most obvious criticisms of the psychological considerations. The first is my assumption of an archaic temporal association between an unexplained phenomenon that could be a poltergeist model, and a neurotic conflict. There is no scientific evidence as yet that the poltergeist reaction has as its basis infantile experiences. The second is more general and involves all of the proposed psychological explanations of psi phenomena. That is, most of our current theories of personality privilege us to witness behavior only at the level of conflict and psychic breakdown, thus forcing us to find or invent sufficient psychopathology to engender the paranormal.

JOEL L. WHITTON

21-A Pretoria Avenue
Toronto, Ontario, Canada

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ON THE REVIEW OF *The History of Psychotherapy: From Healing Magic to Encounter*

To the Editor of the *Journal:*

Authors are not usually called upon to write reviews of their own books. If they were, they would not find themselves in a position to ask for a chance to take issue with their respective reviewers. As it happens, Dr. James F. McHarg's review (October, 1977, issue of the *Journal*) of my *The History of Psychotherapy: From Healing Magic to Encounter* [New York: Aronson, 1976] is so thoughtful and perceptive that I myself could not have given the book a fairer shake. That is, up to a point. Up to the point where my comments on the Freud-Jung relationship seem to touch a raw nerve. Quoting from my text where I allude to Freud's reference to the apparent PK incidents in his (Freud's) bookcase as a "sweet delusion," McHarg apparently takes it for granted that I myself consider Jung's experience as frankly delusional. It is in this context that the word "ominous" comes in. It was Freud's, not my reaction to Jung's preoccupation with the occult. Again, as far as Jung's brush with mental illness is concerned, I have *his* word for this (in his *Memories, Dreams, Reflections*). It is his, not my gratuitous interpretation of his case.

Yet it is precisely this which has deepened my respect for Jung's clinical judgment, and my admiration for his accomplishments as a student of the human predicament. He succeeded in looking at the psychotic experience from without and from within, and he knew both the creative and the destructive aspects of psi.

A word about what Dr. McHarg describes as my "Newtonian delusion." Reviewers are under no obligation to acquaint themselves with their respective authors' other writings. Had he done so, however, Dr. McHarg would have found that, contrary to his expectations, we are in the same camp. I too am fundamentally opposed to the causal, reductive approach of materialistic science; I too am among the impassioned critics of the classical Newtonian, Euclidian, pre-Einsteinian propositions of time, space, and causality. Indeed, in earlier writings and in a recent paper[1] I go all-out to slay the seven epistemological dragons which stand in the way of a novel, non-Euclidian, post-Einsteinian, post-Freudian approach to psi phenomena and to the human mind in general, closely akin to Jungian concepts.

JAN EHRENWALD

11 East 68th Street
New York, N. Y. 10021

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[1] "Parapsychology and the Seven Dragons: A Neuropsychiatric Model of Psi Phenomena." In G. R. Schmeidler (Ed.), *Parapsychology: Its Relation to Physics, Biology, Psychology, and Psychiatry*. Metuchen, N. J.: Scarecrow Press, 1976.

COMMENTS ON THE CRITICAL EXCHANGE BETWEEN DRS. STANFORD AND TART

To the Editor of the *Journal:*

The recent critical exchange in the *Journal* between Drs. Stanford and Tart is healthy because the field of parapsychology is suffering from a lack of open criticism among its own members. As an outsider entering the field I have found that certain forms of criticism are viewed as heresy and simply are not socially acceptable. Consequently I commend Dr. Stanford's openness in his review[1] of Dr. Tart's monograph[2] and wish to make it clear that my views are much more in line with those of Stanford than with Tart's, even though it appears from Tart's "Reply"[3] that I agree with him. After over a year of informal association with Dr. Tart, I was quite surprised to

[1] R. G. Stanford, "The Application of Learning Theory to ESP Performance: A Review of Dr. C. T. Tart's Monograph." *Journal of the American Society for Psychical Research*, 1977, 71, 55–80.

[2] C. T. Tart, *The Application of Learning Theory to ESP Performance*. New York: Parapsychology Foundation, 1975.

[3] C. T. Tart, "Toward Humanistic Experimentation in Parapsychology: A Reply to Dr. Stanford's Review." *Journal of the American Society for Psychical Research*, 1977, 71, 81–101.

see references in this "Reply" (see pp. 89 and 92) to some unpublished results I had shared with him and the use of my name in the context of arguments with which I do not agree. I was not consulted before the publication of these statements. Therefore it is necessary to set the record straight or confusion as to my views will result.

In his review Stanford states:

> Most importantly, the randomicity tests reported [in the monograph] are inadequate to demonstrate the randomicity of the targets. . . . Tests of sequential dependency based upon sequences of much longer length should have been undertaken since it is reasonable to assume that some form of pattern recognition with sequences considerably longer than two items (pairs) is possible with human subjects. This is quite crucial since if there is any form of sequential patterning of targets, with immediate feedback subjects might detect this, thus invalidating the study as a test of ESP and of ESP training (p. 68).

Stanford is correct. The randomness tests used by Tart were totally inadequate. Davis and Akers[4] suggest the testing of sequences of lengths at least four to 10 times the length of the longest test series; this would be 2000 to 5000 digits for the Tart data. But Tart tested sequences of only 1000 digits, and only up to the doublet level of bias.

As early as the fall of 1975 I had pointed out to Dr. Tart that his target sequences were significantly patterned up to at least the triplet level under my information theoretic measures,[5] which have detected pattern in DNA sequences previously overlooked by classical statistics.[6] Dr. Tart steadfastly refused to believe that his subjects could have been using this pattern to inflate their scores. However, I agree with Dr. Stanford that such sequential patterning would be highly useful to the guessing strategies of subjects under immediate feedback conditions and could contribute significantly to the scoring.

In his reply to Stanford, Tart states:

> Dr. Lila Gatlin and I have done extensive tests on the randomicity of the target sequences. . . . To see whether coincidental bias matchings were actually inflating the hit scores, we matched *every* target sequence against *every* response sequence and computed a Z-score on the number of hits. There were not more significant Z-scores appearing off the intended matchings than were expected by chance, so we can reject the hypothesis that the ESP scores were inflated by the subjects' calling biases (p. 89).

Although it is true that Dr. Tart and I worked together on these analyses, I disagree sharply with the conclusion he reaches. What these tests show is that coincidental matching of the *global* bias in the target source with the *global* guessing biases of the subjects is not a sufficient explanation of the high scores. However, it in no way precludes the possibility that subjects could have uniquely matched the *local* biases of their own guess sequences to the *local* biases of their own target sequences. In this sense, the subjects' calling biases, educated by recognition of the patterns unique to their own specific target sequences, could have played a significant role.

Tart continues:

> As to subjects learning target biases and using them, there are several considerations. First, if the subjects had indeed figured out the target biases, we should have gotten much stronger inclines than were actually obtained! (p. 89).

Why? There exist all kinds of interacting factors working to raise or lower scoring rate. Perhaps the subjects' estimates of target biases were just accurate enough to cancel a decline effect.

Tart then says:

> I suspect that carrying out randomicity tests on extremely long sequences . . . of targets is irrelevant and a waste of time. . . . If it takes thousands or millions of trials to detect a statistically significant bias, is this at all relevant to a given subject who only works with a tiny fraction of that number of trials? (p. 89).

This argument is statistically misleading. One simply cannot decide if a coin is fair by tossing it once. Yet if the coin is, in fact, biased and you lose your money on one throw, this bias is certainly "relevant" to you. The reader is referred to the excellent paper on this matter by Davis and Akers.[7] Tart's argument tacitly assumes that the human mind is quite limited in its ability to exploit a statistically small bias. However, as an information theorist I believe that the human mind can do fantastic things—far more than we now believe possible—with a statistically small amount of information.

Tart then describes a naive guessing strategy based only on singlet frequencies which gave chance results and states: "I think it is safe to conclude that the possibility of the subjects learning anything of significance in this way can be dismissed" (p. 90).

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[4] J. W. Davis and C. Akers, "Randomization and Tests for Randomness." *Journal of Parapsychology*, 1974, 38, 393–407.

[5] L. L. Gatlin, *Information Theory and the Living System*. New York: Columbia University Press, 1972.

[6] L. L. Gatlin, "Evolutionary Indices." In *Proceedings of the Sixth Berkeley Symposium on Mathematical Statistics and Probability*. (Ed. by L. M. LeCam, J. Newman, and E. Scott.) Berkeley: University of California Press, 1971. Pp. 277–96.

[7] *Op. cit.*, footnote 4.

Thus ... Tart feels "safe" that since his strategy did not score significantly, no other strategies exist which can do better. But Tart is a poor strategist at this game because he believes it cannot be done. I have found an elementary guessing strategy which scores significantly with eight out of the 10 target sequences from Tart's data. For example, the following simple strategy will score significantly 20% of the time with the target sequence Tart chose for his test. *Strategy:*

1. Toss a 10-sided die.
2. Guess a monotone of the number thrown for the entire experiment. If the number 9 or 1 comes up, the player will score CR = 2.09 and CR = −4.32, respectively.

Tart states:

> Further, analyses of internal scoring patterns that Dr. Gatlin independently carried out on my data, which will be published later, show conclusively that the obtained results cannot be accounted for on the kind of discrete information transmission model that would occur from sensory cueing (or from deliberate fraud) (p. 92).

My analyses in no way "show conclusively" that sensory cueing could not have contributed significantly to the scoring rate. The "obtained results" to which Tart refers which cannot be accounted for by discrete information transmission have to do with the phenomenon of synchronicity or matching patterns in the target and guess sequences. I have already presented this theory in detail in the *Journal,*[8] and over a year ago I placed on Dr. Tart's desk a possible joint publication applying the theory explicitly to his own experimental data. I am still waiting for his response.

These matching patterns are properties of the sequences *as a whole.* For example, when we compare the target and guess sequences in all registers (other than the experimental register), we often obtain a far greater number of significant CR values than predicted by chance. It is difficult if not impossible to imagine how this could be accounted for by simple discrete information transmission as to the identity of specific target symbols. It is, of course, virtually unthinkable that it could have been devised by fraud; but the point of primary significance is that neither can it be accounted for by ESP conceived in the traditional manner as discrete information transmission regarding the identity of the target symbol being offered to the subject. The phenomenon, however, is quite easily explainable under concepts of coincidental pattern matching aided by active pattern recognition as the subjects build their guess sequences to match their own target sequences. The possibility is still open, however, that only part of the scoring rate comes from this pattern matching and the rest could come from "traditional" ESP, sensory cueing, fraud, or whatever.

[8] L. L. Gatlin, "Meaningful Information Creation: An Alternative Interpretation of the Psi Phenomenon." *Journal of the American Society for Psychical Research,* 1977, 71, 1–18.

A control experiment might have furnished extremely valuable information in sorting out the relative magnitudes of these factors. Dr. Tart's argument, as expressed in his reply to Dr. Stanford, that in his feedback studies no control experiments without feedback are necessary because the omnipotence of psi makes them impossible and meaningless is a challenge to the fundamental scientific methodological principle of minimal assumption.

It is the business of science to operate under minimal assumption. This established methodological principle demands that we run control experiments rather than simply assuming how they would have turned out or arguing about their outcomes from post-hoc analyses. When parapsychologists argue against simple fundamental scientific principles such as these for the sake of being "humanistic" or because psi can "do anything," we might as well establish a Church of Parapsychology, where everything is based on psychic revelation and assumption, and forget about experiments entirely. Not that I am arguing against churches, but rather against the confusion of these with science.

Lila L. Gatlin

Department of Psychology
Stanford University
Stanford, California 94305

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Dr. Tart's Reply to Dr. Gatlin

To the Editor of the *Journal:*

I welcome this opportunity to reply to Dr. Gatlin's comments on the exchange between Dr. Stanford and myself (January, 1977, issue of the *Journal*) as it may help to clarify some important issues with respect to my feedback research. I only regret that Dr. Gatlin did not wait until certain analyses that she knew were in progress, and which are too technical to describe fully here, had been published.

First, I apologize for unintentionally misrepresenting Dr. Gatlin's interpretations of the analyses she did on the data of my first Training Study.[1] I mentioned some of these analyses in passing, analyses

[1] C. T. Tart, *Learning to Use Extrasensory Perception.* Chicago: University of Chicago Press, 1976.

on whose i[illegible] pretations I thought we were in agreement. I had already written to Dr. Gatlin twice, offering to publish a letter of correction and apology on this point, once she had informed me of her feelings, but never received any direct reply to this offer.

The heart of Dr. Gatlin's criticisms is her contention that there was an important lack of randomicity, a patterning, in some of the target data used in the Training Study such that the percipients might have been able to figure out these patternings, and so score significantly by mathematical inference rather than by ESP. The answer to this contention involves a consideration of what is meant by randomicity.

Two meanings are generally associated with the concept of randomicity. The first is that no patternings or dependencies of any sort can be found in a sequence of random numerical data. The second is that randomicity means a *lack of predictability* of a numerical sequence: that is, given a sample of the sequence, one cannot predict subsequent numbers in the sequence with greater than chance success.

While the second meaning associated with the concept of randomicity is important for both psychological and parapsychological research, the first is false. Mathematically, one can take *any* sequence of numbers of *any* finite length, even if they have been generated by a truly random process, and find an algorithm which would deterministically generate that exact sequence of numbers. This seems to imply that the sequence of numbers was not random, but resulted deterministically from that algorithm, and thus had a pattern to it that could be detected and made use of. However, the algorithm so determined will *not* successfully predict further numbers gathered from the same random source at a level beyond chance expectancy. To put it another way, we can always find some kind of pattern in *retrospect*, a process akin to the psychological process of rationalization or projection, but that does not mean that the sequence was actually generated in that fashion or that it is predictable.

Thus the question of whether Dr. Gatlin's post-hoc analyses can find any kind of pattern (in the sense of departures from p equaling exactly one-tenth) in my target data is not really the relevant question: such patterns can be found, to varying degrees, in the data of any and every psychological and parapsychological experiment. The relevant question is whether such patternings, sequential dependencies, or biases exist in the target data to a degree strong enough to have allowed percipients in the Training Study to figure out these biases *as they went along* (not post hoc), and make use of them to boost their scores to a level high enough to make unnecessary the occurrence of ESP as an explanation.

I do not understand why Dr. Gatlin implies that I w[illegible] insensitive to this problem, as when she says "Dr. Tart steadfastly refused to believe that his subjects could have been using this pattern to inflate their scores." I have repeatedly made the above point to Dr. Gatlin, namely, that *post*dictive finding of pattern is always possible, but that it is not equivalent to finding patterning that has any *pre*dictive value.

While Dr. Gatlin states her belief that "the human mind can do fantastic things—far more than we now believe possible—with a statistically small amount of information," I do not find this statement to be a satisfactory scientific hypothesis. The ESP hypothesis is that an individual can have contact, via an unknown but nonsensory information channel, with the targets, and so increases his calling score above chance expectancy. Unless the alternative hypothesis of fantastic, apparently unconscious, computing powers capitalizing on bias patterns is stated in a mathematically specified form and then empirically tested to see how well it works, it is not a scientifically testable hypothesis.

I made an *a priori* decision to utilize the results of pre- and post-experimental randomicity tests to insure the adequacy of the electronic random number generator (RNG) used, because of the possibility of PK effects on the generator by percipients during the Training Study, and such results were satisfactory at the singlet and doublet levels. As I have never seen anyone propose a mechanism for an electronic RNG of the type I used to develop doublet or higher level sequential dependencies, it did not seem necessary to test for triplet and higher level biases. As a secondary analysis that might be useful when questions about the mechanism of ESP were asked of the data, however, I needed randomicity and other tests done on the target sequences actually used in the Training Study, so that any departures from randomicity could be compensated for in more sophisticated analyses than those done under the *a priori* decision. This led to my collaboration with Dr. Gatlin, and some of her analyses have been most helpful and stimulating.

As I mentioned in my Reply to Dr. Stanford (p. 90), Eugene Dronek of the Computer Sciences Department of the University of California at Berkeley and I have been working on the development of a computer calling program to detect patterns/biases in numerical target data, including fluctuating local biases, and we have now implemented a very powerful calling program and applied it to the target data of the Training Study. This program is a way of precisely specifying one form of an alternate hypothesis of powerful (albeit unconscious) computing powers in the individual that could capitalize on small biases, and testing this hypothesis. Further, this is a very [illegible]erful test, for we believe that our calling program is probably far more powerful than abilities we could expect in a human being: it has perfect memory for all target digits it has received up to a given trial, and perfectly categorizes, stores, and retrieves these digits at the singlet, doublet, triplet, quadruplet, quintuplet, and sextuplet levels; it accurately calculates exact binomial probabilities of all relevant events (target singlets, doublets, etc.) up to a given trial in order to precisely weigh these indications so as to use the most powerful calling strategy we have been able to devise for that particular trial; it accurately modifies and updates this strategy on every single trial, etc.

Nevertheless, while our computer calling program can score significantly on some of the target sequences from the Training Study, its scores are well below those of the percipients who scored significantly in that study. Thus, although it is conceivable that some sort of bias pattern estimation *may* have occurred, this could at best account for only a small part of the data and, we believe, leaves the bulk of the scoring and the internal relationships found attributable to ESP. We plan to submit our findings for publication in the near future.

Dronek and I do not know whether we have worked out the *most* powerful computer estimation program possible, but following publication of our results, we believe that the proper response of anyone wanting to hypothesize that the percipients in the Training Study took advantage of biases will be to devise a more powerful program and demonstrate empirically that it can account for the bulk of the data, not simply refer to mysterious, unspecified computing powers of the human mind.

The monotone guessing strategy which Dr. Gatlin devised and which she claims "scores significantly with eight out of the 10 target sequences from Tart's data" is irrelevant to the question of predictability of the target sequences, and unfortunately demonstrates a lack of understanding of the difference between *pre*diction and *post*diction. As I understand this monotone strategy (from her Letter and from an unpublished paper, "Game Theory and ESP," which she sent me for comment), it consists of looking at *all* the target data *after* they have been recorded, counting which particular target has occurred most frequently, and then scoring responses as if that most frequent target had been called on every trial! In any random sequence it is almost always the case that one particular number will come up more frequently than another, so one can almost always generate above-chance scores by this post-hoc process, but it is trivial postdiction.

To give a concrete illustration of this postdictive procedure, consider the following sequence of 25 numbers obtained by randomly entering a widely used random number table:[2]

8 7 6 1 9 2 6 9 0 1 6 0 2 8 7 9 7 4 7 6 8 6 0 6 3

If we count singlet frequencies, we find that the digit 6 has occurred most frequently (six times), and if we then use Dr. Gatlin's monotone strategy of going back and pretending we had called a six as a response to each target, we obtain six hits, with a spurious exact binomial probability of .03, one-tailed. In line with Dr. Gatlin's stress on higher order biases, if we tabulate all the doublets occurring and then go back and use the resulting table as a guide to calling trial by trial (except for the first trial, when we must guess) and always calling the most frequent doublet found in our tabulation, then, given target identity feedback on the previous trial (breaking ties randomly), we obtain 10 hits, with a spurious binomial probability of 8×10^{-5}, one-tailed. Obviously going on to the triplet and higher levels would continue to improve our score until we would get almost the entire sequence correctly.

If we work at the same number sequence more appropriately with a *pre*dictive procedure, building up singlet and doublet tables as calling guides trial by trial, the results do not differ significantly from mere chance expectation (2.5 hits). The procedure of calling the most frequent singlet to date, for example, yields only two hits. Using doublet information to date, supplemented with calling the most frequent singlet to date when no relevant doublet information is available, yields only three hits.

There are also a number of minor points in Dr. Gatlin's Letter to which I wish to respond: First, I do not understand why Dr. Gatlin makes a point of stating that the off-diagonal matching tests would deal only with global bias, as I myself explicitly stated in my Reply to Dr. Stanford (p. 89) that this was what they dealt with. I dealt with the problem of local bias separately.

Second, I object to Dr. Gatlin's description of the singlet estimator strategy described in my Reply (p. 90) as "naive." I stated there (p. 90) that it was merely a start on more extensive testing—testing which Dr. Gatlin knew that I was working on. I had also informed her as to why I considered it a reasonable start: namely, because I considered the magnitude of the biases at the doublet and triplet levels that she found in her analyses as quite small, such that they would probably not contribute much more than a singlet estimator program. Further, some of the apparent bias at the doublet

[2] H. Arkin and R. Colton. *Tables for Statisticians*. New York: Barnes and Noble, 1950.

and triplet levels seems to be a straightforward reflection of singlet biases, rather than always establishing the existence of independent higher level patternings. The empirical results that Eugene Dronek and I have obtained confirm this initial impression.

Third, I am puzzled by Dr. Gatlin's repeated implications that I have ignored her findings, as in her statement that "over a year ago I placed on Dr. Tart's desk a possible joint publication applying the theory explicitly to his own experimental data. I am still waiting for his response." Dr. Gatlin and I have had many written and oral exchanges about all of the points raised in her Letter to the Editor and about the possible joint publication she refers to. I have told Dr. Gatlin that I felt the manuscript was premature in its present form and required both further thinking to clarify the assumptions behind some of the analyses and some further analyses in order to clarify what these results meant. I regretted that other commitments kept me from giving this work as much time as I would have liked to. Nevertheless, Dr. Gatlin informed me, more than a month before submitting her Letter to the *Journal*, that she was impatient to publish, with or without my collaboration, so I told her she could go ahead and publish singly, asking only that she fully share the results of her analyses of my Training Study data with me, along the lines of procedure I have proposed to the P.A. Council for the establishment of a Parapsychological Data Bank.

Fourth, Dr. Gatlin seems to think that my points about the psychology of the experimental situation, raised in my Reply to Dr. Stanford, call for some abandonment of scientific methodology. I believe she has missed the fundamental point of that discussion. Experimental procedure in any field of science consists of manipulating a *precisely known* set of independent variables and observing what effects, if any, result on the dependent variables. My point was that what we now know about the experimental situation means that poorly specified but very important psychological variables might be altered simultaneously by the provision or lack of provision of immediate feedback: until we can specify what these variables are more precisely, simply adding a no-feedback "control" condition might not control for anything, but simply give us a confounded result. It is *not* the business of science to operate under "minimal assumption" if the assumptions clearly lead one to error by failing to recognize the complexities of the situation. I have not argued against the use of control procedures *per se*, but against the use of naive, even if standard, procedures when it is not clear what they control for.

Finally, Dr. Gatlin also takes issue with my statement (page 92) that her analysis showed that the obtained results could not be accounted for on the kind of discrete information transmission

model that would obviously follow from alternative hypotheses of sensory cueing or deliberate fraud. I was surprised by her reaction, since I thought I was quite clear on Dr. Gatlin having told me and several colleagues numerous times that the patterns she saw in the data did not fit a discrete information transfer model. This is, however, a highly technical argument, and until Dr. Gatlin has published her analyses in detail it would be premature to discuss it further.

As I mentioned earlier, the technical detail behind some of my brief comments above will be fully presented in papers that will soon be submitted for publication by Eugene Dronek and myself. In the meantime, I trust that Dr. Gatlin's comments and my brief responses to them will prove to be of interest to other researchers.

CHARLES T. TART

Department of Psychology
University of California, Davis
Davis, California 95616

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Publications Committee of the A.S.P.R.

The Publications Committee of the A.S.P.R. consists of Dr. J. G. Pratt (Division of Parapsychology, Department of Psychiatry, University of Virginia), *Chairman*, and the following members: Dr. Irvin L. Child (Department of Psychology, Yale University), Mrs. Laura A. Dale (A.S.P.R.), Dr. T. N. E. Greville (Mathematics Research Center, University of Wisconsin), Mr. Charles Honorton (Division of Parapsychology and Psychophysics, Maimonides Medical Center), Dr. Edward F. Kelly (Department of Electrical Engineering, Duke University), Dr. R. A. McConnell (Department of Biological Sciences, University of Pittsburgh), Dr. Robert L. Morris (Tutorial Program, University of California at Santa Barbara), Dr. John Palmer (John F. Kennedy University), Dr. Gertrude R. Schmeidler (Department of Psychology, City College of the City University of New York), Dr. Rex G. Stanford (Center for Parapsychological Research, Austin, Texas), Dr. James M. O. Wheatley (Department of Philosophy, University of Toronto), and Miss Rhea A. White (A.S.P.R.).

DR. GATLIN'S REPLY TO DR. TART

To the Editor of the *Journal*:

I wish to respond briefly to Dr. Tart's "Letter to the Editor" in the January, 1978, issue of the *Journal* (pp. 81–87) in which he replies to my "Comments on the Critical Exchange Between Drs. Stanford and Tart," published in the same issue (pp. 77–81).

Dr. Tart begins by defining a finite random sequence as one that is unpredictable. This is a fairly common practice, but the definition can be made much more precise and workable than this. For example, Chaitin[1] has defined a finite random sequence as one which cannot be specified by or compressed to a computer algorithm whose length in bits is less than that of the sequence. Dr. Tart states that *any* finite sequence has an algorithm that can generate it, but this algorithm will not predict further numbers from the same source. The length of the algorithm is crucial to the discussion here. If the algorithm is about the same length as the sequence, then of course it has no predictive value and the sequence is, under Chaitin's definition, random. One might as well simply transmit the sequence itself. But if the algorithm is shorter than the sequence it generates, it definitely has predictive value because of the pattern in the sequence on which the algorithm is based.

It is not possible to detect the kind of patterning I have found in the target data from Dr. Tart's first training study[2] in any arbitrarily chosen sequence from any random source. I have tested this specifically by generating random sequences on the computer and searching for the same patterns, but none were found. More significantly, I found that the guess sequences have the same general kind of patterning as the targets. No amount of post-hoc searching for patterns in sequences will yield the same pattern in two independent sets of sequences.

Dr. Tart states (in his "Letter to the Editor") that since he has never "seen anyone propose a mechanism for an electronic RNG of the type I used to develop doublet or higher level sequential dependencies, it did not seem necessary to test for triplet and higher level biases" (p. 83). Operating under the principle of minimal assumption, I did test for them. Taking all 5,000 trials in the training study as a measure of the target source and applying the

[1] G. J. Chaitin, "Randomness and Mathematical Proof." *Scientific American*, 1975, 232, 47–52.

[2] C. T. Tart, *Learning to Use ESP*. Chicago: University of Chicago Press, 1976. (See Ch. 4.)

generalized serial test of Good and Gover,[3] I obtained no significant values of $\Delta^2\psi_1^2$ and $\Delta^2\psi_2^2$, but $\Delta^2\psi_3^2 = 1072$, 810 *df*, $p < 10^{-8}$. Thus, the experimental fact is that the generator produced sequences which are highly patterned at the triplet level.

Dr. Tart also states that "some of the apparent bias of the doublet and triplet levels seems to be a straightforward reflection of singlet biases, rather than always establishing the existence of independent higher level patternings" (pp. 85–86). I assume that this statement refers to my *D*-measures since these are the broadest evidence of a generalized target patterning at *both* the doublet and triplet levels. This being the case, his statement is mathematically incorrect because the *Dn* are independent variables and, as I have defined them elsewhere,[4] they measure precisely the existence of higher level *n*-tuple patterning independent of lower level bias.

Dr. Tart believes that I do not understand the difference between prediction and postdiction. The essential element in prediction is that of *estimation*. As an example of a "postdictive procedure," he describes (pp. 84–85) an algorithm where *n*-tuple frequencies are first counted for the entire target sequence and then used as a basis for a simple Markov prediction to form a guess sequence; i.e., we guess the symbol most likely to occur, given *m* preceding symbols in the target where *m* is the memory of the Markov prediction. Let us call such strategies Post-Maximal Markov-*m* or PMM*m* strategies. Such strategies are not a model of the actual experiment. If they are interpreted this way, they are obviously trivial postdictions. The value of such models lies in their *usefulness as analytical tools*.

For example, let us use a PMM3 strategy to calculate how accurately the subjects must *estimate* the triplet bias in the target sequence to obtain the observed scores. Obviously they do not have access to *all* the information about target triplet frequencies in the early stages of the experiment; hence they must make *estimates* based on small samples. We can calculate how accurate the estimates must be. In the Tart data the CR values of the PMM3 strategies range from about 17 to 19, which is considerably higher than any observed in the experiment. If we simply divide each observed CR by the PMM3 CR, we see that the significantly scoring subjects would only have needed to make estimates with an overall accuracy of about 22% to 59%. Why is this impossible?

Dr. Tart thinks that Eugene Dronek's computer algorithm is

[3] I. J. Good and T. N. Gover, "The Generalized Serial Test and the Binary Expansion of 2." *Journal of the Royal Statistical Society, A*, 1967, 130, 102–107.

[4] L. L. Gatlin, *Information Theory and the Living System*. New York: Columbia University Press, 1972.

"probably far more powerful than abilities we could expect in a human being" (p. 84). I do not share this view. Computers cannot yet perform some of the simplest pattern recognition feats which are second nature for children. The human mind has far more powerful problem-solving abilities than a computer and is extremely ingenious in devising winning strategies in any game situation. The fact that a computer algorithm cannot duplicate the human mind quantitatively does not prove that the mind cannot outperform a computer. The human mind probably uses completely different strategies than a computer.

Dr. Tart misinterprets my monotone strategies. This is a simple *predictive* game. The game is *not* to count the singlets post hoc and deterministically construct a monotone. This is obviously trivial post-hoc prediction and Dr. Tart has set up the proverbial "straw man." The game is played by first tossing a 10-sided die which yields a *prediction*. The point of the game is that in eight out of the 10 sequences the *probability* of scoring significantly is 10% to 40%, which is well above the null level; hence a simple statistical model is no longer valid. This level of monotone success will not be found in the sequences from any random number generator.

The monotone strategies are, in essence, pattern-detecting algorithms which pick up patterns that statistical tests miss. They show that Dr. Tart's target sequences are patterned not only at the triplet level but at the singlet level as well. We tend to believe because of our conditioning that if statistical tests cannot detect patterns in finite sequences, any residual or "hidden" patterns are necessarily weak and thus of very little use to the subject toward extrachance scoring in ESP experiments. This is not the case; in some data I have analyzed, certain target sequences which were random by statistical tests displayed information-theoretic patterns containing more information useful in scoring than did other sequences which were very nonrandom statistically.

LILA L. GATLIN

Department of Psychology
Stanford University
Stanford, California 94305

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