TECHNOLOGY:
A MIXED BLESSING FOR MODERN PSI RESEARCH

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Introduction

Technology is sometimes blamed for society's ills. Water and air pollution, the threat of nuclear destruction, and the greenhouse effect (to name just a few) are considered to be the result of technological advances. Of course, technology itself is not inherently evil; rather, difficulties such as these result from our misuse of it.

Subtle difficulties, in fact, arise with the use of technology. As we become more dependant upon it, we risk losing basic knowledge about the world. For example, who can remember how to hand-compute the square root of a number, now that we have calculators? As we rely more on the expertise of others (in this case, the individual who programmed the square root function), we become dependant upon their view of reality and lose the ability to make independent judgements. It is all too easy to take as fact the answers our technology provides.

Having warned against some of the pitfalls of technology, we consider some of its benefits. When carefully applied, our technology has enabled us to make advancements across most of human experience. In the physical sciences, our rapid increase in understanding has resulted primarily from an accelerated growth of technology. In the behavioral sciences, the single most important technological contribution has been the invention of the computer. Fifty years ago, handling large databases and computing intricate analyses was nearly impossible: now our microcomputers do it with ease. Complex statistical analyses such as ANOVA and MANOVA can be performed with a simple push of a return key.

In this paper, while we provide a brief overview of two examples in psi research where the reliance upon experts and technology have led us momentarily astray, the primary focus is on a technologically sophisticated experiment to explore the effects of feedback in a remote viewing (RV) experiment.

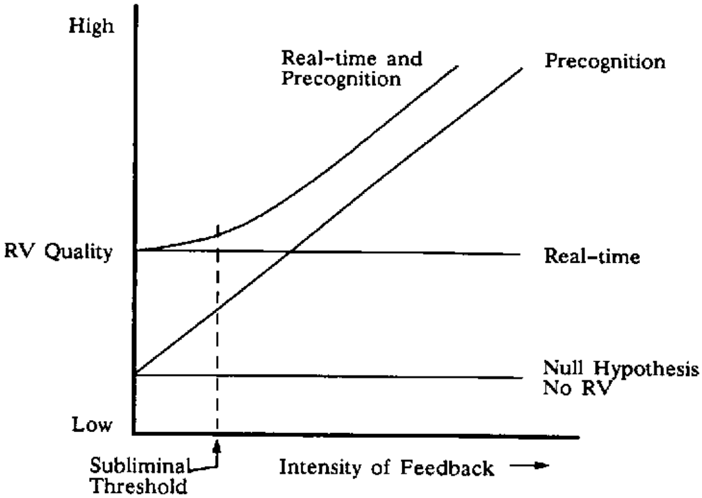

Figure 1. Idealized curves of potential relationships between RV quality and intensity of feedback

Geomagnetic Effects on Psi Performance

In recent parapsychological literature, researchers have shown considerable interest in observed correlations between psi performance and the geomagnetic field (GMF) indices (Adams, 1985; Persinger, 1977, 1979, 1985a, 1985b, 1985c, 1985d, 1986). Providing a complete analysis of this literature is beyond the scope of this paper, but a description can be found in Hubbard and May's 1986 paper. They described the origin of the GMF measurements, the hardware involved and the reporting practices of the National Oceanic and Atmospheric Administration and showed the distributions for the *ap* and *aa* indices.

If one simply used the *ap* or the *aa* index and attempted to compute correlations with a psi performance measure by standard statistics (e.g. ANOVA), it would be possible to underestimate residual variances because of the important underlying structure in the GMF. In a test sample, Forbush et al. showed that p-value (computed with standard ANOVA) of 10^{-15} increased to 0.33 when the correct residual variance was used (Forbush, Pomerantz, Duggal, & Tsao, 1983). Hubbard and May called attention to the extremely low frequency (ELF), ultralow frequency (ULF), and GMF literature, which demonstrates that blind reliance upon the GMF indices ignores the contribution from local sources, and ignores the strong spatial dependences (coherence is less than 25% 600 km away from a measuring station).

We do not mean to imply that technically difficult problems should not be addressed. In a recent paper, Persinger specifically examined these potential problems and demonstrated that a significant correlation may indeed exist between the GMF and psi performance—a very important result, if true (M. A. Persinger, personal communication, August 1988). But even in his latest paper, a problem may remain with the statistics. While MANOVA can deal with the statistically dependant data points, it assumes that the covariance matrix of the data set is stationary (i.e., the variances and covariances do not depend upon when the data sample was measured). In brain wave data this assumption is completely false, but for GMF data over a few days, it *may* be valid. The point is that the vast literature and considerable expertise available to psi researchers must be utilized before we can begin to contribute to the general research literature. It is simply a mistake to find published GMF indices and calculate various quantities with ANOVA to search for correlations with psi performance—a very tempting thing considering how easy it is to accomplish.

Observation Theories

The observation theories assume that room-temperature macroscopic (i.e., $>10^{23}$ atoms) objects are governed by the formalism of quantum mechanics. In particular, these bodies can exist in indefinite states (i.e, not in any of their allowed states). In other words, macroscopic bodies, prior to observation, exist only as a set of possibilities rather than as unobserved actualities. As in the GMF case, it is beyond the scope of this paper to describe the variations on this theme that constitute the observation theories; a broad overview and references to specific papers can be found in Edge, Morris, Rush, and Palmer (1986).

Current quantum mechanical formalism does not prohibit macroscopic superposition, but there is substantial evidence against the idea. It is true that some quantum mechanical effects can seem macroscopically (e.g., the single-photon interferometer, tunneling) but they are, in fact, manifestations of single quantum events rather than a phase-related macroscopic phenomenon. Phase-related events are required before macroscopic indefinite states can be observed. Washburn and Webb (1986) and Chakravarty (1980) have demonstrated true macroscopic phenomena, but under exceptional circumstances. Cooled to 0.050 degrees above absolute zero, 10^5 atoms maintained quantum coherence, and thus Washburn and Webb and Chakravarty were able to prepare systems in indefinite states. These states decay rapidly when the temperature is slightly increased. The implication is that quantum coherence is completely lost even at a few tenths of a degree above zero. Since coherence is required before a body can exhibit truly macroscopic quantum effects, saying that room-temperature devices can exist in indefinite states contradicts experimental results.

Why this is so, is well understood. The quantum mechanical mathematical description of a macroscopic body has on the order of 10^{23} terms and each term has it own coefficient. There are no observed indefinite states of macroscopic bodies because, at room temperature, the relative phases of the coefficients are random. Even so, quantum mechanics does not *prohibit* macroscopic bodies from being in indefinite states. But the experimental evidence, so far, does not support the idea.

The observation theories are based upon an incorrect assumption. Room-temperature macroscopic systems are not in indefinite states. They cannot "collapse" under observation by humans, fish or any other forms of consciousness. Random number generators, ROM chips, computers, and so forth are not indefinite states. They may be in *unknown* definite states, but they are definite, nonetheless. A ROM-chip bit is either 1 or 0, but not both even if no one looks.

In this example, misuse of technology, per se, was not responsible for the error. Rather it was the reliance upon a few experts in quantum theory. We are not suggesting that we should refrain from speculation using unsubstantiated theories. In fact, one might argue that we are obligated to speculate, given the nature of psi data. But we must understand the orthodoxy in detail before we can refute it.

Feedback Dependency Experiment

Beginning in 1986, SRI conducted a 2-year investigation of the dependency of RV quality upon feedback.[3] The experiment was conceptually quite simple, but to address precognitive issues it became technologically complex. In addition to the feedback question, we were interested in determining from what time frame a viewer accessed a target.

Conceptual Description. During the feedback portion of a RV session, the viewer is usually presented with a complete description of the target material and participates in a complete debriefing of the RV experience. In our experiment we eliminated all discussion of the target material and presented the feedback tachistoscopically. The intensities varied

[3] We would like to thank Dr. T. Piantanida for his valuble assistance with the psychophysics and visual details in this experiment.

from zero to a level that just exceeded recognition threshold. Extreme care was taken in order to insure that the viewer was the only individual who was simultaneously aware of both the target and the response.

Figure 1 shows a number of potential feedback dependencies. If a viewer acquires information about the target from the future feedback experience, then one might expect the relationship labeled as "Precognition." Likewise, if the information is acquired in real-time, then there should not be a dependency upon feedback ("Real-time" curve).

One important implicit assumption must be true before the various models shown in Figure 1 can be valid. Namely, the feedback experience is assumed to be proportional to the *cognitive* awareness of the feedback material. Under this assumption, the amount of information available at feedback time constitutes the independent variable.

Detailed Description-Calibration. The crucial independent variable is the amount of feedback perceived by the viewer. We assume that the magnitude of the feedback is directly proportional to the duration of the viewer's exposure for a given level of luminance. In a calibration experiment, subjects were presented with slides and asked to say when they were aware of the presentation. We manipulated the magnitude of the feedback from zero to a value where the viewer could recognize the gestalt of a scene. Each feedback slide was presented for 50 microseconds (ms), and the magnitude of the feedback information was adjusted by attenuating the luminance of the feedback slides over a range of two logarithmic units. In adjusting the magnitude of the feedback, we relied upon Bloch's Law, which says that for presentation times shorter than about 100 ms, the product of time and intensity is constant (Marks, 1975). Thus, varying the luminance of the feedback slide is equivalent to varying its duration.

For luminance calibration, the tachistoscope was loaded with 80 photographic slides (5 opaque and 75 having various luminance contrasts) of natural and man-made scenes (photographs from *National Geographic*) randomly chosen from a larger pool of 400. We varied the luminance contrast of the slides by duplicating them at one of twelve f-stops (including 0) to provide a target pool having variations in intensity covering two logarithmic units. The contrast in luminance for each slide, which may be considered to be the ratio of the brightest to the darkest part of the slide, was further attenuated in pilot trials so that some of the slides were above and others below the observer's detection threshold.

The 75 feedback slides and five opaque slides were back-projected by a Gerbrands G1170 two-field projection tachistoscope onto a 14-inch-square frosted glass window. The tachistoscope was programmed to present each feedback slide in numerical order for 50 ms, followed by a 5-second pause during which the next slide was cycled into position. Slides were attenuated by projecting them through a pair of plane polarizers: one fixed and the other variable. The luminance of the projected image varied as the cosine of the angle between the two polarizers.

Two naive female subjects participated in the calibration. A complete data set was obtained from one subject, and data trends were confirmed by the second subject.

The calibration procedures were as follows. The subject was seated approximately three feet from the projection screen, which was positioned at eye level in the wall between the room in which the apparatus was housed and the room in which the subject sat. The subject was permitted to view the screen and the other contents of the room freely for several minutes to ensure that she adapted to the ambiant illumination level. To screen the sounds of the tachistoscope, the subject listened to white noise through earphones. The response was registered by a foot switch that the subject pressed to indicate detection of the feedback slide. In a typical session, the variable polarizer was set at a predetermined value and each of the 80 slides was presented 5 times. Two sessions were conducted at each polarizer setting, providing ten data points per slide per polarizer setting. An alternative procedure was used when the variable polarizer was set near one of the extremes of the experimental range. (Under the extreme conditions, the subject saw nearly all of the slides or very few of them.) To reduce the tedium, only those slides near the detection threshold were presented.

Each time a new slide was presented, the subject reported whether the presentation was detected. Counters recorded whether a particular slide was detected as well as the proportion of slides detected. From these records, a psychometric function was generated relating the proportion of time each slide was detected to the contrast in luminance for that slide. This function, which relates the contrast in luminance for the slide to its detection threshold, is an index of the detectability of the geographic scene depicted in the slide. By using this psychometric function, it is possible to specify not only which slides are subliminal (i.e., never detected), but also how far above or below the detection threshold each slide lies.

Figure 2 shows a series of six psychometric curves generated by plotting the probability of detecting a given feedback slide as a function of the variable polarizer setting. The magnitude of target slide information was estimated from a psychometric function relating target slide contrast as abscissae and target slide detectability as ordinates.

Normally, data would be collected from a larger sample of individuals in order to arrive at an average function, but in this experiment, data from two persons were sufficient for several reasons. First, pilot studies indicated that interperson variability of target slide detection was quite low. Second, to collapse interperson variability even further, we generated a steep psychometric curve by sampling the abscissae coarsely. For example, if we sampled target slide contrast at only two values—0 and 100 percent contrasts—all observers would respond identically, thus eliminating interperson variation. In this study, we sampled target contrast at intervals that were found in pilot studies to produce low interperson variability. Finally, for the purposes of this study, interperson variability was not significant because it only shifts the psychometric function along the abscissa by some unknown amount without changing the shape of the function. Thus, interperson variability could only result in an erroneous estimate of feedback magnitude. While these errors may influence the intercept of the function relating the dependent variable (RV performance) to feedback magnitude, the slope of RV performance versus magnitude of feedback is independent of these errors.

Detailed Description—Protocol. Forty targets (selected randomly from the pool of 200 *National Geographic* magazine photographs) were prepared into eight intensity groups of five targets each using the calibration data described above. Each intensity group represented the cognitive awareness that each viewer would experience (on the average) at feedback time. Of the eight intensities, one was zero (i.e., no feedback at all), one was below subliminal threshold (SL), one was low SL threshold (25% recognition), one was mid SL threshold (50% recognition), one was high SL threshold (75% recognition), and three were of in-

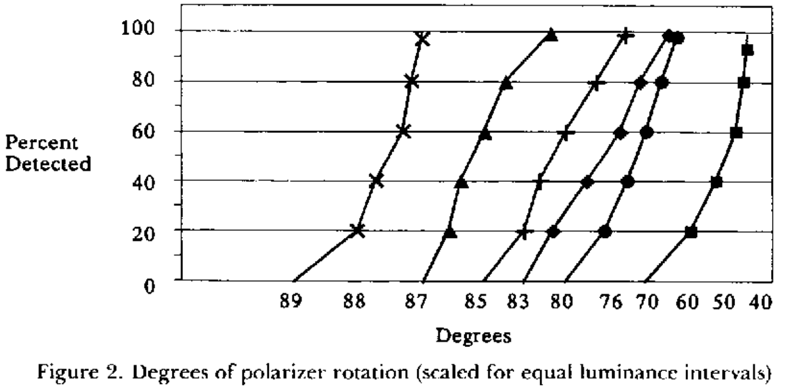

Figure 2. Degrees of polarizer rotation (scaled for equal luminance intervals)

creasing intensity above 100% recognition. The top two intensities were sufficient to experience nearly complete cognitive awareness of the feedback material. By definition, those below SL could not be cognitively sensed.

To attempt to maintain some control over precognitively available "answers," we arranged that at no future time would a response be cognitively compared to its intended target. Three pieces of information are needed to provide complete knowledge of a session: (1) the target, (2) the response and (3) the comparison between them. The target system was prepared by individuals who had no access to the responses. The RV monitor, the assistant and the viewers had no access to the targets. Finally, the analysts were never informed which were the correct results on a trial-by-trial basis.

The slide tray in the tachistoscope (the device to display the feedback material) was controlled by a computer (Sun Microsystem 3–160) in such a way that everyone was blind to target selection during a trial. For example, the tray always began and ended in the zero position. When the computer moved the tray, an independent electrical unit, which could be accessed by the computer, counted the tray steps to assure us that the intended target was displayed at the correct time.

Three experienced viewers (Viewers 009, 105, and 177) each contributed 40 trials (five at each of the eight intensity levels). A novice (Viewer 137) also contributed 40 trials.

A random order of intensities of feedback was determined (by computer) once (and differently) for each viewer prior to the start of the start of the viewer's first trial. Once the order had been set, the trials cycled through the list of intensities until the 40 trials were complete. The sequence of events for each trial was as follows:

1. A monitor and a viewer entered a laboratory that contained a table, two chairs, a computer terminal and a covered 14-inch-square frosted glass window. The window served as a projection screen for the tachistoscope in the adjacent laboratory.

2. When the viewer was ready for the session, the monitor initiated an automatic target selection program on the terminal.

3. The computer randomly selected (with replacement) a target from within the set of five for the given intensity, stepped the slide tray to that target and notified the monitor that the trial could begin. Because of the closed tachistoscope shutters, no illumination of the slide was present on the frosted screen.

4. At the conclusion of the session, the monitor collected the response and the viewer opened the screen cover in such a way as to shield the monitor from the feedback material.

5. When the viewer was ready, he or she pressed a button that initiated a single tachistoscope display of the target. One, and only one, display appeared on the translucent window screen. (Electronics prevented the viewer from receiving more feedback after the first button press.) The monitor was instructed *not* to discuss the experience with the viewers in any way at any time.

6. The monitor ended the session, and notified the control program. After the computer had returned the slide tray to zero, then, and only then, did the monitor and viewer leave the room. All target data were preserved in a computer file.

Detailed Description-Analysis. The rank-order analysis used in this experiment has been described elsewhere (Humphrey, May, & Utts, 1988), so only an overview is presented here. Using cluster analysis, all 200 targets had previously been assigned to orthogonal clusters of similar targets (i.e., every cluster of similar targets differed from every other cluster.) An assistant prepared packages (one for each viewer) consisting of all the responses randomly ordered. Next, the assistant generated a list (ordered on target number) of seven targets for each response consisting of the actual target and six decoys (a different set of seven for each response). The decoys were chosen from clusters different from each other and different from the target cluster. The decoy clusters were shown randomly from a set of 18, weighted by the number of targets in each cluster. Once a cluster was selected, the decoy was randomly selected from within the cluster. This procedure assured that all targets were equally likely to be chosen as a decoy.

The response material, and the target lists were presented to two analysts for judging. The analysts arrived at a consensus to rank order each set of seven targets for each response in accordance with the best to the worst response/target match. For each viewer, a sum-of-ranks statistic was computed for the sessions. In addition the data were plotted as RV quality (i.e, one minus the assigned rank) versus feedback intensity.

Detailed Description-Results and Discussion. Table 1 shows the sum of ranks, associated p-values and effect size for the tachistoscope feedback experiment.

Viewers 009 and 177 produced independently significant results (1-tailed). We can combine the data for all viewers in many ways, but the most conservative is a binomial calculation assuming an event probability of 0.05. Two success in four trials corresponds to an exact p-value of 0.014. A more realistic estimate is provided by a minimum p-value technique (Hedges & Olkin, 1985) which yields 1.4×10^{-4}. The

TABLE 1

Tachistoscope Feedback Experiment

	Results		
Viewer	Sum of Ranks	p-Value	Effect Size (r)
009	131	0.012	0.357
105	182	0.962	−0.281
137	159	0.484	0.006
177	104	3.5×10^{-6}	0.711

important point, however, is that this experiment produced strong evidence for an informational anomaly.

Figures 3 through 6 show RV quality (one is low, seven is high) plotted against intensity of the feedback of the four viewers. Shown also is the regression line and its associated linear correlation coefficient for each viewer. These figures should be compared to Figure 1, the idealized expectations. The result that is easiest to understand in Figure 1 is the positive correlation showing increased RV performance with increased feedback intensity. We did not observe any such correlation with either of the significant viewers. In fact, the linear correlation coefficients were not significantly different from zero.

The lack of positive correlation in the light of significant evidence of RV complicates the interpretation considerably. The most obvious conclusion is that the viewers obtained their data in real time and not from their later feedback. Another hypothesis is that the underlying

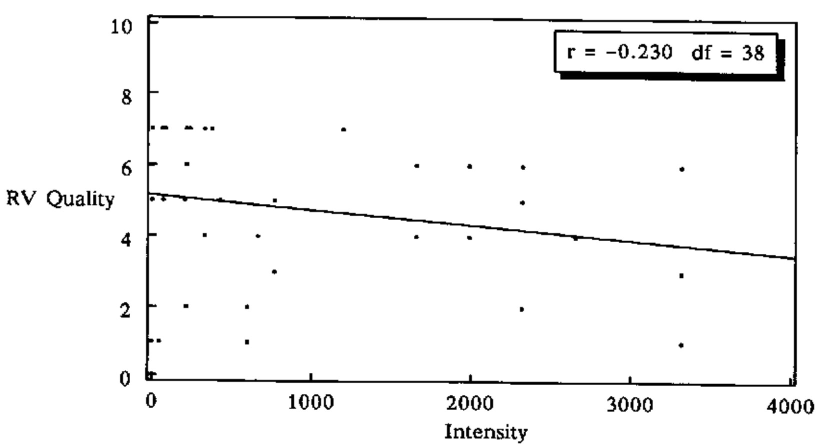

Figure 3. RV quality vs. feedback intensity: Viewer 009

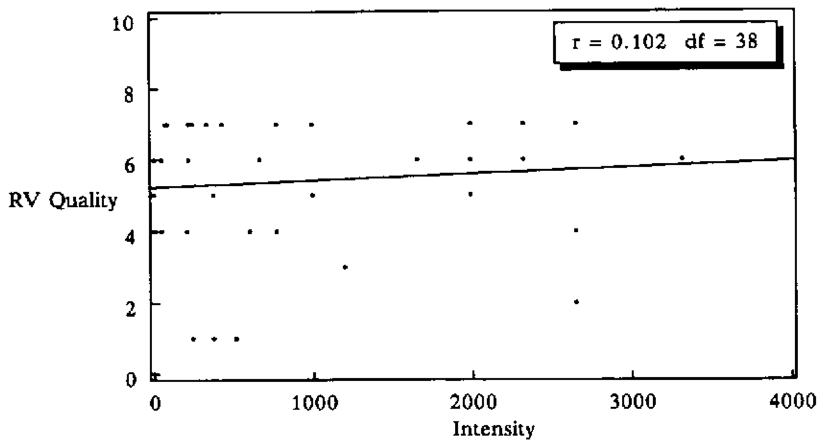

Figure 4. RV quality vs. feedback intensity: Viewer 177

assumption that the cognitive awareness constitutes feedback information is incorrect. If this were true, we would expect to see no correlation with intensity even if the precognition model were correct.

Conclusions. Modern technology, correctly applied, allows psi researchers to address questions that were difficult or impossible a few years ago. While there are certain pitfalls, technology's benefits far outweigh its drawbacks. Technology itself, though, may not provide the answer to difficult questions. In psi research, there appears to be a lack of symmetry. Had the experiment described above supported a

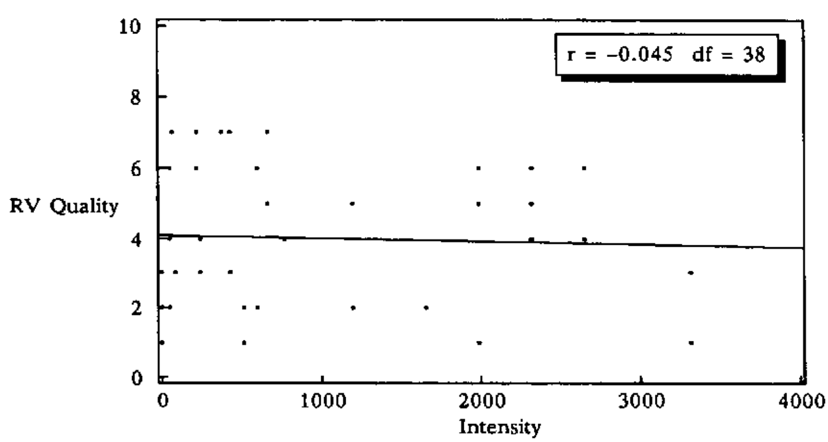

Figure 5. RV quality vs. feedback intensity: Viewer 137

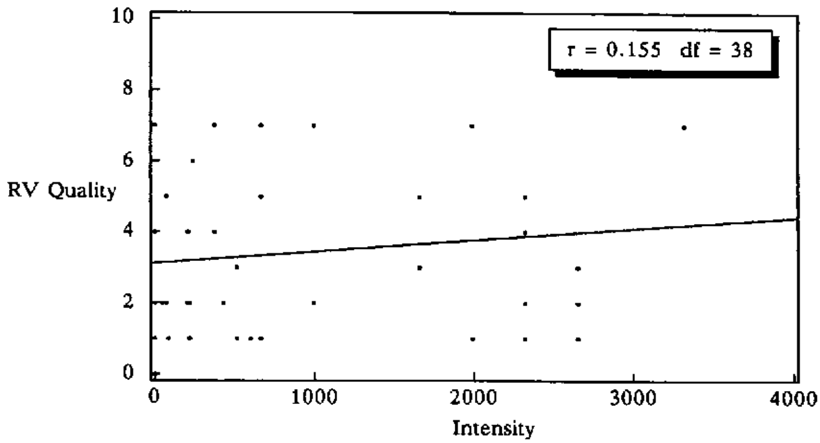

Figure 6. RV quality vs. feedback intensity: Viewer 105

precognition hypothesis, the result would have been far less ambiguous. There would have been little doubt that feedback is cognitive and that RV processes must include a precognitive component. Given that no correlation with feedback intensity existed, alternatives (to real-time) must be considered. In a broad sense, significant correlation between variables implies information about the system; therefore, it is not surprising that this conceptual asymmetry exists.

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DISCUSSION

RAO: Ed, I am very glad you were able to moralize about the possible problems with complex technology in our research. I was a student of Gandhi and shared his concern about the advance of technology to which man might become a slave. So this reminds me of that. But what scares me more is what you said in your philosophical introduction relating to PK research. You say that you spent a quarter of a million dollars to do an experiment to discover so many variables that you do not know what they mean and how to control them. You therefore shifted your research focus from PK to ESP. Now one of the research areas that excited most of us in recent years is the RNG research. Much of it was done with less sophistication than your quarter of a million dollar experiment, but we believe it has provided quite a solid piece of evidence for a facet of parapsychological phenomena. Are you suggesting that we should write off most of this evidence? Or do you believe like us that there is some validity to this kind of experimentation? If there is and it can be done less expensively, why could it not be done again?

MAY: Thank you, Ram, for pointing out to me something that I unfortunately left out of the main body of the talk. First of all to answer your question, yes, the RNG work is, I think, substantial evidence for psi. My criticism of the PK work really—although I did not say it and I apologize to you—was aimed primarily in fact exclusively at what we might call macro-PK investigations. What would be generally called micro-PK investigations involving random number generators or any system where you are basically looking at statistical differences between control and other groups, is perfectly OK. Because there you can control by looking at control groups, but if you are dealing with a high technology experiment where you are trying to, say, do a strain gauge experiment replicating Julian Isaacs' work or trying to do any large scale system, that is where the problems lie. And thank you for allowing me to correct that. That was an oversight in the presentation.

PALMER: One of the points that came to my mind as I was listening to your paper was the whole problem of attempting to rule out with one experiment all the often multitudinous explanations of a particular outcome. I think this is rarely possible, but it seems that in our research, experimenters often attempt to do that, or that is what they claim in their reports. Also, referees often demand it. Someone may do an experiment that is a contribution to knowledge, but since it does not quite rule out all the alternatives it ends up not getting into the literature. Perhaps what we need to consider is experimenters being more modest about what they claim for particular studies and to simply state out front in their discussion sections that not all of the alternative explanations have been ruled out, which should be okay as long as they have made a good faith effort to rule out the ones that are consistent with experimental competence. This approach encourages series of integrated experiments where you successively rule out the remaining interpretations. This is something I would like to see much more of in our research, a programmatic series of experiments. We really do not have anything in parapsychology comparable to the old parapsychological or psychological monographs, where a series of experiments are published together. I think our research literature would be more impressive if there were more of this kind of research being done and published.

MAY: Your point is well taken. First off I think it is maybe impossible to do an experiment where you have excluded all of the alternatives, because you do not know what all of the alternatives are to begin with. The best you can do is the best you can do. I do not mean that flippantly. You can certainly take into account the things that you know about. But one point that I tried to make in the body of the paper, but not in the presentation is that there is a certain asymmetry in the kind of research that we do—maybe in everybody's research. Had this result fallen in line with my particular bias the way that I thought it should, I think then the interpretation of it would not have been quite as cloudy. I do not know if anybody would agree. It is certainly my own speculation that you end up getting a moderately null result—and I mean by that not that you did not see any evidence for psi, but rather you did not see any evidence for psi in accordance with the model you were testing. Then you have much wider opportunity for interpretation. It is less specific than if you had a definite model in mind, tested it and it all fell right along with that model. Then you are more constrained in your analysis. So there is a certain kind of philosophical asymmetry in interpreting results, but I completely agree with you. I mean I do not consider this experiment a failure in any sense. I think it deserves to be reviewed by our colleagues and published.

SCHOUTEN: Let me first say that I am really happy that you pointed out the potential pitfalls in using high technology. But I think there is another side of the coin too. What occasionally happens is that when non-psychologists enter the field they can do things with psychological instruments like scales which are horrible.

MAY: Physics, too.

SCHOUTEN: You manipulated feedback levels and you did it by setting a threshold and I think you used two subjects to set the calibration level. It is of course known that threshold levels vary widely between subjects. Did you check with your subjects whether the feedback levels you manipulated really worked? In your paper you gave two examples where misunderstanding led to conclusions or research which perhaps has basic flaws. About the observation theory, it is supposed that only because the subject observes the outcome you can talk about it in terms of quantum mechanical processes. I always considered that very strange. I was impressed when I read your paper. And what you wrote about it. Does that mean that you consider the observation theories in that respect as invalid? And another question I have always tried to ask physicists but never got an answer to, is that as far as I know quantum physics never said that an observer could change the probabilities as described by the state vector. As I understand the observation theories maintain that probabilities would be changed due to the wishes of the observer. Is that not in contradiction to quantum mechanics?

MAY: You brought up a number of issues there. The last one could be the study of a course in quantum mechanics lasting many months which Dr. Walker would be far more qualified to teach than I. Let me take them in the order in which you gave them if I can try to remember them. First off on the variation of subliminal thresholds, an exquisitely important point, brought up a methodological problem for us. We did not want to ever show even subliminally in later tests the actual target material we used in the real study to try to determine what the individual participant's subliminal threshold was. That was a methodological issue involving precognition. So clearly that would have been a better thing to have done. On the other hand the way that we tried to address this question (and I am a little out of my surroundings here) Dr. Piantanida arranged the contrast ratios of the various slides to make the isometric curve very, very steep so that the intersubject differences in the threshold would not matter too much. Basically all it would do would be to slide the curve back and forth horizontally. Since we were not doing it across subjects, I really did not care where along the tensity access the 50 percent recognition threshold came, so we made the experiment insensitive along that line. On the quantum mechanical issue, the reason I brought the observation models in was really to point out a difficulty that both Dr. Walker and myself have and one that you have which is even worse. One of the exciting and negative aspects of doing the interdisciplinary research that we are all involved in here is that if you take a complex issue such as quantum mechanics where reasonable people can disagree on the interpretation of experiments and the interpretation of the theory, you have standing before you a number of physicists arguing with each other that you have a problem on your hands. You know I think I am right, he thinks he is right and we are trying to do experiments and working very close together with each other to try to determine some aspect of truth on that issue. I can tell you what my opinion is and note that as is well known Harris does not agree with all of this. My opinion is that at least the quantum mechanical aspects of the observation theory are silly. There is, in my view, just no evidence that observation of a large scale quantum system does anything to it. Now I can see him wincing over there, but I have given him his due. The other aspects of the observation theory, particularly from your facility, I have not frankly taken as careful a look at as I should. OK?

MORRIS: First, as I think we have discussed before, there may be a confound about the duration of exposure of the information of the feedback to the viewer. In terms of the viewer's own imagery therefore and the details and the extent of their elaboration of their own experiences, that makes it a very difficult measure ever really to apply.

MAY: Terrible, I agree.

MORRIS: Secondly, suppose the thing had worked. Then a set of alternative interpretations might have been dependent upon when and how the feedback duration condition was assigned in terms of real-time alternative interpretations. I think this is a general problem with a set of strategies whereby you vary the properties of the feedback and attribute meaning to any correlation obtained. It could be that if, in fact, the condition is determined before the person generates his imagery, then that information is then available in real time. So they will generate better protocols whenever the duration is going to be longer because that information is already available. If in fact they have already generated a protocol, then when later on the assignation condition occurs, there could be psi influences at that time lining up the condition to match the good protocol.

MAY: There was an underlying assumption in this experiment which takes up some of the points which you were making. One of the underlying questions was, first of all, what constitutes feedback? That is a question to which I have not a clue. And that is of great interest to me. What I assumed constituted feedback in this particular experiment was somehow related to the subliminal or cognitive realization of getting the answers. Well, we were at the wrong end of the spectrum for that. On account of these things you would not come away with a very profound internal cognitive experience looking at even the most robust of these feedbacks. So one of the criticisms that Piantanida has given us on this particular experiment is the violation of that particular assumption. Maybe we should just have slid the curve way over and varied the more robust aspect of the feedback. Had we done that your comments would even be more true than they are already. So it is a big question as to not only where the data come, from which is of interest to me as a physicist, but what constitutes feedback is even worse. Now just as a point we did not vary time. It turns out that if you are in this weak presentation environment in a regime, you can trade off time of presentation with intensity of presentation, holding time fixed. So our presentation was 50 milliseconds long and we varied the intensity. But it is effectively the same.

HONORTON: Ed, one of the problems that I have become increasingly concerned about in doing the kind of systematic process-oriented, free-response study that you just reported is the impact of target variability. In any kind of free-response situation like this you have a very limited number of trials per subject. To what extent did you know the success characteristics of the particular targets that were used? That would at least minimize the likelihood that you could completely throw off any systematic relationship in terms of the kinds of targets that were used.

MAY: Well, I have to say you got me! I will use that as a reason why we did not get my expected curve out of all of this. The serious answer to that question is that we have only observed preferences for some targets in a casual way and, in fact, I think your criticism is extremely valid. It really calls into question the interpretation of these kinds of results. But, since your work where you have seen variations in some of the target material you used, we carried out last year, and are continuing to carry out, investigations of the differences between dynamic and static targets. We are beginning to see some differences that are similar to what you have reported, but that is an extraordinary confound for the interpretation of this experiment.

HONORTON: I did not mean it as a criticism. I wanted to ask you what do we do for free-response experiments when we want to find out more than that there is a psi effect going on, given the likelihood that there are variations of that type?

MAY: I wish I knew the answer to that. Even though you may take a particular individual who shows a preference for a certain class of targets and design the target pool for that individual, that may not hold for your individual. It may not hold across procedures. I think the only answer must be that you must do within-subjects kinds of experimentation and not look at the global issues. At least you have some way of getting an independent measure, over a long period of time with a given individual, of how well that individual does on these targets and not on those. Maybe you can hopefully control for that condition within a subject. I would throw up my hands, thinking across subjects—too hard for me.

BRAUD: I was going to ask two questions that you essentially anticipated in your response.

MAY: See, precognition is real after all.

BRAUD: Let me ask them anyway, though. You said that the feedback was wholly unsatisfactory to the subjects.

MAY: Terrible, they complained bitterly.

BRAUD: The two questions are one, what effect do you think that so negative a factor could have had on your experiment? And secondly could it be that feedback is having an effect upon performance but that the function is non-linear and you happen to be working at a portion of the curve where you did not expect any differences?

MAY: That was one of the things that occurred to us after the fact. I am not making a big deal out of it because one of our individuals psi-missed, scoring significantly below chance. It turns out—and this I have to qualify as simply a laboratory anecdote—that this individual was the most loudly complaining person about the nature of the feedback. But one point makes not a theory. So it must somehow be connected there, but with four people you can't answer questions like that.

STANFORD: I think we are seeing another example of anticipation of questions here, so you can take another bow about precognition. William anticipated my question pretty strongly. I too was concerned about negative reactivity to low levels of feedback. This is a way of getting you off the hook about precognition, if you do not mind. Obviously it is purely *ad hoc*, but experimenters suppose that subjects are frustrated by lack of feedback and that, in some sense, they can anticipate that precognitively and so they step on the psychic gas a little bit harder. They know that there is an obstacle, that they are not getting all of that feedback. That can directly counteract the curve that you are talking about.

MAY: I do not want to leave the impression that I think this experiment proves or disproves precognition. Clearly after the meta-analysis that we learned about yesterday from Chuck there is no question, at least in my mind, that precognition is a fact of nature. I just wanted to put that in.

ROLL: One of the sections in your paper on geomagnetic effects does not, I think, take into account Michael Persinger's paper that he presented at the PA convention.

MAY: I took some swipes at the geomagnetic correlations that have been reported in the literature by a number of authors—including some of us—claiming correlations between geomagnetic activity and certain psi abilities. Talking with Michael Persinger up at the Montreal PA Meeting and reviewing his paper in detail, frankly in my view he is the only one who has really done the job reasonably well. One of the problems is if you use ANOVA to look at those data it is just a mistake. And it is a mistake because it violates at least one or possibly two underlying assumptions. Number one is that the data from point to point are not statistically independent and a procedure called MANOVA can fix that for you and Michael and others who are beginning to use that. But there is still a question, at least in my mind, because MANOVA still assumes what is called a certain degree of statistical stationarity. In other words, no matter how screwed up the data actually are as you are sampling them, it does not depend on when you sample them. That is a really rough way of describing what stationarity means, with apologies to my colleague Jessica down there. But nonetheless that condition which is a requirement for ANOVA, for MANOVA to be true may not be so strongly violated over the short period of time like seven days that he uses. But it is clearly a mistake if you are going to do any kind of brainwave analysis. Brainwave analysis is very tricky if, in fact, you want to show statistically significant differences in anything, because those data are horrible. The points are not statistically independent and they are by no means stationary. Now there are some mathematicians who can address that from turbulence and hydrodynamics and other areas in physics, who are well versed in how to deal with such crummy data, but if you want to do brainwave research to make those kinds of measurements, please, please be very careful.

BROUGHTON: I would like to make a comment and ask a question. The comment is that I am glad that you are cautioning us about the pitfalls of high technology. I just wanted to note that there are very strong echoes in what you were saying of the last Parapsychology Foundation conference in which I participated—1981—on the use of computers in parapsychology.[4] A number of us who use computers argued the same thing: that we really have to understand what we are dealing with when using a computer program in studies of psi. There is a very strong temptation to just get it off the shelf. Most of us here who work with computers have received unsolicited psi tests some of which were really appalling and made very naive mistakes. If these things go out and people do not really understand what is behind their simple computer program, we end up with some really embarrassing gaffes. On a completely unrelated topic, just to get back to the tachistoscope experiment you mentioned, I wonder if we really will get some of the answers without looking at subject differences. As we talked about this several times during the conference so far, we have been very concerned about how different subjects are going to react to our experiments and we talked about the perhaps negative aspects of the feedback. Following on Bob Morris's comments, is there any way we could really account for things like the Poetzl effect? Even though you are trying to control the conscious feedback, suppose your two subjects who did very well dreamed about the target or incorporated, for hours on end, little aspects of your target 12 hours later. I do not know how you could control it, but it might be of relevance.

MAY: Well Richard, you and I have discussed at length one aspect that I personally find extraordinarily unsettling about models based on precognition. I have a favorite one, Intuitive Data Sorting and the problem with models based on precognition is that you can look into the future and virtually anything you want to have is almost unfalsifiable and very unsatisfying. You gave one nice example of that. I can't control for that. I simply do not know how to do that. So research in a systematic way on precognitive models is exceptionally tricky, very tricky indeed. I want to make just a brief comment about your computer observation. It is not just a problem of computer neophytes learning how to use this new technology. It is a problem that spans all computer disciplines. In

[4] The 1981 conference was *Parapsychology and the Experimental Method*, edited by B. Shapin and L. Coly and published by the Parapsychology Foundation in 1982.

physics there are huge computers called CRAYS that are very, very fast and will do calculations for days on end and come out with a number which is an answer but not quite the answer. How in blazes do you know whether that number is right or not? You built the CRAY in the first place because you can't do it by hand so, do you check it? And if it is making mistakes, how do you know? Those are very serious questions. There are disciplines growing up in physics and other areas where computer models are being substituted for animal models, for example, or computer models are being substituted for nuclear explosions and the like. How in the world do you know your big fancy computer is giving you the right answer? Tricky questions, so we need not be at all apologetic to meet the same questions about psi.