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Abstract—Three models of mind-matter interaction (MMI) in random number generators (RNGs) were tested. One model assumes that MMI is a forward-time causal influence, a second assumes that MMI is due to present-time exploitation of precognitive information, and a third assumes that MMI is a retrocausal influence.

A pilot test and a planned replication study provided significant evidence for MMI, allowing the models to be tested. The outcomes suggest that MMI effects on RNGs are better accounted for by a backwards-in-time rather than a forward-in-time process. Whether this finding will generalize to other experimental designs and MMI phenomena is unknown, but it raises the possibility that teleological pulls from the future may sometimes influence present-time decisions and events. This raises questions about commonly used scientific methodologies and assumptions.

Keywords: mind-matter interaction—random number generator—models—retrocausal

Introduction

People like us, who believe in physics, know that the distinction between past, present, and future is only a stubbornly persistent illusion.—Albert Einstein

Researchers investigating mind-matter interaction (MMI) phenomena have studied correlations between mental intention and a broad range of target systems ranging from single cells to humans, and from photons to bouncing dice (Schlitz et al., 2003). The largest class of studies involves the outputs of electronic random number generators (RNGs) (Radin & Nelson, 1989, 2003; Radin et al., 2006). RNGs produce sequences of truly random events, usually bits, that are both independent and identically distributed (*iid*), and where the source of randomness may be noise in electronic components, radioactive decay latencies, or the direction a photon takes when striking a partially reflective mirror.

Most RNG-MMI experiments use protocols in which human participants are asked to mentally influence an RNG's output—generally sequences of truly random bits—to deviate from an empirical or theoretically expected baseline condition. A participant may be asked to, say, press a button while holding the intention of causing an RNG to generate more 1 than 0 bits in a sample of 200 bits.

Meta-analyses suggest that the results of these experiments are not due to chance, are independently repeatable, and that potential design flaws and selective reporting practices are insufficient to explain the observed outcomes (Jahn et al., 1997; Radin & Nelson, 1989, 2003). Criticisms of these meta-analyses tend to argue that selective reporting is a more plausible explanation (Bosch et al., 2006; Ehm, 2005; Jeffers, 2003; Scargle, 2000; Schub, 2006; Steinkamp et al., 2002), but that point of view is valid only if one assumes that MMI-RNG effects exclusively "operate" at the individual bit level, i.e. that mind is modeled as a causal force that somehow pushes random bits around like puffs of air nudging billiard balls.

Early investigators studying these effects concluded that simple causal models of MMI-RNG effects were inadequate. To see why, consider an experiment where a single button-press, or trial, generates 200 random bits, and the "target" bit, i.e. the bit that mind would try to force the RNG to generate, is a 1. Now assume that MMI operates probabilistically so as to nudge 1 bit in 200 to flip towards the target. Then on average one would observe 101 1-bits and 99 0-bits, or a 50.5% hit rate where chance expectation is 50%. On the basis of an experiment consisting of 100 such trials, the statistical outcome would produce a deviation from chance associated with $p = 0.08$. The same experiment with 500 trials would result in $p = 0.0008$, and an experiment with 3,000 trials would result in $p = 4.7 \times 10^{-15}$. In other words, by simply increasing statistical power a very simple experiment could easily produce unequivocal statistical evidence for a genuine effect, even with a minuscule MMI bias. After hundreds of experimental replications the evidence does not support this idea, and thus the assumption that MMI operates causally and uniformly on individual bits is almost certainly wrong. Other explanations are required.

As investigators began to realize that bit-level causal models did not match the data, they proposed alternatives. Teleological models assuming concepts like "goal-orientation" (Schmidt, 1974) and "psi-mediated instrumental responses" (Stanford, 1974a,b) were among the first of these alternatives. A later model based on precognition, called Decision Augmentation Theory (May et al., 1995), also provided a successful fit to the experimental data by assuming that people can exploit future information to mimic force-like effects. In recent years some theorists have avoided both force-like and passive perceptual models in favor of a unitary phenomenon that manifests in different ways according to context (Storm & Thalbourne, 2000).

One reason for the continuing confusion between causal and retrocausal models of MMI-RNG effects is that the majority of these studies were designed to observe only the end-state of a sequence of random bits, or to record only summary statistics of a collection of bits. To our knowledge, no studies have been conducted to explore the *temporal* details of how a particular trial resulted in a hit or miss. And indeed, because RNGs are designed to provide bits that are completely independent of every other bit, even if all data were available for study one could not infer temporal directions by simply examining the sequence of bits.

This paper describes results of pilot and replication experiments designed to test models that can distinguish between simple forward-in-time versus backwards-in-time models for RNG-MMI effects.

Three Models

Simple models for RNG-MMI fall into three general classes: chance, causal, and retrocausal. *Chance* models assume that observed publications are a selected subset of a larger population of experiments. The "filedrawer effect" model has been examined in meta-analyses, but in my opinion it does not appear to be able to explain the overall experimental results, especially because a large RNG-MMI database with a positive outcome does not have a filedrawer problem (Jahn et al., 1997).

Causal models assume that MMI can be explained as a (possibly exotic) form of what Aristotle called "efficient cause," i.e. the cause-+effect, force-like, forward-in-time relationships commonly used to understand mechanistic processes. In this sense MMI "pushes" on a target system to cause it to confirm with intention. The term often used for this concept is psychokinesis or PK.

Retrocausal models also assume that MMI effects are causal, but in the sense of Aristotle's "final cause." Retrocausal MMI may manifest in a passive and an active form: (1) *Precognition* passively exploits future information, resulting in present-time decisions that probabilistically favor a future goal. (2) *RetroPK* actively causes a target system to be "pulled" towards a future goal that is established in the present.

Pilot Study

Study 1: 3-Stage Markov Chain

The basic experimental design, illustrated in Figure 1, is to use a random system with sequential probabilistic dependencies; the behavior of such a system is mathematically formalized as a Markov chain. In this MMI "target," an RNG is used to generate sequences of random bits that are used to switch a random system between equally likely “1” and “0” states.

The RNG used in this test was the Orion (Amsterdam, The Netherlands), an electronic noise-based circuit that provides sequences of truly random bits to the serial port of a personal computer (PC) at 9600 baud. The Orion RNG has been used extensively in previous MMI research; these devices pass the "Diehard" tests, a gold-standard suite of randomness tests (Marsaglia, nd). The experiment was conducted on a PC running the DOS operating system, and the test was programmed by the author in Microsoft QuickBasic 4.5.

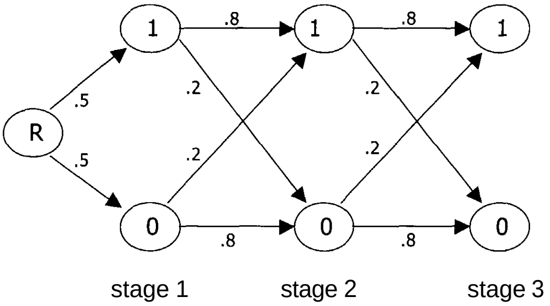

Fig. 1. Markov chain as a 2-state, 3-stage random target system for an MMI experiment. The circles represent states, the lines represent transitional probabilities.

The first random decision, shown as state "R" in Figure 1, is produced by an Orion RNG generating a “1” or “0” with probability = ½. Then the RNG produces a series of random decisions which are processed sequentially through a Markov chain. The Markov chain causes the random system to tend to persist in the existing state (with p = 0.8) and to switch to the other state occasionally (p = 0.2). Thus, even if this system started out with a highly biased RNG output at Stage 1, say, p(1) = ¼ and p(0) = ¾, as the decisions proceeded to later stages the outputs would progressively move towards equal probabilities, p(1) = p(0) = ½. The longer the Markov chain, the more the final hit rate will asymptotically approach p = ½ (with no "bounce" or oscillation in the decline).

Imagine that we sweep through this 3-stage Markov chain N times. After many such sweeps, or trials, we can calculate the hit rates at Stages 1, 2, and 3, where hit rate at Stage x is defined as hr, = sum(1's)/N, where the sum refers to the number of times that the Markov system was in the “1 state" at Stage x over N trials. With large N and an RNG designed to generate random bits with equal probabilities p = 0.5, this Markov chain will produce chance mean hit rates at Stages 1, 2, and 3 with hr = 0.5.

Now consider the experimental task. The participant is asked to press a button with the intention of hearing a sound. Unbeknownst to the participant, after the button is pressed an RNG makes three random decisions, and these are applied to the three stages of the Markov chain. If the last transition arrives in the 1 state, then the PC plays an interesting or humorous audio clip, generally 5 to 10 seconds in length. Otherwise the PC is silent. Each audio clip is randomly selected by the PC, with replacement, from a pool of 500 available clips; most of the clips are segments from popular television shows and movie soundtracks.

For the first binary random decision, the PC retrieves a random byte from an RNG (or equivalent for a pseudorandom RNG [PRNG]). If the byte is ≤127 then the random system goes into the 0 state, otherwise it goes into the 1 state. For succeeding transitions, if a random byte = 0 or >100 then the byte is rejected and another byte is retrieved. When a byte with value 1 to 100 is

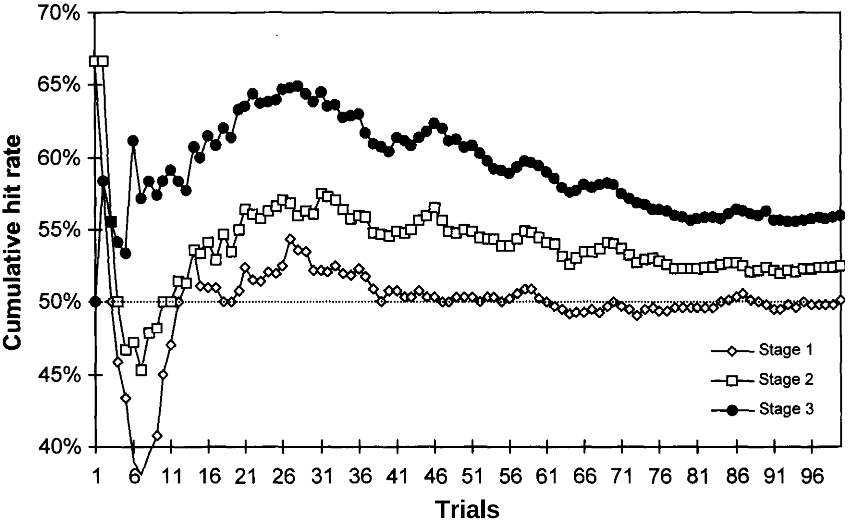

Fig. 2. **Cumulative average hit rates obtained in six runs of 100 trials each.**

obtained, if it ranges from 1 to 80 then the link $p = 0.8$ is followed, otherwise the $p = 0.2$ link is followed.

After many repeated trials N, a successful participant will generate a hit rate at Stage **3** that is greater than chance expectation, i.e. $hr_3 > 0.5$. If hr_3 and N are sufficiently large, this would provide evidence that the random Markov chain system behaved in alignment with the intentional goal.

Results. Figure 2 shows the cumulative average hit rates, per trial, over the course of six 100-trial pilot runs contributed by the author. The graph shows that by trial 100 the cumulative average hit rate at Stage *3* ended up at $hr_3 = 0.55$. Stage 2 showed somewhat less of a bias, and Stage 1 was close to chance expectation, $hr_1 = 0.50$.

Figure **3** shows the likelihood that the results of the pilot test were due to chance. Stage **3** ended up with overall odds against chance of about 1,000 to 1, and at about 50 trials Stage 3 peaked with odds of over 50,000 to 1.

Say that these results were caused not by some form of MMI, but by a malfunctioning RNG. What bias would have been required at Stage 1 to produce the terminal hit rate observed at Stage 3? Calculation shows that with a sufficiently large hr_1 bias at Stage 1, as shown in Figure 4, we could end up with the observed terminal value for hr_3. But notice that this bias around trial 30 would have to be as high as 90%, rather than the 50% expected from a binary RNG. Given that the observed data show that hr_1 at trial 30 was about 54% and not 90%, the observed test outcome is inconsistent with a Stage 1 biased RNG.

Instead of assuming that a large RNG bias appeared only at Stage 1, what if a small but constant bias was operating in Stages 1, 2, and 3? Calculations,

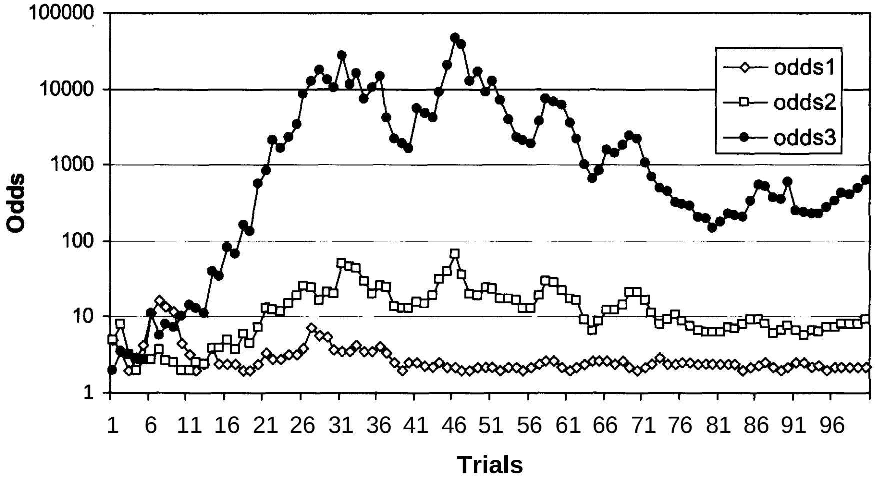

Fig. **3.** Results of pilot experiment in terms of odds against chance.

illustrated in Figure 5, indicate that a constant, above chance bias of +3% applied to Stages 1 and 2 of the Markov chain would result in the observed terminal $hr_3 = 56\%$ (the new hr_1 would be the original $hr_1 * 1.03$, and then $hr_2 = hr_1 * 1.03$). Unfortunately, the curves shown in Figure 5 do not match those of the observed data, so a small but persistent RNG bias does not provide a viable explanation.

What if the terminal $hr_3 = 56\%$ somehow bypassed the ordinary flow of time via intention and manifested a bias "in the future"? We could then run the

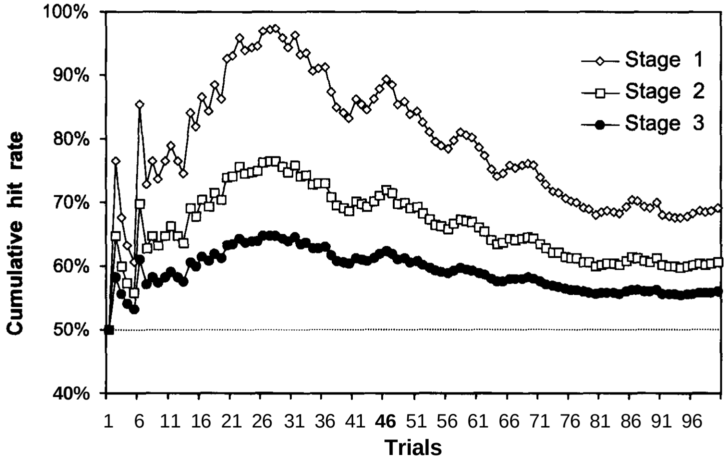

Fig. 4. Hit rate required at Stage 1 to produce the observed hit rate at Stage **3.**

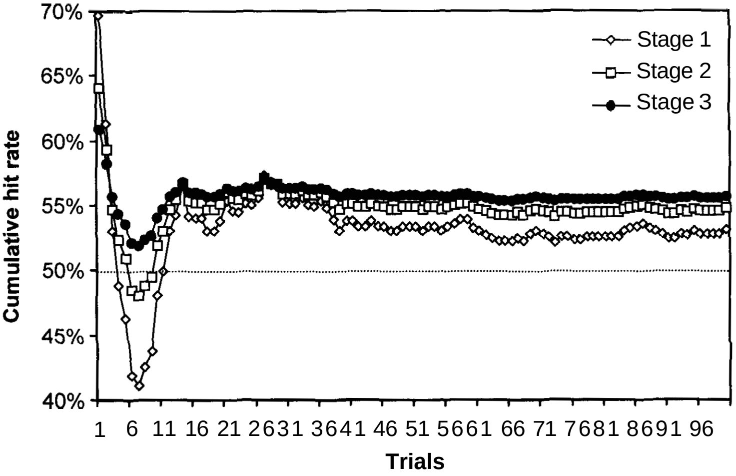

Fig. 5. Results of a constant **3%** forward-time bias. This produces the observed terminal hit rate for Stage **3** (56%), but the shapes of the resulting curves do not resemble the observed results.

observed hr_3 curve backwards through the Markov chain to see what hit rates would have resulted at Stage 2 and Stage 1, assuming no other influences. Figure 6 shows the outcome. The line labeled "Stage 3" shows the results originally observed at Stage **3.** The line labeled Stage 2 shows what would have happened at Stage 2 by running through the Markov chain backwards, starting from Stage 3. And the line labeled Stage 1 is what results after running backwards from Stage 2. These curves are now much closer in appearance to the observed

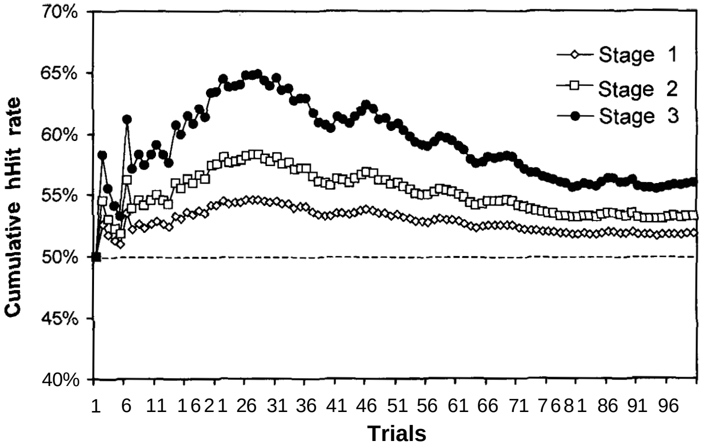

Fig. 6. Results obtained by running Stage **3** "backwards" through the Markov chain to see what Stage 2 and Stage 1 would look like. These curves are closer to the observed data than either Figures 5 or 6.

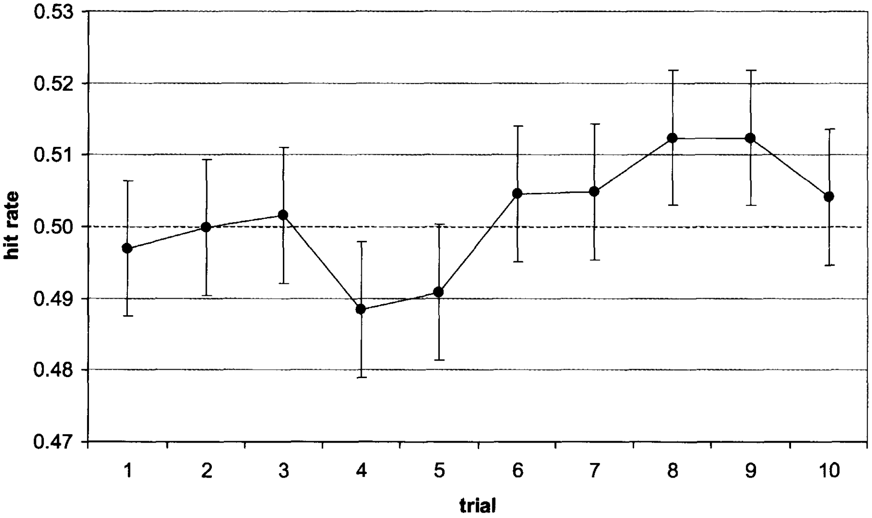

Fig. 7. Terminal hit rates and ±1 standard error bars for experimental data at each stage in a 10-stage Markov chain experiment, N = 2,787 trials.

results. This simple analysis suggests that the observed results may be better modeled as a process running backwards in time from a future "target," rather than as a more complex process running forward in time trying to hit that target.

After this initial study, two more pilot studies were conducted, followed by two replication experiments. Studies 2, 3, and 4 did not result in significant MMI effects, but to avoid selective reporting problems, they are included in this report.

Study 2: 10-Stage Markov Chain

This pilot study involved 17 participants (not including the author) who together contributed 2,787 trials in a 10-stage Markov chain experiment. The design was an extension of the 3-stage chain used in the initial pilot experiment, except that feedback was provided at Stage 10 rather than Stage 3, and participants were asked to click on one of two buttons. If they selected the "correct" button, they would hear a sound clip. What they did not know was that on each trial each button was randomly assigned to predict that the state at Stage 10 would either be a 1 or a 0. If the button they selected matched the final state at Stage 10, then they would hear a sound clip.

Each participant was asked to contribute at least one run of 100 trials, but he or she was allowed to contribute an unspecified number of additional trials if they wished. As shown in Figure 7, this experiment did not provide a significant deviation at Stage 10 ($hr_{10} = 0.504$, $z = 0.436$), so further model testing was not possible. However, note in Figure 8 that the hit rate at Stage 10, shown in terms

Fig. 8. Z score per contributed trial for the target Stage 10 in a 10-stage experiment.

of a cumulative z score, peaked around trial 50. This pattern of increasing hit rate up to about 50 trials, followed by a decline, was also observed in the first pilot study, and will be seen again later. Such "serial position effects" have been observed in previous MMI-RNG experiments and seem to reflect a combination of initial novelty followed by boredom, a problem common in all repeated-trial forced-choice tests (Dunne et al., 1994; Thompson, 1994).

Study 3: 1-6-10 Markov Chain

In this pilot study a 10-step protocol was employed, but rather than linking the intentional goal only to the last stage, the target stage was blindly and randomly assigned to Stages 1, 6, and 10. The pilot experiment involved two participants, each of who contributed a single run of 50 trials. Neither participant was aware of the experimental design, which followed that used in Pilot Study 2. As shown in Figure 9, none of the stages resulted in significant deviations, so model testing could not be applied to this data.

Replication Experiment

Study 4: 3-Stage Markov Chain

This test was conducted on three laptop PCs running Windows 98, 2000, and XP operating systems, each using an Orion RNG, and programmed in Microsoft Visual Basic 6.0 by the author. As in Pilot Study 1, participants were asked to press a single button with intention to cause the PC to play a sound clip. Unlike

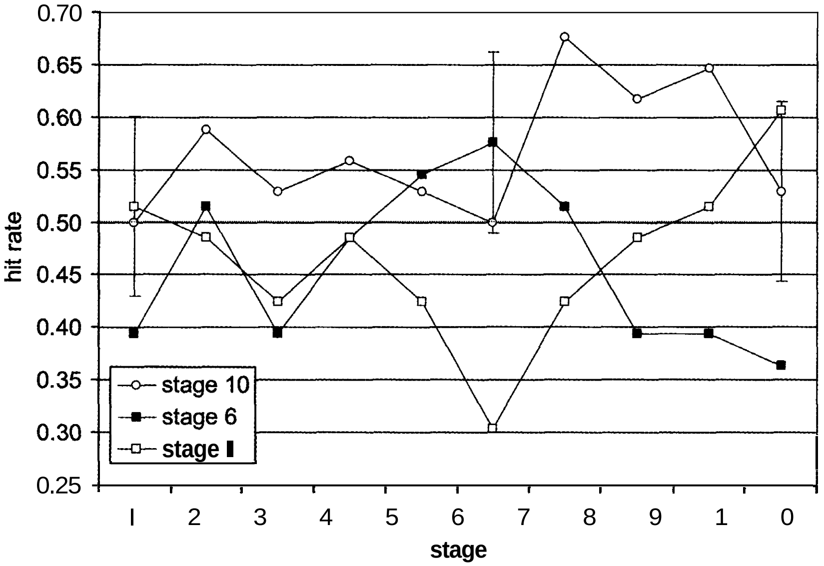

Fig. 9. Hit rates at each stage in a 10-stage Markov chain experiment, with one standard error bars shown at the target stages. The hit rate on each stage is above chance expectation, and the overall hit rate across all three stages was 54%, but with only 100 trials this was not a significant deviation.

in Studies 1 to **3,** in this study a trial that "missed" the target (i.e. the random system ended up in state 0 at the target stage) was signified by the PC playing a short click sound. This feature was added to provide a positive indication of a "missed" trial.

It was explained that the experiment would take about 15 minutes to complete, but nothing was said about how many trials should be contributed or about the underlying test design. It was suggested that each trial might be conducted in a lighthearted manner, trying to maintain a sense of curiosity and excitement about the audio clip they might hear, and to approach each trial as though it was being conducted for the first time.

A persistent challenge in conducting these experiments is that a significant deviation from chance is required to conduct meaningful tests of explanatory models. In an attempt to optimize the likelihood of obtaining significant results, the following rudimentary "talent threshold" scheme was explored: If after the 50th trial in the 3-stage Markov test, a participant had achieved a Stage 3 hit rate $hr_3 < 56\%$, then that person's experimental session would end (it would appear as though that was the natural end-point). If a participant obtained $hr_3 \geq 56\%$, then the experiment would continue on to 150 trials. Note that under the null hypothesis extending data collection beyond 50 trials cannot increase the overall results.

Results. Thirty participants provided a total of 2,118 trials; 23 people provided 50 trials each, five people provided 150 trials, one provided 79 trials,

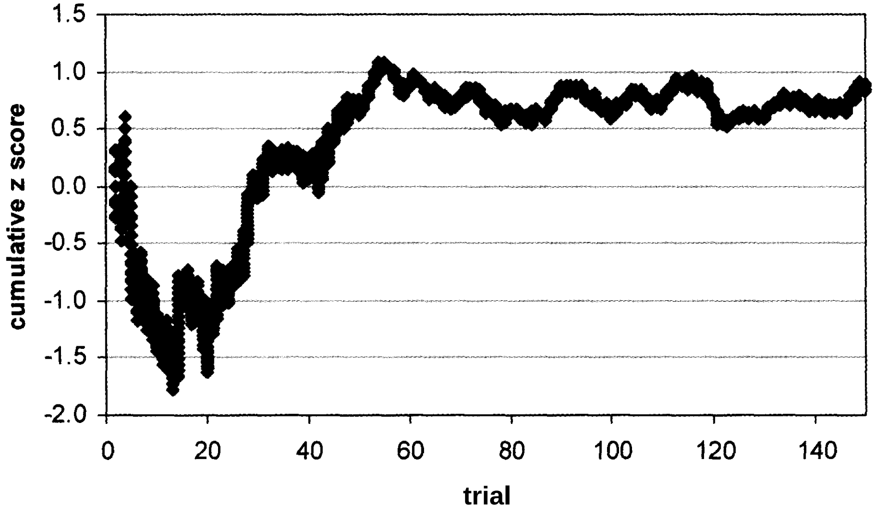

Fig. 10. Cumulative z score for observed hit rate at Stage 3, across all trials, N = 2100, contributed by all participants in the replication 3-step Markov test.

and one provided 139 trials. The latter two incomplete runs were due to the PC unexpectedly freezing while sampling from the RNG. One suspected cause of these failures is that the PC occasionally interpreted an incoming random byte as an operating system command; another possible cause is that the PC's serial input buffer overflowed as the RNG continuously outputs random bytes.

The terminal z scores in this experiment were not significant either for the six participants who surpassed the talent threshold, or for all participants combined (terminal $hr_3 = 0.509$, $z = 0.826$). As shown in Figure 10, and as observed in the pilot studies, the statistical results for Stage 3 peaked around trial 50 with $hr_3 = 0.514$, $z = 1.075$, and then remained flat. This indicated that the talent threshold scheme failed to work; it also meant that the models could not be tested with these data.

Study 5: 10-Step Markov Chain Test

This experiment was a 10-stage Markov chain test conducted on the same three laptop PCs as in Study 4, but with two additional RNGs: (1) an idQuantique RNG that provided random numbers based upon the path that individual photons took upon striking a half-silvered mirror (Stefanov et al., 1999), and (2) a Microsoft Visual Basic 6 PRNG algorithm, which was reseeded on each successive trial based upon the PC's clock at the time of the participant's button press. A high-resolution clock was used to generate this seed-number; on the Windows 98 PC the timer had millisecond resolution, and on the Windows 2000 and XP PCs it ran at the CPU clock speed, providing sub-microsecond resolution.

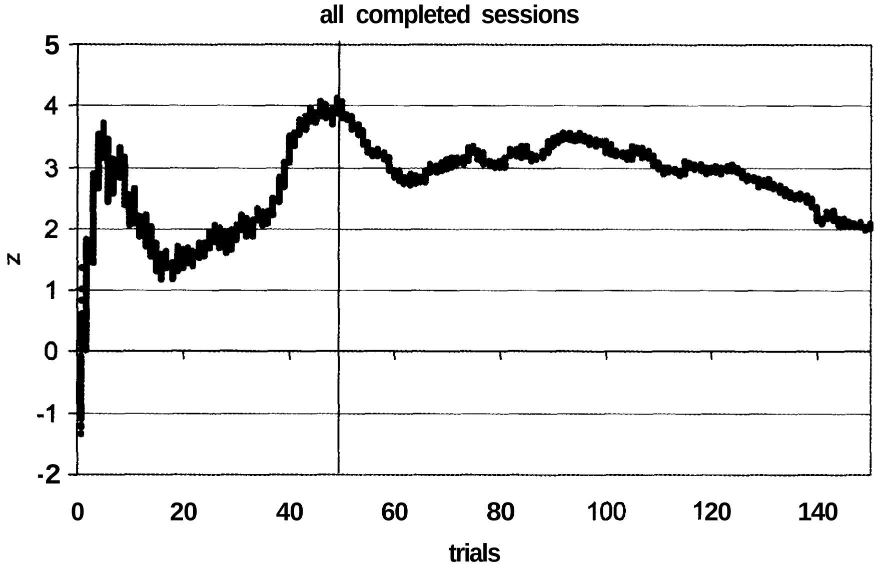

Fig. 11. Cumulative z score for all completed trials, N = 15,937.

A new design element was introduced in this study. In addition to use of sound clips for feedback, a trial-by-trial updated hit rate was shown on the PC monitor, and an initially gray rectangle slowly turned green if the hit rate remained above 50%, or slowly turned red if the hit rate fell below 50%. This was added in an attempt to offset the decline in performance noted in previous experiments.

Results. A total of 15,930 trials were contributed in 218 sessions by 163 people (not including the author): 14,091 trials in 149 completed sessions of 50 or more trials and 1,839 trials in 69 incomplete sessions with fewer than 50 trials. Figure 11 shows the cumulative z score based on the overall hit rate for all 149 completed sessions by trial number, regardless of which target stage (3, 6, or 10) the hit occurred in or the type of RNG. The terminal hit rate was significant at $z = 2.03$, $hr = 0.509$, $p = 0.02$.

Notice a decline in the cumulative z score after the 50 trial "talent threshold," suggesting that boredom eventually dominated performance. At 50 trials, a total of 7,450 trials (149 sessions X 50 trials) had been contributed, resulting in $z = 3.82$, $hr = 0.522$, $p = 0.00007$.

Table 1 shows the statistical results for the first 50 trials per participant for each of the studies reported here, and combined. While these are post-hoc observations, the combined deviation of 52% is almost certainly not due to chance ($p = 3 \times 10^{-6}$). Thus, to provide a significant subset of data with which to test the models, the subset of "the first 50 trials" per person was used in the subsequent analyses. Obviously, if this were a proof-oriented experiment such

TABLE 1
Hit Rates (hr) and Total Number of Trials Contributed at Trial 50 per Participant, and Associated Z Scores and P Values (One-Tailed) for the Five Studies Reported Here

Study	hr	Trials	Hits	z	P
Pilot 1	0.61	300	182	3.70	0.0001
Pilot 2	0.52	1,437	742	1.24	0.11
Pilot 3	0.54	100	54	0.80	0.21
Formal 4	0.51	1,500	763	0.67	0.25
Formal 5	0.52	7,450	3,889	3.80	0.00007
Total	0.52	10,787	5,630	4.55	3×10^{-6}

selection of data would not be permissible, but given that the purpose of the present study was to explore different models, the selection seems justified.

"Failed" sessions. Before discussing the model testing, Figure 12 shows that the cumulative z score based on the 1,839 trials in 69 incomplete sessions was nearly significantly negative, $hr = 0.478$, $z = -1.889$, $p = 0.059$ (two-tailed). The most likely primafacie reason for such a negative result is optional stopping, as participants in this test received trial-by-trial feedback, so it is conceivable that those who were doing poorly simply opted out of the test before finishing their 50 trials. However, in this case participants were specifically encouraged not to voluntarily opt out, and in fact none did. Thus, all of the incomplete sessions were

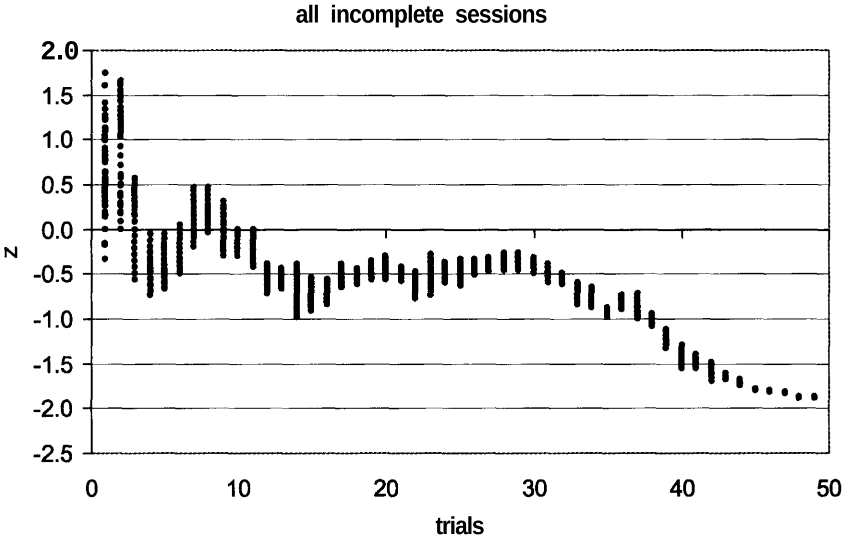

Fig. 12. Cumulative z score for all incomplete sessions with <50 trials, total = 1,839. A range of z scores is displayed for each trial because, e.g., say 22 people each provided 10 or more trials. The cumulative z score at trial 10 is recalculated as each of these 22 responses are successively added to the cumulative dataset; the resulting new z scores are thus all plotted at trial 10.

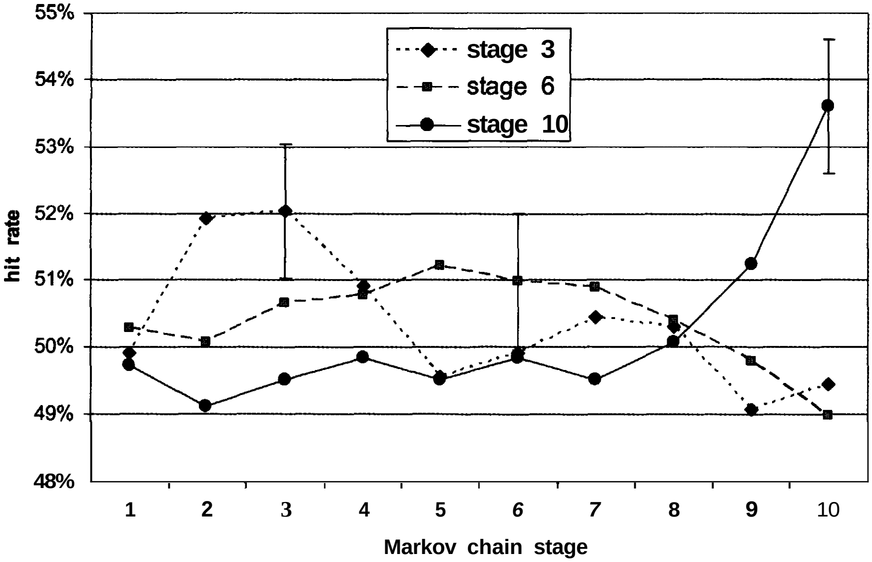

Fig. 13. Hit rates and one standard error bars associated with each stage in the Markov chain, for each of the three target stages.

cases in which the PC froze and the experimental program could not be restarted without rebooting the PC. Whether it is merely a coincidence that individuals doing poorly in this test opted out "by accident" is unknown. But the observation suggests that the assigned intention in an experiment, which is often assumed to be stationary, is more likely dynamically modulated by psychological factors as the experiment progresses. In support of this speculation, we noted a "first timer" blush of success within the first few trials in both the completed and incomplete sessions. The combined z score at the fifth trial for all participants combined peaked at $z = 3.00$, $N = 1{,}001$ trials, $p = 0.001$.

Hit rates on the target stages. Figure 13 shows the terminal hit rates for the three target stages based on the 149 completed sessions of 50 trials each. The observed hit rates are associated with $z_3 = 2.04$, $z_6 = 0.97$, $z_{10} = 3.60$, thus participants hit particularly well on target Stage 10. The Stage 6 results were not significant, but for the sake of completeness the models were applied to all three curves. These results indicate that somehow those participants who completed at least 50 trials were able to "cause" the random Markov system to be in the 1-state at either Stage 3, 6, or 10, even when they were blind to the target state.

Calibration tests. Potential biases in the true RNGs and experimental design were checked with calibration tests. The Orion RNG calibration consisted of 1 million trials (about 10 million bytes) run with the identical programming code used in the experiment, except with a timer in place of a human. This resulted in an overall $hr = 0.5003$ for targets at Stages 3, 6, and 10 combined, $z = 0.686$,

$p = 0.25$. Thus, the Orion RNG did not appear to exhibit any inherent biases that could have produced the observed results.

The idQuantique RNG is a relatively new device and has not been previously used in MMI research, so a total of 3 million calibration trials (about 30 million bytes) were run. These were randomly distributed among the three target stages to provide about 1 million trials in each condition. Results showed an overall $hr = 0.5002$, $z = 0.597$, $p = 0.275$. Hence, there was no evidence that the true RNGs or the testing software was biased.

Model testing. With x referring to a target stage number, the three models tested were as follows:

- ☐ Model 1: Push *forward* or PK, a forward-time causal model. This assumes that we start at the observed Stage 1 hit rate (hr_1) and the RNG is influenced by MMI at each successive stage with a constant bias until we reach the observed hit rate at hr_x.
- ☐ Model 2: Relax backwards or precognition, a passive retrocausal model. This assumes that individuals "know" the right time to take advantage of fortuitous fluctuations in the RNG output so as to select biased deviations at hr_6. This model imagines that we start at hr_x and passively "relax" backwards from Stage x to Stage 1 and then forward again from Stage x to Stage 10 by following the Markov chain transition probabilities. This curve, r_i is defined as $r_{i-1} = (0.8\ r_i) + (0.2\ (1 - r_i))$ for $i = 2$ to x, and $r_{i+1} = (0.8\ r_i) + (0.2\ (1 - r_i))$ for $i = x$ to 10, and $r_x = hr$,.
- ☐ Model 3: Pull backwards or retroPK, an active retrocausal model. This assumes that MMI both sets the observed hr_x and applies a constant bias to the RNG output backwards in time. That is, $p_i = k * r_i$, where i refers to the stage number, r_i is the value determined by the relax backwards model at each stage, and k is a constant MMI bias. The constant k is determined such that it minimizes the value $s = \sum_{i=1-x} |(r_i - o_i|$, where o_i refers to the experimentally observed hit rate and r_i refers to the hit rate predicted by the relax backwards model.

Figures 14 to 16 show the observed and modeled hit rates for the three target stages. As in Pilot Test 3, goodness-of-fit for each model was determined by forming a linear correlation between pairs of curves and then ranking those correlations by magnitude. Table 2 indicates that the best fit in each case was the pull backwards curve.

Comparison of hit rates for true RNGs and PRNGs. Another way of examining the results of these tests is to compare performance on the truly RNGs versus PRNGs. Hit rates on successive states within the Markov chain are biased by design, but with true RNGs they are not pre-determined. By contrast, successive states derived from the outputs of PRNGs are determined at the moment of the button press that begins each trial, because that is when the

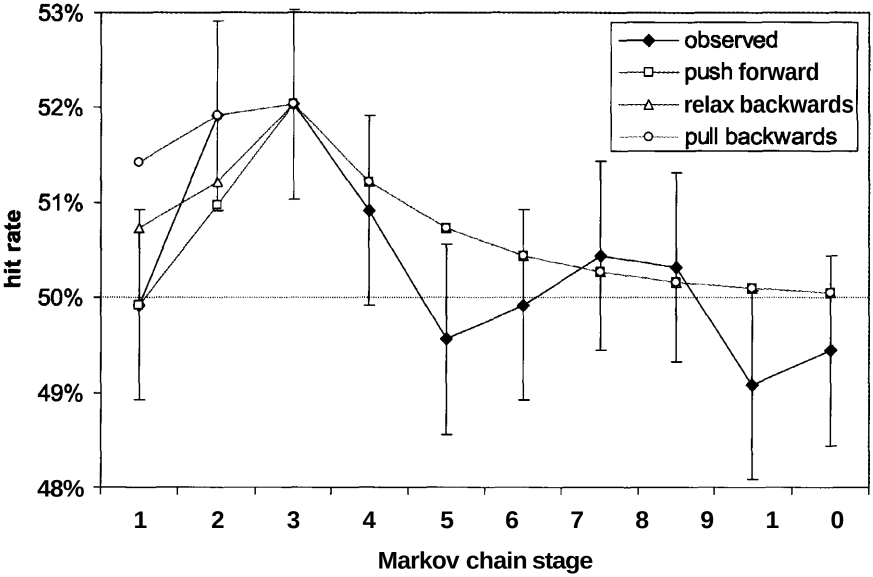

Fig. 14. Observed hit rates when Stage 3 was the target (solid line), and three curves (dashed lines) predicted by the three models. Models were tested only for the hit rates at Stages 1, 2, and 3, as all of the models assume that once the target stage is reached that the remainder of the stages passively follow the Markov chain transition probabilities.

PRNG seed-number is generated, and that seed-number is the only truly random component of those trials.

There are more opportunities for a true RNG to be influenced than there are for a PRNG. If we are dealing with a mechanism that is causally influencing the random system, then this should presumably lead to higher hit rates with true RNGs than PRNGs. On the other hand, if we are dealing with precognition or retroPK which allow the participant to respond in such a way as to probabilistically favor a desired future end-stage, then the hit rates with the two RNG types should be approximately the same.

Figure 17 shows that five of the six effect sizes ($e = z/\sqrt{N}$) are statistically indistinguishable. This again suggests that a PK model is not a good explanation, and that a precognition or retroPK explanation is more likely. The exception was from an Orion RNG running on a Windows 98 laptop. That PC was also responsible for most of the session failures (45%).

Discussion

Analysis of the two successful experiments, Studies 1 and 5, suggest that the observed deviations may have been due to a retrocausal effect. Because this still may not be clear to some readers, to illustrate why this conclusion seems warranted, consider Figure 18. This shows the observed data, five forward-in-time

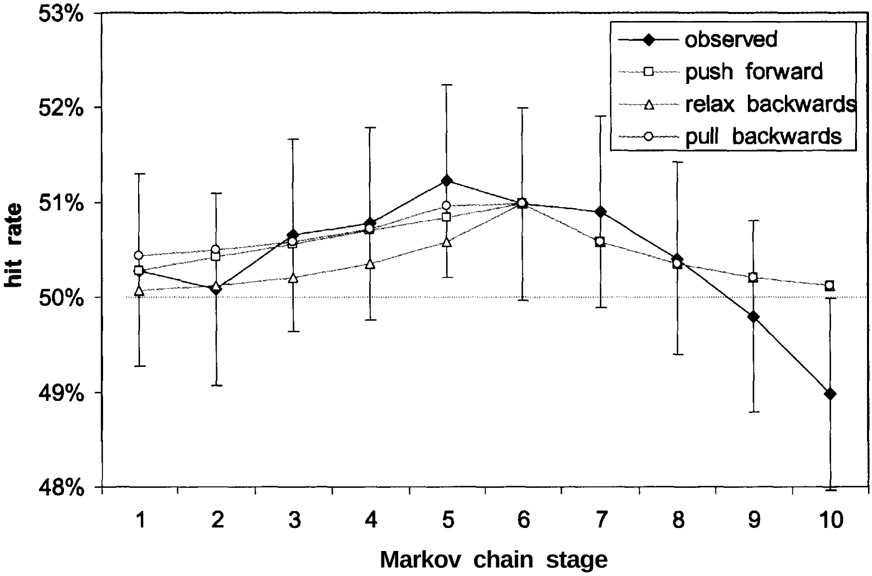

Fig. 15. **Model testing results when Stage *6* was the target.**

models, and one backwards-in-time model, all ending on the hit rate observed at Stage 10 in Study 5.

The five forward-in-time models start at Stage 1 with an initial hit rate of 0.50, 0.52, 0.54, 0.56, and 0.60. We then add a constant factor to each successive stage so the curve ends up at the experimentally observed hit rate at Stage 10 of 0.536. Calculation shows that the required constants range from 1.41% to 1.45%. That is, given the constraints imposed by the transitional dependencies of the Markov chain, if we began with a 60% hit rate at Stage 1, we would have to add a 1.4% bias to the hit rate resulting at each stage in order to end up at the empirically observed 53.6% hit rate. Likewise, if we began at a 50% hit rate at Stage 1, we would have to add a 1.4% bias at each stage to end up at the same spot.

A 1.4% added bias per stage means that instead of the RNG producing 0’s and 1’s each with 50% probability, it has to produce l's, on average, 51.4% of the time. This is not an especially large bias, so imagining that such an MMI effect could be imposed on an RNG is not unthinkable. The problem is that the shapes of the theoretically expected forward-going curves differ dramatically from the observed results. By contrast, a backwards-going curve *without any bias added* is much closer to the observed results. This is why a retrocausal model appears to be superior to models following ordinary causal processes.

Reconsidering Causality

There is no need to argue that common sense is highly effective in negotiating the mundane activities of daily life. But when faced with understanding the

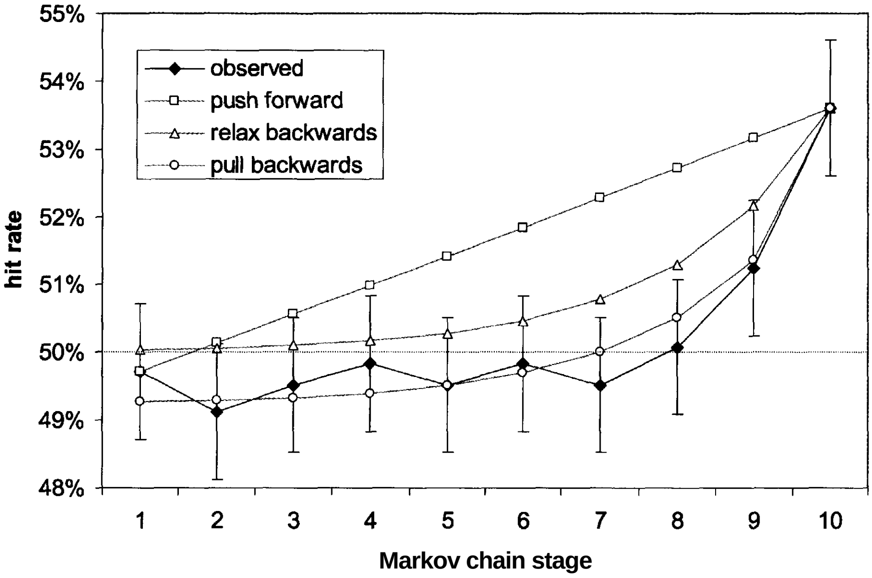

Fig. 16. Model testing results when Stage 10 was the target.

fabric of reality, common sense regularly fails. Modern physics has revealed, for example, that matter, energy, space, and time are not the separate entities suggested by common sense, but rather deeply intertwined relationships.

Given how many common sense assumptions have cracked under scientific scrutiny, it should not be surprising to learn that when causality is closely examined, it too breaks down. Indeed, causality has generated more disquiet among scientists and philosophers than is commonly appreciated. As Bertrand Russell put it in 1913,

> All philosophers imagine that causation is one of the fundamental axioms of science, yet oddly enough, in advanced sciences, the world 'cause' never occurs .... The law of

TABLE 2
Model Testing Results in the Form of Correlations (with Bias Constants in Parentheses)

	Target stage		
Condition	3	6	10
Push forward	0.89 (1.021)	0.88 (1.003)	0.73 (1.008)
Relax backwards	0.82	0.78	0.95
Pull backwards	**0.99** (1.014)	**0.94** (1.007)	**0.97** (0.985)

Note: Results for each of the three target stages indicate that the best fit is the pull backwards models, suggesting retroPK. For example, the best fit to Stage 10 requires that Stage 9 follows the Markov chain transitions backwards and in addition is "pulled down" with a 0.985 bias, or $hr_9 = 0.985 * M_9$, where M_9 is the Markov chain calculation for Stage 9, $hr_8 = 0.985 * M_8$, etc.

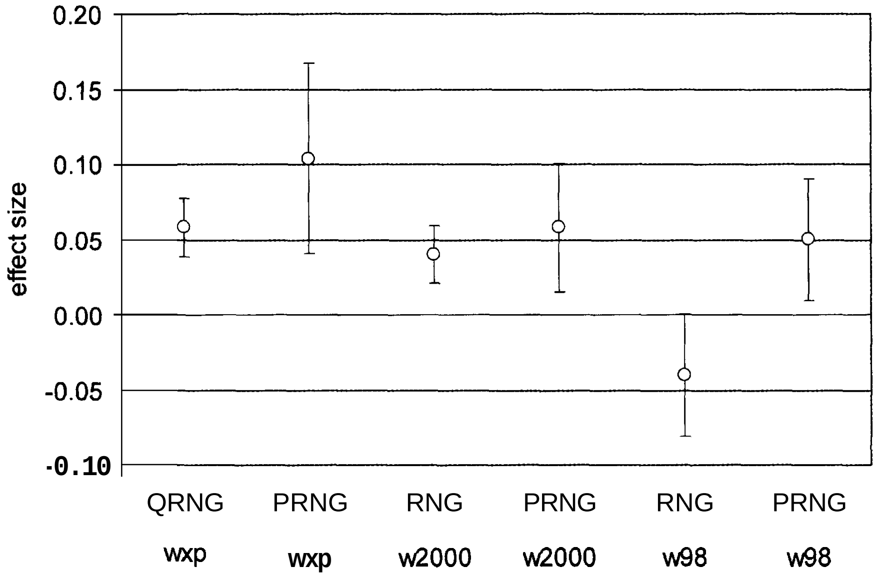

Fig. 17. Comparison of effect sizes on the three different PCs, showing that five of the six RNGs produced statistically indistinguishable result. wxp refers to Windows XP operating system, w2000 refers to Windows 2000, and w98 refers to Windows 98. QRNG refers to the photon-based quantum RNG, PRNG refers to the Visual Basic 6 pseudorandom number generator, and RNG refers to the Orion noise-based **truly** random RNG.

> causality, I believe, is a relic of bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm. (cited in Pearl, 2000: 337)

And as John von Neumann explained in 1955,

> We may say that there is at present no occasion and no reason to speak of causality in nature – because no [macroscopic] experiment indicates its presence . . . and [because] quantum mechanics contradicts it. (cited in Rosen, 1999: 88)

In spite of questions about the fundamental nature of causality, cause→effect sequences certainly seem to be adequate for understanding experience at the human scale. We might expect that hitting a nail with a hammer would provide proof-positive of a force-like, unambiguous causal event at the macro scale. You hit the nail with the hammer and it moves--end of argument. But what if the nail were stuck in a steel bar that resembled wood? Or if the nail were close to its melting temperature, or if the hammer were made out of foam rubber, or . . .. We soon see that any example proposed as an irrefutable case of "absolute" causality can be qualified. And when we begin to entertain conditionals we are forced to redefine causation as a special form of asymmetric relationship, or as a correlation with a higher probability link in one direction than the other. In this sense, the

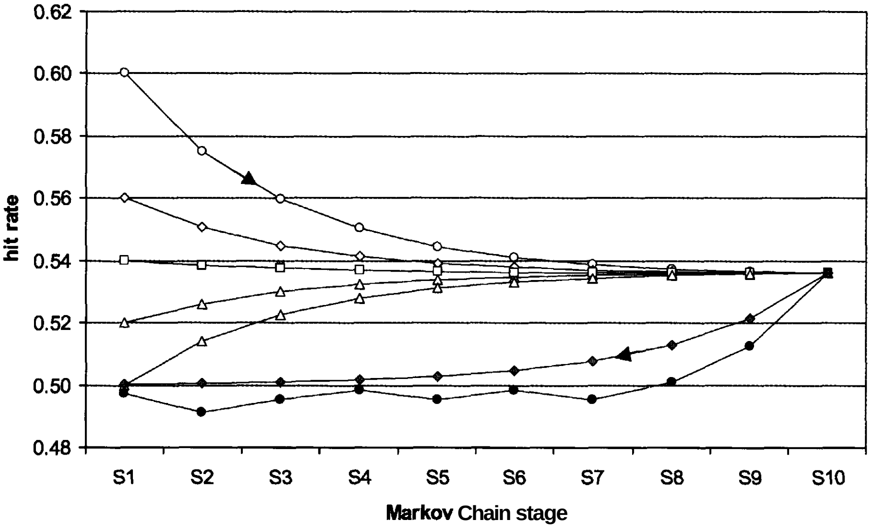

Fig. 18. Comparison of five forward-in-time models (white symbols), each requiring a constant MMI bias imposed at each stage of the Markov chain, a backwards-in-time model that does not require any bias (gray diamonds), and the hit rates observed when Stage 10 was the target in Study 5 (black dots).

ordinary notion of simple causality may be viewed a caricature of what is actually a set of highly complex relationships.

While absolute causality may be an oversimplification, most social, behavioral, neuroscience, and medical scientists would regard such arguments as philosophical quibbles. Common sense causality is regularly used to explain virtually all facets of human experience (Pearl, 2000), and I agree that common sense causality is a useful heuristic tool. But I suspect that it does not provide an adequate explanation for *all* experiences. Why? Because there is a growing body of empirical evidence indicating the presence of *acausal* processes. "Acausal" in this context means "not causal in the unconditional, unidirectional notion of cause—effect, but in the sense of conditional, time-reversed, cause←effect relationships."

Some philosophers regard the concept of time-reversal as a logical impossibility. Anthony Flew wrote, concerning the apparent time-reversed phenomena of precognition and retroaction, that if

> both are to be defined in terms of "backwards causation," this admission becomes the admission of a conceptual incoherence .... In these cases, although anomalous and statistically significant conditions may indeed have been found, these correlations most categorically cannot point to *causal* connections. (cited in Broderick, 1992: 134)

Flew may be correct when it comes to caricatures of absolute, unidirectional causality. But as we've seen, absolute causality dissolves like the Cheshire Cat when deconstructed. And in spite of what common sense insists, many physicists and philosophers are far less certain than Flew when it comes to the meaning of "causal" (Price, 1996). In fact, hundreds of publications in physics journals can be found which consider the implications and properties of time-reversed and time-symmetric phenomena. These include effects described by the formalisms of classical mechanics, general relativity, electrodynamics, and quantum mechanics (e.g. Elsasser, 1969; Etter, 1999; Rietdijk, 1987; Schulman, 1999; Tipler, 1974; Travis, 1992). The solutions to these physical theories are usually assumed to manifest only in exotic domains, e.g. under extremes of temperature, gravity, energy, mass, or speed, or at very short time-periods. And as a result, retrocausality is viewed as being possible in principle, but irrelevant for all practical purposes, or when it comes to understanding human experience.

However, if such exotic concepts did lurk deep within us, how might such experiences manifest? Consciously, they may emerge as precognitions of future events. Unconsciously, they might be experienced as intuitive hunches, gut feelings, or synchronicities. From an anecdotal perspective, there is little doubt that time-reversed phenomena have been reported throughout history and across all cultures (Radin, 1997b, 2006; Rhine, 1969). Many such reports can undoubtedly be completely explained by prosaic psychological reasons, like coincidence, misperception, distortions, and wish-fulfillment. But for over a century researchers have investigated these phenomena under controlled laboratory conditions.

Experimental Evidence for Retrocausal Effects

It is beyond the scope of this paper to thoroughly review the evidence for retrocausal effects, so I will simply mention five classes of experiments that support the concept of time-reversal within human experience. Collectively, this body of evidence consists of hundreds of experiments. They include

(1) forced-choice precognition tests (Honorton & Ferrari, 1989),
(2) free-response precognitive remote perception tests, including one large-scale study involving 653 trials (Dunne & Jahn, 2003; Jahn & Dunne, 1987; Targ & Katra, 1998; Targ & Puthoff, 1974),
(3) psychophysiological experiments studying central and autonomic nervous system correlates to future stimuli (Bierman, 2000; Bierman & Radin, 1997, 1998; Bierman & Scholte, 2002; Hartwell, 1978; Levin & Kennedy, 1975; May et al., 2005; McCreaty et al., 2004; Norfolk, 1999; Parkhomtchouk et al., 2002; Radin, 1997a, 2004; Spottiswoode & May, 2003; Wildey, 2001),
(4) retroPK experiments involving RNGs (Bierman, 1998), and
(5) retroPK experiments involving human behavior and activity (Braud, 2000).

Other Experimental Evidence for Retrocausal Effects

If time-reversed effects are genuine, we would not expect them to appear only in special experiments, or only for certain people at special times. They might ordinarily go unnoticed, but if they exist at all they would presumably be ubiquitous. Besides precognition experiments, where else might we find time-reversed effects?

One place is the mainstream psychological and neuroscience literature. The words used to describe possible time-reversed effects include euphemisms such as "exceptional situational awareness," used to describe the performance of jet fighter pilots who respond faster than they "should" be able to in combat dogfights (Hartman & Secrist, 1991). Another is "time-reversed interference" (Klintman, 1983, 1984; Radin & May, 2000; Saava & French, 2002), originally discovered as an anomaly associated with baseline reaction times in an otherwise ordinary "Stroop task," a common experimental method used to demonstrate cognitive-perceptual interference (Stroop, 1935). Other terms include "anticipatory systems," used to describe how biological systems plan and carry out future behavior (Rosen, 1985), and terms like "postdiction" (Eagleman & Sejnowski, 2000), "subjective antedating" (Wolf, 1998), "tape delay" (Dennett, 1992), and "referral backwards in time" (Libet, 1985), all referring to neurological mechanisms proposed to explain how we become conscious of events that actually occurred milliseconds in our past. An example of the latter is the "color phi" effect described by Dennett (1992):

> If two or more small [colored] spots separated by as much as 4 degrees of visual angle are briefly lit in rapid succession, a single spot will seem to move .... What happened to the color of "the" spot as "it" moved? The answer ... was striking: The spot seems to begin moving and then change color abruptly *in the middle of its illusory passage* towards the second location .... (emphasis in the original, p. 5).

How are we able to fill in the second color spot *before* the second flash occurs? In Dennett's words:

> Unless there is precognition in the brain, the illusory content cannot be created until *after* some identification of the second spot occurs in the brain. (p. 5)

Here Dennett recognizes that precognition can straightforwardly resolve the back-referencing problem, but he dismisses it in favor of speculative theoretical mechanisms based on metaphors like "tape delays" and "editing rooms."

Despite such dismissals, some experiments not explicitly designed to study retrocausal effects nevertheless appear to have encountered them. In an article published in *Science* I noticed that an experimental result closely resembled what I had observed in my presentiment experiments (Bechara et al., 1997; Radin, 2004). I mentioned this in passing to Prof. Dick Bierman (University of Amsterdam), who subsequently located three datasets from previous published experiments, all using skin conductance measures and design protocols similar to the presentiment experiment (Bierman, 2000). These involved an animalphobic study (Globisch et al., 1999), a gambling study (Bechara et al., 1994, 1996, 1997), and the effect of emotional priming on the evaluation of Japanese characters (LeDoux, 1996; Murphy & Zajonc, 1993). In all three of these datasets, Bierman found anomalies closely resembling the presentiment effect: skin conductance levels preceding randomized emotional stimuli were higher than before calm stimuli. The combined result across the three studies was associated with odds against chance of 300 to 1, suggesting that retrocausal effects may "contaminate" experimental studies being conducted for other purposes.

Implications

The effects considered here raise the specter of a logical paradox. Wouldn't information from the future change the present and thereby change the very future from whence the information originated? And wouldn't that create an intolerable temporal recursion?

The answer is yes, but only if the future were destined to occur in only one way. In that case it would not be logically possible to change a foregone conclusion. However, if the future were inherently probabilistic, then the "hard" paradox blurs into a softer and more tractable puzzle. In any case, it is not suggested here that time-reversed effects necessarily change the past. As Braud (2000) put it,

> Once an event has occurred, it remains so; it does not "un-occur" or change from its initial form. It appears, instead, that the intentions, wishes, or PK "efforts" influence what happens (or happened) in the first place.

In other words, time-reversed effects may probabilistically influence past events that were disposed to being influenced at the time, but the same influence cannot change what actually did occur, nor can it change events that are not susceptible to probabilistic influences in the first place.

If retrocausal phenomena did exist, then among other things the meaning of "controlled scientific experiment" must be reconsidered. Any reasonably designed experiment controls for factors like environmental influences and experimenter expectancies. However, even gold-standard randomized, controlled, double-blind trials do not (and perhaps cannot) control for what might called "transtemporal" influences. In fact, any experiment involving any human decision made before, during, or after data collection may be hypothetically vulnerable to time-reversed influences. This includes virtually all studies in all of the experimental sciences. While the magnitude of time-reversed influences is not well understood, it is plausible that some well-known experimental biases presently attributed to other causes, such as Rosenthal's "experimenter expectancy effect," may actually be due to time-reversed components (Barber, 1976; Rosenthal, 1976, 1978).

A second implication of retrocausation is that it justifies a reassessment of Aristotle's "final cause." Sidestepping difficult questions about whether we should more profitably think of causality in terms of force or correlation, most scientists today assume that of Aristotle's four causes, the only one worthy of attention is "efficient cause." Efficient cause in, say, building a chair involves the act of a hammer hitting a nail into wood. Material cause refers to the wood and nails involved in making a chair, formal cause refers to the design of the chair, and final cause refers to the underlying purpose of making the chair in the first place.

Science considers material and formal causes to be interesting but largely irrelevant in understanding the mechanisms underlying chair construction, and final cause is dismissed altogether because teleology is thought (by some) to suspiciously resemble theological guidance. But what if such guiding goals are not imperatives from the gods, but rather influences from our future selves? This idea is similar to the thrust of research on "anticipatory systems" in biology and computing (Rosen, 1991), but here, of course, I mean more than present-time influences created by our capability to inferentially forecast the future. I mean goals that actually come from the future, through retrocausal processes.

Conclusion

Some forms of apparent MMI may involve processes that are more consistent with retrocausal "pulls" from the future than with causal "pushes" from the present. Determining which process best fits the data is important if one wishes to develop MMI for pragmatic purposes, because causal mechanisms are amenable to investigation using familiar experimental methods, but retrocausal and acausal effects are not.

Acknowledgments

The designs in these experiments were inspired by the ideas of Thomas Etter (1960, 1977). I am also indebted to Richard Shoup, Edwin May, and Russell Targ for many conversations about models of MMI effects. The pilot tests were conducted under the auspices of Interval Research Corporation, Palo Alto, CA. The formal replications and preparation of this report were funded by the Samueli Institute for Information Biology. I am also grateful to Dr. Cassandra Vieten, Charlene Farrell, Tina Amorok, and Jenny Matthews for helping to recruit and run participants in this experiment.

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