
CLINICAL ISSUES IN THE PARAPSYCHOLOGY LABORATORY

JULIAN ISAACS

1. Introduction

I must first disclaim any pretensions to expertise in the area of clinical psychology. I was originally a philosopher who, bloodhound-like, followed his nose on the trail of two questions, the first being the mind/body problem, the second being whether psi phenomena are Kuhnian anomalies and herald a revolutionary development in our worldview. Following my nose led me to parapsychology, where I became an experimentalist, because I thought, and still believe, that no one has seriously addressed the problem of developing an adequate methodology for investigating the physics of psi, which I see as being accessible through the laboratory-based study on non-statistical PK effects, "Directly Detectable PK" (DDPK) (Isaacs, 1986; Hubbard et al., 1987). This led me to PK training, in which endeavor I have specialized for more than a decade.

Recently, my experiences in PK training have increasingly convinced me that the problem of obtaining "big psi," certainly of the PK variety, has an important clinical dimension. This has forced me to start assessing the existing psychotherapeutic tools which are potentially available to be pressed into the service of PK training. I would not pretend to have made more than a very limited initial foray into the clinical field as yet, and all my ensuing thoughts should be viewed as preliminary ruminations regarding possible new directions for exploration, reflecting an early phase of my thinking in this area. What is already clear at this stage, I think, is that clinical considerations are unavoidable in the quest for superior psi performance. I shall in section 4 review some of the phenomena we have observed which seem to suggest that clinical techniques may be relevant to PK training.

A more general underlying thesis I advocate is that the division made by parapsychologists between, on the one hand, the study of spontaneous psi, where clinical, depth psychological and anthropological ap-

proaches are more or less accepted and, on the other hand, experimental studies, which generally ignore clinical aspects, is arbitrary and artificial. This division is unnecessarily restrictive and ignores some salient facts, because psi performance seems to be affected by emotional factors (Palmer, 1978) as well as cognitive and other factors, so that a clinical dimension exists in all psi, whether occurring in the laboratory or in the field.

To underscore this point that the division between "real life" psi and laboratory psi is arbitrary, I would remind the reader that parapsychology laboratories are "real life" entities too, where "real life" parapsychological studies are performed, in the context of their own nexus of "real life" emotionally toned interpersonal interactions, including those between subject and experimenter and between the experimental personnel. The realization that parapsychology laboratories are subject to the emotional vicissitudes of real life should make us all more acceptant of the possible contribution which clinical approaches could make to experimental parapsychology. Now, lest the reader automatically assume that I am advocating Freudian psychoanalysis sessions in the laboratory, I should hasten to make it clear that I am not, and that the approach I am advocating is rather eclectic and tentative.

What I am advocating is, firstly, the greater use of qualitative data collection techniques. Here, clinical techniques are included, but qualitative data collection paradigms developed from other areas of inquiry are also relevant. Parapsychologists tend to be obsessed by quantitative methods, to the exclusion of qualitative ones, and this represents the waste of a potentially powerful range of data collection techniques, the use of which could also lead to the enrichment of the quantitative ones, as I argue below.

Secondly, I am advocating a careful and discriminating examination of the various clinical psychotherapeutic techniques of observation, diagnosis and therapy, including especially those of behavior therapy, which seems to have a rather good record of outcome success (Kazdin & Wilson, 1978). The adoption of techniques of data collection and therapy developed in the clinical arena might aid parapsychology in understanding salient psychological dynamics and may also make more potent our stock of psi-facilitating treatments. These are the goals of my approach here.

Since my own area of experimental specialization is DDPK training, I shall illustrate some of my themes directly from my own work, but to have point, the issues I discuss from this area must have general applicability to other experimental paradigms and psi tasks. I hope that the generalization of my group's findings, suitably modified, may be

fruitful stimuli to other parapsychological experimentalists. From my experience in other areas of psi, discussions with colleagues and reading of the literature, I would argue that similar issues to those arising in DDPK training crop up virtually universally in experimental parapsychology. I briefly review this evidence in section 5.

PK training is a relatively new concept in parapsychology, and is unlikely to be widely accepted until either some other group than my own performs formal studies to prove its reality and effectiveness, or else its use becomes adopted for purely pragmatic reasons by groups who aim to study DDPK. As yet, no one has attempted to replicate my initial studies of DDPK training (Isaacs, 1983, 1984b). However, as I have argued at length elsewhere (Isaacs, 1986), if we are to investigate the physics of PK, DDPK tasks are likely to be essential research tools, and training of DDPK ability seems, in my own experience, *prima facie*, to be effective (Isaacs, 1983, 1984b).

A recent collaborative proof-oriented experiment (Hubbard et al., 1987) to demonstrate the existence of the Piezo-PK effect was performed at John F. Kennedy University (JFKU) and SRI International. Although at this time of writing (September 1987) the results are not yet known, the study clearly demonstrates that very high quality proof-of-principle investigations can be performed using DDPK effects as the target phenomena. The task of the JFKU-based research team was to select and train Piezo-PK agents to be run under the independent control of SRI personnel, using SRI-designed and built Piezo-PK instrumentation. The JFKU-based section of this collaborative project was termed the "Remote Action Project." The SRI-based experimentation is of major importance in illustrating the very high degree of evidentiality that it is possible to achieve in well designed DDPK proof-of-principle experiments. The brief psychological case histories I cite in section 4 are derived from the training phase of the Remote Action Project. In passing, I would like to make it clear that the themes, opinions and viewpoints I present in this paper are my own, and that nothing I say here should in any way be taken as representing the opinion of SRI or its parapsychological researchers regarding any of these matters.

Two major tasks would seem to be fundamental to the further development of DDPK training methodology. The first is to identify, describe and, as far as possible, measure the psychological phenomenology and underlying psychological factors associated with PK training. The second, using the knowledge gained in the first step, is to attempt to improve the DDPK training procedures. Clinical approaches can be fruitful in both tasks—the delineation of psychological factors—and attempted optimization of training procedures. In both domains

the most efficient strategy necessarily involves multidisciplinary convergent approaches, employing clinical, experimental, quantitative and qualitative methods, because no single methodology shows a sufficient combination of comprehensiveness and precision.

The use of quantitative psychometric measures, whether derived behaviorally or by self-report, obviously has an important role to play in the exploration of the psychology of PK performance and training. Quantitative measures have been the workhorse of psychological factors research in parapsychology and nothing stated here should be construed to deny or belittle such techniques. They will continue to play an essential and central role in this field and show obvious potential for development and refinement as our understanding of psychological factors grows. However, I want to start my consideration of the possible role of clinical approaches in PK training and experimental parapsychology generally by briefly making a case for qualitative data collection methods. My motives in doing so are to try to point to the important potential role that qualitative data collection could have in broadening the scope of psychological data collection in experimental studies, in alerting experimenters to unsuspected psychological factors and in identifying clinical phenomena arising in the laboratory context.

2. A Role for Qualitative Data Collection in the Laboratory

By qualitative data collection, I mean essentially the collection of information which results in a product having the form of non-numerical written descriptions. The data can be collected by several means, including, but not limited to, interview, participant observation and the self-reports of experimental participants. Various techniques for eliciting behaviors which are subject to observation and qualitative analysis, such as hypnosis or projective tests of various sorts, may also be employed. There are many varieties of qualitative data collection and analysis. Some are explicitly psychological in origin, such as those used in behavior therapy (Gambrill, 1977), social psychology (Maccoby & Maccoby, 1954) clinical psychology (Matarazzo, 1965) and phenomenological psychology (Colaizzi, 1978; Polkinghorne, in press). Others were developed by cultural anthropologists (Spradley, 1980). Anthropological techniques of participant-observation and interview have been pioneered by parapsychologists who have anthropological backgrounds (Giesler, 1984, 1985; Winkelmann, 1981) and they seem to have much potential.

First, perhaps it would be useful to examine some usually ignored prejudices which seem to infect experimentalists. Experimental para-

psychologists at present seem to show little interest in qualitative data collection. To a great extent this may be because of their lack of training in these techniques. However, I suspect that many parapsychologists probably regard qualitative data collection and analysis as somewhat of a sham—a poor substitute for the concrete “exactness” of quantitative data collection, which appears to provide a reassuring sense of objectivity relating to hypothesis testing in terms of P values and measures of distribution, etc. Their training has led them to value quantitative outcomes above any other form of data collection, because of their clear relation to hypothesis testing.

But the crucial point to grasp is that quantitative and qualitative data collection are complementary, not competitive, because both can gather data which the other technique is unable to deal with. It is not a case of “either-or”: appropriate domains exist for both methods, although these domains can overlap. Qualitative data collection techniques can yield data concerning participants’ and experimenters’ constructs: for example, their attitudes concerning participation in experimentation, towards the psi task, and experimental personnel, their values, world-views, social relationships, conflicts, problems, subjective experience and psi facilitation strategies. Quantitative data collection methods often cannot adequately address the complexity of these high level, global domains. In most current studies, this information is usually ignored.

The present almost exclusive concentration on psychometrically gathered data has the effect of putting blinkers on researchers whereby they tend to conceive of psychological factors only in terms of those measurable by already-existing psychometric tools. The traditional parapsychological quantitative experimental paradigm excludes the systematic gathering of qualitative data. The experimenter therefore often remains wholly unaware of possibly crucial psychological determinants, which, because they are not trapped by the psychometrics selected for use in the study, escape attention. This is especially true if the relevant factor is not one chosen by the experimenter to be the focus of their investigation in the initial design of the study. But irrelevance to the principal hypothesis under test certainly does not prevent any given psychological dynamic from dominating the results. Indeed, this type of factor may be responsible for some of the mystifying elusiveness and apparent inconsistency of psi. Just this point establishes a very strong case for supplementary qualitative data collection, a case which deserves being spelled out in some detail at this point.

Qualitative data collection enables a very broad range of psychological and social factors to be investigated concurrently. Quantitative psychometric measures carry with them the danger of over-analysis,

significant correlations being generated by chance if large numbers of factors are sampled. Qualitative data collection cannot produce exactly this error, although the cognate errors of the seeming identification of, in fact, chimerical or noncausally determinative psychological factors is obviously possible. However, a strong point in favor of the qualitative approach is that, for what it is worth, the parapsychological informants—the experimental participants—can themselves rank or otherwise indicate to the investigator the relative importance which they ascribe to each of a large number of psychological factors. Although, obviously, the opinion of the participants could never be a sufficient criterion for the identification of major psychological factors, they may thus supply at least some kind of indication of which factors would *prima facie* be worth evaluating as causally salient. Whereas if no psychometrically measured factor or combination of factors accounted for very much of the variance in the quantitative data from a study which employed only quantitative measures, the experimenter is left uninformed.

The anthropological field study approach, as described below, would potentially enable the experimenter to identify major psychological dynamics common to the responses of his experimental subjects even if these dynamics had not been foreseen during the period of initial hypothesis formation and experimental design prior to initiation of the study. The use of qualitative data collection and analysis would enable the experimenter to become aware of and examine such psychological factors in spite of these dynamics being unexpected and not trapped by the psychometric tools used. The results of such additional data collection may therefore be of considerable heuristic value, since psychological factors initially identified through qualitative data collection may later be measurable using existing or specially developed quantitative psychometric tools. In this way, relevant, but unanticipated psychological factors can be trapped by the qualitative “fishing net,” which is literally open-ended. One of the most important roles of qualitative data collection could thus be to alert experimenters to factors which their conventional psychometrics missed, but for which new psychometric tools could be developed.

I next want to point out a paradox in experimentalists' attitudes and behavior towards qualitative data. Frequently, the experimenters who are publicly committed to exclusively quantitative data collection do, even so, perform an unacknowledged type of informal qualitative data collection and analysis, in the form of formulating and exchanging the “lore” of parapsychology in behind-the-scenes discussion at parapsychological conferences. My point here is that the techniques of quali-

tative data collection developed deliberately, whether, for example, as a methodology for anthropological field observation and analysis, or for clinical purposes, are likely to be much more rigorous and disciplined than are the very informal and undisciplined observations of individual parapsychologists which give rise to the "lore," the looseness of which, incidentally, may be responsible in large part for the negative attitudes of experimenters towards qualitative data.

To support this claim, consider that it is probably not yet appreciated within the ranks of parapsychological experimentalists that, for example, although ethnographic field observation methods (Spradley, 1980) do not employ experimental controls, the hypothetico-deductive method is nevertheless utilized. Hypotheses are formulated as the data are collected and subjected to analysis. Further data are then collected to test hypotheses deriving from the first level of analysis. The method is cyclical and may involve several cycles of data collection, analysis and hypothesis development, followed by renewed data gathering. This method does not just employ passive observation on one pass. It is an active reiterative probing of the field situation. The targeting of data collection responds to and tracks regularities and questions exposed by successive analyses of the data recovered from interview and observation. Attempts are made to try to identify not just the surface phenomenology, but also the causal determinants of the situation under investigation, even if these are hidden and not necessarily obvious at first glance. Given a choice between the current "method" of informal parapsychological "lore" and the ethnographic method, why not adopt the more rigorous technique, since it has already been developed by other workers with similar interests to our own parapsychological concerns?

It is also worth considering whether parapsychologists' views of qualitative methodologies might also be skewed because of certain intellectual preconceptions held by some experimentalists. There are several polarities in this area which should be distinguished in order to clarify our discussion. I have alluded to the quantitative-qualitative distinction, but many experimentalists seem automatically to equate this pair of polar opposites with three others. These are the laboratory-field, experimental-observational and testable-non-testable distinctions. Yet it is simplistic to equate these polarities, because they are clearly distinguishable and studies fulfilling every permutation of these categories could be performed. As our own ethnographic study of PK training demonstrates (Faithorn et al., 1987), qualitative studies of quantitative laboratory experiments are possible, just as are quantitative field studies and the testing of hypotheses by qualitative means, as occurs in some

ethnographic studies. The gathering of qualitative data does not commit the experimenter to working in a field setting, nor to working with untestable hypotheses, even if the validation of such hypotheses is not performed using quantitative criteria.

I shall now turn to some clinical issues which have emerged in our recent DDPK training investigations. The use of qualitative data collection in this research has sensitized us to the existence of clinical dimensions in DDPK training. Our attempts to counter inhibitory factors, rather than just passively note them, has led to an increasing realization that the psychotherapeutic disciplines may have already developed techniques for facilitating positive mood and attitude changes, reduction of inter- and intra-psychic conflict, and the facilitation of psychological adjustment and psychological growth which may be of direct relevance to the quest for very superior psi performance. It seems that perhaps many parapsychologically valuable psychotherapeutic techniques may have been developed which could be of great application in facilitating psi, but because of the compartmentalization of knowledge and specialization in parapsychology, this psychological technology is unavailable until its application has been pioneered and demonstrated to the parapsychological community.

3. (i) The Clinical Phenomenology of PK Training

PK training inevitably introduces clinical issues because many of the apparent inhibitors of PK performance in the training situation seem to be emotional factors which might respond to psychotherapeutic approaches. Here I must make the cautionary statement that the following description and analysis of psychological factors in DDPK training are based on the qualitative data collection of myself and my co-workers and have not yet been tested using quantitative techniques. The quantitative confirmation of our conclusions based on qualitative data collection is in an early stage, partly because of the problems of insensitivity and lack of power created for psychometrics by small subject numbers, and partly from a desire to achieve some qualitative understanding of this phenomenology before attempting to measure it using psychometric tools.

Six primary factors having clinical implications seem to emerge from the qualitative analysis as possibly influencing PK performance, although other types of factors also appear to influence performance which are not so clearly clinical in type. The basis for these hypotheses are the observations made of participants undergoing Piezo-PK training at JFKU in 1986 and 1987.

3. (ii) *Performance Anxiety*

The first factor is one which I hypothesize to be the primary reason why psi performances are not easily obtainable. If psi were to be as easily demonstrable before critical witnesses as are, say, athletic skills, parapsychology would be in a very different position now from where it is. This factor is, in my view, one of the principal reasons why parapsychology has remained rejected for so long, because high level psi performances are not available "on demand" for skeptical witnesses to view. This factor, performance anxiety, is familiar to sports psychologists, sex therapists and behavioral therapists. In the PK training context it seems clear from observations of trainees that they are subject to performance anxiety, triggered easily if the trainee construes the performance situation as being a test of his or her PK ability.

It seems very strange that parapsychologists have not mounted a major investigation of the role of performance anxiety in inhibiting psi performances under test conditions—say, where an effect is being demonstrated within a proof-of-principle study, or a demonstration of psi is given to a critic or distinguished witness. If performance anxiety is indeed a major factor inhibiting psi in these types of situations, surely it behooves us as parapsychologists to investigate this factor and find ways to counter its inhibitory effect, so as to improve the replicability of psi under "demonstration" conditions. This seems especially true of DDPK and macro-PK abilities which seem to be, perhaps, more easily inhibitable than other psi capacities.

But here, the news may be encouraging because Behavioral Therapy techniques for the reduction of anxiety and performance anxiety have been proven to be rather effective in non-parapsychological contexts (Kazdin & Wilson, 1978). It would not in principle be difficult to utilize similar techniques in an attempt to reduce performance anxiety in PK agents, as is discussed immediately below.

3. (iii) *The Relationship between Belief and Performance Anxiety*

Batchelder (1984) and Stanford (1977) have both referred to the role of belief in intentional PK performance and Batchelder makes belief a central factor in his theory of PK induction. Belief seems likely to play several roles. One is to provide, as Batchelder states, suggestions which may cue PK effects (Isaacs, 1984a). The beliefs in question obviously fall into several distinguishable categories, for example, a general belief in PK, or a belief that PK is possible under the circumstances of the study in which the participant is involved, or the very situation-

specific belief that PK is just about to occur, generated by an apparently responsive PK target system. It appears that it is this specific belief which is associated with PK success in our investigations, which would be consistent with Batcheldor's hypothesizing of expectation as an effective eliciting agent for PK.

But another way in which expectant belief may function as a PK facilitator is by reducing performance anxiety. If success is perceived as being within the immediate reach of the PK agent, this seems likely to act as an anxiety reducing factor. Clearly, this hypothesis is potentially testable within suitably designed studies. This factor would provide one mechanism to explain why artifactual signals of PK success appear to act as releasers for real PK to occur, Batcheldor's "artifact induction hypothesis" (Isaacs, 1981, 1984a).

Much of the behavioral therapy approach to control of performance anxiety consists of equipping the client to replace his or her lack of confidence, feelings of defeat and incompetence towards the task with coping strategies of various kinds, central to which is relaxation and the interior recital of statements expressing confidence and competence. The locus of such a strategy is clearly to effect changes in the beliefs of the individual, which ties in neatly to the previous considerations regarding the role of beliefs and performance anxiety. Such strategies appear to be successful in training clients to cope with various non-paranormal demands (Gambrill, 1977) and it seems a fruitful hypothesis that PK performance, too, might be made less easily inhibited by performance anxiety if these procedures were adopted. Given the notorious frequency of disappearance of macro-PK performance under rigorous test conditions it seems only commonsensical at least to check whether clinical techniques for the treatment of performance anxiety might enhance DDPK performance, since test conditions are clearly more likely to induce performance anxiety than are informal conditions.

3. (iv) Negative Emotional Effects of Life Events

Stress resulting from negative impacts in trainees' lives appears to be highly inhibitory of PK performance. The inhibitory effects of negative life events appear to be mediated by their effects on the trainees' mood and general morale. This feature of the DDPK performance of trainees at JFKU has been very pronounced. Any emotionally significant loss or conflict, such as the loss of a loved one, loss of security, a financial loss, interpersonal conflict, or any persistent problem which impacts trainees' morale can seemingly be responsible for inhibiting trainees' DDPK output. This would appear to be potentially a clinical

issue, since the loss of trainee morale from these sources might be reduced by psychotherapy. Total immunity from negative impacts created by events in trainees' lives would be possible, presumably, for an enlightened Buddhist master, but the extent and duration of the emotional effects of traumatic or problematical life events may perhaps be reduced by psychotherapeutic means and, in the longer term, by the psychological growth of the trainee whereby he or she becomes less negatively reactive to life events.

In the DDPK training process, the first 30 minutes of each 90 minute training session is used, when necessary, as a preparatory period during which the trainee attempts, by various processes, to resolve problems brought into the session from his or her life, and/or to redirect his or her attention away from them. The overall purpose of the preparatory periods is to (1) reinforce belief in the trainee's DDPK ability, (2) create a positive mood change in trainee and trainer, and to enable each to free themselves from the negative emotional tone created by unpleasant life events and (3), to orient the trainee towards his or her DDPK induction strategy and the DDPK task.

It seems likely that this process would be made more efficient by the addition of appropriate psychotherapeutic techniques. It was often found that the preparatory period was not sufficiently powerful in freeing trainees (and trainers!) from the distress caused by negatively impactful life events. Three major problems seemed to reduce the effectiveness of the preparatory periods in producing positive mood shifts. The first was that preparatory periods were not long enough, but making the preparatory period an hour long would have demanded that training sessions be two and a half hours long, a difficult requirement to satisfy. The second was that the trainee and trainer often could not readily acknowledge distress. Often the true nature of the distress would only emerge in conversation towards the end of the (consequently not very PK-productive) training session. The third was that if it was the trainer/experimenter who was in distress, there was sometimes role conflict for the trainer between the role of "powerful" experimenter and the need to disclose distressing material. Non-disclosure would frequently prevent the experimenter from being able to accomplish positive mood changes. Since the DDPK results seemed to reflect the state of both trainer and trainee, if either of the two were in a distressed state, results suffered. The use of the preparatory period has suggested the hypothesis that part of the effectiveness of psi-facilitating strategies (visualization exercises, meditation routines, etc.) may be that in addition to orienting the individual towards the task and inducing a psi-facilitating state of consciousness, the attention of the

individual is directed powerfully away from thoughts and feelings having negative components, so that positive mood change is achieved.

Some DDPK trainees are individuals who seem to possess the potential for good DDPK performance as indicated by their ostensible DDPK performances in screenings for DDPK agents and by their profiles on the psi experience questionnaires (PEQ) but the manner in which they lead their lives effectively prevents them from realizing that potential. It is a recognized clinical syndrome that some individuals compulsively load themselves with tasks which overwhelm them, or compose schedules which leave them overwhelmed (Oberndorf, 1951; Maclowitz, 1980). One of the case studies described below in section 4.(ii) details an example of this phenomenon. Frequently it can take some probing to enable an experimental participant to acknowledge that he or she is indeed suffering from overwork or being overwhelmed, although the objective record of his or her activities clearly warrants this conclusion. The reasons why individuals lead chaotic or excessively busy lives seem to have a clinical dimension.

Another syndrome which has also been noted within Piezo-PK trainees is that some individuals who have strong spiritual and psychic values pay scant regard to maintaining an adequate income. The effects of low income are often stressful for the individual and inhibitory to his or her psi performance. This phenomenon seems to have both clinical and cultural aspects, since our culture tends to put spiritual/psychic values and material values into opposition. Some psychically inclined individuals tend to identify with the opposition of these values by rejecting the material world to the point that they are seemingly unprepared to cope adequately with basic issues of survival.

There is a further implication of the way in which the individual's level of integration and maturity affects his or her psi, and this applies especially to parapsychologists. As a group, parapsychologists tend to be academics who have been enculturated to value highly their intellectual ability. The obverse of this is that frequently these highly intelligent intellectuals do not value their "irrational" emotional sides, and refuse to acknowledge the importance of their own emotions, even though their profession is professedly psychological. Intellectuals seem frequently not to learn effective means of emotional self-management. This can lead to a lopsided development of such individuals where they may grow to be intellectual giants whilst remaining emotionally immature. In turn, this may lead to difficulties in interpersonal relations and being prone to extreme emotional reactions to negative life events.

In the Piezo-PK training context the experimenter's state seems to strongly affect the outcome of DDPK training sessions, and impacts on

the morale of the experimenter, caused by life events and, by incidents associated with the experiment as a whole, can affect results. Perhaps this experience is not unique and, if so, the implications for parapsychologists as a group are considerable. The notion that psychotherapy for experimenters and their pursuit of psychological growth might improve the yield of psi in experimental studies might seem surprising, but there is good evidence for experimenter effects (Kennedy & Tad-
donio, 1976; White, 1977). If the logic of the foregoing remarks regarding the connection between characterological growth and reactions to problematical life events does indeed hold, the implication is clearly that less anxious or neurotic experimenters might produce better psi results. There are indications that psychotherapy sessions which presumably created positive mood change in participants in Schmeidler's study (Hudesman & Schmeidler, 1971), reviewed in section 5, enhanced ESP performance, so it certainly seems reasonable to hypothesize that perhaps psychotherapy for experimenters might augment their psi effects in a favorable way.

3. (v) The Trainee-Trainer Relationship

The third important factor is the trainee's relationship with his or her experimenter/trainer. The trainee/trainer partnership seems similar to the athlete/coach relationship, which has been noted to be of crucial importance for the performance of coached athletes (Sternberg Horn, 1986; Chelladural, 1986). Trainee and trainer must develop closeness, trust and faith in each other. They must be able to relate without excessive conflict. As one of the case studies below will show, drastic reduction in the trainee's PK output is likely to occur if a serious conflict develops between the trainee and trainer. But more subtle dynamics can also have a serious impact on the training process. The trainer/trainee pair cannot always solve these problems by themselves and clinical assistance might play a useful role in resolving these situations.

The ability to act sensitively and effectively in the training role is presumably partly due to learned social behavior and partly due to personality factors. Not every experimenter is suited to such a role. We have some preliminary indications that the trainers' personalities affect the trainee's choice of trainer. The trainer's and trainee's ability to accept their partner and successfully relate to them also influences the outcome, so that in a research team, some trainee/trainer pairs might be expected to be less successful than other combinations. Clearly, in the long run, if the capacity to function effectively in dyadic rela-

tionships is partly a function of the individual's emotional maturity and integration, this has a clinical significance.

3. (vi) *Motivation*

The fourth factor is the trainee's level of motivation. Some trainees have appeared to be willing to participate in experimentation, but are very weakly motivated (Isaacs, 1984b). Typically, this type of trainee has not shown a good training performance. Strong motivation seems to be important for good performance in the PK training setting. Evidence for similar motivational effects on randomly acting PK target systems, such as dice and the REG, has been noted within the parapsychological literature (Stanford, 1977).

Both intrinsic motivation, where the trainee values PK performance and training experience for what it is rather than for what it may bring, and extrinsic motivation, where the PK performance is viewed as subserving some other purpose, appear to be effective. Many of the PK trainees in our studies have shown a strong interest in the process of self-discovery associated with their PK training, whereas others have seemingly trained their PK ability primarily as a form of service in order to fulfil some more external mission. The ideology of the group as a whole is to view the establishment of DDPK and its real-life applications as part of a mission to orient Western mankind towards a more spiritual viewpoint which, it is hoped, will rescue the human race from nuclear annihilation. The gaining of psi's acceptance is seen as a step towards the establishment of the reality of the transpersonal realm.

Participants and experimenters can thus be viewed as having hierarchical sets of goals, with the day-to-day concrete achievements in PK perceived as serving more far-reaching goals. The importance of explicit and careful goal-setting has been acknowledged in behavioral therapy (Gambrill, 1977) and by sports psychologists (Gould, 1986) who use eclectic mixtures of behavioral therapy and other psychotherapeutic techniques. We are in process of incorporating explicit techniques of goal setting more fully into our PK training methodology.

3. (vii) *Emotional Resistance to PK*

The fourth factor, emotional resistance to PK, has been identified by Batcheldor as an inhibitor of PK (Batcheldor, 1984). Batcheldor's conceptualization of resistance is that unconscious fear is aroused by the threat of an individual's potential exposure to PK. In order to avoid this fear, and without the fear ever becoming conscious, the PK

is either paranormally inhibited from occurring, or else the individual will behave overtly in such a way as to stop the PK events from occurring, without being aware of the motivation behind his or her behavior. This factor is different from all of the others reviewed so far, in that it is a true depth psychological factor, because of the presumed unconscious nature of the fear and the unconscious motivation of the resistant behavior.

In the sitter group context, in which the concept was developed, a typical example of resistant behavior during the occurrence of ostensible PK events would be that an individual would act so as to disrupt the occurrence of further PK, by, for example, directing the attention of the group away from the PK aspects of the sitting by asking irrelevant questions which would distract the group.

Tart (1986) has discussed the fear of psi in parapsychologists and psychics. Certainly, the DDPK situation, where a quiescent PK target system provides unequivocal feedback of the occurrence of PK, is likely to maximize resistance, if the would-be PK agent is subject to inhibition from this source. But resistance is presumably lessened in our trainee group by our careful selection for PK training of individuals who report rather high levels of spontaneous and/or intentional psi functioning. This group may be much more acceptant of DDPK than a lay group would be.

However, there are certain mysterious failures of PK training which it might be tempting to ascribe to resistance; an example of this type of case is given in 4.(v). But this is a highly problematical hypothesis. The problem is that firstly, no general experimental verification of the reality of resistance to PK as an inhibitory factor has yet been performed. This, of course, assumes that at least some aspects of resistance can be operationalized, which seems arguably possible. However, unless some form of well validated psychometric or behavioral test of resistance had been administered to a PK trainee, it is impossible to know whether it is correct to invoke resistance as an explanatory factor in any individual case. Nor, given the hypothesized unconscious nature of resistance, can the avowals of the trainee be given any kind of privileged status. These kinds of problems tend to infect all explanations based on depth-psychological dynamics.

4. (i) Some Piezo-PK Training Case Studies: Introduction

The selections offered below originate from case studies conducted during the PK training phase of the 1987 collaborative proof-of-principle Piezo-PK study performed jointly with SRI. These case studies

illustrate some of the problems encountered in Piezo-PK training. The research team's contribution was to find, select and train PK agents at John F. Kennedy University in preparation for their being run under independent control at SRI.

Selection of candidate PK trainees was accomplished by three methods. One was by participation in a PK screening operation (Isaacs, 1981), where they were administered an informal test of psychokinetic metal-bending and a brief informal trial on a strain-gauge based strain-sensitive device. The second was by their demonstrating a promising initial PK ability in a laboratory trial, using the Piezo-PK detection system. The third was through personal contact. All potential trainees also completed the Psi Experiences Questionnaire (PEQ), a psi-related inventory specifically developed for selection of possible PK agents. The PEQ comprises 30 questions regarding rates of occurrence of (1) spontaneous and intentional psi experiences, especially those which could be PK in type, (2) general belief in PK and specific belief in the informant's own PK ability, (3) experience of the practice of mental disciplines, (4) spiritual orientation and (5) previous experience of participation in behavioral research.

Trainees completed some 25 90-minute Piezo-PK training sessions at JFKU prior to being run at SRI. It should be noted that the piezo sensors utilized as PK targets were located inside an electrically isolated, electromagnetically shielded enclosure. The enclosure was placed in a separate room from that occupied by the trainee, signals being brought to the computerized feedback and recording instrumentation in the participants' room by means of optical fibers. An equipment description of the Piezo-PK detection system and protocol is given in Hubbard et al. (1987). The PK trainees with the best Piezo-PK performance were selected to perform at SRI, so that only 6 out of 10 trainees were finally chosen to participate in the SRI proof-of-principle sessions (Hubbard et al., 1987).

4. (ii) Negative Impacts from Life Events

Mrs. J is a homemaker and professional psychic in her late thirties. She reports becoming seriously interested in psi some eight years ago and has self-developed her ESP and psychometry skills since then. Her profile on the PEQ suggested possible PK ability, and she claimed to be able to paranormally bend metal. She started Piezo-PK training quite promisingly, but soon after starting began to report problems of friction with her family related to her scheduling time for participation in the experimentation.

Over a period of several weeks of twice-weekly Piezo-PK training sessions it became clear that Mrs. J filled her schedule with such a high density of activities that she was permanently overloaded. This seemed to be the origin of the friction with her family. Since she claimed to be very highly motivated towards PK training, it was tactfully pointed out to her by her trainer/experimenter, E1, that if she was to reach her PK potential, she needed to slow down the frenetic pace of her life by reducing her scheduled commitments. However, she continued to lead such a busy life that she frequently arrived at her Piezo-PK training sessions in a state of fatigue. Her performance sharply declined, especially after a skiing accident which left her in almost continuous pain for two weeks.

The effect of her situation was to lead E1 to the conclusion that, because of her lifestyle, she would never reach her potential performance. E1 decided that E1's own negative expectations regarding Mrs. J's PK performance might be limiting Mrs. J's PK. Since Mrs. J was very keen to continue training, it was decided that another trainer/experimenter, E2, should take over her training on a provisional basis. In the first few Piezo-PK training sessions with E2, Mrs. J's PK performance markedly improved. However, a new and severe set of problems in Mrs. J's life then emerged, since the family had decided to relocate away from the US mainland, and she was largely responsible for selling the family's house in California and organizing the situation concerning the family's property on the island they were moving to. Again, Mrs. J's PK performance declined and she was not selected for participation in the proof-of-principle study conducted at SRI.

This case suggests the negative impact which an individual's lifestyle can have on his or her DDPK training performance. It also perhaps suggests how a change of experimenter, from one who had no faith in her PK ability to one who was neutral, may have been responsible for the sudden improvement in her performance.

4. (iii) *Trainee / Trainer Conflict*

Mrs. P is a divorced homemaker who also showed a promising profile on the PEQ. She was first assigned to E2 and showed an inconsistent Piezo-PK performance. In her very first session, she produced two very large magnitude effects. She reported that she had been very challenged in the first session and had been determined to prove that she could perform the PK task. She described herself as having been in "survival" mode, where it was going to either be herself or "it" (the Piezo-PK instrumentation) which would emerge victorious from the confronta-

tion. She then proceeded to produce no identifiable PK effects in 19 subsequent training sessions. At that point, it was decided that she would be transferred to E1, both because of E2's by now negative expectations of her and also because some frictions had developed between herself and E2.

Mrs. P's PK performance improved dramatically and fairly consistently in the first six sessions with E1, and she appeared to be a good candidate to be selected to participate in the SRI sessions. However, she and E1 had an intense conflict in the seventh and following sessions. Her PK scores immediately tumbled in the first conflicted session and never subsequently recovered. This case seems to illustrate two points. The first has been noted in the first case, that transferring a trainee from an experimenter having negative expectations to one who was neutral or positive, seems to improve DDPK performance. The second is that marked discord between trainer and trainee can seemingly be responsible for reducing a PK trainee's PK effectively to zero.

4. (iv) *Performance Anxiety*

The third case hints at the inhibitory role of performance anxiety. Miss X started her Piezo-PK training late in the experimentation. She had previously been chosen by one of the trainees as her "confidant." "Confidants," in the context of the Remote Action Project, are individuals chosen by trainees to be helpers in their PK training process. Duties of the confidant include: consistently showing an interest in the trainee's PK training progress, assisting the trainee in finding solutions to difficulties in his or her training, rewarding him or her for good performance, encouraging him or her, providing an independent viewpoint from outside the experiment, and providing an extra communication channel between the trainee and the experimental team.

Miss X accompanied her trainee to a session one day, and requested a short trial period on the Piezo-PK instrumentation. To the trainer's surprise she then proceeded to produce a definite and moderately large effect. She completed a PEQ, with a promising profile. She was recruited as a trainee and her performance in training was sufficiently good for her to be selected to be run at SRI. There, she produced an effect in her second session. She reported that she greatly enjoyed her sessions at SRI and seemed to regard her PK sessions there as lighthearted fun.

However, between her second and third sessions at SRI she overheard a conversation between myself (the principal investigator) and another member of the research team, who accompanied her to SRI for her

third and fourth sessions. In this conversation, the importance of her contribution to the tally of PK events recorded at SRI became fully apparent to her, for the first time. There is no way of proving that her subsequent failure to produce further effects at SRI was due to the change in attitude caused by her hearing the conversation. However, she certainly reported a change in attitude and seemed to take her participation in the study much more seriously from then on. The moral seems to be: don't induce performance anxiety in experimental participants by communicating to them the importance of the results they obtain.

4. (v) *A Counseling Success*

Mrs. Z is a professional in her mid-forties. She showed a very high psi profile in the PEQ and did well in her early training. During the middle phase of her training, she suffered emotional upsets resulting from conflicts in her relationship with her live-in "significant other." During this period she several times attended training sessions in some distress as a result of events in her relationship, and produced comparatively poor PK performances, compared to her norm, in these sessions. She was referred to an intern in the JFKU Transpersonal Counseling program who had volunteered as the professional counselor for the Remote Action Project. The counseling sessions held with the couple were effective in allowing them to resolve their conflicts. Mrs. Z's PK training performance then continued to improve and she was one of the most successful PK agents at SRI.

5. *Clinical Issues in Other Areas of Parapsychology*

Several psychological factors which have potential clinical implication have been noted in the parapsychological literature. In general, the studies involved have investigated the relationship between particular psychological variables and psi performance, usually ESP. Unfortunately, less research relating psychological factors to PK performance has been reported. Frequently, no attempt was made to manipulate these variables, but clinical approaches to the manipulation of many of these factors so as to attempt to enhance psi performance would be possible in principle. In the case of deep-seated personality variables, it seems likely that only long-term psychotherapy, if anything, would effect significant change. However, perhaps some neurotic conditions might be amenable to medium-term psychotherapy, particularly if the condition was appropriately treated by behavioral therapy, which is no

longer limited to the treatment of monosymptomatic conditions (Rachman & Wilson, 1980). However, other factors, particularly mood, may be accessible to modification through relatively brief situation-specific psychotherapeutic intervention.

The psi-related psychological factors include mood (Carpenter, 1977), anxiety level (Kanthamani & Rao, 1973; Nicol & Humphrey, 1953; Ballard, 1977; Palmer, 1978), relationship with and attitudes towards the ESP test administrator (Anderson & Gregory, 1959), neuroticism (Kanthamani, 1968; Palmer, 1978), openness (L. W. Braud, 1976, 1977), expansiveness (Palmer, 1977; Smith & Humphrey, 1946) and motivation (Schmeidler, 1974). An extensive review of this work is not possible here, but a few examples will illustrate my point.

The fairly robust relationship between extraversion and positive ESP scoring which has been frequently noted (Palmer, 1977) may disguise a fairly strong inverse relationship between neuroticism and ESP scoring which might be responsible for the reported results. Most extraversion scales, such as the Minnesota Multiphasic Personality Profile (Jackson & Messick, 1962) and Guilford scales (Guilford, 1959), show a high negative intercorrelation between extraversion and measures of social adjustment. Kanthamani (1968) reported a correlation of 0.63 among her subjects between the neuroticism and extraversion factors of Cattell's High School Personality Questionnaire. When neuroticism was partialled out of the relation between extraversion and ESP, this relationship became non-significant. Partialing out extraversion had little effect on the neuroticism-ESP relationship. Only in Eysenck's scales are extraversion and neuroticism fairly orthogonal, but these scales have not been consistent predictors of ESP performance.

Several studies have indicated effects of mood upon ESP scoring (see e.g., Carpenter, 1977; Palmer, 1978, for reviews of this work). Since moods are complex and can be categorized into many components by factorial analysis (Nowlis, 1965) and there is evidence (Schmeidler & Craig, 1972) that the relationship between psi performance and mood may be situation specific and may interact with personality, work to date must be viewed cautiously. However, a general trend can be discerned, whereby happier mood is associated with ESP hitting and unhappier mood is associated with ESP missing (Carpenter, 1977).

In a longitudinal study of clock-card forced-choice ESP by Fisk and West (1956) which of all the reported ESP/mood studies most resembles the DDPK training situation (because of the repeated testing situation—all subjects in the Fisk and West study completed at least 96 trials, working on a daily basis), significantly positive scoring was associated with ESP sessions where the subjects' moods were given the

highest rating as "pleasurable." These results are obviously compatible with the findings from Piezo-PK training reviewed above, where the effects of negative impacts in DDPK trainees' lives were taken to be mediated largely by their effects on mood. An implication of this is that the deliberate manipulation of mood may be effective in increasing ESP performance. Clinical techniques may have relevance in this domain, if deficits in mood are caused by psychological factors which are open to amelioration by psychotherapeutic means.

Somewhat surprisingly, only two experimental studies of the effects of psychotherapy on psi performance have been reported (Hudesman & Schmeidler, 1971, 1976) using respectively, a group of three student psychotherapy clients and a single psychoanalytic patient. The second study includes a description of attempts to perform four other studies in the same topic area which were defeated by practical problems. In the two fully reported studies, psychotherapy was not deliberately directed to factors which were believed to affect ESP performance. In the first study, the therapy sessions were rated for therapeutic progress by the psychotherapist. The sessions were divided into four groups according to therapy rating and, although the best quartile was not significantly different from the second best quartile in terms of ESP scoring, using an ANOVA measure, the ESP scoring of the best quartile was significantly better than the two poorest quartiles ($P = .02$ and $P = .05$). For two of the patients, ESP scores were exceptionally good in one session each. While blind to the ESP scores, the therapist identified these sessions as those where the two patients showed the greatest progress. Since measures of mood were not taken, the mechanism of this effect cannot be inferred with certainty.

In the second study (Hudesman & Schmeidler, 1976), a single client in psychoanalytically oriented therapy was administered an ESP test and a mood checklist both before and after 24 therapy sessions. It was found that the client's changes in mood correlated positively with ESP success ($R = .84$, $P = .06$) and the variance of his ESP scores was significantly below chance after the therapy sessions ($P < .002$) and was also significantly lower than the variance before the sessions ($P < .05$). ESP success and ESP variance were not correlated. The results were interpreted as showing changes in ESP scoring as a result of the mobilization of the client's ego defenses.

The experimenter effect has been noted as having a powerful influence on psi scores (White, 1976, 1977; Kennedy & Taddonio, 1976). Since experimenter effects appear to be unavoidable in parapsychology, optimizing those aspects of experimenter performance which are subject to change would seem to be a worthwhile objective. Since some

components of the experimenter effect seem to be mediated by the experimenter's style of social behavior (Schmeidler & Maher, 1981; Honorton, Ramsey & Cabibbo, 1975), it could be argued that psychotherapeutic treatment of experimenters who experienced difficulty in eliciting psi from their experimental participants might be effective in promoting better psi performance. Equally, clinical intervention into the participant/experimenter relationship might also be worth trying in some cases.

6. A Cautionary Note Regarding Psychotherapies

Depth psychologies such as Freudian psychoanalysis are only a small subset of a diverse collection of psychotherapies. There is no single "psychotherapy," but a rather varied assortment of techniques and theoretical constructions showing wide variations in approach. The evaluation of the effectiveness of the various "talking" psychotherapies is made difficult by the fact that virtually no methodologically satisfactory outcome studies have been published (Rachman & Wilson, 1980). The comparative outcome studies which have been published tend to show that all psychotherapies (except behavioral therapies) produce similar remission rates and that it is not clear that their remission rates differ significantly from the spontaneous remission rate in most cases. This sobering thought must be born in mind when considering clinical approaches to psi enhancement. However, the psychotherapies are historically very recent developments and the absence of adequate outcome studies will presumably be remedied in time. Certainly behavior therapy appears to produce outcomes which are better than the "talking" psychotherapies and better than the spontaneous remission rate (Kazdin & Wilson, 1978), and may well be the approach of choice in psi enhancement. Ironically, the use of therapeutic techniques in parapsychological studies may represent a well-controlled domain for testing the efficacy of such techniques in facilitating well-defined behaviors!

7. Conclusions

It has been argued that despite the contrary mind-set of most experimentalists, the laboratory represents as much of an arena for "real-life," with its share of real-life dramas and psychological stresses, as anywhere else. As such, acknowledgement of the psychological and social dynamics of the parapsychology laboratory and their impacts on the experimenters and experimental participants cannot be avoided. Observations from longitudinal training of Piezo-PK have suggested

very strongly that a number of psychological factors which can inhibit PK performance have clinical dimensions. This is to say that these factors appear to relate to the emotional life of the trainees and experimenters in PK training studies and may be alterable by psychotherapeutic means. Disregard of, and the ensuing lack of control over, these factors may be partly responsible for the unpredictability and variability of psi.

Measurement of these factors could be performed in a convergent fashion, using conventional psychometrics, behavioral and (sometimes) physiological indices, projective tests, clinical interview techniques and the methods of anthropological ethnography. The use of qualitative methods for psychological data has been advocated as being complementary to quantitative methods.

In addition to the collection of data relating to clinically relevant psychological factors, the convergent use of psychotherapeutic and other techniques for modifying mood, state anxiety, attitudes and beliefs in attempts to improve the psi performance of the individual would seem worth trying. A brief review was conducted of psychological factors having clinical dimensions which have already been noted in the parapsychological literature. These include mood, state anxiety, expansiveness, motivation and neuroticism. If these factors have an impact on psi performance, which the available evidence suggests they do, the deliberate manipulation of such factors by psychotherapeutic means may prove effective in improving psi performance across a range of tasks. Techniques deriving from behavioral therapy (Gambrill, 1977) and sports psychology may also be of use in this domain (Straub, 1980; Williams, 1986). Those components of the experimenter effect which may be due to social style, mood, state anxiety and expansiveness might be modifiable by therapeutic means, which could also be seen to include a social learning component.

In the present era of the contraction of parapsychology, the exploration of the application of psychotherapy to psi performance may seem to be a luxury. However, at least some of the problems of parapsychology are caused by psi's elusiveness, so that any means whereby psi performance could be improved and made more reliable are potentially valuable. It was suggested that particular attention to the role of performance anxiety in inhibiting psi performance under "demonstration" conditions might prove of value in permitting the development of methods based on behavior therapy for alleviating performance anxiety. The long term benefits of making psi performances more robust to observation by skeptical or critical witnesses are obvious. Outcome studies of the non-depth psychological therapies seem at

present to be deficient, and to not demonstrate a marked differential between treatment and no treatment, excepting some of the behavior therapies.

The application of psychotherapies to psi enhancement should therefore be approached with caution, but parapsychology is in a good position to provide methodologically sound validation of the positive effects of specific therapeutic procedures on psi performance. The only study directly relevant to this hypothesis (Hudesman & Schmeidler, 1971) clearly showed an association between therapy sessions where progress was reported and ESP scoring. It remains for adventurous parapsychologists to explore the application of psychotherapy to the enhancement of psi performance.

REFERENCES

- Anderson, M., & Gregory, E. (1959). A two-year program of tests for clairvoyance and precognition with a class of public school pupils. *Journal of Parapsychology*, 23, 149-177.
- Ballard, J. A. (1977). Unconscious perception of erotic, non-erotic and neutral stimuli on a psi task. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in parapsychology 1976* (pp. 159-162). Metuchen, NJ: Scarecrow Press.
- Batchelder, K. J. (1984). Contributions to the theory of PK induction from sitter-group work. *Journal of the American Society of Psychological Research*, 78, 105-122.
- Braud, L. W. (1976). Openness versus closedness and its relationship to psi. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in parapsychology 1975* (pp. 155-159). Metuchen, NJ: Scarecrow Press.
- Braud, L. W. (1977). Openness versus closedness and its relationship to psi. In J. D. Morris, W. G. Roll, & R. L. Morris (Eds.), *Research in parapsychology 1976* (pp. 162-165). Metuchen, NJ: Scarecrow Press.
- Calaizzi, P. (1978). Psychological research as the phenomenologist views it. In R. S. Valle & M. King (Eds.), *Existential-phenomenological alternatives for psychology*. New York: Oxford University Press.
- Carpenter, J. C. (1977). Intrasubject and subject-agent effects in ESP experiments. In B. Wolman (Ed.), *Handbook of parapsychology* (pp. 202-272). New York: Van Nostrand Reinhold.
- Chelladural, P. (1986). Styles of decision making in coaching. In J. M. Williams (Ed.), *Applied sport psychology* (pp. 107-119). Palo Alto, CA: Mayfield Publishing.
- Faithorn, L., Edison, R., Jenks, S., Tyndall, S., & Isaacs, J. (1988). An ethnographic preliminary study of the "Remote action project." In D. H. Weiner & R. L. Morris (Eds.), *Research in parapsychology 1978* (pp. 27-31). Metuchen, NJ: Scarecrow Press.
- Fisk, C. W., & West, D. J. (1956). ESP and mood: Report of a mass experiment. *Journal of the Society for Psychological Research*, 38, 320-329.
- Gambrill, E. D. (1977). *Behavior modification: Handbook of assessment, intervention, and evaluation*. San Francisco: Jossey Bass.
- Giesler, P. V. (1984). Parapsychological anthropology: I. Multi-method approaches to the study of psi in the field setting. *Journal of the American Society for Psychological Research*, 78, 289-330.
- Giesler, P. V. (1985). Parapsychological anthropology: II. A multi-method study of psi and psi-related processes in the Umbanda ritual trance consultation. *Journal of the American Society for Psychological Research*, 79, 113-166.

- Goldstein, A. (1960). Patients' expectancies and non-specific therapy as a basis for (un)spontaneous remission. *Journal of Clinical Psychology*, 18, 399-403.
- Gould, D. (1986). Goal setting for peak performance. In J. M. Williams (Ed.), *Applied sport psychology* (pp. 133-148). Palo Alto, CA: Mayfield Publishing.
- Guilford, J. P. (1959). *Personality*. New York: McGraw Hill.
- Honornton, C., Ramsay, M., & Cabibbo, C. (1975). Experimenter effects in extrasensory perception. *Journal of the American Society for Psychological Research*, 69, 135-149.
- Hubbard, G. S., Bentley, P. B., Pasturel, P. K., & Isaacs, J. (1988). Instrumentation and protocol for a remote action experiment. In D. H. Weiner & R. L. Morris (Eds.), *Research in parapsychology 1987* (pp. 31-35). Metuchen, NJ: Scarecrow Press.
- Hudesman, J., & Schmeidler, G. R. (1971). ESP scores following psychotherapeutic sessions. *Journal of the American Society for Psychological Research*, 65, 215-222.
- Hudesman, J., & Schmeider, G. R. (1976). Changes in ESP scores after therapy sessions. *Journal of the American Society for Psychological Research*, 70, 371-380.
- Isaacs, J. (1981). A mass screening technique for locating PKMB agents. *Psychenergetic Systems*, 4, 125-158.
- Isaacs, J. (1983). A twelve session study of PKMB training. In W. Roll, J. Beloff, & R. White (Eds.), *Research in parapsychology 1982* (pp. 31-35). Metuchen, NJ: Scarecrow Press.
- Isaacs, J. (1984a). The Batchelder approach: Some strengths and weaknesses. *Journal of the American Society for Psychological Research*, 78, 123-132.
- Isaacs, J. (1984b). *Some aspects of performance at a psychokinetic task*. Unpublished doctoral dissertation, University of Aston, Birmingham, England.
- Isaacs, J. (1989). Directly detectable psychokinetic effects: A new category of psychokinesis. In B. Shapin & L. Coly (Eds.), *Parapsychology and human nature* (pp. 81-119). New York: Parapsychology Foundation, Inc.
- Jackson, D. N. & Messick, S. (1962). Response styles on the MMPI: Comparison of clinical and normal samples. *Journal of Abnormal and Social Psychology*, 65, 285-299.
- Kanthamani, B. K. (1968). *The ESP subject: An enquiry into the personality patterns of psi hitters and missers*. Unpublished doctoral dissertation, Andhra Univ, Waltair, India.
- Kanthamani, B. K. & Rao, K. R. (1973). Personality characteristics of ESP subjects: V. Graphic expansiveness and ESP. *Journal of Parapsychology*, 37, 119-129.
- Kazdin, A. E. & Wilson, G. T. (1987). *Evaluation of behavior therapy: Issues, evidence and research strategies*. Cambridge, MA: Ballinger.
- Kennedy, J. E. & Taddonio, J. I. (1976). Experimenter effects in parapsychological research. *Journal of Parapsychology*, 40, 1-33.
- Maccoby, E. E. & Maccoby, N. (1954). The interview: A tool of social science. In G. Lindzey (Ed.), *Handbook of social psychology* (pp. 449-487). Cambridge, MA: Addison-Wesley.
- MacLowitz, M. (1980). *Workaholics: Living with them, working with them*. New York: Mentor.
- Matarazzo, J. D. (1965). The interview. In B. Wolman (Ed.), *Handbook of clinical psychology* (pp. 403-450). New York: McGraw-Hill.
- Nicol, J. F. & Iumphyrey, B. M. (1953). The exploration of ESP and human personality. *Journal of the American Society for Psychological Research*, 47, 133-178.
- Nowlis, V. (1965). Research with the mood-adjective check list. In S. S. Tomkins & C. F. Izard (Eds.), *Affect, cognition and personality* (pp. 352-389). New York: Springer.
- Oberndorf, C. D. (1951). The psychopathology of work. *Bulletin of the Menninger Foundation*, 15, 77-84.
- Palmer, J. (1977). Attitudes and personality traits in experimental ESP research. In B. Wolman (Ed.), *Handbook of parapsychology* (pp. 175-201). New York: Van Nostrand Reinhold.
- Palmer, J. (1978). Extrasensory perception: Research findings. In S. Krippner (Ed.), *Advances in parapsychological research: Vol. 2. Extrasensory perception* (pp. 59-243). New York: Plenum Press.
- Polkinghorne, D. E. (in press). Phenomenological research methods. In R. S. Valle & S.

- Halling (Eds.), *Existential-phenomenological perspectives in psychology*. New York: Plenum Press.
- Rachman, S. J. & Wilson, G. T. (1980). *The effects of psychological therapy*. New York: Pergamon Press.
- Schmeidler, G. R., & Craig, J. G. (1972). Moods and ESP scores in group testing. *Journal of the American Society for Psychological Research*, 66, 280-287.
- Schmeidler, G. R., & Maher, M. (1981). Judges' responses to the nonverbal behavior of psi-conductive and psi-inhibitory experimenters. *Journal of the American Society for Psychological Research*, 75, 241-257.
- Smith, B. M., & Humphrey, B. M. (1946). Some personality characteristics related to ESP performance. *Journal of Parapsychology*, 10, 269-289.
- Spradley, J. P. (1980). *Participant observation*. New York: Rhinehart & Winston.
- Stanford, R. G. (1977). Experimental psychokinesis: A review from diverse perspectives. In B. Wolman (Ed.), *Handbook of parapsychology* (pp. 324-381). New York: Van Nostrand Reinhold.
- Sternberg Horn, T. (1986). The self-fulfilling prophecy theory: When coaches expectations become reality. In J. M. Williams (Ed.), *Applied sport psychology* (pp. 59-73). Palo Alto, CA: Mayfield Publishing.
- Straub, W. F. (Ed.). (1980). *Sport psychology: An analysis of athlete behavior*. Ithaca, NY: Movement Publications.
- Tart, C. T. (1986). Psychic's fears of psychic powers. *Journal of the American Society for Psychological Research*, 80, 279-292.
- White, R. A. (1976). The limits of experimenter influence of psi test results: Can any be set? *Journal of the American Society for Psychological Research*, 70, 333-369.
- White, R. A. (1977). The influence of experimenter motivation, attitudes, and methods of handling subjects on psi test results. In B. Wolman (Ed.), *Handbook of parapsychology* (pp. 273-301). New York: Van Nostrand Reinhold.
- Williams, J. M. (Ed.). (1986). *Applied sport psychology*. Palo Alto, CA: Mayfield Publishing.
- Winkelman, M. (1981). The effect of normal education on extrasensory abilities: The Ozolco study. *Journal of Parapsychology*, 45, 321-336.

DISCUSSION

STANFORD: I certainly agree with you that personal and interpersonal factors, phenomena, consequences of interaction, interpersonal interaction and so forth need to be considered, but I want to point out that there is a sore need that they be considered in other areas than just parapsychology. The reason some psychological researchers act this way is because it is the way they have been trained in graduate school and undergraduate school. People are treated like parts of a machine that we put together like tinker toys. We get what we want through manipulation, but you can still be very rigorous. You can be objective. You can keep things constant across experimental conditions, not getting sloppy, but still consider the needs of the individual. In fact, I would maintain that many experimentalists engage in spurious experimental control in the sense that things do inadvertently vary across conditions because investigators may be totally unaware of the way

subjects are reacting. When I teach my graduate social psychology course I emphasize as part of the segment on the social psychology experiment, that the matter of communication, rapport between subject and experimenter, must be established beforehand so that you can bring people to a common starting point for your study so they have a common understanding of what you are trying to do together. So I don't think it stops with parapsychology. Those who are experimentally inclined sometimes fail to take heed of where the individual is when you begin your experiment. This is simply a source of error variance in the studies that reduces the chance of any statistical significance at all. And yet this kind of thing is too much ignored in human psychology experimentation. We recognize it in rat experiments when we let the animals fully explore the maze before we ever start the study so that they are familiar with it, but in human work, subjects walk in and press the button, all the stimuli come on the screen, and so forth. Now you did wake me up, Julian. I would like to suggest that there is a potentially very important phenomenon that we need to take into consideration in several respects in parapsychology. It comes out in social psychology. This is the self-handicapping phenomenon. First, I am a little bothered that you may be taking it too seriously in terms of the way you literally state it. I know you don't really think this way, but in an enthusiastic way you are stating the fact that people will tell you about their horrible day at work, for example, but it is more likely to occur after they have failed the test.

ISAACS: Yes, that is what we found.

STANFORD: There is a lot of research outside of parapsychology that shows that same thing. This is not a unique characteristic of parapsychological subjects. The very same phenomenon could tell us something that is extremely important, because self-handicapping is a circumstance in which an individual is in a situation where he or she is not sure of the source of his or her success, how closely it depends upon their own efforts. This has actually been studied outside of parapsychology and it is the paradigm in parapsychology. I mean that people typically do not know exactly how to control their psi performances. They do not know how the crucial events depend on their efforts. What has been shown in the literature outside of parapsychology is that under those circumstances, given their "druthers," given what they really like, people will often tend to select a handicap. Work under a difficult circumstance. Work on an impossibly difficult task. Why? Because if you fail there is no threat to self-esteem or to others' views of your ability. What I would suggest is that you might allow people an opportunity for self-handicapping. Let them know that this is a little beyond ex-

pectation. You do not put limits on what they might be able to do, but there are different ways to do it. We can talk about some of this at other times. But nonetheless it has been shown in work outside of parapsychology that sometimes when people are allowed to help self-handicap in certain ways they actually put more effort into it than otherwise. In terms of psychological measurements of what is going on in the experiment, we need to consider the distinction of obtrusive and unobtrusive measures. Unobtrusive measures are coming into play more and more outside of parapsychology. For instance, when tape-recording utterances during a session and doing content analysis where subjects are not focused upon it, you don't ask them questions about what is going on or they become self-conscious. Rather you look at what goes on and analyze it in a way that does not make them self-conscious and therefore alter their internal space.

ISAACS: Thank you! That is a good point. We were involved in longitudinal studies with the same individuals over a period of time and therefore what we sowed in terms of interpersonal relationships, we reaped. The kinds of relationships we generated were made very clear to us because we observed, week after week, the effects of what was going on there. I agree with you about the self-handicapping phenomenon. There is a problem which we face, especially in the context of Batcheldor's theory, as Jürgen would be aware. Batcheldor's theory says the participant must believe that PK is happening and that success is possible. One can try to build the subject's confidence up and say "yes, you can do it, yes, you can do it," but that may be too threatening because it may seem to permit no acceptable excuse for failure. This is a complex situation where we have to take that into account. One of the factors that I wanted to comment on and did not have time for in my paper was that maybe one of the releases for performance anxiety is the belief that the events are already occurring. And this may be how the Batcheldor approach works rather than or maybe in addition to suggestion. In other words, Batcheldor has always said "belief acts as a suggestion which makes the unconscious mind create PK." Maybe what is happening is that when the participants perceive the system is responding to them (and we had trouble in creating this perception because we did not make our systems at SRI look labile enough) at the point where they see the system going their way they then don't have to try anymore. They can afford to sit back and become more passive and less anxious about their performance. So I think that these are some of the complex factors which interact with the issue of self-handicapping, although I certainly take your point.

SCHLITZ: I think that our emphasis on experimental work in para-

psychology perhaps was a little over-stated in your observation that qualitative methods have their greatest impact in terms of providing experimental stimulation or ideas for hypotheses to be tested. In fact I think one of the beauties of qualitative methodologies is that they provide a means of mapping consciousness, both implicit and explicit, of psi experiences, whether that be objectively validated or not. I think also that it provides a means of getting at some of the social and cultural factors that lead to doing this kind of research and having these kinds of experiences in the first place. The second thing I would like to say is that I think we need to be careful about making generalizations based on some of these phenomenological insights that you gain through your qualitative analysis. One of the things that I think qualitative methods provide is a sensitivity to idiosyncrasies amongst people. In my own work I have been doing interviews with successful psi experimenters like William Braud, Helmut Schmidt, Charles Honorton. You find that the phenomenological detail that goes into their accounts of what makes a successful experiment is very different from one experimenter to the other. The issue of stress, in particular, came up because someone like Braud finds that it is absolutely necessary to preclude any kind of stress from the situation. Somebody like Honorton, on the other hand, just reported to me on an experiment he had done with Malcolm Bessent where he ended up with highly significant results in his experiments under circumstances where he was absolutely sure that he was not going to get any results at all. These expectations and the subjective qualitative detail is not always definitive of any nature of psi *per se*.

ISAACS: I hope that I prefaced my talk with the qualification that these were informal observations and therefore not necessarily generalizable. I certainly agree with your point.

SCHLITZ: Another thing about the qualitative method is that it helps us to identify our own biases and presuppositions. One of the things that comes through both in the comments earlier and in your presentation is that we are really emphasizing generalization, uniformities and law-like relationships about psi. It may be that we are forcing psi through a theoretical or methodological filter that has nothing to do with the actual properties of the phenomena themselves.

ISAACS: I think that what we are looking for are generalizations, but I do not expect to get instant generalizations that hold true for simple factors or simple sets of factors. If we had some very complex mathematical description of the factors affecting an experiment, we might find that elements of that factor set would generalize. But what we are doing now is much more primitive, less precise and less complex.

Therefore it would be unrealistic to expect currently known psychological factors to be simply generalizable. Subject populations differ and there are lots of other factors which are currently not trapped by the quantitative methods that we use. This is one of my big beefs about quantitative methods. What cannot be done is to produce a quantitative constrained description with enough complexity to get something which will be generalizable, especially if most factors as they seem to be now are rather weak. If you have a single really strong factor, maybe that might be more generalizable, but what I expect to find is a whole complex of weaker factors which together determine the outcome. It is messy and it is difficult and it is going to take a lot of time to do it because psi seems to be a phenomenon which is determined by very highly complex determinants and therefore we can not generalize simply.

NEPPE: Julian, I must say you have stimulated and disappointed me. You have stimulated me because I was thinking that the kind of environment that you have created with your enormous enthusiasm may be just the right kind of environment that you need for that special experiment that you were referring to beforehand that you wanted to do. But you have disappointed me because we agree and because of this we will have difficulty debating a certain issue! A lot of what you have said today I am going to talk about tomorrow from the point of view of trying to quantitate qualitative analysis. I think that what you emphasized from the point of view of psychological variables is something that is very often ignored in parapsychology. When I try and speak to non-parapsychologists about parapsychology they at times look askance implying what kind of science is this, a pseudo-science that people seem to be interested in? I have tried to explain to them that in fact parapsychology ought to be a prerequisite course in any form of psychological endeavor and possibly any form of scientific endeavor. It has formulated the essence of trying to eliminate areas for alternative experimental error. Yet with that formulation of essence, certain aspects have at times been ignored. These are the psychological factors; I think that you are emphasizing a lot of this today. You mentioned that at times subjects will state afterwards that they did not feel so good today or something had upset them beforehand and that is why they could not succeed. How often do experimenters really record that kind of result? It is something which we notice a lot in psychiatry. In fact I call it the "by-the-way syndrome." At the end of the interview, as the patient is going out the door, the patient says, "by the way, Doctor, you know I am having a problem with my husband." It is this that is the essence of what the patient has really wanted to talk about.

ISAACS: I wish I could develop a technique which would enable people to divulge that "by-the-way factor" right at the beginning of their PK session.

NEPPE: But then it is not a "by-the-way factor." If after the "by-the-way factor" you then did another session with the subject it would be very interesting to look at the results.

ISAACS: We thought of lengthening the sessions, but we were already at an hour and a half and if we were going to lengthen it up to two hours or so it would become impossibly cumbersome.

NEPPE: Well, maybe you ought to shorten the sessions to 25 minutes, wait for the "by-the-way factor" and then have another 25 minute session.

ISAACS: That is a helpful proposal, thank you.

MORRIS: One important area is the idea of coding the quantitatively rich aspects of our procedures more. I think that one of the most valuable things that we can and need to learn is a better way of expressing these aspects so that they will be registered in experimental literature more effectively. On more than one occasion I have had portions of my own papers truncated by editors for space reasons that would have contained that kind of material. Secondly, you noted performance anxiety as one factor that you regarded as very important. I think it is a fairly major factor, but it also is likely, perhaps, to be best understood in terms of what lies behind it. There are a couple of other factors that you have implicitly acknowledged, but have not commented on much, that might illustrate this. One that is being discussed in some psychological research over in Britain is the resistance to being learned about. I think this is very important especially as far as experimenters are concerned. They always had their own role perceived as the one that does the learning about rather than the one being learned about. There is a sort of one-sided power-sharing dynamic that oftentimes can go on there. Secondly, there is the phenomenon, drawn a lot from sports psychology, of the fear of success. The fear of success as discussed in the growing literature on it can sometimes be based on concerns that are very realistic. Some people fear success because it means they will have to be on the road and away from their families. They just do not want to do it any more. Other kinds of fear of success can have somewhat deeper, perhaps more psychoanalytically related roots. As they grow up one of their parents may conclude that there is only room for one expert in this family and that is yours truly, the parent himself, and anyone else should not even consider it. So they still have that baggage with them. Exploring what lies behind many of these surface concepts such as performance anxiety, the reluctance to being learned

about and fear of success will really take some digging. I think it is eminently worthwhile.

HARARY: I think it is perhaps most important to look at the fear of success on the part of the experimenters. I once wrote a paper called "Fear of Success in Psi Research." It is not the participants that I am worried about, it is the people doing the experiment. For example, you had a massive "row" with one of your experimental participants when she was doing well. I would not view that as being a spontaneous random event. I think that it probably has something to do with your personal experience of watching this person perform. You could probably give me all sorts of reasons for what was going on, but I think that there is an emotional resistance not only on the part of the designated participant, but more often on the part of the others involved in the experiment. It has certainly been my experience in the psi field, that when you really start to do something serious the people around you start to fear the perceived relative loss of their own power. They are afraid that you are taking it away from them because you are doing the very thing that is supposedly being studied. So in the field we have seen a kind of dichotomy. If you are the person being studied, you could not possibly know what is going on. So we can keep our power that way because I will study you. If you also know what is going on, if you also have a brain in your head and you are also showing some kind of actual psi functioning, then you are really a threat and you are apt to get into some really transparent arguments with some very insecure fellow researchers. If you understand the frame of reference of the participant, then the experimental situation should relate to that. If you require a subject to look calmly at an image on the computer, or whatever it is, then by definition you have structured your experiment to ask for a certain state of mind. It is not then at all fair to say that that state of mind is responsible and necessary for the functioning in general. Many people have made that mistake throughout the history of psi research.

ISAACS: You are quite right. In fact, we did it in the sense that, in some cases, when we had participants who encountered really hot emotional issues we found that they got signals while they were talking about the hot issues, rather like a poltergeist event. We would do the processing of the hot issues as the way of inducing PK. We used different types of induction strategies for different types of participant. This could be developed further. I agree with you that experimenters are resistant to psi. Remember that wonderful comment by D. J. West perhaps the most resistant psi investigator of all time. This is Professor Donald J. West who is ex-president of the British Society for Psychical

Research. He wrote that he spent years as the experimental officer of the British SPR chasing around after psi, but strangely whenever he found it he did not stay. He went off on the chase again. I also remember one of my early subjects who was producing colossal amounts of PK in training sessions six and seven, but in the two days before session eight she got drunk twice, had no sleep and therefore emerged in a totally wrecked state for her eighth training session and we got no PK whatsoever. I think that that was probably a symptom of resistance in response to our fear of "too much PK" happening in session eight. The row with the subject was over an issue which is very germane and interesting to American parapsychological research. I will tell you the inside scoop on that row. The subject said that she had a concern that our research might be used for military purposes. At the time I was considering applying to the Army for a grant to study PK, not for military purposes, but for doing pure research in PK. But it was to a military authority. When she threatened my ability to go to an agency for a grant I freaked and it was not resistance to psi. It was a reaction to the threat of having no money. I think that was the actual issue rather than resistance to her PK.