent compromise. However, when we note that at least in some cases (e.g. Honorton, 1976) experimenters almost certainly used their own psi to produce the desired result, the confidence this proposal suggests immediately collapses. If experimenters have psi at all, their results cannot be taken at face value.

An unfortunate result of the importance of the experimenter, for whatever reason, is that experimenters are well aware that their results are critically dependent on their own mood and enthusiasm. When most special experimenters are not in a fired-up state no scoring is obtained. Consequently, experiments are hardly ever repeated exactly and the hum-drum repetitive jobs that are the daily bread of most psychological work never get carried out. Even if the experimenter effect is a conventional sensory one, if experimenters are always forced to jump from topic to topic, no really solid data-base can ever be laid.

If an experimenter effect, of whatever kind, is the over-riding factor in psi experiments then something has to be done. In short, now that we know we have it we must get rid of it. Any experiment with only a single experimenter is not worth very much, because of the possible sensory contribution. If the experimenter effect is a sensory one, however, it is relatively easy to minimise it: a number of data collectors should be employed. At the very least, a routine feature in psi experiments should be that someone else runs some of the subjects. However, this kind of precaution is of little use against experimenter-psi. Sometimes lack of sensory contact with the principal investigator seems to block his contribution (e.g. Pratt and Price, 1938) but more often it does not (e.g. Fisk, 1951). Perhaps this manipulation has an effect on the experimenter's psi contribution only if it changes his attitude.

Can the observational theories be of any help in detecting or eliminating experimenter-psi? At the present state of the theories no cut and dried answers are possible. It would be surprising if they could provide any certain prescription for parapsychology's ills without the aid of experiment. As in other scientific work the theory should suggest experiments which lead to a more useful theory and so on. Some effort has been expended on the theories in the hope of improving on them in this respect (e.g. Millar and Hartwell, 1979) and this work continues; however in the virtual absence of empirical evidence this is very much a guessing game.

However, the theories do offer strong suggestions. In particular they assert that feedback is a necessary condition for anyone to exert a psi influence. Consequently, if the feedback and/or the experimenter, is manipulated at feedback, then his contribution should change. One complication is that E typically obtains feedback at a large number of points in the experiment. Thus, while it is likely that he usually exerts his major PK effect somewhere near the final analysis, for the purpose of experimental feedback manipulations it would be advisable to limit his contact with the data so far as is practicable, so that there is only one point of feedback. It will be noted that this is consistent with the parapsychological lab lore that the experimenter should not examine his data until they are all in and then only when he is in a good mood.

The modification suggested by the observational theories is that the data should be split, unseen, into two subsets. For example E then looks at one set when he is in a good mood, the other when in a bad mood: he could be drunk or sober, in ganzfeld or not etc. If experimenter psi is an important factor then these manipulations will probably affect the results obtained. It would be advisable to adopt this general procedure as an integral part of all psi experiments. The easiest process would be that two different people observe the data.

Such manipulations do indeed produce scoring differences (e.g. Broughton, 1977; Bierman, 1978a). What exactly these differences mean, however, and how we can proceed further, are not so clear. The experimenter (or someone else) may look later at the difference between subsets, which he eventually does see. Because the observational theories are formulated so that psi is space-time independent he may possibly use his PK then and treat the whole thing just like any other experiment. It is clear, however, that factors in the real world limit this space-time independence and the task of experiment is to determine what these may be. If these factors can be isolated then they can be deliberately used to control experimenter psi.

What sort of evidence would suggest that a subject was playing some part? A strong indication would be if the subject could score with other experimenters. If the experimenter himself were unable to score but could do so only in combination with a particular subject, this would strongly suggest the importance of the subject. In general if different subjects scored at grossly different rates with the same experimenter, this would suggest the subjects had some influence, especially if the scoring rates were much in excess of those the experimenter could produce unaided.

The question of the combination of psi sources is of considerable importance (Millar, 1978, p 323). A most interesting case would be if neither S nor E could score separately, yet they could when acting together. If such an effect should prove to be other than a psychological effect, then the suggestion would be that a psi source was being created from two non-sources.

It might be thought that the ideas proposed here would be alien to our special experimenters. On the contrary, these workers seem always, like poltergeist agents, to have harboured some deep suspicion about their own involvement in the results they observe. However, it is only lately (e.g. Honorton, 1975) that this has attracted such attention. There are a number of comments in the literature which come very close to the position outlined in this paper. Having accidentally noticed that if data are checked by different people the results are different (i.e. what the observation theories suggest) Feather and Brier (1968) comment "If this situation is true, the checker in the present may have an effect upon the calls which the subject made in the past". It can only have been due to the weight of pre-conceived opinion that they did not think to ask "If the checker is more important to the results than the 'subjects[1], whom should we be studying?".

One of the basic assumptions in parapsychology has been that psi is dependent on a constellation of delicate psychological factors. After 50 years of attempts to define these variables, parapsychology has little or no more control over the phenomena (Pratt, 1978, p 135- 137). It is the contention of this paper that the failure of parapsychology to progress, results rather from a fundamental misconception about the psi process, in particular where the psi is coming from. Whether this is true, or not, will ultimately be determined by whether the ideas expressed here lead to greater control over psi effects.

Most experimenters are handicapped by the inability to produce any psi to study: on the other hand what they see probably reflects the true state of play in the world. One possibility for this majority is to initiate large-scale screening projects to detect the psi-star subjects. Another is, in effect, to use special experimenters as subjects. A compromise might be to obtain the co-operation of a special experimenter to help with screening, since, as noted above, his psi should attract others.

For the special experimenter, it would be premature to discard his current theories for untried ones. Diversity of view, rather than unity is more likely to prove productive in parapsychology. Nonetheless, it seems the special experimenter would be well advised to "take out insurance" against his own decline by some investment in feedback manipulation.

ABSTRACT

The topic of this paper is the distribution of psi in the population. Although the basic strategy of experimental work depends upon this, surprisingly little attempt has been made to examine the question experimentally. Instead, since the Rhine revolution, it has been assumed that most people have some small degree of psi ability. The assumption is questioned here.

Drawing heavily upon the observational theories, and Walker in particular, it is suggested that the principal psi differences between people are determined at feedback. The degree to which the feedback stimulates the psi source is proposed to be crucial. The

main experimental consequence is that suitable manipulation of the state of the subject at feedback may lead to improve psi scoring.

If psi exists, the traditional distinction between subject and experimenter can no longer be drawn. The results of special subjects are almost independent of the experimenter, while in group experiments he is crucial: only a handful of special experimenters can elicit psi from unselected groups with any regularity. Explanation of this in terms of experimenter psi is decidedly no worse than the usual idea of sensory-mediated effects. Experimenter psi has also the logical advantage that it alone can be. tested (by removing the possibility of a sensory component). In a few cases this procedure has left experimenter differentials intact. This section is independent of the observational theories.

The problem with experimenter psi is not any logical or empirical deficiency, but rather what to do with it. To this end a scheme of psi distribution is proposed which uses the observational theories to suggest ways out of parapsychology[1]s dilemma. The reason most experimenters do not obtain convincing evidence of psi with unselected groups, is that there is none. Instead, this is an artifact of the psi abilities of the few special experimenters. This scheme accounts for many of the observed features of parapsychological experiments and some novel deductions can be made. Only psychic experimenters should be able to get group experiments to work: this is strongly supported by the micro-PK literature.

On this basis, the majority of experimenters who cannot "elicit" psi in the conventional way, will never be able to and should turn their attention to attempts to find special subjects or use special experimenters as subjects. Special experimenters, on the other hand, should attempt to manipulate their own psi contribution by working with the feedback they get, within the context of the observational theories.

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