LETTERS (Tart)

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LETTERS

To the Editors:

Dennis O'Brien, reviewing my recent monograph, *The Application of Learning Theory to ESP Performance* in this *Journal* (O'Brien, 1976), states that ". . . a comprehensive study of learning" (of ESP) "is long overdue." While my own studies are only a beginning compared with the extensive investigations ultimately required to look into all aspects of learning improved ESP ability, including comparing it with the learning of more conventional kinds of skills, I believe there is now excellent evidence that the typical decline of ESP ability can be eliminated, and good evidence that some subjects can learn improved ESP ability. Unfortunately, Mr. O'Brien's review will not alert most readers to this. He has argued that learning was not shown, and that the exceptionally significant amounts of ESP manifested were probably just experimenter effects. I want to reply to these points in order to further stimulate discussion of this important area, and also to correct a number of inaccuracies in Mr. O'Brien's review.

It is important to realize that we do not start from some neutral or steady performance level with ESP performance, but rather from the fact that the amount of ESP shown by subjects frequently declines in repeated guessing tests (Pratt, 1949), an observation I argue shows that extinction of ESP ability is common. I have never seen a precise estimate of just how common decline effects are, and the relative rarity of individual subject data in published reports would make its exact frequency hard to estimate, but it is certainly very common. "Incline" effects are apparently very rare, and I cannot find an index entry for incline effects in any of the parapsychology books on my shelf, although all list the decline effect.

Further, we start from a position where the absolute amount of ESP usually manifested in the laboratory is minute, even if statistically significant. In engineering terms we have a very poor signal to noise ratio in most studies. Given most parapsychologists' goal of understanding the nature of ESP by doing functional studies of it, we are in a very bad experimental position: you can't correlate anything very well when your main variable comes and goes sporadically, manifests very weakly under a huge amount of noise (chance guessing and effects of other psychological factors), and fades away as you continue to work with your subjects. I admire the persistence of investigators who keep working at finding relationships when the magnitude of the ESP in most studies is minute, but I don't expect very rapid progress from such studies.

My personal goal is to understand the nature of ESP, and technically that requires finding ways to make it manifest at a much higher level and much more reliably than is currently common. Thus any procedure which promises to greatly increase the amount and reliability of ESP calls for extended study. Whether a basic learning theory approach is applied in conjunction with other supporting factors or not is secondary to our need to make ESP stronger and more reliable. My studies of immediate feedback obviously used other factors than immediate feedback alone, so let us look at the most important one, experimenter effects.

The most basic way of defining "learning" is as an improvement in performance with repeated practice. Conversely, the most basic definition of "extinction" is a decline in performance with repeated practice, culminating in chance-level performance. Obviously other factors can also produce temporary improvements and declines in performance. While we can discuss learning in the abstract, it actually must occur in a real human being whose motivation, interest in the task, distractability, aptitude, boredom, resistance, etc., are probably continually varying and affecting his performance in addition to any possible effects of learning per se. Further, our potential learner is always interacting (to some degree) with an experimenter who shows similar kinds of variations which affect his or her interaction with our learner. J. B. Rhine (Rhine et al., 1940) long ago pointed out the importance of experimenters possessing the requisite qualities to elicit and encourage ESP in their subjects if they were to contribute anything to our field.

I have emphasized the importance of these complex properties of the experimenter and his or her interactions with the subjects for many years (see, e.g., Troffer & Tart, 1964), and consequently described these factors at considerable length in the monograph to give the readers a feeling for these complexities. Thus I was surprised at Mr. O'Brien suggesting that all the results might have been due to experimenter effects: *of course* the experimenters believed learning was possible and communicated this to the subjects. The traditional recipe for rabbit stew says "First, catch a rabbit." It is standard

operating procedure in parapsychology to be warm, encourage better performance, etc., with subjects, and this is hardly unique to my studies. Taking this as a norm in much successful ESP research, the real question becomes whether the provision of immediate feedback increases performance *given* experimenters who encourage ESP in their subjects.

Obviously manifesting or improving ESP is a delicate, easily disturbable, and at least partially nonconscious process for a subject.[1] While it might be theoretically interesting to see if immediate reinforcement per se is a strong enough treatment to eliminate extinction and produce learning using experimenters and subjects disbelieving in this possibility, I would not personally want to spend time investigating it, as my goal is producing reliable, high-level ESP performance so we can then study the nature of ESP.

Now let us consider the question of evidence for learning in my own studies and for other studies reviewed in the monograph. Mr. O'Brien feels that there was ". . . no real sign of learning in these data" because certain "sure signs of learning" were absent, viz.: ". . . intrasession invariability relative to the acquisition stage; intersession retention, indicating that something is being retained from session to session, thus increasing initial performance at each session; and finally a plateau in the curve beyond which little performance increase is seen."

Two important things should be noted about this criticism. First, it implicitly deals with a different question than I was concerned with in the monograph, viz., whether the possible learning results I obtained *were identical in nature to learning curves for known psychological functions.* This is certainly an interesting question, but it goes beyond the more basic definition of learning (increases in performance with practice) and the experimental design used in the monograph. Second, the criteria Mr. O'Brien gives for assessing learning (of conventional psychological functions) are criteria for the *terminal* stages of a learning process. Since I stated in the monograph that 20 training runs (the practical limit of what we could do at the time) was too short to do more than start a possible learning process, applying these terminal criteria is inappropriate at this stage of the research.

The basic findings presented in the monograph, which I believe were not adequately conveyed by Mr. O'Brien's review, were as follows. My application of basic learning theory to ESP performance

[1] I am preparing a theoretical article on these internal processes.

predicted that for individual subjects *who had some initial ESP ability to begin with* and who wanted to learn to improve it, the provision of immediate feedback: (1) would stabilize ESP performance (eliminate declines) in short to moderate length experiments where loss of motivation, boredom, or other psychological factors which might affect performance do not become major factors; (2) could produce learning (performance increases) in *some* subjects; and (3) the more initial ESP talent a subject had, the more the learning possible under these conditions will predominate over the extinction process in being falsely "rewarded" by being right by chance alone, so there should be a positive correlation between overall ESP ability and the slope of performance curves. All three predictions received substantial support.

By far the most solid finding of the monograph, combining my own and others' results, is the elimination of decline effects. Recall that while we do not have a precise estimate of their frequency in repeated-guessing ESP tests, they are considered common. Combining my and others' studies where data were available on individual subjects, ignoring niceties like ESP talent level, only one significantly negative slope was found for 195 subjects. To be very conservative, if *steady* ESP performance were the norm and if immediate feedback or the lack of it had no real effect at all, we would expect (using the .05 level) about five significant negative slopes and five positive ones. The actual result is one significantly negative slope and fourteen significantly positive slopes ($p < .025$ by Fisher exact test). If we modify this comparison to reflect the idea that declines are common (10%, for example), the reversal is far more dramatic.

As I stated in the monograph, I favored interpretation of my and others' results in terms of my learning theory application in order to maximally provoke discussion, a strategy which has been quite successful. If I wished to interpret the results more conservatively, I would argue that: (1) the evidence is excellent that the provision of immediate feedback generally eliminates decline effects; (2) the evidence is good that some subjects can learn better ESP performance under these conditions; and (3) the evidence is good that the degree subjects benefit from immediate feedback is proportional to the general level of their ESP ability. Insofar as the commonness of decline effects is attributed to repeated guessing without immediate feedback being an extinction procedure, as I choose to interpret it, the very strong finding of the elimination of declines further argues that the beginnings of learning occur with immediate feedback.

I must note again, as I did in the monograph, that there seems to be a persistent misunderstanding of my theory: I have never predicted that the simple provision of immediate feedback *guarantees* learning of ESP. It interacts with a subject's initial ESP ability and other psychological factors, such as general learning ability and motivation, so the predictions are those summarized above.

There are numerous minor points in Mr. O'Brien's review that call for a reply, but for the sake of brevity I shall respond to only three of the more salient ones.

In discussing Chapter 2, my review of other studies using immediate feedback as a variable, Mr. O'Brien criticizes me for citing studies with no significant overall ESP scores to support my theory. While the theory does predict that immediate feedback will not produce learning in subjects with no obvious ESP, this is obviously a very weak prediction, and these studies were reviewed primarily for the sake of completeness, and also because we might see some declines or inclines by chance alone that would be of help in establishing a baseline. I do not agree, however, with his criticism that four studies involving feedback training with closed decks (Honorton, 1970, 1971; McCollam & Honorton, 1973; Kreiman & Ivnisky, 1973) should not be counted. I pointed out in the monograph that this is not a legitimate procedure for *assessing* ESP ability because of statistical artifacts, and not the best way of providing feedback training, but these same studies showed that this procedure did produce significantly increased ESP scoring on subsequent ESP tests that did not involve feedback with closed decks. Nor do I agree that two studies with machines that were not totally fraud-proof at the time should be totally discounted, for, as discussed in the monograph, the data simply did not show the type of effect (a sudden step increase of one extra hit per run) that could be generated by misusing the machine. Other studies that did show serious methodological flaws were specifically excluded in the final integration of results in Chapter 6.

Mr. O'Brien repeatedly miscalculates the number of subjects run on the two devices in the Confirmation Study as 138, apparently by adding the sums in Table 2. It is stated several times (pp. 42, 58, 59) that each subject was routinely tested on both training devices and (p. 59) that the slight discrepancy (68 vs. 70) between the sums in Table 2 was due to a few subjects not completing testing on both machines as planned.

I can appreciate Mr. O'Brien's desire for a control group in my study, run without immediate feedback. I felt, however, that

Honorton's studies (1970, 1971; McCollam & Honorton, 1973) and Kreiman and Ivnisky's (1973) replication of Honorton's work, all employing a false feedback or a no feedback control condition, adequately demonstrated that feedback produces improved ESP performance (on confidence calls or overall ESP scores), but no feedback does not, *given conventional approaches to experimental design which assume a passive subject and that the experimenter is not part of the experiment.* I believe we now have excellent evidence, both from conventional psychological studies of experimenter bias and from Kennedy and Taddonio's (1976) recent review of experimenter effects in parapsychological research, that this is a very questionable assumption. The conventional provision of a false or no feedback control group then becomes a tricky proposition. If the experimenter knows false feedback is being given, might his knowledge alter his interaction with the subjects and consequent results? How can we be sure that a subject will not use his ESP to figure out that he is being tricked with false feedback or being put in the "less valuable" group with no feedback and react accordingly? These and related questions are very complex: since I and my students' goal was the production of high and improving ESP performance, since we took a nondeceptive, humanistic approach, and since our time was limited, we made the tactical decision to not use a no feedback or false feedback condition. I hope that someone can devise a design that gets around the experimenter knowledge and deceptive problems here.

Finally, Mr. O'Brien is impressed (as am I) with individual subjects manifesting so much ESP that they show p values like 10^{-14} or 10^{-28}, and states that "there are few parapsychologists today who can claim to have witnessed subjects scoring at levels such as these; and if the learning of psi ability requires an even higher 'threshold level,' there may be little hope of ever seeing it achieved." That few parapsychologists see scoring like this succinctly illustrates the problem that has always plagued us, the low level and unreliability of ESP. I estimated, in the monograph, that the talent threshold for the learning process to predominate over the extinction process (inherent in repeated guessing because of being correct part of the time by guessing alone) corresponded roughly to a psi-coefficient (Timm, 1973) of .1, which is indeed high for most subjects. I hope I am wrong here and that the needed talent threshold will be much lower: I would prefer that most subjects could learn improved ESP performance, but it may be that only a few can. However, I also pointed out that this threshold is not absolutely fixed, but interacts with other psychological factors, such as general learning ability and

motivational level, so skillful psychological work with such other factors may extend the possibility of learning improved ESP performance to more subjects. Note too that immediate feedback does stabilize ESP performance in a much larger number of subjects below this talent threshold.

In conclusion, I believe that we now have excellent evidence from my own and others' studies that immediate feedback can stabilize ESP performance (eliminate decline) in many subjects, and good evidence that it can result in at least the start of learning in some others. Since reliable, high-level ESP performance is one of the most important needs of parapsychology, I again reiterate Mr. O'Brien's call for a *comprehensive* study of the possibilities of learning improved ESP performance, and I hope that his review and these comments will stimulate further research.

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