Journal of Scientific Exploration, Vol. 12, No. 3, pp. 407–423, 1998 0892-3310/98

Evidence that Anomalous Statistical Influence Depends on the Details of the Random Process

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Abstract — Within the field of anomalous statistical influence it has become widely accepted that the susceptibility to human influence of a (non-pseudo) random event generator (REG) is independent of the details of its construction. This view was formalized by Schmidt, after obtaining similar experimental results from very different REGs, in the Equivalence Hypothesis that all REGs are equally susceptible. Since then, a number of models of anomalous influence have built upon this hypothesis to predict a scaling of the anomalous statistical yield with other factors, assuming the REG details can be ignored. For example, "time-normalized," and Decision Augmentation Theory (DAT) models predict that the anomalous *Z*-score scales as the square-root of, respectively, the time invested by the human operator, and the number of operator initiations of the random process (button pushes).

This paper is a report of an experiment to test the equivalence hypothesis, and, by implication, the validity of any derivative model. Human operators were invited to affect "trials" formed from accumulations of binary random events wherein the method of accumulation was randomly switched between two different modes. In one mode, a trial equal to the sum of 200 bits was presented to the operator as the target of intention (to elevate or depress its value). In the other mode, the target of intention was a number distilled from the sum of 2,000,000 bits, having the same chance statistics as the 200 bit trial. By keeping the display and time between trials identical for both modes, both operator and experimenter were blind to the randomly alternating method of computing the trial value.

A surprising and important finding from our results ($p = 0.00037$) from 140,000 such trials is that anomalous influence of a random process does strongly depend on the method of generation. As a consequence, these results significantly refute the equivalence hypothesis, and therefore the time-normalized and DAT models. Instead, it must be concluded that "anomalous interaction" (Psychokinesis) is innately (partly or wholly) a property of the machine, and therefore its description lies partly or wholly in the domain of a (future) physics.

Keywords: anomalous influence — psychokinesis

Introduction

Earlier Work at PEAR

The original motivation for this experiment was to find out if statistically significant anomalous influence on random process could be achieved more quickly by gathering data at much higher rates than has been the custom. In an exploratory investigation of this question (Jahn *et al.,* 1996), data were gathered at 20, 200, and 2000 bits per trial, where the time per trial — about 0.8 seconds — was t he same for all three. In those pilot experiments, not enough data were gathered to come to achieve a significant difference in the susceptibility to intention of the different methods. Continuing on from that earlier work, this paper is a report of a more recent and conclusive effort to answer the same question using a much larger database and more sophisticated protocol.

Historical Background

Following the pioneering work of J. B. Rhine and others, it was natural that workers in the field of psychokinesis investigate the dependency of anomalous influence on the physical characteristics of the random process. Notable among these efforts was the work of Forwald (1976) and Cox (1971) who found that the susceptibility of dice to human influence did not seem to depend in a straight-forward way on their mass. From this, and subsequent failure to find a dependency on the spatial separation between the human operator and the source of the random process (Dale & Woodruff, 1947; Nash & Richards, 1947; Mitchell & Fisk, 1953; Dunne & Jahn, 1992), there arose the view that such anomalous behavior could not be the result of a physical force. It has since become widely accepted that human intention correlated with anomalous statistics (anomalous statistical influence) is an irreducibly statistical phenomenon. However, this still leaves undecided the scaling of the effect with the number and psychological state of the human operators, the time and effort invested, and the statistical (temporal) characteristics of the random process.

In early experiments, the temporal characteristics of the random process as a significant factor were explored by subjecting variable numbers of dice per throw to human intention (*e.g.* Rhine & Humphrey, 1944). These studies found no significant differences in the statistical yield (the anomalous Z-score) per die. However, as pointed out by Stanford (1977) these experiments confound the variance of the random process with the interval between events, since it is to be expected that the time required to record the results depended on the number of dice per throw. This difficulty has since been overcome with the adoption of the electronic random event generator and computerized recording. Schmidt (1973) found that if binary trials were presented at two different rates, operators performed significantly better (per trial) at the lower rate. Clearly, however, there remains a possible psychological confound; operators could not be blind to the two different rates of presentation,

and therefore one cannot discount a possible inequality of effort due to psychological factors. A later study by Schmidt (1974) endeavored to overcome the possibility of such psychological confound. Since our work is a refinement of that experiment, it is fruitful to review Schmidt's experimental design and findings in some detail.

Schmidt's 1974 Experiment

Operators were presented with binary trials randomly alternated from one of two different REGs under conditions such that they were blind to which source was responsible for the presented trial. One of these sources produced a single binary trial once every three seconds by a single digitization of an analog noise source (the "simple" generator). The other ("complex") generator also produced a trial once every three seconds, but this was derived from the majority vote of 100 digitization events of a *different* source in the three second period. Both REGs were run in parallel; which REG was responsible for the trial displayed to the operator was decided by a pre-recorded random binary sequence (decision variable). The time interval between trials was fixed at three seconds, independently of the value of the decision variable.

In a pilot experiment of 1,011 trials, originating approximately equally from each of the two REGs, operators achieved positive Z-scores (*i.e.* in the direction of intention) of 3.7 and 0.5 on the simple and complex generators respectively. In the subsequent formal experiment of 3,304 trials, again originating approximately equally from each of the two REGs, operators achieved positive Z-scores of 4.4 and 3.0 respectively. On the basis of the formal results, Schmidt concluded that operator performance did not depend on the complexity of the REG.

Stanford (1977) claims that if Schmidt's pilot and formal data are pooled, then the differential Z for the two generators is significant. Our calculations indicate a differential Z on the pooled data of 1.83, and a Stouffer (aggregate) differential Z of 2.3, the positive signs indicating a better performance on the simple generator. However, without an in-going hypothesis preferring either generator, these Zs should be subjected to a two-tailed test, which yields a two-tailed probability on the pooled data ($Z = 1.83$) of 0.067, which is not significant. We conclude that Stanford's claim is based either on a one-tailed test, or on an un-weighted Stouffer differential Z.

Although Schmidt's conclusion that the data do not show a dependence of operator performance on the complexity of the REG is formally correct, the pooled data do at least suggest that there may be a difference for the two generators, which could become significant in a larger database.

Comparison with this Experiment

The putative independence of induced statistical anomalies with respect to the random generator is of crucial import. Its confirmation, or more precisely,

its lack of refutation, severely confines the domain of a successful theoretical model to, and downstream from, the observed data. In contrast, a single example of an unambiguous demonstration of non-independence would refute the equivalence hypothesis, and thereby demand that a successful model encompass events upstream from the observed data.

Against this background, our experiment is a further test of the equivalence hypothesis, but with two major differences from the experiment of Schmidt. In our experiment we use the same noise source and same digitized random bit stream for both types of trial. This has the effect of narrowing the field of possible sources of divergence to the algorithm for computation of the trial values from the random bit stream. The other major difference is that we can draw on a much larger database, and so give any possible divergence a greater opportunity to manifest. The important differences are summarized in Table 1.

TABLE 1
Comparison of this and Schmidt's 1974 Experiment

Experiment	Formal Trials	Summed Bits per Trial	ratio: (complex/simple) Digitized Bits per Trial	Time per Trial (secs.)	Total Time Invested(hours)
Schmidt 1974	3,304	1	100	3.0	2.75
Ibison 1997	140,000	200	10,000	0.8	31.11

Design

Overview

A wide bandwidth Elgenco micro-electronic analogue noise source is digitized to give a bit stream of random binary events (Figure 1 and 2). A contiguous block of bits from the stream is processed in two very different ways (see

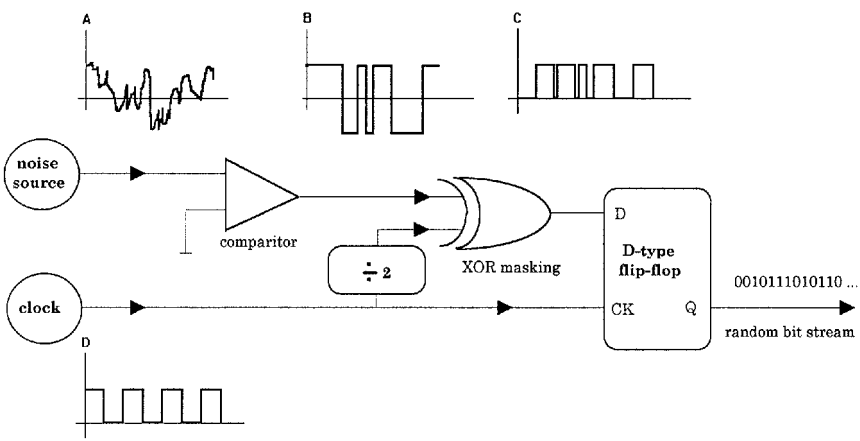

Fig. 1. Schematic of random event generator.

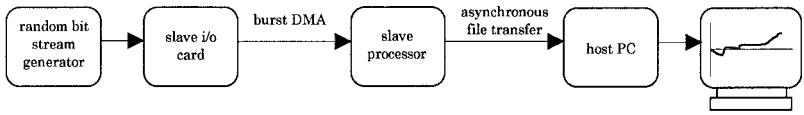

Fig. 2. Hardware Schematic.

below) to give two numbers with the same chance distribution. At a rate of around once per second, just one of these two numbers is presented as a "trial" in digital or graphical form to a human operator. Which of the two is presented is decided by a single random bit; the other number is neither presented nor recorded. The noise source delivering the random bit-stream is described in detail in Appendix 1 and the slave processor used to capture and process the random bit-stream to give the trial values in Appendix 2.

The Two Methods

To avoid controversy on the characterization of complexity employed by Schmidt, our two types of trial are designated low and high density, corresponding very closely to his simple and complex trials. In the "low-density" method, a trial value is constructed by summing 200 of these bits. It follows that these trials have mean 100 and variance 50. In the "high-density" method, trials are constructed by summing a second set of 2,000,000 bits, and then re-scaling and truncating the value so obtained, to give a trial value with the same chance distribution as that of the low-density method. Which trial value is displayed — *i.e.* from the high or low density method — is decided by another pre-recorded random process.

Protocol

Both operator and experimenter are unaware of the origin of the trial value being displayed, and so are constrained to treat all trials similarly. A record is kept of the trial value and its generation method, the latter made available for subsequent analysis only upon completion of the entire experiment. With the operator blind to the origin of the trials presented, he/she attempts to influence their values in accord with a standard tri-polar protocol, details of which can

be found in Jahn *et al.,* (1996). Briefly, the protocol demands an equal number of trials subjected to an effort, in "runs" of 100 or 1000 trials, to elevate, depress, or leave unaltered, the trial values. These efforts we will refer to as "high," "low" and "baseline" intentions respectively. On average, by virtue of the randomization, there are an equal number of trials, regardless of the intention, generated by the two methods.

Following collection of both operator data and a large volume of control data generated with no operator present, the experimental design calls for a subsequent statistical analysis to determine if there exists a dependency on the method of data generation of the operators' ability to influence the trial values according to the tri-polar protocol. To minimize the risk of experimenter influence on the outcome on the differential test, the un-blinded results for the high and low density trials were first observed by a disinterested, but statistically knowledgeable, third party.

Results

70 series totaling 210,000 trials, of which 140,000 were subjected to intention, were collected from 13 operators. Figure 3 shows, chronologically, the combined high and low density results for the high, baseline, and low intentions. These data, summarized in the fourth row of Table 2, show a negative going Z-score of –1.7259 ("psi-missing") for the difference between the high and low intentions. However, no formal significance can be attached to this

TABLE 2
Summary of Results

Density	Intention	Trials	Z	Probability	Tails
combined	high	70,000	–1.5893	0.94400	1
combined	low	70,000	0.8515	0.80275	1
combined	baseline	70,000	0.3418	0.73250	2
combined	high-low	140,000	–1.7259	0.95782	1
low	high	35,012	0.6906	0.24491	1
low	low	35,032	–1.1432	0.12648	1
low	baseline	35,047	0.8951	0.37076	2
low	high-low	70,044	1.2967	0.09736	1
high	high	34,988	–2.9388	0.99835	1
high	low	34,968	2.3490	0.99059	1
high	baseline	34,953	–0.4126	0.67992	2
high	high-low	69,956	–3.7391	0.99991	1
high-low	high	70,000	–2.5661	0.01028	2
high-low	low	70,000	2.4689	0.01355	2
high-low	baseline	70,000	–0.9249	0.35504	2
high-low	high-low	140,000	–3.5603	0.00037	2

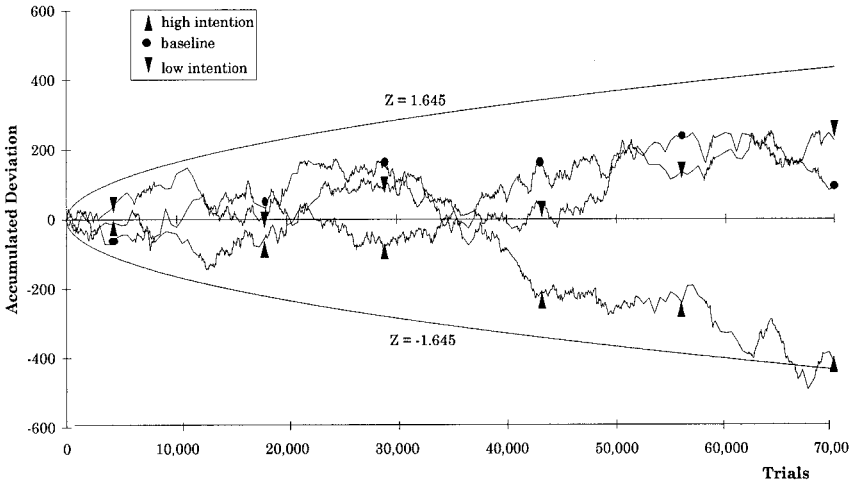

Fig. 3. Effect of intention on combined high and low density noise.

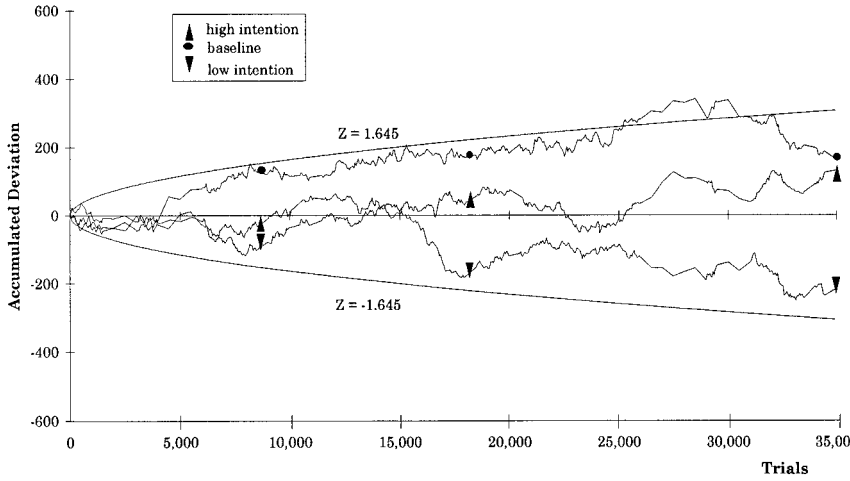

Fig. 4. Effect of intention on low density noise only.

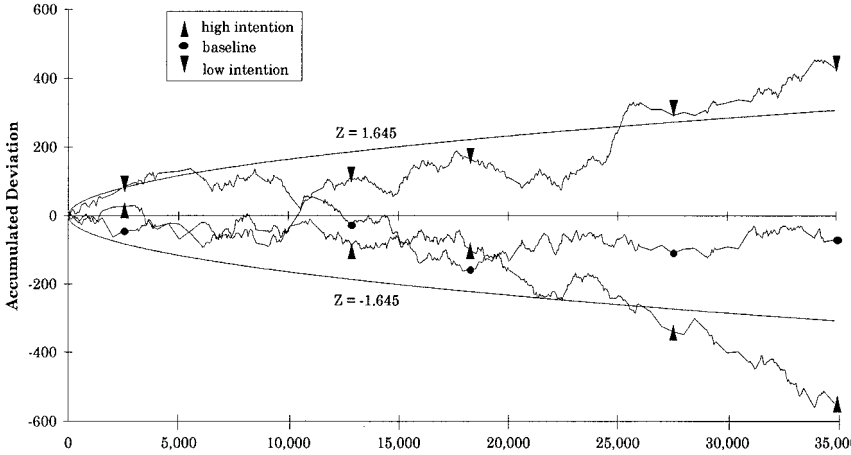

Fig. 5. Effect of intention on high density noise only.

result because the design calls only for a differential test between the high and low density data.

With reference to Figures 4 and 5, it is readily apparent that the combined data negative Z-score is a result of a mild positive response to intention in the low density data (high-low $Z = 1.2967$), and strongly negative response to intention in the high density data (high-low $Z = -3.7391$) as summarized in Table 2.

The two traces in Figure 6 show chronologically, for the high and low densities separately, the accumulated deviation from expectation of the high - low

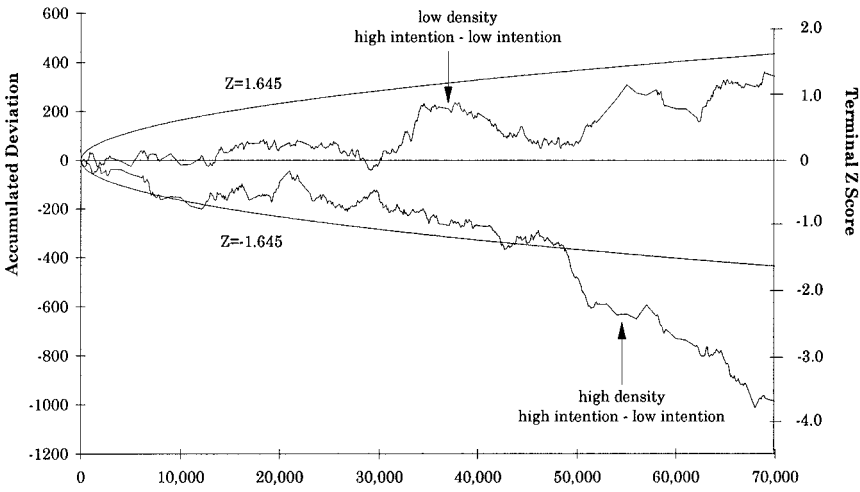

Fig. 6. Effect of noise density on susceptibility to intention

intention difference. Clearly there is a significant dependence on the density. Formally, from the last row of Table 2, the null hypothesis that there is no difference between the methods is refuted with $Z=-3.5603$, $p=3.7\text{x}10^{-4}$ (two-tailed).

Positive Z indicates a positive mean shift (independent of the direction of intention). The one-tailed probability for the high intention accumulates over values greater than the observed Z. The one-tailed probability for the low intention accumulates over values less than the observed Z.

Discussion

These results are indisputably anomalous; the protocol precludes an explanation based on systematic bias. The high density trace in Figure 6 establishes that two data sets may diverge when the only change is a human intention to score high or low. The significant difference between the two traces in Figure 6 derives from the difference (high-low density) of the difference (high-low intention) of two data sets, and is therefore (doubly) immune to systematic bias.

There are essentially two possible explanations of this result. One is that not only is intentional anomalous influence from operators acting to affect the trial values, but there is also an unintentional anomalous influence acting to affect the outcome of the whole experiment; *i.e.* is acting at the level of the hypothesis being tested. The other explanation is that given two different random processes with equal efforts of operators toward both, they do not respond equally to intention (though it may be that this differential response may only emerge when the two processes are randomly juxtaposed). These two explanations are discussed in more detail below.

Possible Influence of Bias at the Level of the Hypothesis Under Test

These results can be explained without attributing a difference in susceptibility to intention in the random process methods by supposing that anomalous human influence is acting not only at the level of trials, but also at the level of the hypothesis under test (*i.e.* specifically to distinguish between the two data sets). However, there are severe constraints to be satisfied by such a proposed influence:

- It must be effective despite originating from an *unconscious bias,* since there could be no conscious bias from operators and experimenters towards the data sets from either random process.
- The influence must be effective *without feedback* at least until the time the double-blind is broken and the data are analyzed.
- The influence must be able to *override* the effects of a *first observation* by a disinterested party.

This explanation seems far less parsimonious than simply assuming a differential susceptibility of the random processes.

An additional argument against this explanation is the difficulty in establishing any *consistent and falsifiable* theory. First, note that one would have to admit that a test of such a theory would itself be vulnerable to "observers" other than the operator. If these observers were biased toward refutation and capable of affecting the results of the test, the theory could be falsified by the very mechanism it proposes.

Nonetheless, an explanation of our results based on the influence from biased observers *is verifiable.* Imagine an experiment exactly as described in this report except that there is only one method of trial computation (*e.g.* high density). But suppose that the randomized binary decision variable is retained so that the data can be partitioned for subsequent differential analysis (even though there should be no difference). Then if a significant difference between the two data sets is discovered, the explanation can only be that anomalous influence is acting at the level of the hypothesis under test. (But note: absence of a significant difference between the two data sets does not indicate the explanation is invalid, for the reason given above.)

The Role of Setting

In rejecting the above explanation one must conclude there must be some difference in the response of the two random process methods to intention, and therefore that the equivalence hypothesis is invalid. However, this does not mean that two electrically and/or mechanically different REGs *must* have different susceptibilities, or even that the two REGs used in this experiment will respond differently to intention in all settings. Rather, it means that in some settings, differently constructed REGs can respond differently to human intention. An important role for the experimental setting is thus suggested by the finding of this experiment, in contrast to the apparent uniformity of susceptibility to intention of dissimilar REGs reported in the literature.

The Role of Juxtaposition

Consider for example the possibility that anomalous influence is adaptive to the target in a manner analogous to that of human vision. A consequence of adaptation at the front end of the vision system is a poor ability to distinguish between different shades of gray unless they are juxtaposed (in space or time). By analogy it is conceivable that the differential susceptibility demonstrated in this experiment is a consequence of the close juxtaposition of the two random process methods.

As a second example, consider a mechanical oscillator formed from a mass and spring subjected to (manual) impulses from a human operator with the goal of maximizing the amplitude. An intelligently adaptive operator will apply impulses at the correct phase and at the natural resonant frequency of

the oscillator (or a sub-harmonic thereof). Now let the spring constant be randomly switched between two values at most once in a period, and to which the operator is blind. The optimal impulse strategy is now unclear. It is conceivable that the operator may decide to persist as if the oscillator had a single fixed resonance, so that on average he or she was successful in increasing the amplitude only when one of the spring constants was in place. Further, this strategy can result in a reduced amplitude when the other spring is present, thus explaining the *psi*-missing on one of the data sets.

For both of these models, a testable implication is that operators will achieve similar yields on the two densities when they are not juxtaposed. Efforts are underway to test this prediction, and we hope to report in the near future the results of an experiment using only the high density random process.

Status of DAT and Time-Normalization Models

Any model claiming that anomalous susceptibility depends *only* on a factor other than the inner workings of the REG and its setting is categorically refuted by these results. This applies to the DAT model because it claims that the anomalous yield (Z-score) scales as the square root of the number of operator interventions and depends on nothing else. Likewise the time-normalization model is refuted because it claims that the anomalous yield scales as the square root of the time invested by the operator in exerting his/her intention and depends on nothing else. This is true quite apart from the relative standing of these scaling predictions on *identical* REGs. Either of these models can be modified to survive refutation if its claim to exclusivity is dropped. Thus, it may be that the anomalous yield scales in the particular manner predicted by one of these models provided the REG is "held fixed." It follows from our result that tests of the scaling laws predicted by these models (hypotheses) should be conducted with the REG held fixed.

Status of the Bit-Wise Influence Model

The bit-wise influence hypothesis claims that the a shift in the binary probabilities of the digitized bit-stream is a constant of the anomalous human-machine interaction (see for example Dobyns, 1996). It implies that the anomalous yield measured as a Z-score scales as the square-root of the number of digitized bits subjected to intention. Since this model predicts that the anomalous yield *does* depend on the inner workings of the REG, the bit-wise hypothesis opposes the equivalence hypothesis and therefore is not automatically refuted by our results.

To investigate the impact of our results on the bit-wise hypothesis, we compute the bit-wise effect-size ε, defined as Z/ square root of the number of bits. From Table 2 we find

$$\varepsilon_{\text{high\_density}} = -9.996 \times 10^{-6} \pm 2.673 \times 10^{-6} \tag{1}$$
$$\varepsilon_{\text{low\_density}} = 3.464 \times 10^{-4} \pm 2.672 \times 10^{-4}.$$

According to bit-wise model, these two should be equal. The Z-score associated with their difference denoted by Z_{bwh} is given by

$$Z_{bwh} = \frac{\varepsilon_{\text{high\_density}} - \varepsilon_{\text{low\_density}}}{\sqrt{\left(\delta\varepsilon_{\text{high\_density}}\right)^2 + \left(\delta\varepsilon_{\text{low\_density}}\right)^2}} = -1.334 \tag{2}$$

which should be subjected to a two-tailed test, and is clearly not significant. This result is dominated by the relatively large values of effect-size and uncertainty of the low-density process as can be seen from the following

$$Z_{bwh} = \left(\frac{Z_{\text{high\_density}}}{\sqrt{n_{\text{high\_density}}}} - \frac{Z_{\text{low\_density}}}{\sqrt{n_{\text{low\_density}}}}\right) \Bigg/ \sqrt{\frac{1}{n_{\text{high\_density}}} + \frac{1}{n_{\text{low\_density}}}}$$

$$= \frac{Z_{\text{high\_density}}\sqrt{n_{\text{low\_density}}} - Z_{\text{low\_density}}\sqrt{n_{\text{high\_density}}}}{\sqrt{n_{\text{high\_density}} + n_{\text{low\_density}}}}$$

$$\Rightarrow Z_{bwh} \approx -Z_{\text{low\_density}} \text{ since } n_{\text{high\_density}} >> n_{\text{low\_density}} \tag{3}$$

where the n are total the number of bits collected at each density. From this we conclude that despite the significantly different susceptibilities of the two densities, our results do not disqualify the bit-wise hypothesis, though an implication of Equation (3) is that the bit-wise hypothesis would be refuted in the event that the low-density results alone achieve statistical significance.

Conclusion

Our results provide strong evidence (p = 0.00037) against the hypothesis that all REGs are equally susceptible to intention. In other words: though two REGs may be constructed to provide identical displays and may be subjected to identical efforts to perturb their output, if their inner construction differs, then their response to anomalous influence may also differ. However, we do not know to what degree the manifestation of unequal yields from differently constructed REGs depends on the experimental setting, and in particular on the temporal juxtaposition of their output to the human operator.

Appendix 1 - REG Design Details

The REG is a stand-alone unit comprising a digital noise source, clock, XOR filter, and latch. The digital noise source is itself comprised of an analog noise source, amplifier, and comparitor (Figure 1). The output (Q) of the latch delivers the raw bit-stream to a slave processor, the operation of which is described in Appendix 2.

The Digital Noise Source

The source of randomicity is a modular 70 MHz bandwidth 1 mV RMS Elgenco analog noise-source. The noise is amplified to the order of 1 Volt RMS with zero mean and then digitized using a high-speed comparitor with reference zero (Figure 1 traces).

Clock and Latch Sampling

The digitized signal is XOR gated (see below) and then clocked into a flip-flop (*i.e.:* latched) at a fixed frequency of 40 MHz supplied by a quartz oscillator. The output (Q) of the flip-flop is then the source of the raw bit stream.

Hardware XOR Filter

Ideally, the output of the comparitor should spend as much time at positive saturation as at negative saturation, and therefore the raw bit-stream should have as many 1's as 0's. In practice, various sources of asymmetry cause the behavior to depart from the ideal, including, for example, a non-zero comparitor input offset, and asymmetry in the power consumption of bipolar semiconductor logic which impacts the comparitor reference voltage. For this reason, as with previous PEAR designs of similar REG equipment, following digitization by the comparitor, but prior to latching, the bit-stream is subject to an exclusive OR (XOR) with an alternating template which is just the digitizing clock (frequency) divided by 2. In the absence of any anomalous behavior, this should force the latch Q to have as many 1's as 0's, regardless of the statistics of the bits leaving the comparitor.

Symbolically, let $d_k^{(0)}$ be the kth data bit following digitization, and let the hardware template be c_k where

$$\begin{aligned} c_k &\equiv \frac{1+(-1)^k}{2} \\ &= 1 \text{ if } k \text{ even,} \\ &= 0 \text{ if } k \text{ odd} \end{aligned} \tag{A1.1}$$

then the data following the hardware XOR masking is

$$d_k^{(1)} = d_k^{(0)} \oplus c_k$$

where

$$\begin{aligned} x \oplus y &\equiv \bar{x}y + x\bar{y} \\ &= 1 \text{ if } x \neq y \\ &= 0 \text{ if } x = y. \end{aligned} \tag{A1.2}$$

Appendix A2 —Slave Processor Functionality

Due to the high data rates involved, it was necessary to employ dedicated hardware for the acquisition and processing of the bit stream. We used an Alacron Digital Interface board (i/o board) directly connected to an Alacron Intel i860-based motherboard hosted by a PC (Figure 2). Let the high and low density trial values be denoted T_{HD} and T_{LD}, and the trial value delivered to the operator be denoted T_{op}. The sequence of events in the slave processor is:

I. high density acquisition
II. software XOR
III. sum and re-scaling -> T_{HD}
IV. low density acquisition
V. software XOR
VI. sum -> T_{LD}
VII. randomly choose $T_{op} = T_{HD}$ or T_{LD}

Each of these stages is described below.

High Density Acquisition

The i/o board buffers blocks of 32,768 bits from the REG. Once full, the buffer is emptied into the motherboard RAM via interrupt-driven DMA. Construction of the high density trial commences with 62 acquisition cycles to give 2,031,616 bits in i860 RAM, which takes in total around 100 ms. The first 2,000,000 bits of these constitutes the raw data from which the high density trial value is constructed. During DMA time (around 2.5 μ), bits generated by the REG are ignored. Therefore each block of 32,768 bits within the 2,000,000 bits in RAM is a contiguous sub-string of bits delivered by the REG, but contiguity is not maintained across the block boundaries.

High Density Software XOR

It was discovered that the acquired bit stream was still biased after the hard-

ware XOR with the alternating template. We speculate that this may be the result of capacitively coupled feedback from the output to the comparitor reference and supply, rendered more potent because of the higher frequencies in this, as compared to PEAR benchmark, REG designs. To counter the bias, the random bits in i860 RAM are subjected to further XOR filters in software to ensure symmetric (50:50) bit probabilities.

We use two successive stages of XOR alternating template filtering at different frequencies. The first template is at half the frequency of the hardware XOR alternating template. *i.e.:* the data following the first software XOR stage is:

$$d_k^{(\mathrm{HD},2)} = d_k^{(\mathrm{HD},1)} \oplus c_{k/2.} \tag{A2.1}$$

The second template is at a frequency of 1/1024 times the hardware XOR template, so the data following the second software XOR stage is:

$$d_k^{(\mathrm{HD},3)} = d_k^{(\mathrm{HD},2)} \oplus c_{k/1024.} \tag{A2.2}$$

High Density Sum and Re-scaling

To construct the high density trial score T_{HD}, 2,000,000 bits are summed and then re-scaled with the aim of achieving a trial value having the same distribution as that of T_{LD}. The latter, though binomial, is very nearly normally distributed, with mean 100 and variance 50. The sum of 2,000,000 random bits is also normally distributed, with mean 1,000,000 and variance 500,000. The re-scaling must therefore adjust the mean and variance of the latter, whilst generating an integer having a normal distribution consistent with the low density binomial distribution.

Let:

$$S_{\mathrm{HD}} \equiv \sum_{k=1}^{2,000,000} d_k^{(\mathrm{HD},3)} \tag{A2.3}$$

then the requirements described above are satisfied by the procedure

$$\begin{aligned} T_{\mathrm{HD}} &= \left[\frac{S_{\mathrm{HD}} - 1,000,000}{100} + 100\right] \\ &= \left[\frac{S_{\mathrm{HD}}}{100} - 9,9000\right] \end{aligned} \tag{A2.4}$$

where here $[x]$ stands for "the nearest integer to x."

Low Density Acquisition

For construction of the low density trial just one block of 32,768 contiguous random bits is loaded into i860 RAM. Only the first 200 bits of this block are used.

Low Density Software XOR

The same XOR procedure is applied to both the high and low density data sets. Clearly however, the final XOR process at 1024 times the period of the hardware XOR plays no role in the construction of the low density trial since the period is longer than the number of bits used.

Low Density Sum

The low density sum is just the sum of the first 200 bits:

$$S_{\mathrm{LD}} \equiv \sum_{k=1}^{200} d_k^{(\mathrm{LD},2)} \tag{A2.5}$$

where the superscript (LD,2) refers to the low density data set following the second XOR process (*i.e.* the first software XOR process).

Random Switching Between the Two Methods

It should be emphasized that during the period allotted to each trial — around 0.8 sec. — t here are two data sets loaded into i860 RAM from which high *and* low density trial values are respectively computed. However, only one of these trial values is displayed and recorded. Which value is decided by a random binary variable obtained from a 1000 element pre-recorded table which is constant for the whole experiment. At the initiation of a run, an initial table index (effectively the seed) is obtained from the system clock. Subsequent table values are decision variables for the trial methods. The table index is "wrapped around" at 1000 so that all 1000 values are used, regardless of the initial index. By design, the table has exactly 500 each of 1's and 0's, so in a run of 1000 trials, there will be 500 each of low and high density origin.

A property of this design is that there is just one statement (a conditional assignment) in the whole computer program which is "sensitive" to the value of the decision variable. As a result, the computation time for each trial does not depend on the value of the decision variable, so there is no opportunity for the operator, consciously or unconsciously, to become aware of the origin of the trial value from the interval between trials. Less obviously relevant, but conceivably of importance, is that the internal electrical spatio-temporal dynam-

ics of the slave and host computers are, apart from the conditional statement, identical for trial values of either origin. Internal activities associated with the conditional statement occupy less than 1 part in 10^7 of the trial time.

Acknowledgments

M. I. gratefully acknowledges the financial support of the McDonnell Foundation. Princeton Engineering Anomalies Research gratefully acknowledges the tireless contributions of its volunteer operators, and the financial support of the IGPP Mind-Machine Interaction Consortium, Lawrence Rockefeller, D. C. Webster, and several other philanthropic organizations and individuals.

Credit goes especially to Roger Nelson whose idea it was to blind the operators to the trial method, and John Bradish, who spent considerable time building and refining the high frequency digitized noise source. Credit also goes to Robert Jahn whose made an important design decision concerning the different methods of trial value computation.

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