CORRESPONDENCE (*DRAFT*)

To the Editor:

The question about the alleged elusiveness of psi phenomena is an important one and deserves extensive discussion. Kennedy's article (*JP*, **65**, 219–245) is a good starting point, but there are a number of lacunae that should be pointed out.

First of all, not every psi researcher would agree with the position that psi is elusive and weak. For instance, Utts (1996) in her assessment of the Stargate remote viewing (RV) research claimed that this line of research shows great stability over time. May (1996; personal communication, June 28, 1996), who was responsible for a large part of this research, supported her assessment. The same can be said for the effect size, which, according to another review by Utts (1991), is not weak at all but can be characterized as moderate (in the order of 0.2).

Thus, when proposing a model, as Kennedy did, to account for the general elusiveness and weakness of psi phenomena, it would be a good idea to also take into account the apparent exceptions to the general rule (see also Bierman, 1980, about negative reliability).

I welcome Kennedy's position to view the elusive nature of psi not as an obstacle to overcome but as an intrinsic aspect of psi, although I am not sure why that would follow from a correspondence principle. One should realize, however, that in order to study (rather than to avoid) the elusiveness, it could be that repeating the same (boring) ganzfeld experiment is a *good* idea. The general attitude in the psi community, however, is that we should not stick to the same paradigm all the time. Changing a paradigm has even been proposed as a way to "overcome" the elusiveness. Following this suggestion, the proponents of changing paradigms would never learn what the dynamics are behind the alleged elusiveness.

Kennedy discussed a number of hypotheses that would account for the observed unreliable and weak character of psi. These correspond largely and overlap to some degree with the hypotheses I put forward in my invited contribution at the PA convention in 2000 under the title "Descartes Error? The Nature of Psi and the Relation Between the Subjective and Objective World" (published, slightly modified, in *The Physical Nature of Consciousness*; Bierman, 2001). In this contribution, which apparently escaped Kennedy's review, I discussed long-term drifts of effect sizes in several psi databases and concluded that, apparently, effect sizes can decline but also rebound over time. Skeptics would claim that this shows that we are unable to replicate studies: A significant decline implies

nonreplication of an effect size, even if the overall database is still significant, as is the case for the ganzfeld database.

If psi researchers are asked about the meaning of the findings of parapsychology, they tend to respond that the reality of psi suggests that things are connected that we thought (based on classical physics notions) were not connected. This "connectedness" transcends space but, considering the evidence for precognition, also time. If we take that kind of generally claimed meaning seriously, and why shouldn't we take our own words seriously, then it follows that replicability *cannot* be expected. Replicability assumes and requires independence of experiments. But built into psi intrinsically is a connectedness that connects experiments with each other—*not* only in a trivial way, causally forward, by variables like increased experimenter boredom, but also in a way that we cannot even conceptualize, where (aspects of) future experiments in some way have a relation with current experiments.

This is not the place to comment on each of the hypotheses put forward by Kennedy. Most of these hypotheses are just "not crazy enough." I focus on the hypothesis that the observational models (Millar, 1978) might provide a framework to understand the elusive character of psi. And I would like to add von Lucadou's (1990) system theoretical approach to account for elusiveness. I will not discuss Palikari's balancing model, although it might also be a relevant framework.

Under the title "Psi Is Influenced by Many or All of the People Who Are Interested in the Potential Results," Kennedy discussed the observational theories as explaining elusiveness. He based his analysis on two sets of data: checker effects and effects on prerecorded targets (for a quantitative review of these, see Bierman, 1996).

It can be concluded from these data that

a. Experiments are not finished at the time that an experimental psychologist would say that the experiments are finished.
b. This time extends beyond the first observation of the results.

Kennedy incorrectly suggested that "many or all people who are interested" (p. 234) are involved. From the experimental results, this cannot be concluded. The observational theories come in many flavors (Millar, 1978), and only one claims that all future observers matter to the same degree. It has been shown mathematically that this leads to a divergence problem (Millar & Hartwell, 1979), and Kennedy referred to this problem by stating "the hypothesis of backward influence does not explain why the net integrated psi effect would make psi elusive rather than completely suppressed or enhanced" (p. 235). But, as argued above, such a complete suppression or enhancement only occurs if there is no limit to the number of relevant observers.

If we take a bit more abstract point of view by interpreting unreliability as a consequence of error variance due to uncontrolled variables, then it

seems to me that the fact that we generally do not control future variables in our experiments could account easily for the apparent unreliability.

It is interesting that, within the framework of the observational theories, error variance ought to decrease in cases in which the future of the data is better controlled. This is where the apparent stability of the Stargate RV experiments enters the picture. These experiments were largely classified and followed a very strict protocol before the results could be released.

It can be shown easily by Monte Carlo methods that, given a mean effect size around 0.2 (averaged over widely varying individual outcomes of, let's say, 32 sessions), an associative RV experiment using the roulette as a random number generator determining the target would result nearly always in a riskless profit in the long run (using a safe betting scheme). The fact that since the end of the Stargate episode we have not seen the use of the claimed stable effect size in this way suggests to me that this effect size, once outside the closed environment of classified research, cannot be trusted to be as stable as inside this environment. This supports to some degree the observational theoretical "explanation" of elusiveness.

In his analysis of Complex Meaningful Information processing systems, von Lucadou (1994) stumbled on a remarkable similarity between the descriptive formalism and the well-known quantum formalism. The observational theories as discussed above had their roots in an analogy with quantum systems (Walker, 1975, 1985), while this new theory turned out to be similar to quantum theory without explicitly starting from it. In von Lucadou's systems theory, one gets psi as an equivalent of nonlocality in quantum theory. According to von Lucadou, there is no (classical) information transfer, but correlations arise. Once one tries to *use* the information, the correlations disappear, just as they disappear if one tries to use quantum nonlocality to transmit classical signals. In quantum systems, this remarkable effect occurs as soon as one creates the possibility of using the nonlocality for information transfer, even without actually doing so. This is very awkward and really looks like psi elusiveness.

That we still find correlations in psi experiments can be accounted for if one realizes that, in most cases (with some exceptions; see for example Carpenter, 1991), we do not manipulate the "source or target" information, but we let a random decision determine what the source information is. In this situation, meaningful information transfer is impossible, because the random process deprives the information of meaning (if one defines meaning as the aspect of information that allows one to act upon it).

A fuller incorporation of the aforementioned frameworks into Kennedy's arguments would probably have resulted in a slightly different model and, more importantly, in slightly different suggestions for future research. With these comments, I would like to point future psi researchers, especially those with a physics background, to further explore the

abovementioned theories as a possible explanation for the elusive character of psi.

References

Bierman, D. J. (1980). Negative reliability: The ignored rule [Abstract]. In William G. Roll and John Beloff (Eds.), *Research in parapsychology 1980* (pp. 14–15). Metuchen, NJ: Scarecrow Press.

Bierman, D. J. (1996). Do Psi Phenomena Suggest Radical Dualism? In: Stuart R. Hameroff, Alfred W. Kaszniak, and Alwyn C. Scott (Eds.) *Toward a Science of Consciousness II* (pp. 709-714). MIT Press, Cambridge, MA.

Bierman, D. J. (2001). On the nature of anomalous phenomena: Another reality between the world of subjective consciousness and the objective world of physics? In Ph. van Loocke (Ed.), *The physical nature of consciousness* (pp. 269–292). New York: Benjamins.

Carpenter, J. (1991) Prediction of forced-choice ESP performance: III. Three attempts to retrieve coded information using mood reports and a repeated-guessing technique.*JP.* **55**, 227-280**.**

Kennedy, J.E. (2001). Why is psi so elusive? A review and proposed model. *JP,* **65**, 219–245.

May, E. (1996). The American Institutes for Research review of the Department of Defense's STAR GATE program: A commentary. *Journal of Scientific Exploration*, **10**, 89–107.

Millar, B. (1978). The observational theories: A primer. *European Journal of Parapsychology*, **2–3**, 304–332.

Millar, B., & Hartwell, J. (1979). Dealing with divergence [Abstract]. In W. G. Roll (Ed.), *Research in parapsychology 1978* (pp. 91-93). Metuchen, NJ: Scarecrow Press.

Utts, J. (1991). Replication and meta-analysis in parapsychology. *Statistical Science*, **6**, 363–403.

Utts, J. (1996). An assessment of the evidence for psychic functioning. *Journal of Scientific Exploration*, **10**, 3–30.

von Lucadou, W. (1990, June). *Some remarks on the problem of repeatability of experiments dealing with complex systems.* Irsee, Germany: NATO Advanced Study Institute, Information Dynamics.

von Lucadou, W. (1994). Wigner's friend revitalised? In H. Atmanspacher & G. Dalenoort (Eds.), *Inside versus outside* (pp. 369–388.) New York: Springer.

Walker, E. H. (1975). Foundations of paraphysical and parapsychological phenomena. In L. Oteri (Ed.), *Quantum physics and parapsychology* (pp. 1-53) New York: Parapsychology Foundation.

Walker, E. H. (1985). On the mathematics of scientific belief systems. In B. Shapin & L. Coly (Eds.), *The repeatability problem in parapsychology* (pp. 98–143). New York: Parapsychology Foundation.

Dick J. Bierman

Chair of Parapsychology
Bestuursgebouw, rm. 271
University of Utrecht
Postbus 80125
3508 TC Utrecht
The Netherlands
Bierman@accu.uu.nl, bierman@psy.uva.nl