A PARAPSYCHOLOGICAL TEST OF ECCLES' "NEUROPHYSIOLOGICAL HYPOTHESIS" OF PSYCHOPHYSICAL INTERACTION

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1. The Problem of Psychophysical Interaction

How are we to account for the reality of our own self-conscious experience? Does mind "emerge" out of or represent some "inner" aspect of physical states? Or is mind an independent entity that interacts with but is not reducible to physical states? Is the brain a generator or a transmitter of mind? If the former, what is the alchemical algorithm through which this remarkable transformation takes place? If the latter, what is the mode of interaction between mind and brain? Is it one-way or two-way: are mental states always effects and never causes of brain states, or are brain states sometimes effects as well as causes of mental states?

The problem of psychophysical interaction has traditionally been relegated to speculative philosophy because it has been empirically inaccessible. In this paper, I will suggest some ways in which psi research may contribute to the development of an empirical approach to this ancient and fundamental problem. My contention is that the methods and findings of psi research provide science with a unique opportunity to bring this problem into empirical focus. As an illustrative exercise, I will focus on one aspect of dualistic interaction theories that is central to the viability of such theories: the concept of two-way interaction between mind and brain. I will suggest that this concept has, as a direct consequence of psi research methodology, become amenable to experimental study, and I will offer some crude beginning thoughts on how these methods may be extended to enable us to ask more penetrating questions about the nature of psychophysical interaction.

2. Physical Detection of Mind Influence

Theories of dualistic interaction can be brought into empirical focus if—and only if—the hypothesized mind influence can be physically detected under conditions that render explanation through physiological reductionism untenable. It is here, I believe, that psi research methodology can make a unique and important contribution.

2.1. Random Generator PK Studies

Psychokinesis experiments with quantum mechanical random generators provide one such method. This line of research is now sufficiently extensive to warrant at least tentative support for the hypothesis that goal-directed mental activity can serve to induce small but measurable changes in the normal operation of remote physical devices.

These devices use fundamentally random processes such as radioactive decay or thermal noise in semiconductors to provide an electronic analog of "coin-flipping."[10] In a typical device of this type, electrons emitted by Sr-90 trigger a Geiger counter and the momentary position of a high-speed binary counter at the time of the electron registration determines whether a "head" or a "tail" has been generated. This output is latched to a target bit and if they match (e.g., both are "heads"), the trial is a hit, otherwise it is a miss. The target bit may be constrained to remain constant (e.g., "heads") over a series of trials, or it may be complemented (i.e., alternated between "heads" and "tails") on every trial. The latter strategy provides a built-in control to cancel out any side bias favoring one of the two output channels. The expected probability of a hit under these conditions is, of course, 0.5 and this may be routinely verified through control calibration tests run without observers attempting to influence the device.

These devices offer a number of advantages including imperviousness to outside ("nonpsi") influence, a high data rate (up to approx. 10^3 samples/sec., currently), automatic data recording and analysis, and sensitive feedback to the observer of the momentary internal state ("heads"/"tails") of the device.

Subject/observers monitor the current physical state of the device through a feedback signal that occurs whenever the device is in, say, the "heads" state, with no—or different—feedback when it is in the "tails" state. The range of feedback displays currently in use includes simple digital readouts, light and tone displays and computer graphics displays. One such computer display in use in our laboratory, e.g., involves a race car simulation. The subject/observer sees two cars racing

up a TV screen. One car is "live," i.e., is driven by a quantum mechanical random source. This is the target car. The other car is driven by a deterministic pseudorandom source. This is the control car. Each car advances up the screen, one step at a time, as a function of the number of hits (target-source matches). The subject's goal is to make the target car "run faster" than the control car.

Superficially at least, these experiments are similar to biofeedback procedures. The subject/observer's task is to monitor the feedback display and to enforce the presence of a given feedback contingency, according to preset experimental instructions. Unlike biofeedback, the only direct physical connection between the subject and the physical system he attempts to influence is the feedback signal itself. The only apparent instrumentality available to the subject consists of his cognitive dispositions and strategies. If successful, properly controlled experiments of this type would seem to indicate that purely subjective states are capable of interacting with remote physical systems so as to directly influence the selection of values permitted by the probability distribution of the physical state.

Since this line of research was inaugurated in 1970 by the physicist Helmut Schmidt,[11] approximately five dozen experiments of this type have been reported by investigators in eight different laboratories. I have surveyed these studies elsewhere.[6] Approximately 65 percent of these studies yielded independently significant evidence of direct observer-instrument interaction, compared to the expected chance rate of 5 percent. None of these studies show similarly significant results in control calibration tests with the equipment run without observers or intended influence.

Given some of the more interesting potential implications, these findings must be treated with great caution. Considering especially the relatively high data rate, there is no way to determine with certainty how many nonsignificant experiments may have been conducted and not reported. We know that in the behavioral sciences generally, there is a pronounced and well-documented bias against the publication of nonsignificant findings.[1,13]

There are, however, several considerations that militate against the hypothesis that these findings are due to the selective reporting of "positive" studies. First, because of the controversy surrounding psi research, workers in this area tend to be more sensitive to the need for replication and unbiased reporting of *both* positive and negative findings than are scientists working in more conventional problem-areas. The Parapsychological Association is, to the best of my knowledge, the only professional scientific society that has an official

policy against the selective reporting of positive results. Second, the fact that 35 percent of these experiments represent negative outcomes shows that *in practice* failures *are* reported. Third, even if we assume, as a worst case estimate, that for each of the significant experiments reported to date, there are 10 unreported and nonsignificant studies (i.e., over 350 unreported failures), the observed results would still be significant. Finally, this estimate is actually quite conservative since it is based on a 5 percent criterion of significance: 35 percent of these experiments are significant at the more stringent 1 percent level and 20 percent of them are significant at the 0.1 percent level.

Thus, these studies of observer influence on physically-remote random devices show a healthy level of replicability by any reasonable standard of behavioral science experimentation. Before proceeding to discuss how this methodology might be profitably extended to more directly address the mind-brain relation, it will be useful to briefly review some of the secondary findings that have emerged thus far through this line of research.

2.2. Goal-directedness and the Role of Feedback

These effects, like those studied in biofeedback, appear to be "goal-directed." The observer's task is to increase the frequency of a feedback signal. He need not know or be concerned with what is "inside the box," i.e., the internal mechanism of the random generator, in order to influence its output. This is indicated by studies in which physical parameters of the device have been systematically varied.[12]

For example, several studies in my laboratory have used a random generator that automatically complements the target *1 microsecond* prior to each trial. This feature was incorporated into our design as a control to cancel out any possible side bias in the output of the device. It serves this function quite adequately: in 7 million control trials, we have observed a total excess of 37 "heads" (observed mean = 50.00053 percent, or 0.027 standard deviations from the expected mean). Some of our experiments explored the role of device feedback in guiding these observer effects. In several studies,[7,15] subject/observers received tone feedback over headphones while relaxing in a room adjacent to the random generator. Their task was to keep the tone ON as much as possible. Unknown to the subjects, the feedback tone was sometimes contingent upon above chance scoring (hits) and sometimes upon below chance scoring (misses). The subjects' goal was to keep the tone ON and they did so to a statistically significant degree, regardless of the feedback contingency. The fact that in these experiments the target

was defined *1 microsecond* in advance of the trial would seem to rule out any mechanistic "push-pull" interpretation of these effects, since this operation is approx. *3 orders of magnitude* faster than the speed of human neural transmission, which operates on the order of *milliseconds.*

Successful subjects often describe their state in these experiments as being characterized by passive intention or "wishing," as contrasted with effortful striving, and several experimental studies have shown superior performance with goal-directed as contrasted to process-oriented subject strategies.[9]

While in most of these studies subjects have received feedback to the momentary state of the random device, such feedback does not appear to be necessary in order for these effects to occur. In an experiment with Ingo Swann,[8] we found that his scoring rate improved and was independently significant in part of the experiment in which we increased his distance from the random generator and gave him only total score feedback at the end of each 500 sample run. Swann was separated from the instrument during this phase of the experiment by two 4-inch thick steel walls and a distance of approx. 4 m. Braud and Braud[2] have similarly reported significant random generator effects with subjects who received only delayed and partial feedback. The specificity of these effects is presently unknown. If two independent random sources are run simultaneously such that one drives a subject-monitored feedback display while the other is "silent" (i.e., unobserved), does the effect occur only on the displayed source or does it generalize to the "silent" source? We are currently in the design phase of an experiment to address this question. Needless to say, systematic research on the role of feedback is just beginning in this area. The current "best guess" is probably that momentary device feedback, while not necessary for these effects to occur, does facilitate directional outcomes.

3. Psi Correlates of Volition: A Feasibility Study

To what extent can this methodology be extended to apply more directly to the study of mind-brain interaction? If we provisionally accept the validity of this work, there are several important consequences for further research, the most interesting of which, it seems to me, are these: 1) The findings constitute *prima facie* evidence for two-way interaction between mind and brain, since it would seem most unlikely that mind could directly influence external physical systems and not be able to influence its own brain; 2) it suggests a

plausible mechanism of interaction and accounts for the apparent elusiveness of exosomatic psi interactions and 3) it provides a methodological base from which to launch more intensive studies.

What follows is a feasibility exercise. It is loosely based on the speculations of the neurophysiologist J. C. Eccles. Eccles has long argued for a dualistic solution to the problem of psychophysical interaction.[3–5] The brain, according to Eccles, is a *detector* rather than a *generator* of mind. He suggests that "weak mind influences" psychokinetically modify the pattern of discharge of large neural networks.[4] ". . . the neurophysiological hypothesis," says Eccles, "is that the 'will' modifies the spatio-temporal activity of the neuronal network by exerting . . . 'fields of influence' that become affected through this unique detector function of the active cerebral cortex."

This hypothesis suggests that a primary function of psi phenomena is mind-brain communication. This idea is not new with Eccles. It has been proposed in various forms by psi researchers from F. W. H. Myers to J. B. Rhine. The most detailed formulation, prior to that of Eccles, was the "Shin" theory of Thouless and Wiesner.[14]

3.1. General Design

In a preliminary attempt to empirically formulate and test the hypothesis that there is a measurable psi component to volition, we have conducted two pilot studies. This work was carried out primarily by Lawrence Tremmel and myself. In these studies, a noise-driven random generator was used to detect PK activity in relation to subjects' volitional efforts to influence their ongoing EEG activity through biofeedback. Random generator output was sampled and gated ("triggered") by the subjects' success/failure in meeting preset EEG feedback conditions. If Eccles' hypothesis is correct, significant PK effects (i.e., deviations from randomness) should coincide with periods in which the subjects are volitionally active, i.e., when they meet the specified EEG feedback conditions. This was our only prediction in Experiment 1.

3.2. Description of Experiment 1

Ten volunteer subjects each contributed a single session. The subject was seated in a reclining chair in an Industrial Acoustics Corp. 1205A Sound-Isolation Room and was set up for ganzfeld stimulation. EEG recording was monopolar with the active electrode on the left occipital area, referenced to the opposite earlobe. Electrode impedance was 7 Kohms or lower. The subject was informed that the presence of EEG

alpha rhythm activity (8-13 Hz) would be associated with an audible tone. The subjects' volitional task during the session was to keep the tone ON as much as possible.

EEG activity was recorded on a Beckman Type R Dynograph. Alpha detection and feedback was via a digital frequency discriminator requiring two alpha waves with a minimal amplitude of 10 uV (peak-to-peak). The random generator was run at 10 trials/sec., with the target bit logically complemented on each trial. Each run consisted of 100 EEG alpha gated trials. Ten runs were taken in each session.

Since no random generator feedback was given, we had no a priori reason to predict directional PK outcomes. PK scores were obtained by squaring the z-scores for each run and summing across runs to obtain a chi-square value with 10 *df*. The overall PK results were significant, with $\chi^2(100) = 145.7$ and $p = 0.002$ (2-tailed). Independently significant ($P_{0.05}$) random generator results were obtained in three of the 10 experimental sessions.

The results of this experiment, while demonstrating a significant PK effect, are ambiguous with regard to Eccles' hypothesis. Since we did not monitor ungated random generator output (i.e., when subject's EEG was outside the feedback range), it is possible that the observed PK effect was unrelated to subjects' EEG. Even had we monitored ungated random generator output and found the PK effect to be isolated to the gated EEG feedback condition, the results would still be ambiguous with respect to Eccles' hypothesis, since such a result might indicate an intrinsic EEG-PK relationship rather than a psi correlate of volition.

3.3. Description of Experiment 2

We addressed these issues in our second experiment. We monitored both gated (alpha) and ungated (nonalpha) random generator output and added a no-feedback "baseline" condition. The "baseline" condition was identical to the EEG feedback condition except that subjects were asked to simply relax and were not given EEG feedback or volitional instructions. This provided a basis for discriminating between the intrinsic alpha-PK hypothesis and the hypothesis that there is a psi component to volition.

Several procedural changes were made in Experiment 2: 1) EEG electrode placement was C_z to linked earlobes; 2) EEG recording and feedback was via an Autogenic Systems 120a EEG Analyzer; 3) the total number of random generator trials per run (gated + ungated) was increased to 500; 4) soft music was provided subjects over headphones with EEG feedback superimposed over the music during EEG

TABLE 1

Experiment 2: Mean Chi-squares/Run by Condition

Condition	Gated RNG	Ungated RNG	$t(16)$	$P(t)$
EEG Feedback	1.33 ± 0.47	0.80 ± 0.31	2.58	<0.01
Baseline (No Fbk)	0.97 ± 0.22	1.01 ± 0.35	<1	n.s.
$t(16)$	1.95	<1		
$P(t)$	<0.034	n.s.		

feedback periods and 5) subjects' EEG feedback was shaped by the experimenter by increasing the minimal amplitude threshold so as to maintain EEG alpha feedback approx. 20–30 percent-time during the feedback runs.

We tested the following predictions in Experiment 2: 1) the significant PK effect of Experiment 1 would be replicated in the gated feedback condition; 2) gated feedback PK scores would be significantly larger than ungated feedback scores; 3) gated feedback PK scores would be significantly larger than gated baseline PK scores and 4) for the gated feedback condition, there would be a significant relationship between EEG frequency and amplitude changes within the session and overall PK scores.

Seven subjects each contributed a single session. One subject contributed two sessions and another contributed three. Thus, 12 sessions were completed altogether under these conditions. In each session, the subject completed 10 runs of 500 random generator trials in both the "baseline" and EEG feedback conditions.

Results by condition are shown in Table 1. For the gated feedback condition, the PK results are significant, with $\chi^2(120\ df) = 159.2$, and $p = 0.005$ (1-tailed). Thus, the alpha gated PK results of the first experiment are replicated, confirming our first prediction. PK results for the remaining three conditions (feedback ungated, baseline gated, ungated) are nonsignificant.

Predictions 2 and 3 were examined by t tests for uncorrelated means. The PK scores for gated EEG feedback trials are significantly higher ($p = 0.01$, 1-tailed) than the ungated feedback PK scores. This result indicates that the PK effect is related to subjects' EEG alpha activity, confirming our second prediction. Moreover, and very important considering Eccles' hypothesis, the gated EEG feedback PK scores are significantly higher ($p = 0.034$, 1-tailed) than the gated baseline PK scores, suggesting a relationship between PK and EEG feedback rather than an intrinsic relationship between PK and EEG

alpha activity (prediction 3). These latter two findings isolate the PK effect in this experiment to the gated EEG feedback condition.

Prediction 4 was not confirmed. Although in the anticipated direction, degree of change in EEG frequency and amplitude parameters were not significantly related to PK scores. Pooling the PK results of Experiment 1 with the comparable gated feedback condition in Experiment 2, we find $z' = 4.04$, and $p = 2.56 \times 10^{-5}$.

As I stated earlier, this work constitutes no more than a crude feasibility study. We are now in the process of designing an extensive replication of the second experiment. We are aware that there are many conceptual and methodological problems to be overcome. I welcome your criticisms and suggestions.

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DISCUSSION

TART: I'd like to make two brief comments. One is supporting Chuck's comment that he doesn't think these PK results might be a sample of only the successful experiments, the ones that happen to get selectively published. In a general survey of the field some years ago, I did a questionnaire survey of members of the Parapsychological Association and asked them how many experiments they had done *altogether* in the field, as well as how many they had published. In general, I found that something like one out of every three experiments is published, rather than the approximately one out of

every twenty you'd expect if only positive results were getting published.

Secondly, I think there's a methodological point you made that's really very important and I just want to emphasize it. That is that results of PK tests on generators that are running several orders of magnitude faster than we can imagine neurological processes to run are specially important in arguing for some kind of dualistic interpretation of the results. That's really an exceptionally important point.

DIXON: I hestitate to ask this question as a non-parapsychologist, but it seems to me there is a philosophical problem here and also a methodological one—you can have an animate system that is affecting an inanimate one—the PK effect. You can have an inanimate affecting an animate system—the clairvoyance situation. You can also of course, have two animate systems affecting each other at a distance. It follows, therefore, that it would seem logical to expect that two inanimate systems could affect each other, but if they can, then this disposes of the parapsychological hypothesis entirely, *unless* you postulate, and you may have evidence of this, I don't know, that the presence somewhere of an animate system is somehow a catalyst for relationships between two inanimate systems. I don't know if this experiment has ever been done, but it seems to me it's one that should be done.

HONORTON: The problem with your proposals, it seems to me, is that so far (at least) no one has come up with a way in which you could test a synchronistic correspondence, let's say, between two computers, that could not be explained in terms of the final observation by some experimenter. There is a great deal of discussion among the physicists who are entering this area now as to the role of the observer. If momentary feedback to the temporary state of the device is not necessary, then maybe the end result is. What I didn't mention is that when we do the control tests, where we're looking at the "normal" operation of the machine, there's a lot of superstitious behavior that goes on. My standard procedure is to cross my fingers, hope it will come out very close to chance and leave the room. And I suspect, for me as a PK subject, my motivation is probably much higher in the control test than it would be in the experimental one, because if that machine is not behaving normally, then that casts doubt on any of the experimental results. I don't see how you could do that experiment to get the observer out of it. At some point somebody is going to come and look at the results, and that may be all that's necessary. I wouldn't know how to get the observer, who is going to come and look at the results at some point, out of it, so that we could isolate the effects to the inanimate

systems rather than to the experimenter, who has needs or motivations in relation to the outcome of the experiment that might be satisfied in this way. The same problem occurs in animal research in this field. Some years ago there were some studies between a human and a paramecium, making a paramecium move to one side of the Petri dish. What was going on there? Was that the paramecium having the effect or was it the experimenter? There really is no clear way of resolving that. Over the last few years we've become very self-conscious over our inability to get the experimenter out of it and I've come to at least a temporary conclusion that it may be impossible in a fundamental sense to isolate the psi effects to a specific organism. It's quite clear that we can demonstrate that there is an interaction going on, but to say conclusively that it's the subject rather than the experimenter who goes through a randomization procedure or some other aspect of the experiment that could provide the basis for the result, there's really no clear-cut way of doing that, at least at present.

KELLY: Departing from what you just said, Chuck, I would reinforce that by saying to the non-parapsychologists particularly, that once one begins to take these things seriously, they intrude upon one's life as an experimenter in the most unexpected and horrible ways; and in this experiment I think we have a clear case of this kind of thing. It's impossible to resolve on any clear-cut and decisive logical grounds what the true source of the effects of this experiment are. I must say that I personally feel an almost overpowering compulsion to suppose that it's you, Chuck. One reason is that it seems to me entirely implausible that mind influence could be distributed in this kind of spatial way. For example, let us suppose that I were talking to Dr. Bigu; as I approach him might my mental processes begin to interact with his in such a way as to confuse their normal operation? Or, for example, in the kind of experiments that Graham Watkins did with anesthetized mice, where the mice were lying side by side on a platform—why should the intentions of the subject not have affected both mice equally? It just seems to me implausible that an intentional effect of this sort could be distributed spatially. I would much rather see you as the subject.

Also, I think it's implausible in more specific ways. For example, in your experiment, the suggestion is made that when a person is successfully willing something, he's more likely to have this kind of a field effect in the neighborhood. If there were to be any such effects, I would almost have expected exactly the opposite. It seems more likely that, if a subject is successful, he is affecting the particular system that he's trying to affect, whereas, if he's not successful, he may be just missing in a physically adjacent way. On that basis one might have

expected the scoring on the generator to occur when he's *failing* at the task.

HONORTON: Well, I certainly cannot deny the possibility that I might be a major source, or Tremmel, who was the other experimenter, but why should either of us have that effect? It's not a clear choice between a plausible and an implausible explanation. It's really a choice between several implausible possibilities and it seems to me that it's much too early, given the small amount of systematic research that's been done in this area, to suggest one or the other interpretation with regard to specificity here. There haven't been any studies to my knowledge. Maybe you know of some in which two independent generators are being monitored simultaneously, where one is being observed and the other isn't. I would say that such a study could be crucial to the interpretation of this kind of study. If there's a generalized effect, then that, I think, would tend to support the idea that we have in these random generator studies a crude detector of, for lack of a better term right now, mind influence. As far as the Watkins studies are concerned, why didn't the subjects affect both mice equally? I don't think we can say that they didn't. At least some of that work seemed to involve a differential effect where the results weren't clearly significant on the target mouse, but it was a difference between the target mouse and the control mouse that was significant.

BELOFF: I cannot let the occasion pass without wanting to congratulate Chuck Honorton on being the very first parapsychologist who has attempted experimentally to test what I would still like to call the Thouless/Wiesner theory, although he's related it much more to Eccles' recent speculations. This theory was put forward twenty years ago, but, so far as I know, no one has ever seen a way to try and test it experimentally. But I am still a bit worried about the point Ed Kelly raised and which I don't think you've quite met yet, which is why—and I'm sure Eccles would have difficulty seeing this sort of inference drawn from his theory—if we are successfully controlling our brain rhythms, we should or should need to influence some external physical system. I can't quite see how you make this deduction from the theory. Perhaps you could elaborate on this more clearly.

HONORTON: I don't have a good answer to that. The best that I have at the moment is that the random source is in circuit here. It's been gated by the subject's meeting or not meeting the EEG conditions and may therefore be more likely to show some peripheral influence than it would otherwise. What I'm trying to do here, and it's admittedly very crude, is simply to start seeing how we might apply some of the psi

research methodology to this question and begin some empirical work where previously it has been entirely speculative. I don't have any tremendous investment in the Eccles' type of interpretation of these effects, and it could be just as valuable or important if it turned out to be an intrinsic relationship between psi and EEG activity. In the second experiment, the effect is isolated very clearly to the condition in which the subjects are given feedback and are successful in meeting the feedback conditions. So there seems to be something about feedback which is not contingent on the random source here that is important.

PRIBRAM: You said the first experiment, suggested a plausible mechanism of interaction and I wondered what that plausible mechanism is.

HONORTON: Well, maybe it's plausible only to me. I think that for all its vagueness and speculative quality, what intrigues me about Eccles' speculation is that he's suggesting here a way in which the phenomena that we in parapsychology are studying might have a natural home. That is, psi phenomena are characterized in the way that we've been studying them, by their sporadic nature, and if it were to turn out that a primary function of psi phenomena is to provide a means of communicating back and forth between brain and mind—whatever mind is—that would provide a plausible explanation, from the parapsychological end, at least, in terms of making psi phenomena fit in.

PRIBRAM: That's not a plausible mechanism.

HONORTON: No, it's not a mechanism.

PRIBRAM: You said plausible mechanism.

HONORTON: I will take that back.

PRIBRAM: Or even a possible explanation, if you will. What's the explanation? You're simply saying Eccles has "mind" wandering about the association cortex and this somehow relates to what you're saying. I don't see the connection. By the way, that's the only thing concerning what you said today about which I have some reservations. I think there is an explanation which I'll present tomorrow.

HONORTON: What are you asking for in terms of explanation?

PRIBRAM: Well, an explanation of synchronicity; an explanation in terms of reasons.

HONORTON; Perhaps explanation is too strong. It seems to me to be a plausible possibility given the fact that we do have good evidence that

human beings are able in some anomalous way to interact with remote physical devices.

PRIBRAM: We know that we can interact with physical devices. I can write a computer program, and that computer program can then manipulate the switches in the machine. That isn't the problem. The problem is that in your experiments we don't know the "connection" between the events. Somehow the connection seems mysterious and unexplained. This, not the fact the events occurred, is what produces doubt in the scientific community. Thus, I repeat—it's the "connection" between events that remains mysterious.

HONORTON: It clearly is at this point.

PRIBRAM: Then you don't have a plausible explanation.

HONORTON: O.K., I don't have a plausible explanation.

EHRENWALD: Thank you. I think we have now witnessed the enormous philosophical or epistemological difficulties into which we are running when we try to account for certain parapsychological phenomena. I advisedly say "account," because I get cold feet at the thought of "explanations." I think we can find a way out of this if we try to make, first of all, two propositions. One proposition would be (and this is perhaps in response to Dr. Dixon's remark) that psi does not happen between two machines—it cannot, because unless there is, sooner or later, a human observer to register the fact that something happened which was not just random, independent of any human being, no statement can be made. When psi happens, it invariably is predicated on the involvement of the human factor. Now this leads to a nearly unmanageable source of error in every parapsychological experiment. I called attention to it many years ago when I talked about para-experimental telepathy, telepathic leakage, para-experimental PK or doctrinal compliance. This was later on taken up by Robert Rosenthal's description of observer bias influencing even normal psychological experiments. We know today in the parapsychological setting, the experimenter's expectations invariably have an effect. It is one of our main concerns to find out how much the observed effect is due to experimenter expectations and how much is due to other intrinsic factors which are independent of the observer. As far as the continuity or the connection between my ability to influence my EEG pattern and influencing an object "out there" is concerned, I have tried to make allowance for this fact by proposing my Extension Hypothesis, leaning closely on the theories of Thouless and Wiesner. I submit that we have gotten so used to the fact that my "selfconscious mind" can

influence my body and my brain that we have stopped wondering about it. Honorton's experiments redirect our attention to it. They are striking illustrations of the extension hypothesis. Unfortunately, we cannot account for them in strictly mechanistic terms. Nor can Karl Popper's reference to what he calls "promissory materialism."

TART: First, there is the point raised by Dr. Kelly about the specificity of psi. I think that we should remember that, when we ask someone to do a psi task, we're asking someone to basically do a "miracle" that he's had no practice on, and he doesn't really know what to do, so it shouldn't be surprising if the result comes out rather non-specifically. If we look at an infant first attempting to move something, we get a lot of crude movements rather than the controlled motor skill we're used to later. I think there's a lot of evidence that indicates that psi is often used in a way that's poorly focused, that's not very controlled.

Second, I think Chuck should use PK even more strongly as an *explanation*, and not simply as a paradoxical thing we can observe. I think saying that we use PK to influence brain mechanisms is not really a different order of statement than saying the reason we have light coming out of these ceiling fixtures is because we have "electricity." In either case we have an invisible something that we never observe directly. We see effects and we postulate something behind them. I'll go out on a limb tomorrow when I give my paper and use PK in a much more positive sense rather than simply as experimental anomalies.

Third, on Dr. Dixon's question about whether two inanimate systems might use something equivalent to psi. I've been associating with a lot of physicists lately and their world view is so totally weird and unusual that parapsychological phenomena seem much more commonplace to me! One effect that has intrigued me very much lately that sounds exactly like psychic effects between two inanimate systems is an experiment based on the Einstein-Podolsky-Rosen paradox. As I understand it, you generate two particles that move apart at the speed of light and have absolutely no connection; you perturb one and the perturbation affects the other one. Now, that sounds to me like a classic PK experiment where you totally isolate two physical systems and then show an effect that's non-explainable in terms of current explanatory systems. My physicist friends assure me this is not hypothetical; the experiment has been done, and works this way. Naturally, they simply now try to modify the explanatory system, but it sounds very much like a classical PK experiment, ignoring the later effect of the experimenter in making the experiment happen. Of course, the very fact that we, as animate systems, ask the question, immediately involves us in it.

SMALL: To go back to the specific experimental design that you were talking about, if I understand you correctly, it seems to me that there are two things involved. One would be the direct application of Eccles' hypothesis and a question of volition—in other words, the intention of the subject to create that effect. The other thing would be feedback, and I'm not sure whether that's clearly distinguished here. The test of whether the feedback is necessary seems to me to relate to Helmut Schmidt's idea that in order to get psi, there has to be feedback involved. On the other hand, it seems to me Eccles' idea would have implied that only intention was necessary without necessarily the feedback. Can you comment on that?

HONORTON: I think Schmidt has either severely modified or has abandoned his feedback postulate because there have been a number of studies in which there is no momentary feedback to the random source. That's the kind of feedback Schmidt is talking about. The kind of feedback that was given in these experiments was to the subject's EEG activity, not to the random source. The random source was being gated on the basis of whether or not the subject was meeting the prescribed EEG conditions, but there was no feedback from the random source except, of course, to the experimenter who took down the data, and is an alternate possible psi source here. I said that there are many methodological problems here defining volition in a way that is empirically useful. That is a very big problem. If anybody has any suggestions on how to do that better, I'd appreciate hearing about it. The subjects' task was to keep the tone on and to the extent that they kept the tone on we defined that as satisfying the volitional condition of the experiment.

KELLY: Despite my earlier critical remarks, I fundamentally admire these experiments, and just wanted to raise a question about their interpretation. I think, particularly, it's encouraging the way you're going about developing it. Clearly you're focusing in on the relevant aspects of the proposal, but I'd also like to suggest there might be other ways to go about the same kind of thing. It seems to me, for example, to flow much more directly from the theory, that there would be certain kinds of systems that should be much easier to affect. In particular, systems that become more neural-like in their structure might be easier to affect. Majority vote systems, for example, have a distant connection to neural networks. You can think of a majority vote as a particular kind of decision structure. You can also loosen the requirements on that structure to have more neural-like patterns of connectivity among the elements that you're deciding with, and, of course, the ultimate

generalization of that would be to work on brains themselves. It might be by doing allo-biofeedback experiments such as William Braud has done, but doing them with EEGs or something of that sort. I also believe it's now technically possible to maintain fairly sizeable clumps of neural tissue *ex vitro*, as it were, and have them functioning. That might constitute a particularly interesting system to try a PK experiment with. Of course Eccles himself might counter negative results by retreating further into his notion of "openness" to liaison.

HONORTON: Yes, we're also thinking about a possible parapsychological version of the Kornhuber experiment where the subject, at his own discretion, just wiggling his finger, gates the random generator, to see whether there is a specific PK effect associated with that kind of volitional act. In closing, my own enthusiasm here, which is quite strong, is not toward any data that we have, but the potential of some of our psi research methodology for making a positive contribution to help develop an empirical approach to some of these problems. I believe that had Eccles and Popper in their book and Penfield in his made use of parapsychological data, they would have a much stronger case.