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EDITORIAL

As many JSE readers know, there was a considerable hubbub recently over a research paper scheduled to appear in the *Journal of Personality and Social Psychology*, written by Cornell University psychologist (and *JSE* Associate Editor) Daryl Bem. In that paper, Bem describes a series of nine experiments, involving more than 1,000 participants, apparently showing that subjects react physiologically to stimulus events whose random selection has yet to occur. Thus, the experiments seem to show (as Bem puts it) "the anomalous retroactive influence of some future event on an individual's current responses."

In my view, Bem's experiments are methodologically elegant and exceptionally clever. One reason this study is so notable is that Bem took several extensively analyzed psychological effects already vetted in the professional literature, and simply tested for them in a seemingly time-reversed direction, by collecting the subjects' responses prior to randomly choosing the corresponding stimulus events. Furthermore, four of the nine experiments replicate the experiments immediately preceding them in Bem's study.

Bem's report has attracted an extraordinary degree of national and international attention. He's been both excoriated and praised in prominent print media, blogged about endlessly, and interviewed on major television programs. Needless to say, this is an unusual degree of attention for academic parapsychological research, and presumably that's because (a) Bem is a prominent and widely respected member of his field, and (b) Bem's study is being published in a major mainstream journal, not a specialist parapsychological journal, or (I regret to say) the *JSE*.

Rather than echo the issues already discussed at length about Bem's report (e.g., his statistical methods of evaluation, the proper application or viability generally of Bayesian techniques, or the respects in which the often shrill and ill-informed criticisms of the study reflect stupidity, conceptual panic, dishonesty, or intellectual cowardice within the scientific establishment), I'd like to mention two issues which (as far as I've seen) have escaped general attention.

The first is that nearly all parties in the dispute seem to accept what for many would be the default interpretation of evidence suggesting precognition—namely, that (if the evidence is reliable and authentic) it indicates a form of backwards or counter-clockwise causation. I can't explore the relevant issues here in detail (for that, see Braude, 1997). But I can at least remind readers that what Jule Eisenbud used to call the "active analysis" of precognition often (and perhaps always) seems to be a live option. Let me indicate very briefly why that is.

According to the active analysis, we don't need to suppose that a future state of affairs is a causal condition of an earlier event (e.g., that tomorrow's plane crash caused someone at an earlier time to have a precognitive dream of the crash). One alternative is to describe the earlier (precognitive) event as a form of clockwise, psi-mediated inference. That is, we suppose it results from presumably unconscious—and possibly imagery-rich—future projections based on real-time psychic scanning. It would be a parapsychological analogue of an engineer dreaming about the collapse of a building under construction after examining that building's blueprints and current condition at the construction site. So in the case of the plane crash, the "precognizer" might simply use ESP to determine the *present* mental state of relevant passengers or crew (or the precarious physical state of the plane), and then unconsciously draw a reasonable inference that bubbles up to awareness in the form of a dream or hunch. Among other virtues, this analysis avoids the so-called *intervention paradox*, which seems to arise when we want to say that an *accurate* precognition of an event *E* allowed us to prevent *E* from occurring. On this interpretation, the precognition (or prediction) would not be represented semantically by the categorical future-tense statement, "Event *E* will occur no matter what." Rather, the assertion in question would be the conditional or hypothetical statement, "*E* will occur unless __," where the blank is filled in by a description of steps for preventing *E*.

Now this gambit won't work for cases like those described in Bem's study, where the ostensibly future cause is selected by random processes that are non-inferrable in principle. For those situations, the active analysis posits a different possibility—namely, that the future event was *brought about* through clockwise psychic influence—for example, psychokinesis or telepathic influence.

There's quite a bit more that needs to be said in order to make the active analysis seem like a genuinely viable option. For example, one familiar, but very flimsy, criticism of the analysis is that it's simply implausible to think that people would bring about (or want to bring about) the sorts of disastrous events that often seem to be precognized. Quite apart from the fact that ostensibly precognitive experiences of (say) plane crashes or mine collapses can be interpreted as psi-mediated inferences, Eisenbud replied convincingly to that argument, as follows:

> . . . there is no disaster, of whatever magnitude of degree or horror, that has ever been foreshadowed in dream, premonition, or Delphic utterance that cannot be matched in effect by one that has been brought about by some individual deliberately and with full awareness of the consequences. . . . The record on this score is so extensive and so clear—from fatal child abuse to Hiroshima, from capriciously started wars to shocking acts of political terrorism—that there can be no reasonable argument about human propensities in

> this domain. The only question is whether there is a hidden part of the average well-acculturated human being, who cannot consciously imagine himself battering a child or bombing a school building, that is subject to the same impulses that actuate persons who are openly destructive. (Eisenbud, 1982:175)

Another quite different and thorny set of issues concerns the concept of causality itself, along with the common assumption—expressed in various ways by philosophers of science and physicists—that retrocausation is no different from clockwise causation except for the temporal direction of the causal arrow. However, this *mirror image* view of retrocausation is actually exceptionally problematical. For one thing, the positing of causal connections is a form of explanation, and the activity of explaining is irreducibly pragmatic and appropriate only relative to a surrounding context of inquiry.[1] And for another, events are not items in a perspective-independent warehouse of ontological furniture. The pie of history may be sliced in an indefinitely large number of ways, none of which is inherently privileged. But that means that ordinary clockwise causal connections must be parsed, pragmatically, out of an intrinsically undifferentiated web of happening running in the same temporal direction. That is, each causal connection is merely and necessarily a part of a more temporally extended clockwise causal story. And there will always be an indefinitely large number of extended stories we could tell for a putatively identified cause and effect, each of which makes sense in its own way of how the former event leads causally to the latter, and none of which is appropriate *simpliciter*, or succeeds as an explanation no matter what.

However, retrocausalists treat ostensibly precognitive links as isolated from a presumed surrounding web of retrocausal happening—that is, as having no retrocausal antecedents stretching indefinitely into the future and no retrocausal consequences extending indefinitely into the past. For example, we're not told what events retrocausally led to the earlier plane crash, or what retrocausal consequences flowed backwards from the precognitive experience. In fact, the events described in allegedly retrocausal chains (e.g., plane crashes and dreaming) are even *described* using clockwise causal terms. Plane crashes and dreams are sequences of events running from earlier to later.

The second point is that the concept of causation, like every other concept, can't be isolated from an extensive network of additional related concepts—in this case, the concepts of explanation, understanding, intention, decision, action, to mention just a few. So we can't radically revise the concept of causation to allow causal links to be isolated from a surrounding history without making far-reaching and arguably gratuitous changes to members of the enormous conceptual network of which it's a part.

So retrocausalists appear to be caught on the horns of a dilemma, neither

of which seems attractive or feasible. On the one hand they could try to do what no one has come even close to doing so far—namely, explain what kind of retrocausal history in fact surrounds ostensibly isolated retrocausal links. (I explore, in Braude, 1997, reasons for thinking this is a dead end). On the other hand, retrocausalists could abandon the mirror image view and defend the position that retrocausal links differ radically from clockwise causal chains. That is, they could argue that retrocausal links can indeed be isolated from a surrounding mass of happening, and that they are not necessarily pragmatically selected points within one or more members of an indefinitely large number of possible and larger retrocausal stories. But (as I noted above) since concepts are not isolable entities, the concept of causality can't, in fact, be amended or abandoned in that way without forcing deep revisions elsewhere in an extensive network of related and apparently otherwise acceptable concepts. Therefore, retrocausalists would have to justify the need for a large-scale conceptual revision when the data can be explained without it, and when the alternative active analysis merely requires a much less sweeping change to our world view—namely, simply extending the stage of operations for forms of psychic functioning for which there is already a considerable body of evidence. Therefore, retrocausalists would need to defend what at best seems to be an unnecessary and unparsimonious position.

At any rate, I doubt that many of Bem's critics, who recoil at the suggestion of retrocausation, would be any happier with alternative interpretations of Bem's results that posit refined or extensive exercise of ESP or psychokinesis. And I know, in any case, that my position is definitely a minority opinion—although of course I believe that it has the virtue of being correct.

So let me turn to the other aspect of the reaction to Bem's paper that seems to have received too little attention.

In a nutshell, the problem is that the storm over Bem's paper was thoroughly predictable, and in my opinion it illustrates a point I've been harping on for many years—namely, that typical (even if solid and creative) quantitative laboratory experiments in parapsychology are doomed to be both unconvincing to the scientific community at large, and also conceptually unilluminating. The usual complaints (even from open-minded parapsychological fence-sitters) about the quantitative results are variations on the claim that, somehow or other, the math is wrong or at least suspect. Typically, critics charge that improper statistical measures were employed and that with more appropriate or sophisticated analyses, the alleged positive effects evaporate. Others, with no particular methodological axe to grind, simply have trouble shaking off the sneaking suspicion that the allegedly significant odds against chance point to a mistake somewhere. Parapsychologists even debate these points among themselves and frequently argue with one another over whether there's *any*

convincing quantitative lab evidence for psychic functioning.

Moreover, meta-analyses certainly don't put an end to these disputes, because parapsychologists and non-parapsychologists alike debate whether the meta-analyses track the relevant features of the experiments. For example, when evaluating a collection of micro-PK tests, should a meta-analysis focus on individual random bits and assume (as some have) that the probability of a hit is the same per bit across all studies in the sample? Or should the meta-analysis focus also (or instead) on the number of bits generated per sample, the generation rate of bits, the duration of the experimental session, or the psychological conditions of the task? This is an especially tricky topic, because at bottom it's a version of the deeper and complex debate over what counts as an experimental *replication*. (For some thoughts on that issue, see Braude, 2002.) And that's just a specific instance of the even more fundamental philosophical debate over the nature of *similarity*.

In my view, what's always been needed are, first, psi effects so impressive that quantitative analyses are beside the point. These effects are plentiful enough, and include some ganzfeld and remote viewing hits that are so spot-on or so reliable that it's simply absurd to attribute the successes to chance. They also include PK results so dramatic and obtained under conditions so obviously clean that allegations of fraud are clearly and merely lame cries of protest (see Braude, 1997, Braude, 2007). And second, what's needed are gifted subjects who can produce results relatively consistently, with different experimenters and on numerous occasions. This helps diffuse the notorious "source of psi" problem, which arises acutely when conducting tests with unselected subjects. In those cases, there are so many unidentifiable and uncontrollable variables in the underlying causal nexus that it's never clear why the experimental results turned out as they did (see my Editorial in *JSE* 23:3, Fall 2009). These are cases for which ineffective quantitative analysis is the only way of defending the claim that something paranormal has occurred. But if you have a star subject who is regularly associated with conspicuously anomalous effects, we have at least a prima facie case for assigning that subject a key role in the causal nexus, and quantitative analysis quickly becomes irrelevant. For example, Joe McMoneagle's remote-viewing track record speaks for itself, irrespective of its statistical improbability, and no matter what other psychic influences might have partially contributed (positively or negatively) to the observed result.

I certainly don't pretend that this brief rant is the last word on the issues I'm discussing. As my considerably extended treatment elsewhere of all of them indicate, there's quite a bit more that can and needs to be said. I'm merely lobbying for expanding the dialogue over Bem's paper into important territory that so far seems to have been neglected.

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One last note. I'd like to welcome two new members to the *JSE*'s team of Associate Editors. The first is Robert Bobrow, M.D., Clinical Associate Professor of Family Medicine at Stony Brook University, and author of the book *The Witch in the Waiting Room: A Physician Examines Paranormal Phenomena in Medicine*. The second is Jeremy Drake, Ph.D., a noted astrophysicist at the Harvard–Smithsonian Center for Astrophysics in Cambridge, Massachusetts. Among other activities, Jeremy is the principal investigator of the Center's recent research project showing that pulverized planetary dust may lie around double stars. I'm hoping that these illustrious additions to our team will help the *JSE* deal more expeditiously with the growing number of submissions to the *Journal*.

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Note

[1] Analogously, a request for directions can take many forms, and which form we choose—or which form succeeds—will depend on such things as who is asking for directions, what that person already knows, and what (under the circumstances) would be considered as too much detail or too little detail.

References

Braude, S. E. (1997). *The Limits of Influence: Psychokinesis and the Philosophy of Science, Revised Edition*. Lanham, MD: University Press of America.

Braude, S. E. (2002). *ESP and Psychokinesis: A Philosophical Examination (Revised Edition)*. Parkland, FL: Brown Walker Press.

Braude, S. E. (2007). *The Gold Leaf Lady and Other Parapsychological Investigations*. Chicago: University of Chicago Press.

Eisenbud, J. (1982). *Paranormal Foreknowledge: Problems and Perplexities.* New York: Human Sciences Press.