The PEAR Proposition: Fact or Fallacy?

For twenty-five years a group of researchers at Princeton University has been making claims that humans can affect electronic and mechanical devices with their minds. They claim their experiments are conducted in a rigorous, scientific manner and yield above-chance results. However, a close examination of their primary random event generator calls the data into question.

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"THIS TIME WE'RE TRYING VULCAN MIND MELD."

For twenty-five years a remarkable group at Princeton University, the Princeton Engineering Anomalies Research (PEAR) group, has been pursuing a research program in what many would characterize as parapsychology. A recent article by this group, "The PEAR Proposition" (Jahn and Dunne 2005) summarizes this quarter-century effort. The bulk of the research has been to show that human intent can remotely affect mechanical and electronic devices in a manner consistent with their intention. They have also reported experiments in remote perception. However, in this article I will take a critical look only at the first group of experiments. I first came upon the work of PEAR while on sabbatical leave in 1992. While browsing in the library, I came upon an article in the journal of the Institute of Electrical and Electronics Engineers (IEEE) with the eye-catching title "The Persistent Paradox of Psychic Phenomena: An Engineering Perspective" by R. Jahn (1982). A number of things struck me about this article: it appeared in a reputable, peer-reviewed, credible scientific journal; its author was clearly a colleague with credible scientific credentials, being formerly a Dean of Engineering; and the work was conducted at an institution with impeccable standards, particularly in the sciences. Jahn's article reviewed historical claims for parapsychology and justifiably rejected many of them for a host of obvious reasons, among them poor methodology, poor statistics, outright fraud, and so on. The last section discussed experiments conducted at Princeton whereby an electronic device was designed to produce a random series of pulses. These were counted in a pre-set time interval and it was established that the accumulated counts scattered around a mean and standard deviation, which conforms to a high degree to Gaussian statistics, i.e., the statistics of random numbers.

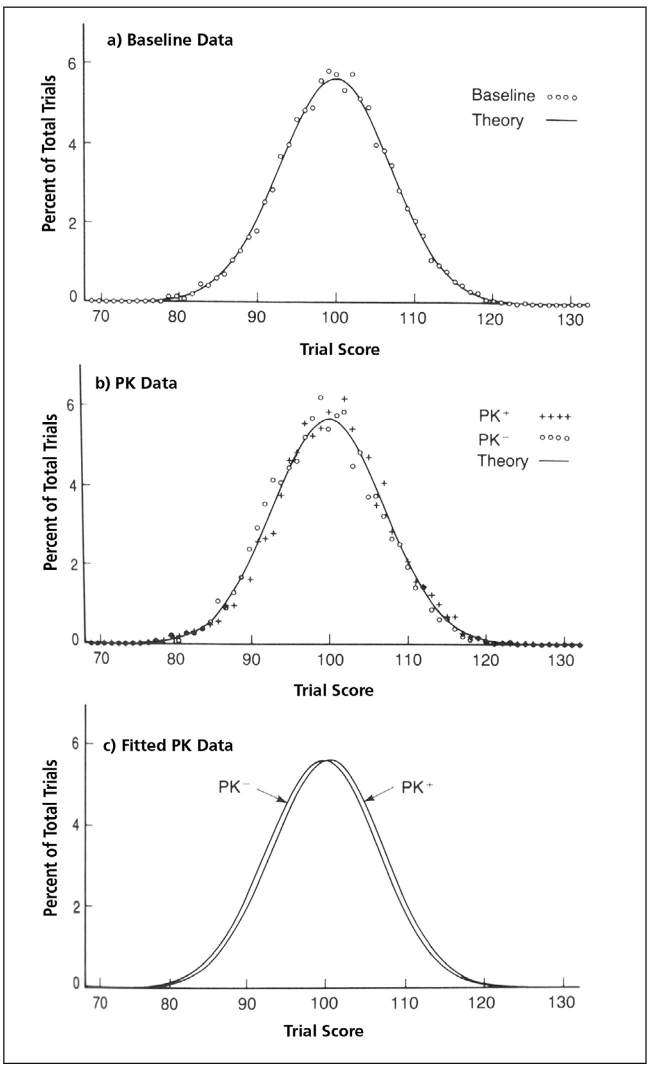

Figure 1. First formal results of one operator's intentions on REG output count distributions, superimposed on theoretical chance expectation: a) baseline data; b) high- and low-intention data; c) best binomial fits to high and low data.

Some of the first results obtained using this device are shown in figure 1. The distribution of the numbers does seem to conform to the expected random distribution about a mean of 100 and with a standard deviation of around seven. A fit derived from the Gaussian distribution with these parameters is shown as the solid line and labeled *theory*. The word *theory* is used in the sense of modeled behavior—here the assumption being that in the absence of any extraneous effects the device behaves in a random manner. *Theory* is not used in the sense of being derivative of a set of physical principles. In their many publications the PEAR group use *theory* to imply fitting of experimental data to statistical expectations.

The data, as published, appear to show a small offset between the data derived when someone is ostensibly attempting to bias the mean to be higher than it would be in their absence (or, more precisely, in the absence of their mental efforts to produce a bias) and conversely a small offset in the opposite sense when someone attempts to bias the output to be smaller than it would be otherwise.

In attempting to underpin the claims for statistical significance with a theoretical basis, the PEAR group make frequent appeals to quantum mechanics and quote approvingly of Schrödinger, Wigner, etc. They argue, in a metaphorical sense, that the parameters of quantum systems can be mirrored by psychological correlates. Some of the claims advanced by the PEAR group are post-dictions, for example, the claims for gender bias, baseline bind, etc. None of these are actually predicted by any of the many interpretations of quantum mechanics.

Methodological Issues

I have conducted several experiments in collaboration with others in this field (Jeffers 2003). One characteristic of the methodology employed in experiments in which I have been involved is that for *every* experiment conducted in which a human has consciously tried to bias the outcome, another experiment has been conducted immediately following the first when the human participant is instructed to ignore the apparatus. Our criterion for significance is thus derived by comparing the two sets of experiments. This is not the

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methodology of the PEAR group, which chooses to only occasionally run a calibration test of the degree of randomness of their apparatus. We contend, although Dobyns (2000) has disputed our claim, that our methodology is scientifically more sound.

If the claims are credible, it should be possible for other groups to replicate them. To their credit, the PEAR group did enlist two other groups, both based at German universities (Jahn et al. 2000) to engage in a triple effort at replication. These attempts failed to reproduce the claimed effects. Even the PEAR group was unable to reproduce a credible effect.

Baseline Bind or Baseline Bias?

One favored way of analyzing and displaying data from experiments of this type is to calculate the accumulated deviation from normal expectations. If humans had successfully biased the data such that it now has a higher mean than in the absence of their efforts, then by forming successive differences in the data so biased with a data set in which there is presumably no bias, then one would get an ever-increasing sum. Similarly, if the subject had succeeded in biasing the data such that the data now had a *lower* mean, the accumulated sum would increment in the negative direction. No bias would result in a cumulative sum, which would hover around zero. Gaussian statistics allows one to assign a likely probability to the cumulative sum if no bias were present. Typically, if this probability, *p* value, is less than .05, then one concludes that a real bias is present. Inspection of the data in figure 2 appears to bear out the claims made. Curves labeled P+ (corresponding to efforts to bias the data to a higher mean value) do indeed accumulate to a running sum unlikely by chance at a $p<.01$, and curves labeled P- (corresponding to efforts to bias the data such that it has a lower mean) do accumulate in the negative sense with a $p<.01$. The baseline plot, BL, obtained from data in which no effort is made to bias the equipment does indeed, as per expectations, hover around zero.

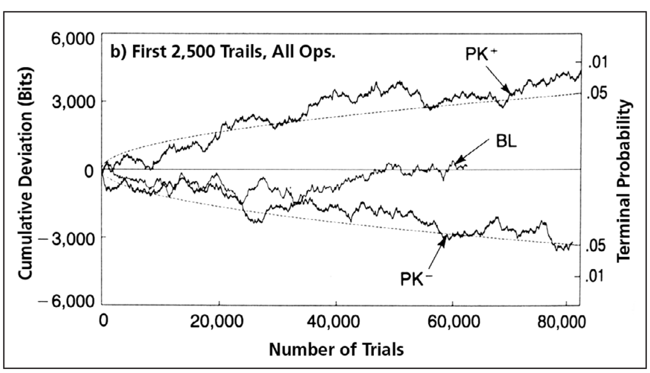

Figure 2. REG Grand cumulative deviations: All operators.

In Jahn and Dunne's book *Margins of Reality*, a short chapter is devoted to "Baseline Bind." It is reported that "namely, of the seventy-six baseline series performed, seven or eight of the means would be expected to exceed the 0.05 terminal probability criterion, in one direction or the other, simply by chance. In fact not one of them does." In other words, the baseline data are too good. The means of the baseline data conform to the means of the calibration data, but the variance of the baseline data is less than that of the calibration data. This baseline bind effect is attributed to "the conscious or unconscious motivation on the part of the operators to achieve a 'good baseline.'" It is instructive to compare the baseline behavior of the data shown in figure 3 with that in figure 4. The data presented in figure 4 show an accumulated deviation which *actually achieves significance* according to PEAR criteria, as the terminal probability lies just outside the $p=.05$ envelope. The data in figure 4 represent all the data accumulated in PEAR's experiments.

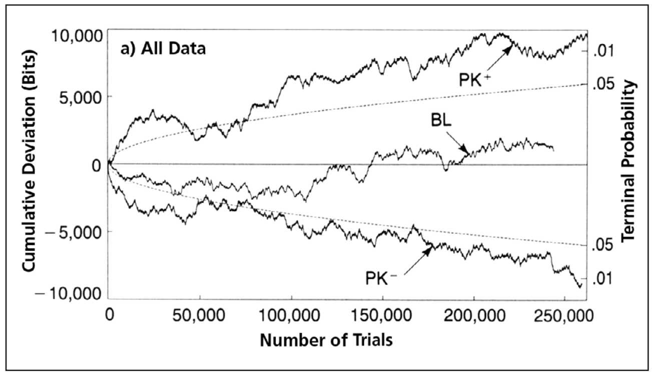

Figure 3. Cumulative deviations of all mean shifts achieved by the same operator as figure 2 over entire database of 125,000 trials per intention.

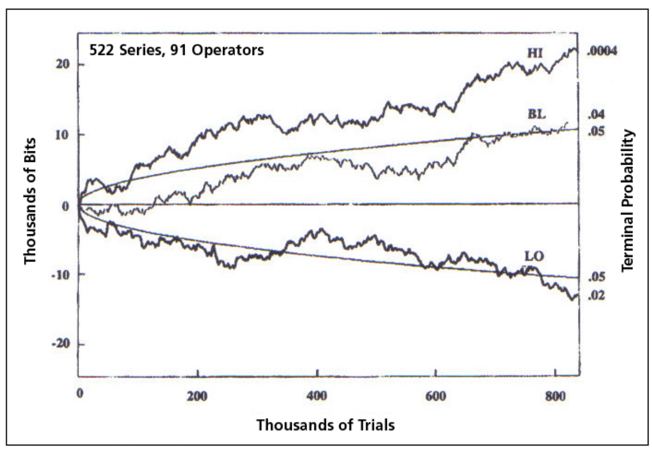

Figure 4. Cumulative deviations of all mean-shift results achieved by all 91 operators comprising a database of some 2.5 million trials.

When the data shown in figure 4 were first published, surprisingly there was no discussion about the behavior of the baseline data given the previous claims regarding "baseline bind." The baseline data in figure 4 violate PEAR's own criteria for significance (i.e., $p<.05$ terminal probability), and consequently—according to PEAR's own standards—must be regarded as evidence for nonrandom behavior in the baseline data. This has to call into question the claimed statistical significance of the data labeled HI and LO in the same plot.

Conclusions

In their book *Margins of Reality* Jahn and Dunne raise this question: "Is modern science, in the name of rigor and objectivity, arbitrarily excluding essential factors from its purview?"

Although the question is couched in general terms, the intent is to raise the issue as to whether the claims of the parapsychological community are dismissed out of hand by mainstream science unjustifiably. This paper argues that in the light of the difficulties in replication (even by the PEAR group itself), the lack of anything approaching a theoretical basis for the claims made, and, perhaps most damaging, the published behavior of the baseline data of the PEAR group *which by their own criteria* indicate nonrandom behavior of the device that they claim is random, then the answer to the question raised has to be no. There are reasonable and rational grounds for questioning these claims. Despite the best efforts of the PEAR group over a twenty-five-year period, their impact on mainstream science has been negligible. The PEAR group might argue that this is due to the biased and blinkered mentality of mainstream scientists. I would argue that it is due to the lack of compelling evidence.

Acknowledgements

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