

DIRECTLY DETECTABLE PSYCHOKINETIC EFFECTS: A NEW CATEGORY OF PSYCHOKINESIS

JULIAN ISAACS

(i) Introduction

I want here to make a case for the existence of an hitherto ignored class of psychokinetic effects, which I shall term "directly detectable psychokinesis"—DDPK. A central feature of my thesis will be that this class of effect could serve as a valuable research tool with which to explore the physics of PK, and possibly that of psi generally. I start by asserting the importance of PK as an object of study. I then offer a critique of the random event generator (REG) as a PK target system. After providing the beginnings of a definition of DDPK effects and outlining some of their properties and useful characteristics as research tools, I shall cite some examples of DDPK effects which have already been reported. I will then very briefly refer to some of the research I have conducted with one particular DDPK effect and outline what I regard as being some crucial psychological requirements applying to successful DDPK induction strategies. Finally, I shall briefly speculate on some possible applications of DDPK effects and their potential social and conceptual consequences, which could directly impact our notions regarding human nature.

(ii) PK and the Physics of Psi

Mankind's ability to manifest events directly in the world of matter by psychokinetic means must surely be considered one of the most surprising aspects of human nature. Yet the existence of PK as a human ability is of extreme significance because it may provide a clue to a deeper understanding of the structure of the world. Psychokinesis must also be of importance to physics because PK's existence demonstrates that physics is seriously incomplete in some way. It is of importance to psychology because its existence demonstrates a human capacity which may ultimately cause us to radically re-assess our conceptions of human

identity. It is of importance to parapsychology both for its own sake as a psi phenomenon and also because the study of psychokinesis may prove to be the royal road to discovering the physics of psi—if such there be. It is with this latter project that I am principally concerned.

Psi is not accepted by mainstream science for many reasons. Some of these are of sociological interest, for example, those having to do with the negative connotations given to psi by our culture, its chthonic associations and its sensational treatment by the media. However, it is the barriers to scientific acceptance which concern me here. Chief amongst these are (1) the bizarre and counterintuitive nature of psi effects, (2) the difficulty or seeming impossibility of their being provided “on demand” for observation by skeptical witnesses, (3) the relatively modest level of replicability of most psi effects in parapsychological experimentation and, finally, (4) the seeming incoherence and meaninglessness of the psi phenomenology when presented to the lay person (and the parapsychologically lay scientist) in the absence of a satisfactory theory of psi’s genesis and mechanisms of operation. If there existed a coherent and well constrained theory of the physics of psi, this would provide a much needed theoretical underpinning for the whole parapsychological enterprise, as well as greatly assisting its acceptance by the scientific community. However, it should be born in mind that such a theory may be impossible to achieve.

It could be, as some parapsychologists have argued, that psi phenomena are simply lawless, on a physical level, or originate from such a radically different realm from the physical that all attempts to develop a physical theory will founder (Beloff, 1980). Perhaps psi is a product of mind (Isaacs, 1977)—perhaps even Mind at Large—which may simply over-ride all lawful regularities, all physical mechanisms, creating events in the physical world by fiat, so to speak, so that the search for regularities will fail in the face of an infinite variability and absence of dependable lawfulness.

This scenario could be reinforced by an obliging ability on the part of PK agents to unknowingly create PK events which serve to validate their experimenter’s pet hypotheses regarding the physical mechanisms mediating PK effects, even if these hypotheses only frame some of the truth. In which case, each experimenter will point to his own database in confirmation of his notion of PK mechanisms, but these will only be a subset of those physically permitted and will not exhaust the possibilities available for realization by other experimenters and their subjects. Now this possibility is, in my view, a real one, and even if things prove not to be that bad, limits to our ability to create a satisfactory

theory of the physics of psi seem likely to be imposed by the intrinsic nature of psi.

The case I wish to argue here, though, is that our attempts to gather data pertinent to the physics of psi have not really properly started yet, because of the limitations imposed by our attending to only one source of data and our failure to consistently develop a methodology appropriate to unraveling the physics of psi. I wish now to briefly examine the origin of this blindness, which is largely historical.

(iii) A Critique of the REG as PK Target System

When J. B. Rhine turned away in disgust from the intricacies and deceptions of the "Margery" affair (Tieze, 1973) and rejected forever the investigation of "physical phenomena" conducted in the dubious darkness of the seance room in favor of the use of dice for PK investigations conducted in well-lighted laboratories, he created a tradition of research which largely ousted other approaches to the study of psychokinesis and which is still dominant today.

It is indisputable that this line of research has been impressively fruitful and it should be carefully noted that I am not in any sense arguing that the research following this paradigm should be stopped, limited, or rejected in any way. My concern is not to attempt to destroy one research methodology, but to plead for the development of another and hitherto unrecognized one, a rather similar form of research methodology, but one which uses a different class of PK target systems and has a more positive attitude towards PK training than that which is evinced by those using the REG systems which are so dominant in PK research today.

Rhine's research paradigm, reconstructed as an action program, is to select some convenient-to-use randomly behaving system. The target systems have been various—dice, counters, coins, water drops and, currently, the electronic random event generator (Rhine, 1970). Having designated a target system, its behavior must be checked for randomness. If adequately random, the system is then exposed to a subject whose task will be to bias its behavior away from randomness. Evaluation of the subject's effect is then performed statistically.

This approach has much to recommend it. Today's electronic REG is unaffected by environmental factors and it can easily be interfaced with a computer which can run the experimental trials with great precision and reliability and also perform the subsequent data analysis (McCarthy, 1982). REG results are easy to validate, since good statistical checks of randomness exist, and the instrumentation can be locked

away from subjects. As a result, REG research is widely accepted and has high status within parapsychology. Consequently, the REG-based PK target system has become the workhorse of modern PK research. However, the ease of use and convenience of the REG for PK studies may have led parapsychologists to ignore certain less desirable characteristics of this target system which constitute very serious drawbacks if the aim is to uncover the physics of PK.

One type of limitation derives from an intrinsic property of the REG as a randomly active system. It is systematically impossible to know whether a given hit is due to chance or due to PK. This is an unavoidable consequence of the random nature of the REG's output. In turn, this property makes it impossible for subjects to associate a given state with REG success, except in a gross overall manner as a sort of mass effect over many trials. My interpretation of learning theory (Melton, 1964) would therefore predict that subjects would not show improvement in REG scoring rates (which are usually very low, down around the three percent level for most REG data (Tart, 1983)) with practice. The REG literature as a whole has not yet been searched specifically for inclines across repeated trial sessions, but some data exist which show long-term trends in subjects' scores (Dunne et al., 1983). These data seem to indicate that, at best, subjects show steady scoring, rather than inclines in scoring with increasing trials number. Certainly, no one has yet reported classic learning-related inclines in performance as a result of long continued practice at the REG task.

The implication of this state of affairs is that the REG seems condemned to produce "small psi" effects. We must not expect that REG biases induced by PK will ever be more than a small percentile shift. This makes REG-based systems unusable as PK detectors in systems exploiting the possible applications of PK for controlling instrumentation by "remote action." Ironically, in the face of parapsychologists' sometimes apparently hostile responses to the purported "big psi" of macroscopic PK, the REG's "small psi" property may be a blessing in disguise and Tart, in particular, has surmised that, as a group, parapsychologists may be covertly resistant to large psi effects, (Tart, 1984) so that the REG results' conformance to the "religion of the .05 probability level," may count in its favor. Be that as it may, the situation with the REG as PK target seems to be one where we should not ever expect to obtain an impressed PK "signal" which is more than a small fraction of the device's random "noise," and this seems intrinsic and final. If "big psi" (in this context, instrumentally detected PK events having good signal to noise ratio) exists in the PK field, it must lie somewhere else than within the realm of REG-based research.

But worse than this is the inscrutable nature of the REG as a PK target system. The original conception which motivated Schmidt to construct his first, radioactivity-based REG (Schmidt, 1970) was the notion of using one of nature's own random systems, the quantum reaction involved in radioactive breakdown, which is inherently unpredictable and probabilistic in behavior, as the source of randomness for the device. Presumably, even the magician Randi would not allege that the radioactive atoms had been bribed or gimmicked so as to perform to order.

But does the effect really originate at the quantum level? Here a besetting problem common to all instrumented PK detection systems is encountered, because unless the PK detection system is monitored in operation, there are limitations to the precision with which the point of entry of PK into the system can be specified. The imprecision is dependent on the particular devices used, but with the REG this ambiguity is very large. How do we know that the PK does not affect the semiconductor material in the integrated circuits in the device, or create temporary conductive paths between electronic components, or directly ionize the gas in the Geiger counter tube, or create electromagnetic impulses near critical electronic components? Any of these mechanisms could be responsible for the results reported to date. And even if we monitor the device in operation, how do we know that PK does not cleverly sneak around the monitoring, or even play fast and loose with the monitoring system to make things appear as they are not?

These questions expose some interesting and frequently inexplicit assumptions made by PK researchers. Schmidt's approach is to regard the REG system as a "black box" (Schmidt, 1975). We need know nothing about events inside the black box. As long as it behaves randomly in the absence of PK, its behavior can be sufficiently described by specifying the changes in the probabilities of its outputs when affected by PK. However, there is another assumption made by many of us in conceptualizing PK (Braud, 1979), which is that PK preferentially affects systems which are random or indeterminate in behavior, since this fits into a commonly held but often inexplicit belief that PK obeys a law of least action—the part of the system affected is always that which is easiest to influence, and indeterminate systems are seen as easier to influence than determinate systems. But, additionally, in the case of the REG, the assumption is made that PK does not add any energy to the system, but alters the system's likelihood of achieving a given state (Millar, 1978). This is the conceptualization of PK being fundamentally a dynamic which biases systems into behaving in quantumly permitted, but lower probability modes than normally they would.

I have no objection to these assumptions, which I regard as reasonable, but the problem posed by the black box approach to REG PK is that this never allows us to open the box and ask how the PK effect is mediated physically, what physical mechanisms are involved, or any one of a number of pressing and highly important questions. One is left with the REG-black box inscrutably altering its behavior as a result of human influence without being able to ask the incisive question on a physical level—"how?"

Now, lest I am accused of being naive with respect to quantum theory, of course this type of non-answer to the fundamental question concerning quantum reactions is an absolute in all of quantum physics, so that the REG is not especially sinful in this regard. But when one looks at other areas of physics where systems are used as detectors of physical processes, much richer data are collected by the monitoring systems, which often monitor for a range of physical parameters which together allow inferences to be made regarding the underlying processes involved in the energetic reaction under investigation. I am emphatically not suggesting here that PK is some simple physical force, like electromagnetism, for example. But the types of question which can be asked regarding the physical properties of PK when the REG is used as the sole PK detection system are very limited. This is not to assert that any of the questions which have been experimentally addressed—the psychological factors affecting PK performance, the effects of distance on PK, the effects of multiple observers, whether retroactive PK exists, etc.—are not very important, since clearly they are (Stanford, 1977). But REG-based research cannot produce data on the effects of PK on materials, or data concerning the paranormal production of force, electrical effects, magnetic effects, or chemical effects etc., and this source of data could be very fruitful, as I hope will become apparent in sections (viii) and (ix).

The final problem associated with the REG is now a topic of considerable controversy. This controversy is very complex, has its roots in the very beginning of Rhine's work with PK, but has been given new life by some recent creative conceptualizations. The challenge is (May et al., 1986) that all the evidence for PK deriving from REG-type target systems (whether electronic or mechanical) can be explained as really resulting from a covert form of ESP, rather than being PK. The "intuitive data sorting hypothesis" (IDS hypothesis) has a lineage in the form of the "precognition vs. PK" ambiguity regarding the interpretation of REG results. But the IDS theorists seriously assert that all the data for the existence of PK from randomly acting systems should be disregarded, since they can be explained as having been generated by

the ESP mediated selection, by experimenters or subjects, or both, of appropriately biased subsections of target strings which in total show a random distribution. On this view, no action of any sort has been exerted on the REG system; the apparent effects are due entirely to the selection process which partitions the data between experimental and control conditions. This controversy is in a very early stage, since experimental tests of the IDS hypothesis have only just started to be executed. However, it is certainly possible that the IDS hypothesis may be proved true. If this occurs, parapsychology will be left with no well developed laboratory system for detecting true PK. To what alternative system, then, could we turn?

(iv) The Vicissitudes of Macro PK

It is here that the obverse side of the REG's dominance now becomes apparent, since, largely because of neglect and lack of professional interest, so-called macro PK methodologies are in their infancy and macro PK has a generally low status within professional parapsychology. A sort of academic Catch-22 situation applies to macro-PK research, where the absence of credibility of macro PK and its methodological under-development deter investigators from investing their limited resources in this area, which prevents the development of adequate methodologies, which deters investigators from researching the area . . . and so on.

It has become the orthodoxy to either totally ignore, or else to regard the 19th and early 20th Century reports of seance room PK as being amusing, but obviously deluded accounts of ingenuous responses to successful deception by the mediums involved. The relatively recent spate of interest in paranormal metal bending has subsided and the general consensus seems to be that metal bending too, has not been proven real, and the dubious reported behavior of Geller has further besmirched this topic. A well known parapsychologist confided a view to me which I think typical of most professional parapsychologists regarding macro-PK, that "the larger the reported PK effect, the less likely is it to be real."

The reasons for this extreme aversion and avoidance of macro PK are not difficult to identify. First, it is very difficult to find individuals who can produce the effects. The effects are very easily inhibited and, to be sure, there has been a widespread and persistent history of fraud associated with macro PK. Probably worst of all, macro-PK methodology is still very primitive, in most cases being still directed to simply proving that the effects are not fraudulent or artifactual. With present

macro-PK methodologies one cannot perform elegant and easily executed automatic testing of PK, nor, as a result, easily design studies using macro-PK tasks which investigate the psychology of PK.

(v) *DDPK: Micro-Macro PK?*

It is at this seeming impasse that I want to point to the existence of a class of PK effects which shares some of the desirable characteristics of macro PK from the point of view of physics-based studies, while being potentially as well behaved, accessible to validation and convenient to use as current REG PK. The basic idea is very simple and I shall give only the bare outline here.

It should be noted that systems fitting the DDPK description have quite frequently been used on a piecemeal basis within PK research (e.g., Schmeidler, 1973; Puthoff and Targ, 1979; Jarrard et al., 1976). But they have been utilized without their potentials being realized. Usually, the ingrained mindset of the Rhinean approach has led to the systems being used with unselected beginning subjects (rather than carefully selected and trained individuals) in a proof-oriented design where the effects were so weak as to require statistical analysis to demonstrate their existence. Section (viii) giving examples of studies employing DDPK systems could be expanded to include more studies, and it is not true that parapsychologists have shown no interest in such systems, for example, see the symposium on PK with stable systems in the 1973 PA Convention (Roll et al., 1974).

However, the essential supplementary methodological requirements of (1), careful subject selection, which demands the development of DDPK subject screening procedures (Isaacs, 1981), (2), subject training and (3) the crucial role of feedback (see section [vi]) appear not to have been appreciated by the experimenters. It is my contention that, as a result, the effects obtained using these systems have not been nearly as powerful as they might have been and, as a consequence, the use of these systems has been dropped, often because of a seeming lack of suitable subjects, which in turn led to a lack of repeatable results (parapsychologists are nothing if not highly pragmatic in this regard!) If it is not realized that performance improves with practice, the number of suitable subjects available is very much decreased compared to the potential pool which is available if one is prepared to increase the magnitude and reliability of effects through subject training. Nevertheless, some experimenters have still been surprised by the effect size they encountered (e.g., Wells and Watkins, 1975), and others have noted apparent improvement with practice (Puthoff and Targ, 1978).

The fundamental concept could be likened to a biofeedback task where the subject affects some measured parameter, except that the system affected lies outside the body of the subject—a sort of exsomatic biofeedback. One uses as PK target some system which is sensitive to small energy inputs or redistributions, such as, say, a piezoelectric crystal strip. It should be noted that I am not claiming that the piezo-PK effect is an ideal or even especially good DDPK task. The optimum DDPK task remains to be discovered. I suggest that many other types of system could potentially be used, since I suspect that a great diversity of systems having the properties listed below will be found to be accessible to PK influence. PK seems capable of coping with complex tasks (Kennedy, 1978), so that complexity of the target system presumably would not constitute a barrier.

In the case of the example presently offered, the crystal strip is mounted in a quiet environment, the electrical output of the crystal (which responds to small forces imposed on it) is recorded and, for the PK agent whose task is to influence it, the crystal's output is converted into some perceptible signal, say an audio tone. The PK agent's task is to influence the strip so that the feedback system is activated.

Ideally, the quietness of the strip's surroundings ensures that no signals above a certain threshold are recorded in the absence of PK. If the environment is not this quiet, then either a twin-sensor system is used in anti-coincidence mode, where one crystal strip acts as the control channel for the other strip, both strips being housed in a common electromagnetically screened environment, the whole system having known noise rejection characteristics, or else an environmental monitoring system is used. The purpose of both techniques is to exclude the acceptance of artifactual signals in the presence of environmental interference. Subject fraud is prevented by locating the target system remote from the subject, or if the subject has not yet reached that stage of performance, the target system is located in the same room, but is protected by shielding and distance from the subject and the subject is witnessed while performing.

Intrinsic to this suggested methodology is the hypothesis that repeated practice by suitably gifted subjects will enhance the magnitude, reliability and controllability of their effects. Much of the conceptual framework for this type of training has already been established within the psychological literature on biofeedback (Shapiro and Surwit, 1976; Schwartz and Shapiro, 1976), operant conditioning (Ferster, Culbertson and Boren, 1975) and behavior modification (Gambrill, 1978). Tart has also commented on PK training (Tart, 1983).

The piezo-PK example is based on the systems I have used, both

here and in England (Isaacs, 1984), but my intent is to direct attention to the general properties of directly detectable PK. An attempt at a preliminary definition of DDPK is embodied in the three primary defining characteristics of DDPK effects listed below:

(1) DDPK effects are paranormally caused perturbations of stable physical systems. The systems should exhibit low levels of spontaneous activity and low noise in the absence of PK inputs.

(2) DDPK target systems must be sufficiently sensitive to be responsive to small inputs of energy or small redistributions of the resting state energy of the DDPK target system.

(3) DDPK target assemblies must be instrumentally monitorable by electronic systems. The monitoring system must supply a signal to the PK agent which provides instantaneous feedback indicating periods of success in affecting the target system. The feedback sensitivity must be sufficient to allow the system noise floor to be perceptible to the PK agent.

Examining these defining characteristics, the first, (1), is the property of low spontaneous activity—low “noisiness” which gives rise to the “directly detectable” nomenclature. A principal feature of the class I seek to delimit is that in the absence of PK the systems remain quiescent. Without a PK input, the DDPK system should simply sit and do nothing (it will of course, as any system does, produce its intrinsic noise, which I deal with below). In probability terms this implies that in the absence of PK, the systems’ probability of emitting a response comparable to that emitted when affected by PK should be either zero or vanishingly small. The choice of interpretation of what probability level to equate to “vanishingly small” is clearly arbitrary.

In DDPK systems, PK can be observed to occur without the need for statistical analysis. This view is tantamount to heresy within the Rhinean paradigm, since a fundamental commitment is made in that tradition to statistical analysis as a means of legitimating claims to paranormality. The Rhinean approach represents an acceptance of poor signal to noise ratios in PK detection systems which I argue do not have to be accepted. It should also be noted that many types of measurements in the conventional physical sciences are certain enough not to require the support of statistical analysis.

The quiescence requirement also removes DDPK systems from being subject to the IDS hypothesis. If no naturally occurring spontaneous events of the order of magnitude of the recorded DDPK events are observed, there can be no experimenter or subject initiated choices (whether ESP mediated or not) to take advantage of such natural fluc-

tuations as are hypothesized to explain the REG data. The hypothesis that PK effects can be obtained on quiescent systems also seemingly contradicts some current assumptions regarding the nature of PK—such as the favoring of indeterminate systems as PK detectors.

Clearly, the use of the concept of signal to noise ratio in (1) implies that to be counted as DDPK effects, the magnitude of paranormally caused perturbations must be several times that of the system's intrinsic noise. In my piezo-PK studies I defined a threshold of five times the peak to peak intrinsic noise level of the piezo detection system to be the magnitude at which perturbations would be considered as candidate DDPK. Candidate PK events are admitted into the PK dataset if, simultaneously with the candidate PK events, there are no acoustic noise and vibration signals above baseline and if there are no mains fluctuations above baseline, using acoustic/vibration and mains monitoring systems.

The largest signal recorded in the English work was 172 times the peak to peak noise of the system when quiescent, but the bulk of the signals lay between about 10 times and 40 times the noise level (Isaacs, 1984). The specification of a signal to noise ratio as the acceptance threshold for potential data is obviously arbitrary, but should in practice be guided by the probability of the obtaining of such levels of output in the absence of PK.

The second definitional requirement, (2), is what sets DDPK apart from macro PK, although it is clear from discussion of the DDPK concept with colleagues that most parapsychologists will unthinkingly equate DDPK with macro PK. This is a mistake, however, since several important characteristics set DDPK apart from macro PK. The "low energy requirement" is extremely important in practice, because it seems likely that the potential population of PK agents who will be capable of successfully exerting effects at these low energy levels will be much larger than the population who can produce true macro effects. In screening US groups for potential piezo-PK ability, it seems that about five percent of selected groups (i.e., groups with an existing interest or familiarity with psi) may show potential for piezo-PK agency, a figure which is comparable with the results of the English screenings (Isaacs, 1981).

It should be noted that in (2) I hedge my bets by defining the energy requirements either in terms of energy input or in terms of redistribution of resting state energy. This feature of the definition was included because it seems possible that PK may act as an informational source—reorganizing systems away from their resting state by redis-

tributing their intrinsic energy—rather than acting as a net source of energy, and the definition has deliberately been left open in this regard.

(vi) The Hypothesized Role of Feedback in DDPK

Statement (2) in practice implies statement (3), since in general, systems showing small fluctuations cannot generate feedback or recorded outputs without being monitored electronically. Although I cannot cite experimental studies which directly support the analysis of the role of feedback in DDPK I am about to present, since these have not yet been performed, it appears that feedback plays an absolutely crucial role in eliciting DDPK responses from potential DDPK agents in training. The requirement regarding the properties of DDPK feedback systems must, in my view, be understood and fulfilled in practice, in terms of the DDPK instrumentation's feedback properties, before DDPK training attempts can stand any chance of success.

The phenomenology of DDPK training appears closely to conform to the principles of operant conditioning where a behavior which is an approximation to the required behavior must be emitted first and then reinforced, before further, more optimal behaviors, are then emitted and can be reinforced. In practice this leads to what at first sight appears to be a paradox, that subjects should essentially be presented with a "noisy" system, but the rationale for this relates closely, in my view, to the reason why it seems relatively easy to obtain PK on REG types of systems.

If we ask the question of what behaviors should be reinforced in order to start the process of operant conditioning in the PK training process, clearly the answer must be PK behaviors, or at least PK-like behaviors. Typically, a DDPK agent may at first produce only relatively few PK events, or even none, over the threshold which defines events as candidate DDPK. In practice the threshold is set high enough for some under-threshold events to be likely to be PK rather than noise or artifact (Isaacs, 1984). But if feedback is only supplied for the over-threshold events, the operant conditioning process will be very slow and a risk is generated that due to lack of frequency of reinforcement, motivation may fail and the PK response may even be extinguished. It thus becomes necessary to provide a feedback signal which is sufficiently sensitive to the state of the system to be able to relay the intrinsic noise of the system in perceptible form to the DDPK agent in training. This will ensure that, however slight the trainee's PK responses may be, reinforcement will occur.

Usually, what happens in DDPK training is that the agent's signals

start out by mostly being in the range of the noise of the system, but over the course of several DDPK training sessions (typically six sessions in the English studies [Isaacs, 1984]), with successful subjects, the signal magnitudes climb out of the noise until many over-threshold events occur in each session. This kind of phenomenology is very compatible with the operant conditioning paradigm, and the crucial point to appreciate, from that perspective, is that in order for the subject to emit a PK-like behavior, the feedback system must be sensitive enough to be able to signal perturbations which are at, or very near, the intrinsic noise level of the DDPK detection system.

The psychology of DDPK training can be examined at different levels of discourse, using different models, since there are other non-paranormal learning models which can be applied as analogs to DDPK training in addition to operant conditioning. So far, I have used the language of operant conditioning, but a more cognitive approach is possible. Batchelder's formulation falls into this category (Batchelder, 1984). His principal hypothesis is that PK is elicited by a particular type of situationally triggered "instant" belief state which is induced by the subject being exposed to a stimulus which he interprets as indicating that PK is already occurring. This is the rationale for Batchelder's "artifact induction (of PK) hypothesis." The REG by its very nature supplies artifactual hits, since by pure chance the REG frequently generates its hit state spontaneously. The REG can therefore be seen as a device which systematically invites subjects to construe (and also misconstrue—it can be an "artifact induction" device) themselves as having been successful in creating PK events. It very clearly fulfills Batchelder's hypothesized requirement for PK induction.

From this perspective it seems to have been a remarkably fortunate accident that such systems were chosen for parapsychological use. But of course this is to ignore the fact that this type of system was in fact introduced by a user who had noticed that he was influencing it—the young gambler who came to Rhine and insisted that when "hot" he could affect the fall of dice (Rhine, 1970)!

From this cognitive perspective, relaying the noise from a DDPK detection system allows the PK trainee to construe (or misconstrue) the system's fluctuations as being due to his or her influence, analogous to the situation I have hypothesized as occurring with the REG. This then may create the "instant" PK-producing belief state, which creates more reinforcing feedback, and so on. Diana Robinson has discussed the possibly psi facilitating role that a subject's felt sense of control could have in maintaining high levels of motivation in the absence of high anxiety or high striving (Robinson, 1981). In support of this anal-

ysis, it should be mentioned that an apparently universal preference shown by DDPK trainees is that they must, through experience of the feedback signal, feel "in touch" with the piezo crystal itself, which is interpretable as implying that the feedback system should be responsive to the slightest alteration in state of the piezo sensor.

This analysis of the psychological role of feedback would also tend to explain why full scale macro PK is so difficult to produce, because apart from the possible physics based difficulty in summoning enough PK action to accomplish a gross object movement, if no encouraging small movements occur, the process of creation of a positive belief state cannot start its upward climb.

In terms of DDPK methodology the lessons from this analysis seem clear. The feedback system must be capable of wide excursions from the instrumental "noise floor" up to several magnitudes above noise, yet still provide sensorially discriminable and aesthetically pleasing results. Presumably too, for motivational optimization, the feedback modality should fit the preferences and cognitive style of the PK trainee, although this too remains unverified by experimentation.

(vii) Learning and Distance Hypotheses

We now come to two subsidiary, but important hypothetical characteristics of DDPK target systems which are listed below. These are hypothesized properties, rather than defining properties, and their truth will depend upon the results of experimental studies, but for the sake of creating a clear overview of the conceptualizations I bring to DDPK they are included here.

(4) If immediate real-time feedback from the PK detection system noise floor is given to the PK agents attempting to influence DDPK target systems, some of the PK agents will be able to utilize the feedback information to identify internal states and cues which will enable them, with continued practice, to improve their success rate in influencing the DDPK target system.

My doctoral research in England was directed to evaluating whether or not piezo-PK constituted a learnable PK task (Isaacs, 1984). The three longitudinal piezo-PK training studies conducted as part of that research produced data very strongly suggestive that suitably gifted individuals can learn to increase the size and frequency of their effects. However, small subject numbers were utilized, and of the total of 16 subjects who participated in the three studies, only three succeeded in producing learning curves comparable with those encountered in other

areas of psychology. In the extensive pilot studies of piezo-PK training conducted in America by myself and my group, US PK agents appear, if anything, to be more successful than the British ones in improving their piezo-PK outputs with practice.

My conclusion is that piezo-PK is definitely a PK task at which performance improves with practice. However, I suspect that two extra components of this research must be established before it would be reasonable to expect the parapsychological community to accept this conclusion. The first, upon which I am currently engaged, but at present do not want to comment, is to prove that the piezo-PK effect is a real PK effect and that my results are not due to fraud or artifact. The second is that independent replications of the studies demonstrating learning with practice should be performed by other groups. I have hesitated to publish my findings except for a brief paper describing my first longitudinal study (Isaacs, 1983), because, given the current attitude of the parapsychological community towards any other forms of PK than REG PK, and given my claims relative to the piezo-PK task being learnable, my news is too good for most parapsychologists to believe. My intention is therefore to publish more freely once I have completed the studies designed to show that the effect truly does exist, using a state of the art piezo-PK detection system and independent witnessing of trials.

The fifth statement, below, is in essence a manifesto for the type of research I see as being opened up by the development of a DDPK methodology. Increasing interest is being shown by the parapsychological community in long distance PK (Tedder and Braud, 1981).

(5) PK is capable of affecting systems located at long distances from the PK agent. It is hypothesized that DDPK systems could be set up in physics laboratories and successfully influenced by PK agents remote from the target site, if real-time immediate feedback is provided to the PK agents. Limitations on creation of DDPK effects over distance are due to psychological effects and are not due to intrinsic physical limitations on PK.

If DDPK effects can be created at long distances from the individual producing them, this opens the way to what may be a very convenient experimental format, where PK agents are able to focus their effects on a variety of detection systems set up in physics laboratories without having to travel in person to the laboratory site. This format would protect against problems of fraud, and may also prove to be the answer to the too frequent inhibition of PK agents by the psychologically less than optimal (for psi purposes) environments of many physics labora-

tories. The PK agents could operate from their favored environment, yet the effects could be produced in the most optimal technical setting for measurement purposes. Feedback could be supplied via the telephone system, so that the sense of contact between PK agent and target system could still be maintained.

(viii) *Examples of Some DDPK Effects*

I present below a number of examples of possible DDPK effects, grouped roughly into five classes. This is not meant to be an exhaustive or definitive review of previously reported DDPK effects, which still remains to be done. It should be emphasized that I am not claiming that any of the effects listed below have been definitively validated as being real. This is not my point; rather, what is offered here should be regarded as being a glimpse of a broad and largely unexplored territory which invites careful and innovative exploration.

Studies Using Light Beams etc. The French researcher Osty (Osty and Osty, 1931) reported the apparent absorption of a beam of infrared light passing through air, occurring ostensibly as a result of the PK action of the medium Rudi Schneider. The English parapsychologist Anita Gregory (Gregory, 1982, 1983) reported an apparently successful replication of this effect, which occurred in the presence of several independent witnesses. The PK agent for this study was Matthew Manning. Stanford (Stanford and Fox, 1975) reported the change in output of a photocell exposed in air to a light source, ostensibly occurring as a result of the release of effort of a PK subject.

Benson Herbert (Herbert, 1963) reported that the English PK agent Suzanne Padfield was seemingly able to cause a decrease in beam strength of a beam of polarized visible light which was passed through a trough of water. Herbert interpreted this effect as being mediated by the creation of turbulence in the water, which apparently deflected the polarized light beam. Adamenko reported that a Soviet PK agent seemingly rotated the plane of polarization of a light beam by some five degrees, while a nearby control polarimeter showed no such effect (Adamenko, 1979). Grad and Dean (Grad, 1967; Dean 1983; Grad and Dean, 1984), and Schwartz and DeMattei (Schwartz and DeMattei, 1986) have reported changes in the infrared absorption spectrum of water samples which had been treated by healers. The hypothesized mechanism is the temporary creation of structural change in the water molecules.

Electrical Conductivity Effects. Many reports claiming that physical mediums could discharge charged electrosopes (voltage indicating ca-

capacitors) have been noted in the mediumship literature (Sudre, 1960). Some of these events seemed to have occurred under good conditions of witnessing. An essentially identical observation was made by Wells and Watkins (Wells and Watkins, 1975) who found a subject who could consistently decrease the discharge time of a charged electrical capacitor by some 10 percent. The English researcher Brookes-Smith reported (Brookes-Smith, 1975) ostensibly paranormal changes in electrical conductivity between copper strips plated onto a high resistance plastic sheet which was mounted underneath a table used for levitation in a sitter group. The effects seemed to occur in association with the ostensibly paranormal levitation of the table. Hasted (Hasted, 1981; Hasted and Robinson, 1981 a and b) has reported a variety of ostensibly paranormal electrical conductivity effects, including the creation of ostensible air ionization and conductive paths through the air between the PK agent and an electrically monitored target, occurring under witnessed conditions. Adamenko reported that the Soviet ostensible PK agent Alla Vinogradova could seemingly induce electrical charges on objects (Adamenko, 1979), although it is presently unclear whether this effect was paranormal or an extension of some normal ability.

Thermal Effects. Schmeidler (Schmeidler, 1973; Schmeidler et al., 1975, 1976) has reported PK effects occurring to thermistors sealed into vacuum flasks. The first study was criticized by Millar, who performed an unsuccessful replication (Millar, 1976). A similar study was performed by a group under the direction of Robert Morris (Placer et al., 1976). Mattuck (Mattuck, 1977) reported the ostensibly paranormal influencing of a thermometer by a Danish "mini-Geller" girl. Adamenko reported that a Soviet ostensible PK agent could change the apparent temperature of temperature-indicating liquid crystal displays (Adamenko, 1979). The same effect was reported by the French physicist Wolkowski (Wolkowski, 1977).

This work, especially Schmeidler's and the Morris group's, illustrates the need for careful selection and training of PK agents, since it appeared that certain subjects could create the effects, but many could not, so that their methodology, which was to use unselected groups of subjects, gave inconsistent results.

Microstructural Changes in Materials. The metal-bending literature (Hasted, 1981) includes a large number of reports of microstructural change occurring seemingly as a result of exposure of the target specimen to the attentions of ostensible metal-benders. Changes in hardness (Hasted, 1981; Crussard and Bouvaist, 1978; Sasaki et al., 1979) have been reported, even in undeformed specimens (Crussard and Bouvaist, op. cit.) as well as phase changes in steel (changes in crystalline structure)

(Crussard and Bouvaist, op. cit.) and brass (Hasted, 1981). The creation of loop dislocations, which normally occur only after radioactive irradiation (Crussard and Bouvaist, op. cit.) has also been reported.

Hawke (Hawke, 1976) reported changes in the magnetic pattern of the iron oxide of a Hewlett Packard programable calculator card occurring ostensibly as a result of exposure to Uri Geller. He interpreted this effect as being due to microstructural change induced by Geller in the oxide layer.

Dynamic Strains. Several reports have been published of dynamic strains being induced in instrumented specimens by ostensibly paranormal means. Much of this literature derives from the investigation of metal-bending and has used resistive film strain gauges as the sensing elements (Bersani and Martelli, 1983, Hasted, 1981). Strain gauges have also been used to detect ostensibly psi-created strains in a seemingly different context than studies of PK (Osis and McCormick, 1980). Piezoelectric strain sensitive elements have also been used (Hasted, 1983; Isaacs, 1983, 1984).

Hasted's work is by far the largest in volume in this area, but has received a generally negative reception (e.g., Stokes, 1982) on several counts. He has usually included insufficient detail in his published accounts regarding the control and the witnessing of subjects and of the anti-fraud precautions taken. Despite his use of a "dummy" control channel to monitor for electromagnetic artifacts, his results have also been interpreted as being due to the ingress of electromagnetic transients into the interference-sensitive strain gauges (Targ and Puthoff, 1982), and concern has been expressed regarding the possibly confounding role of creep in the strain gauge-substrate bonds due to possible overheating of the strain gauge by excessive bridge currents (May, 1985). His mathematical analyses for a few of the reported effects have also been criticized, together with doubt being expressed regarding the linearity of response of the strain gauge systems he used (Wood, 1982).

I have had the opportunity of personally examining Hasted's practices and experimental procedures since 1977 and evaluate his data as certainly not being due to fraud, since the security conditions were in general much better than he has communicated. Hasted did the last several studies within an electromagnetically shielded room, which did effectively prevent the ingress of transients, and the data collected under the screened condition were essentially identical to those gathered prior to the change.

I would cautiously endorse the claim that he was probably observing effects which were psychokinetic in origin. One serious reservation

concerns the data collected from these events if they were of short (<100 mS) duration, since the chart recorders he used would severely attenuate events of shorter duration, leading to great underestimation of the magnitudes of brief events. Additionally, he tended to ignore the confounding effect of psychological variables on the phenomena, so that the theoretical concepts, such as his "surface of action" construct, should be regarded very cautiously (Isaacs, 1984). Nevertheless, Hasted's work stands as a brave example of work which, if repeated and extended under careful conditions of control, could lead to a very fruitful expansion of the physics/PK research area.

(ix) DDPK as a Research Tool

The examples given above can serve to illustrate some of the avenues of inquiry opened by DDPK effects. Here, I will cite a small sample of the potentially fruitful questions they raise. In the case of the infrared occultation phenomenon, what is the identity and nature of the substance which decreases the beam strength; is it a gas, a plasma? Investigation of this question might, for example, include the taking of an infrared absorption spectrum of the occluding material, if such there be. Infrared absorption spectra give valuable information regarding the chemical composition and physical state of the absorbing material. Control measures would, of course, need to be taken to ensure that it was indeed an absorption effect involved, rather than, say, a direct electronic effect on the apparatus. Low intensity infrared sensitive video recordings might be useful in this context. The same type of absorption spectrographic techniques could be used in the case of the visible light absorption in air.

The occultation effect reported for a polarized light beam passed through water could be investigated using schlieren techniques which render water currents visible and accessible to photographic recording, or laser doppler techniques could be used if small particles were put into the water as markers for the currents.

The mechanisms mediating the electrical conduction effects raise some interesting questions. Are the phenomena caused by ionization of air? Conventional techniques for investigating gaseous ion physics could be applied.

The paranormal electrical conductance effect is an important phenomenon to investigate because it still remains an open question whether current REG results could be explained on the basis of being caused by a conductance effect, and the limits and characteristics of

psychokinetic electrical interventions into electronic devices are of relevance to all electronically instrumented PK detection systems.

In the case of the thermal effects, there are important issues concerning the localization of the incoming heat, and whether the heat is derived from some source outside the system affected, or whether a local redistribution of thermal energy is responsible for effects. Cryogenic systems might be fruitful target systems for the investigation of this topic area because their available thermal energy can be reduced to very small values.

In the area of microstructural changes an important type of effect is one where there are no competing normal explanations which can be given for the observed changes. Much of the previous research into the microstructural changes in ostensibly paranormally deformed metal specimens suffers from the limitation that the changes created by paranormal action cannot be distinguished from changes which are due to normal effects, such as the deformation process itself. The French work (Crussard and Bouvaist, 1978) is particularly interesting because effects were observed on undeformed metal specimens. This approach could be extended and techniques employing thin films as PK targets might be especially suitable, since they should be perhaps more readily microscopically characterizable than thick specimens. Both softening and hardening of metal samples have been reported (Hasted, 1981) by several investigators. As yet, no-one has systematically exposed a variety of targets of different materials to PK, nor have chemical systems been used systematically on any scale, although well characterized chemical systems should be very suitable as PK targets.

An important question raised by the work with dynamic strains is the question of whether the strains occur as a result of an imposed external force, or whether they originate as a result of events inside the targets. Localization questions arise here, too, since it is important to know whether Hasted's concept of a "surface of action" corresponds to physical reality.

(x) Applications of DDPK

I have two primary motives for being interested in possible applications of DDPK. One is that, once trained, DDPK agents tend to look for increasingly interesting and challenging outlets for their abilities, and applications would provide this type of motivating environment for DDPK ability to be used in laboratory research. The second motive is that if DDPK applications were to become used in Western society, this would act as an enormously powerful stimulus to the acceptance

of psi. In the discussion below it should be plain that I am not by any means certain that PK can be trained to the point where applications become feasible. The evaluation of possible applications is, however, a high priority of my group's current research program.

It seems clear that two ingredients are needed—a reliable, inexpensive, robust and portable DDPK detection system and the DDPK agents with sufficient skills to operate them. Clearly, too, at first, DDPK has to be used in non-critical applications, where errors in control of the effects would not lead to hazardous consequences. This limitation implies that “fun,” leisure-oriented uses should be developed first. Favorite candidates for PK controllability are toys, games, systems which can interface with music producing devices (such as synthesizers) and PK sensitive devices for martial artists to use to test out their mastery of the so-called “Chi” force said to be involved in martial arts.

If DDPK agents could localize their effects on chosen sensors and produce effects over long distances, a DDPK “telegraph” becomes possible, with messages being encoded in binary or Morse Code, two sensors being used, one for “dit,” the other for “dah.” This may seem an unnecessary use of PK, since effective alternative communication systems obviously exist, but the communication format may well be highly motivating for participants in laboratory research, and offers an excellent task for the assessment of PK skills and the psychological factors which impact performance.

I hesitate to mention the final application, because it currently sounds like science fiction, but then so did heavier-than-air flight, space travel and atomic energy etc. However, the military implications of this application are obvious, and I wish to forestall the suppression of information about this application by making its possibility public at this point, before it has been developed. I should also add that this application is, of course, by far the most uncertain of all I have mentioned and I have no idea whether it will work.

The IDS hypothesis has thrown doubt on the REG results as being PK, so that the data demonstrating the retroactive PK effect (Schmidt, 1976) cannot yet be accepted at face value. However, if retroactive PK really does exist, and if DDPK effects can be created in the retroactive mode, (and if DDPK agents can reliably produce “Morse Code” PK), a “telegraph to the past” becomes in principle possible. From the point of view of those in the past, on the receiving end of the system, they will have a system which will give information about the future. Because the information will be in Morse Code, any verbal or numerical information could be transmitted to them. A final “if” (making four in all—this is a very “iffy” concept) concerns whether the information,

once received, could be used, since it is possible to imagine scenarios where human consciousness of the information would inhibit its transmission (see Schmidt, 1981; Weiner and Bierman, 1982) but if all the "if"s were fulfilled, the human race would then be in possession of an accurate method for predicting the future, with quantitative data on future events becoming available. The uses for such a system are legion and obvious, and if such a system became a reality, the existence of psi would undoubtedly become accepted because of its commercial applications.

(xi) The Implications of DDPK for Human Identity

There seem to be four possible sources of origin of PK effects: the brain, the whole nervous system, the whole-body, or a hypothetical non-physical mind-entity (Thouless and Weisner, 1948). At the very least, a cerebral, nervous system, or whole-body origin for PK involves presently unknown physical processes, or unknown aspects of already known processes. The widespread recognition of PK as a real dynamic in the world will greatly impact our notions of what we are as human beings, even if the physicalist view of human identity—that our minds are coterminous with brain function—proves true.

But if PK originates from some non-physical mind-entity, this implies the possible existence of another realm in which such entities are located, which would lead to a very different model of the universe from the "one storey" model currently accepted in science. The development of proof of such a realm would be an extraordinarily difficult undertaking and even if this view proves to be correct, its establishment will take much time and effort. However, DDPK would be an ideal type of effect to serve as a research tool for the investigation of the neurophysiological sources of PK (if such there be), because the effects are well localized temporally and are therefore suited for EEG studies of active PK agents.

The dualist view invites another question—from whence do minds originate? The issues of survival of death and reincarnation arise at this point. Are minds just individuated subportions of some unimaginable collective mind-entity? This immediately raises questions regarding the existence of a transpersonal entity and its relationship to the explicate order of the physical world. It is not my intention to speculate further on these matters, which do, however, directly impact our conceptualization of what it is to be human, but to point out that one route amongst many which converges onto these central questions starts out from the seemingly innocent question posed above concerning the

source of PK. The study of DDPK may, therefore, produce many positive consequences for our understanding of human nature and its relationship to the physical world. If nothing else, I hope that the present discussion causes parapsychologists to look anew with increased interest and clarity at the existing evidence for DDPK effects and to attempt to extend and elaborate upon this knowledge.

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DISCUSSION

COLLINS: I have a couple of technical points. The first one is this business about improving the signal to noise ratio. I think it is in my talk when I said that one of the problems with parapsychology, one of the things that I would point out if I were a critic, is the failure to improve the signal to noise ratio. That is not the same as asking for the signal to noise ratio to become very high. In other words you could still have a very low signal to noise ratio, but the snag is that if you can see some slight improvement even if it remains low that looks convincing. The difficulty with paranormal research is you have not got any improvement. Not that you have not yet got to a massive signal to noise ratio, because of course it would be convincing under those circumstances.

ISAACS: I think that the studies using deep relaxation to facilitate ESP and the developments in ganzfeld research arguably provide some evidence for improvement in signal to noise ratio, compared to previous free-response ESP scoring. However, forced-choice scoring rates do seem not to have changed. In the case of ESP it is more as if the major limitation is that performance is inconsistent, rather than showing uniformly bad signal to noise ratio and perhaps these treatments somewhat improve reliability. The skeptical community obviously does not accept this view.

COLLINS: The second point is the distinction you are making between experiments like the Schmidt experiment that uses a REG and the macro PK that you are talking about. Now a few years back I seem to remember developments of the Schmidt experiment were going like this. The Schmidt experiment machine was making very rapid choices. I mean the Schmidt experiment made, shall we say, a million choices in a second and all you had to do was to get a slight positive effect and

there is no reason why the effect should not be indicated by some binary choice. So let us say that you got it to make a million and one red choices in a second and you get a red light and nine hundred and ninety-nine green choices. Do you see what I mean?

ISAACS: Yes, I do.

COLLINS: So you can turn the whole apparently statistical matter into a binary effect with the sort of feedback you are talking about.

ISAACS: Do you want me to comment on that?

COLLINS: Well, it seems to damage the distinction you are trying to make.

ISAACS: It does not because Schmidt found that the scoring rate declines in step with the increases in target generation rate, so that similar or worse signal to noise ratios are obtained with accelerated target generation rates than with slow ones. Unfortunately one cannot achieve less uncertainty in REG outputs by increasing the target generation rate as you propose. The problem is the irreducible preponderance of false positive feedback in REG outputs, given achievable scoring rates. Charley has written a paper taking the position that there will be a "talent threshold" constituted by a certain level of PK scoring, below which learning will be impossible because of the misinformation given to the PK trainee by false positive cues—hits due to chance rather than PK. My position is that no one has demonstrated hitting above what one could reasonably view as being the minimum "talent threshold" for learning to be possible. There is, in my view, no way of modifying the feedback properties of REGs to compensate for the unreliability in the feedback signal as a source of information about the effect the PK agent may be having on this type of system. From my perspective this will always prevent REGs being used for PK training or applications of PK to control systems. To my knowledge, no one has yet reported a body of research showing learning effects with REGs. If this had been found in the REG data, I would be surprised if it had not been reported by someone. What you have done is what I hoped nobody would do and well done for doing it, which is that you got us engaged in a very complex argument about the interaction of scoring rate with feedback trial generation. Then came the separate issue of feedback rate and the issue of can you make that system look as if it is an analog system. I think that is the best argument and we shall talk about that privately because that is too technical for some of our audience. The single point that I would like to stop with is that nobody has reported learning on an REG system. The most that you could possibly say is that scoring is steady. That scoring is at a very low rate so that my main substantiated claim that there is not learning shown with those systems is not con-

tradicted by anybody's alternate claim that they have seen learning with an REG machine.

COLLINS: That seems to be an empirical claim rather than a theoretical one.

ISAACS: Yes. I tried to provide a theoretical explanation for this empirical claim in my presentation.

NEPPE: If one looked at the five criteria that you have drawn up for DDPK—stable systems, low noise, monitoring by some kind of electronic system, sensitive potential in terms of learning and distance effects—I am wondering whether you would think that the alleged electronic voice phenomenon would fit the criteria for DDPK.

ISAACS: Electronic voice phenomenon is an interesting and neglected area which has been ignored because of its spiritualistic surface phenomenology and seeming resistance to quantification. Hawke reported, in the book on Geller edited by Charles Panati, that Geller apparently altered the magnetic pattern on a programmable calculator card, which uses a layer of iron oxide as the recording medium, similarly to magnetic audio tape. This suggests that PK might be able to cause structural changes in iron oxide layers. If this were done to an audio tape it would certainly sound very different before and after being zapped. My hypothesis is that some EVP phenomena are pure unmodified noise which is interpreted as having meaning. However, perhaps other EVP "speech" might be due to the PK modification of originally pure noise artifacts by the expectant people who listen for EVP phenomena, who form hypotheses about what is on the tapes and then, over a period of time and many auditions, modify random noise to conform to expectation. I have always wanted to perform a study to check this hypothesis by taking copies of the target tape after successive exposures to the EVP enthusiast, so as to demonstrate that they modify the noise on the tape. However, to answer your question directly, although my experience is very limited, the EVP tapes which I have heard do not show the good signal to noise ratio which I specify as necessary for a PK effect to be detectable without statistical means, so that I do not think that EVP should be classified as DDPK. Also, in my opinion, no one has done a careful study showing that EVP are indisputably paranormal either.

NEPPE: What do you mean by zap?

ISAACS: I mean apply psychokinesis to the iron-oxide layer.

NEPPE: I see.

WICKRAM: I have had some experience with biological feedback, teaching people to influence biological systems. In the early years of biofeedback it was believed that if you gave people enough feedback

they would walk on water. A little more feedback and they would be walking. The issue of feedback is somewhat more complicated than that. There are individual differences in people's ability to profit from feedback. There is some preliminary evidence, three studies to date—two from Australia and one from the United States—demonstrating that people of superior hypnotic ability given immediate feedback get messed up. The gradient of the learning curve is poor. On the other hand, people of low hypnotic ability do profit from immediate feedback. Now, there are no data as far as I know apart from my own clinical observation that people of superior hypnotic ability do profit from delayed feedback. In other words, there is the subjective confirmation of expectancy that something is going on. When feedback is given, not immediately, but maybe after about two or three trials, it seems to verify their subjective sense of efficacy, of something—what ever you want to call it—and escalates the ability to control their biological system.

ISAACS: Yes, I want to thank you very much indeed. We discussed this a bit over lunch and your contribution is really very helpful. We had independently started to get to a similar position in the sense that it is clear that giving some people immediate real-time feedback actually disturbs the state in which they produce the effect and some of our participants will refuse real-time feedback. They stick their hands over their eyes and their ears and they don't want feedback, except that they must have, as you have suggested, delayed feedback to maintain their belief. This is the next step, the next level of understanding of this phenomenon.

WICKRAM: Procedures by themselves may be important, but we have to look at their interaction with subject variables.

ISAACS: Absolutely.

TART: I will echo that and then make another point. There certainly are moments when feedback is disturbing. There are moments, as it were, when you are really trying to concentrate on the exact nature of some internal signal and that outside feedback signal is distracting you so you may want to cut it out. I also need to make a semantic point, since we are dealing with English here. Directly detectable PK is a marvelous term, but in the ordinary meaning it also includes macro PK, which I am not sure you include once you have thrown in these defining characteristics of electronic monitoring. Since we have the macro/micro distinction maybe we need directly detectable milli-PK? I hate to start it, but I think you need to clarify it slightly more.

ISAACS: Thank you. One of the things that worried me very much is that parapsychologists have such a binary bifocation between macro

and micro that I was worried that they would automatically assume that I meant macro PK when I am talking in fact of micro PK instrumentally detected, of good signal to noise ratio.

TART: There is also an important characteristic that you did not mention and that is that it is psychologically more feasible to ask someone to do macro PK. My goodness, how are you going to do that? Work on the random event generator. You do not really quite know when you are doing something, so putting it in this intermediate range it is much clearer to a person that maybe that is something I can affect, it is not totally out of range.

ISAACS: Yes, I would agree with that. My thinking is shifting at the moment rather radically in the sense that I think there are different schemata with which we can view PK training and possibly PK performance overall, where there are different levels of analysis. I have looked at my procedures and compared them to operant conditioning. The requirements of giving feedback from the noise floor are basically the requirements of operant conditioning. If you were trying to shape a behavior, namely PK, what do you do? The answer is you get a PK-like behavior first, which means that you have an effect that is so slight that you cannot necessarily initially distinguish it from a rather large noise pulse. But you give it feedback so that you are shaping that behavior and it is reinforced and then the events get bigger and bigger and get more and more reinforcement. If you then go from the operant conditioning level of looking at this behavior to a more cognitive level, the level at which you were addressing the issue of the participants' beliefs becomes very important. In my paper I give an analysis of why the REG and statistical systems are so successful in inducing PK. You can get PK on these systems because the system is systematically set up to invite yourself to believe that you have created an effect on it. It is telling you you have a hit. At one point, you may, simply for purely superstitious and incorrect reasons, assume that that hit was due to you. Then you have established a pro belief state which allows you to increase the amount of PK that you can get on the system. The problem with macroscopic PK trials is that if the thing is sitting there inertly, it is not giving you any artifactual indications that you are successful at all. It is never allowing you to believe that you are affecting it. So what I am saying is that macro PK is, as you say, a much more difficult task to believe in as a belief *per se*, as a trait, and in terms of the state belief it never engenders the kind of state belief, the instantaneous belief that Batcheldor talks about which is given by the REG system and which is given by DDPK systems because of the instrumentation. So I

give a much more complex analysis in my paper than I have been able to mention verbally in this presentation.

PANCOAST: I would like you to discuss the complementarity or the connection. I am not an experimenter, so my language is really different between the PK effect on things outside of the body and what is one of the effects of a guided Kundalini process that does a number of things. One is to spontaneously move the body in a flow. Two is to move it very precisely to open blocks with a clearly learned effect of the correct manner of the body's structure and functioning. Do you understand what I am talking about? And three, the intuition, and this is where it comes in the control of it that knows ahead which way the energy is going to move it and that is in my own experience. I would love to judge the signal to noise ratio with that one. I happen to be in the process of that kind of an experience myself right now and have been for a number of months and so I am speaking from a process I am intimately connected with. There is also the connection with another process, which is when that energy is not moving the body, but is directly producing whatever electro energy is healing energy, that then gets applied. And if I do not know what I am talking about technically, it is because I only know it experientially.

ISAACS: It is very unclear what the relationship is between sensations of "energy" in the body and PK. This is an area which needs much more study. In parapsychology we have ignored this phenomenological side almost completely until recently, because in its striving for "objective" data and proof, the Rhinean paradigm was founded on a behaviorist methodology. We have employed behaviorist experiments where the detailed inner experience of the subject is ignored, or confined to measurement by very limited psychometric instruments, rather than being studied in an extensive and open-ended way. Parapsychologists have dichotomized their data into reliable, quantitative data gathered by the behaviorist methods and "unreliable," "unanalyzable" non-quantitative data which has then formed the "lore" exchanged informally at Parapsychological Association Conventions, behind the scenes. It is bad form to talk about the "lore" in public, because that is speculation. The lore is non-formal, non-quantitative and uncontrolled. Parapsychologists have had a very ambivalent attitude towards this kind of data because of its qualitative and non-formal character. But there are ways of formalizing it and even hypothesis testing with it. Some of the pioneering studies in this domain have been done by Marilyn Schlitz, using a methodology adapted from anthropological interview techniques, which may provide a pointer to incorporating this type of data in a way that the Rhinean paradigm has ignored.

PALMER: Since I am currently employed at the home of the Rhinean paradigm let me make a couple of points in its defense. First of all, every time I hear a debate whether we should be doing A or B, the question always occurs to me of why we do not do A *and* B? I do think that there is a great deal of potential in a DDPK approach along the lines you have discussed, and I certainly would like to see this approach pursued more than it has been. I hope some other laboratories will pick up on it. On the other hand I would make three points on behalf of the more traditional approach. Number one, the real issue is what has actually been accomplished. The REG work has been going on for a number of years and has been quite successful. I think some of the best and strongest evidence that we have for psi anomalies comes from the REG work. More than any other area of experimental research in parapsychology it has been difficult for our critics to deal with. It is the REG work that so far has delivered the goods. A second argument I would make on behalf of the traditional approach uses one of the points you made against it, namely the IDS model. Whether or not the REG work reflects PK or some other form of psi, the IDS model raises some very interesting theoretical questions which I think are directly related to REG methodology. My point is that some of the interesting theoretical questions you can ask about psi in general are statistical in nature, and therefore we need some research based on models like IDS that address those kinds of questions. My final point has to do with signal to noise ratios. In a narrow sense you are certainly right that, virtually by definition, the DDPK approach has an advantage in terms of signal to noise ratio in the sense that you can detect whether a particular isolated signal is in fact real. However, in the broader sense, and I wonder if this perhaps is what was really meant when this topic was originally brought up, the more fundamental question, I think, has to do with reliability. That is a problem for both approaches and it is not necessarily overcome by DDPK. For example, can the person zap the piezo crystal consistently? Can he do it on Monday? Can he do it on Tuesday? If you are thinking about a Morse Code, can he reliably produce a dash when he is trying to produce a dash, or is he sometimes going to produce a dot dot? I think these are the more important signal to noise questions and they are not necessarily going to be solved more readily by the DDPK approach than by the more traditional approach.

ISAACS: Thank you. I agree with every single point you made. Half an hour is such an incredibly short time that I had to really sharpen up my dichotomies. I do not really want to attack the Rhinean paradigm which has got us to where we are now. Nor do I want in any way to supplant that research because I think, as you said, that there is a real

need for both forms of research. What I am concerned to do is to try and push the balance a little bit. I want to develop a new methodology without in any way getting rid of the old methodology. The other point that I would make is that it is true that the DDPK work has not got the kind of track record, the kind of careful validation of the REG work behind it yet and that is perfectly true. I am hoping that we are actually starting on that path. Those tasks have to be accomplished. Another point that I would make is that in many ways the DDPK approach is much more difficult and much more arduous, more time consuming and more consuming of resources than the REG or statistical approach, because you have to have selected subjects, you have to train them, you have to nurse them along. There is a big investment there. Then finally on your point about reliability, I want to say the same thing as you. The problem is of obtaining reliable PK performance from people, whether we use an REG or a DDPK system.

ROCKWELL: One other advantage of the directly observable device—and this is from the standpoint again of the fraud-proof type of experiment—is that you can record the data directly onto an ordinary audio cassette, the control signal onto one channel, the target signal onto the other channel, and you can feed that in and store the data in that form untouched by human hands. You could then feed the cassette directly through an analog-to-digital device, which is a cheap gadget you can get for any little personal computer. This information goes into your computer and you can then ask the computer to digitalize a piece of it, the data that you want to work with. You can do fast Fourier transforms. You can do differentiation, integration, total area under a curve, etc. You could do any kind of analytical work with the raw data without ever having touched it. The whole business of fudging data, it seems to me, is removed from criticism in that sense. And I think that is a real advantage.

ISAACS: My piezo system at the moment is a computer-based system. The signal is directly converted. But there are problems in using computer systems. I would not personally choose to use the computer system to give people feedback unless the computer was very fast. I would give them feedback through an analog system and then do the data reduction on a computer system. The other sociological point that needs making is that some people are convinced by non-statistical data in a way in which they are not by statistical data. I do not happen to agree with that point of view and I find it difficult to understand how people can take that position, but some skeptics do seem to respond in this way.

McCLENON: I do not wish to negate your presentation. It sounds

as though it is a good method and should be a good orientation because it is testable. But it seems to me that the basis of it is a behaviorist approach. Researchers are not very good at getting PK and so your solution is to give the people feedback and then you will get it and everything will work out. But it seems as if we are really amateurs in getting PK because we do not get it very much and we seem to ignore people who claim to get it a lot. I think if you look around the world cross-culturally at people who are psychic practitioners and folk healers and such, they do not use a behaviorist approach and they do not put great emphasis on feedback. The main method is reaching some kind of state of disassociation, or whatever you want to call it, until you come into contact with some deity and then you do whatever the deity tells you to do. So I wonder if you might just make some comments on that as an alternate paradigm, perhaps.

ISAACS: I would like to. To do the research in America we went out and screened lots of people. We ended up with about a dozen very extraordinary people most of whom are either experienced psychic practitioners, mediums, or shamans, etc. We have a set of people who are immersed in a transpersonal and psi believing culture. They try to use their powers to do PK and many of them use mediumistic techniques. For example, we have people who believe they are channeling the universal mind to do PK. We do in fact use all of that incredibly rich and rather unexplored informal psychological technology which is imported into our laboratory by our participants. I have not mentioned this until you forced me to do so in front of our august audience. I was censoring myself. Although I have been sounding rather behaviorist, concurrently with our quantified research an anthropological team is active, who are doing what Marilyn Schlitz has been doing with, for example, Helmut Schmidt and other people such as Reiki healers. This team is interviewing our participants and the experimenters. They are looking at the beliefs, the concerns, goals, interests and techniques of the participants and the experimenters. We also have members of my research team who have started qualitative research projects looking precisely at the area you mention from an anthropological perspective. I do agree with you that it is the way to go and a very powerful additional factor. The fact that I have been talking about operant conditioning in no way means that the people at the front end of this experiment are not fully committed to similar techniques and beliefs as the groups you cite.

WICKRAM: It seemed to me that the people who profit from feedback are those who are like St. Thomas; skeptical people.

ISAACS: Doubting Thomas.

WICKRAM: Doubting Thomas, the one who said I do not believe that Christ has risen unless I can put my hands in his sides.

KRIPPNER: He was a saint also.

WICKRAM: Some human beings have difficulty in altering their belief systems. They are stuck in certain belief systems and they need ocular proof. All that these feedback devices do for them is that they package observations in ways that then permit them to change their belief structures. These are not just abstract questions for me. I work with this routinely with people who have chronic pain, modifying their belief systems. Feedback works very well with people who have low hypnotic ability. Someone talked about this issue of operant conditioning. It is not going to solve the problems, but what it will do is package the alteration of belief systems in such a form, in such a manner that it will permit us to move these people to a different place in their heads in a way that somehow augurs well in spite of them.

ISAACS: Yes, thank you for your point. I think that the most relevant and so far totally neglected literature which is highly relevant to PK induction is the approach of some cognitive psychologists to biofeedback performance. It seems that very few people in parapsychology have yet looked at this literature, the material from the experimental social psychologists such as you have quoted. I think that Meichenbaum's approach, where he considers that for some form of behavioral cure, whether it be biofeedback or anything else, the patient has to change his cognitions in order to improve his performance vis à vis some bodily state or problem, is exactly what you are referring to. The feedback system allows them to make these cognitive shifts and I agree with you. I think that there is a huge and rather non-explored territory which could very fruitfully profit from these forms of input from cognitive psychology and from experimental social psychology in a way that we have not as yet utilized. Parapsychologists really have a treasure house sitting there that they have not utilized as yet. I am making a true believer confession at this point regarding my previous prejudice against forms of behavioral therapy and operant conditioning, because I saw them as a form of police state psychology and very reductivist. I think now that with the cognitive revolution in psychology we do not have to be reductivist in the way that we were and that we can approach people as people rather than become ratomorphic with them.

ARONS: Of course I have my own reactions about behavioristic approaches. My first comment was going to be that then you get into the whole kettle of fish that the behaviorists get into—what do you mean by a reinforcer for any given person? I want to support this now and I am going to support this more tomorrow. The cognitivists in a strange

way tend to enter into this in a frame of reference. I am thinking particularly of George Kelly who I find a lot more palatable than some of the other information processing cognitives. But we do have tests now such as Kelly's Personal Construct instrument which would allow us to enter into the construal system and then instead of talking about M&M's we can really talk about reinforcement in a very meaningful sense.

ISAACS: One of the reasons for using the qualitative approach of the anthropologist is that you get all of this belief stuff in soft form which you can then formalize within the construct theory and repertory grids of Kelly and other people. The other thing that allowed me to get over my fear of behaviorist operant conditioning was the realization that it is simply one level of discourse, one level of analysis. You can acknowledge that level while still being able to acknowledge the other levels which see human beings as first of all human beings in cognitive psychology and then as much more than that within transpersonal psychology, which I think is going to happen too.

ARONS: Just as a remedy to what you just said, one level that happens to be the simplest and most powerfully affecting level really smothers all other levels, so that fear I think is very justified. When you are speaking of a very reductionistic psychology that takes over an entire field then that level becomes dominant and that is where your fear is definitely justified.

ISAACS: I think parapsychologists are daily reminded that people cannot be reduced in that way.

ARONS: Sure.

NEPPE: May I just add one other interesting comment having brought up the whole area of cognitive psychology. This is very much a simplification. I do not imply that it is anything to do really with cognitive psychology, but it is an interesting one in relation to the group of mini-Gellers that I happen to be investigating. I used to ask these young people, generally eleven, twelve, thirteen-year-old kids, what is it? How can you go about bending these keys? And I would ask them this greatly intrigued because I would get the same response virtually from everyone of them and they would say "I think it so and I do it." And it seems to me it is highly relevant from the perspective of any kind of behavioral alteration, however we want to term it, because somewhere along the line it seems that one needs to radically alter or at least modify a belief system such that there is this potentially positive direction. Or it might be that the statement was completely incorrect. But it would be one kind of hypothesis to test out. Have you got any comments?

ISAACS: Yes, I have a comment because I think it relates centrally to the theme of this conference. What we can do is bounded by our image of what we are. Our view of human nature constrains what we can do. One of the roles of parapsychology is to change the picture of human nature that we have and I think that it will do that hand in hand with transpersonal psychology. What the world needs is a very much less reductivist and very much less mechanistic view of human nature than what we have got. I am very glad that we are having the conference to advance this process.

TART: I want to amplify what you have just been saying, Julian, and what several panel members have said. The provision of feedback is an excellent general purpose technique for learning. I mean there is simply a general sense in which if you want to learn to do something you have got to have some idea of what is happening in order to control it. But as it has become clear, you do not use feedback in isolation. If you want to teach somebody to hammer with a hammer, for instance, you do not only look at his wrist and the motion of the hammer. There is a person holding that hammer, a person with goals and so forth. So this leads into matters such as, for instance, when feedback is appropriate and when it is not. You may learn a strategy that is partially effective or let us say it is effective, but only up to certain limits. Getting feedback then is very helpful. I train in Aikido, for instance, and so I have learned certain techniques to respond to a certain kind of attack. I am certainly making use of feedback to improve my performance, given the concept of what I am out to learn. Sometimes, though, I discover that that concept, that way of doing it, has some inherent limitations built into it. At that point I have got to switch. Now, that is hard because I know I have made some progress with the other one. We get attached to our skills. You have to switch to a technique where clearly you are worse at first. The feedback you get then is terrible. You see how much worse you are doing than what you used to be able to do.

That is a real human relations problem there. How do you get somebody to switch? And I think this applies to a lot of kinds of psychic functioning. A lot of people have techniques that work up to a certain point and they get attached to them. Now, by providing feedback you may want them to get even better, but they may be good enough in terms of why they are there for the experiment. They are doing what their belief system says is right. There are occasional results. They feel fine even if you say it is not 100 percent. We have to be sensitive to those kinds of parameters. We have to switch techniques, drop techniques when it is appropriate.

ISAACS: I agree with you. We did a breakdown of our current nine participants, seven of whom are pretty well dependent on feedback and two of whom do not want feedback when they are actually active. Then the other issue which I think you raised which is highly relevant to our approach is the issue of goals. I think that what we are interested in is to do the kind of careful goal setting that sports psychology has analyzed to some extent. What we sometimes find is a divergence between the participant's goals and the experimenter's goals and what we need to do is to somehow get much higher levels of intrinsic motivation in our participants. There seem to be two ways of doing it. You find people—and we have one or two people like this—whose mission is to do scientific PK. They are just wonderfully motivated and they want to come in every day of the week and work all day. If you cannot find people of that sort then somehow you have to develop a way which is ethically acceptable for everybody, of setting goals for people, for increasing the intrinsic motivation of participants. There again, within psychology there has at least been some looking at intrinsic and extrinsic goals. The other issue which was raised here was the issue of effectance as being another concept which is relevant to belief systems. We find that people have to transcend the methods that they have used at one phase and what we have to do is somehow loosen them up to be prepared to use all sorts of different methods at different phases of their PK training. And, as you say, that can be difficult because people can get very locked into their techniques. The final observation I would make which I think is very interesting, in which we really have not yet started pulling the information together, is that it looks as if getting involved in this form of experimentation has a very transforming effect on people. People start either hitting blocks within themselves that get them "stuck" or they hit them and get through them and then their performance really escalates and we find that people have really experienced quite radical changes. This is a worry for us because we do not want to start creating massive change within the people whom we are working with. But it looks as if, for the people who succeed, that doing this as a discipline is akin to some form of martial art or whatever in the sense that it makes them detect and realize their blocks and then if they can work through that they grow and they go on to the next block and so the thing becomes part of their inner transformational life. This is one aspect where it clearly exceeds by far the normal behaviorist psychology style of experimentation. It is something which I think a lot about and I am also somewhat concerned about, because there are massive ethical ramifications.

WICKRAM: Most of the things I wanted to say have already been

said. But several years ago in the Biofeedback Society of America we organized a symposium with Gary Schwartz and I think Dave Pazkewitz on "Do Beliefs Have Biological Consequences?" We reviewed some of the literature on that topic. If you work in, for example, a rehabilitation ward with patients, you become impressed by the fact that the lack of an adaptive belief system can be more immobilizing for these patients than the lack of an arm or a leg. It becomes a very practical issue that has preoccupied me. How do you modify human belief systems? How do you destabilize the existing belief systems and then oscillate them, before you try to change them? And it seems to me that the people who do poorly on these psychophysiological experiments on PK or on hypnosis tests are people who are stuck in their belief systems. And the development of a technology to change their belief systems around I think will contribute significantly to progress in this field.

ISAACS: That is true. One of the areas that I have been looking at is in the area of coping strategies. Psychologists who are interested in death and dying have examined some of the coping strategies used by people and produced interesting typologies and I am very keen on trying to use coping strategies. One of the ways in which we can approach PK training is to take a behavioral therapy approach and say that what we have got is a behavior where there is not enough PK and what we want is a behavior where there is plenty of PK and how do we get from the one to the other? People's belief systems are blocks on that way. The other thing that you indirectly referred to, which I would love to have the time to explore, is the possibility that we may find that PK is part of an overall syndrome. It looks from our screening data as if PK ability is part of an overall psi capable syndrome. It looks as if people with lots of psi also can do PK. But what interests me is that maybe those people too will be good at biofeedback tasks. Maybe they will be physiological athletes as well as psychokinetic ones, in which case pretraining with biofeedback becomes a very interesting option. First of all as a measure, as a possible correlate of PK ability. Secondly, some of the biofeedback literature has shown an increase in internality of control as a result of biofeedback training. That is very exciting because that may increase the participant's sense of effectance. After you have a couple of biofeedback tasks on which they are pre-trained and then they do PK, we may have a superior response because what we have done is to shift their beliefs by taking them through biofeedback training first.