

Death by Observation: A Reply to Millar

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ABSTRACT: Brian Millar fails in his attempt to defend the observational theories (OTs) against my charge of a vicious causal loop. He claims that the OTs demand only the existence of a feedback channel, rather than feedback itself. But that gambit violates the original spirit of the OTs, undermines its (merely) apparent utility, and suffers from conceptual problems as severe as those it was intended to avoid.

I was very pleased to read Brian Millar's (1988) spirited and interesting defense of the observational theories (OTs). Most critics of my objections to the OTs have engaged in little more than dialectical flailings about. Millar, on the other hand, has displayed both the willingness and ability to examine my *arguments*. (All along, I have provided observational theorists with a much clearer target than they have provided me.) I am grateful, then, for the opportunity (finally) to engage in a relatively straightforward and systematic debate, and thereby deepen our understanding of the issues surrounding the OTs. Millar recognizes the need for clarifying the conceptual underpinnings of the OTs, and he (apparently) is one of the few who actually grasps what the relevant issues are.

As it happens, I think Millar has also helped to ensure the demise of the OTs, by demonstrating their inability to avoid the problems I originally noted. Naturally, that is not what he hoped to achieve; but it is a step forward nevertheless. And although I admire Millar's attempt to rebut my argument systematically, I confess I found parts of his critique rather difficult to understand (especially the sections on varieties of retrocausation and the analysis of ESP). But rather than try to deal with all the respects in which I find myself puzzled or in disagreement over Millar's paper, I will confine my remarks to what seem to be the matters of greatest importance. For simplicity, I will also focus on the OTs' explanation of PK. If (as it appears) that can't be saved, there is no need to worry further about their derivative account of ESP.

To begin with, Millar claims that my original criticism of the OTs suffers from two related "infelicities" (p. 264). The first is that I imply that "something different occurs within the psi source if the feedback on some trial is a hit as compared to when it is a miss" (p. 264). The second is my repeated reference to hit "events." Millar counters that the OTs are concerned, instead, with ensembles of trials and the changing probabilities of hits, not with individual hits and misses (and corresponding variations in the subject's response). He seems to suggest (though it is by no means clear) that these infelicities weaken my argument that the OTs contain a

logically incoherent causal loop. So, just in case Millar did want to make that claim, let me set the record straight.

First, it is obvious that trial-by-trial observation of hits and misses *will* produce different internal *perceptual* states in the subject, and it is probable that those differences would result in further differences in the subject's attitude or manner of attempting to influence the random event generator. But those differences play no crucial role in my argument. Nor is it essential that my use of the term "event" apply to discrete hits and misses (or observations thereof). I had thought this was clear enough in my original criticism. But, to make it perfectly clear now, "event" can also apply to the subject's total PK effort over a series of trials, as well as to the presentation of feedback for the whole series. My central proof for the logical absurdity underlying the OTs still works.

But Millar's principal response to that proof rests ultimately on a more sweeping and daring strategy. He tries to formulate the OTs so that they avoid even the *appearance* of positing a vicious causal loop. Millar argues that the OTs can analyze psi functioning by demanding nothing more than the existence of a feedback *channel*, rather than feedback itself. He writes, "The important thing is not that *S* sees a hit or a miss event, but that *S* is in the position of seeing either, according to the outcome" (p. 258). But I fail to see how this gambit could work. To see why, we must first notice the ambiguity in saying that *S* must only *be in the position of* seeing a hit or miss. Millar could mean either (a) that *S* is so situated relative to the feedback display that he will or does observe a hit or miss (e.g., that *S* is in front of a visual feedback display with open eyes and an attentive mind), or (b) that it is merely *possible* that *S* observes feedback (whether or not he in fact does).

Millar's problem, then, may be illustrated by the following situation. Suppose that *S* is facing a visual feedback display, but that he is blindfolded prior to the presentation of feedback, so that he will not and does not observe the results of his trial. Notice, however, that although a blindfolded *S* will not observe feedback, it is still possible that he does so (e.g., by removing the blindfold). But what happens, under these conditions, to the "psi circuit"? Clearly, there are two principal options: Either the circuit is broken (and hence the blindfolding will prevent PK), or else it is not broken (and blindfolding is no obstacle to whether PK occurs). But it appears that Millar's version of the OTs can tolerate neither option; it is impaled on the horns of a dilemma.

Suppose, first, that the circuit is broken—that is, that by blindfolding *S* we would be unable to satisfy a necessary causal condition for PK. In that case, the OTs must demand more than the mere possibility of *S*'s observing feedback. An unbroken circuit requires *S* actually to observe feedback. But requiring the observation of feedback would force Millar to reintroduce the fatal causal loop. Hence, on the first option, Millar's attempt to deflect my original criticism fails; both presentation of feedback and the resulting causal loop turn out to be essential to the OTs. Millar's second option, however, fares no better. If the circuit remains unbroken even

after *S* is blindfolded—that is, if *S* need not actually observe feedback to satisfy the requirement of being in a position to do so—then *S* needn't ever have been in front of the display. In fact, in that case, it doesn't matter where *S* is. If the psi circuit is unbroken while *S* is blindfolded, then saying that *S* must be in the position of observing *F* means only that it is possible that *S* observes *F* (whether or not *S* actually does so). But then a psi source in a Schmidt-type experiment could be wearing a blindfold and earplugs, or leave to go to the toilet, or even to another country. Since the integrity of the psi circuit does not require actual presentation of feedback, that would make no difference to the experimental outcome.

This result might seem innocuous at first glance, because according to the OTs, feedback can occur considerably *after* generation of targets. But one would think that if *S* never observes feedback (e.g., if the blindfold or earplugs are never removed, or if *S* never leaves the toilet or returns to the country), a vital experimental condition remains unfulfilled. The OTs were developed to analyze the results of a particular set of experimental situations originally conceived by Schmidt. These seemed puzzling from certain traditional theoretical points of view; and it was one of the OTs' apparent strengths that they had something to say about them (see Braude, 1979b, 1979d). But in those experimental situations, the subject must observe feedback in order to play his role in the experiment. Millar even concedes this in his operational definition of PK. He writes: "For a successful subject, the proportion of hits generated is greater when *S* observes the REG outputs than in the absence of such observation" (p. 257, italics added). In fact, had it been possible to dispense with the observation of feedback, then there would have been no point in ever involving the subject at all with the feedback display. *S* might as well be in another country and have nothing whatsoever to do with any experimental apparatus.

Hence, Millar's second option leads indirectly to the same conclusion as the first. Claiming that the psi circuit is unbroken while *S* is blindfolded leads to intolerable results. First, it is incompatible with Millar's own definition of PK. And second, it violates the original spirit of the OTs and the experiments that inspired them by eliminating the need for any involvement of the subject with a feedback device. Apparently, then, Millar must once again concede that a blindfolded subject is, as he puts it, "disconnected" (p. 258). The OTs' psi circuit is unbroken only if the location or condition of *S* is such that observation of *F* actually takes place. As Millar's operational definition of PK betrays, the mere existence of a feedback channel is not enough to close or complete the psi circuit. It is not enough that *S* be so positioned that he merely *can* observe *F*, *whether or not he ever does*. To repeat, that weak requirement can be satisfied by an eternally blindfolded or remote *S*. But then, since actual observation of feedback is necessary, after all, in order to avoid breaking the psi circuit, the OTs require causal loops in just the form both Millar and I recognize to be pernicious. Therefore, both horns of Millar's dilemma lead to the very result he had hoped to avoid.

I suppose Millar could still insist that the OTs do not require actual

presentation of feedback, even if that forces him to deny the relevance of *S*'s having anything to do with the feedback display. That would, of course, break the causal loop; and it is a position that many parapsychologists would find agreeable. But I wouldn't expect proponents of the *observational* theories to go along with it. For one thing, it would deprive those theories of their unique application to the Schmidt-type experiments that inspired their development. Moreover, by denying the relevance of actual feedback, the OTs would lose an even more fundamental distinctive feature. Many people, after all, have suggested that psi can occur without the subject learning about it by ordinary sensory means. But ironically, once the OTs drop the requirement of actual feedback to the subject, there is no need to insist, as Millar does, that the psi circuit be a *feedback* channel. If actual feedback is not necessary, it is gratuitous to claim that PK requires a causal network *for the presentation of feedback*. In the case of PK, the only channel required is some sort of causal link running *from* subject to target. But in that case, the OTs would be making no more than the thoroughly uninteresting claim that PK requires a PKer. That wouldn't even deserve the title "theory," much less the technical and quasi-philosophical fuss lavished on it by physicists and others in the parapsychological community.

Now of course the OTs do not want, and never did want, to make such a toothless claim. The PK psi source *S* is not simply the person *S*, *wherever he is and whatever he is doing*. *S* is the person actually receiving feedback. And all along the problem for the OTs has been to explain *why* that matters. That is the step Millar has heroically tried to avoid. He realizes that to explain the relevance of actual feedback, the OTs must posit their fatal causal loop. As we have seen, however, Millar's escape route is hardly worth the effort. If he drops the requirement that the subject actually receive feedback, he deprives the OTs not only of their distinctiveness, but of their substance. Millar will not have rescued the OTs; he will have emasculated them.

As I noted above, Millar believes that PK can be defined operationally. He says "PK is measured by the difference in the behavior of an REG in different setups. For a successful subject, the proportion of hits generated is greater when *S* observes the REG outputs than in the absence of such observation" (p. 257). Notice, however, that nothing in Millar's account explains why the presence of a psi source matters at all (i.e., *how* its presence is causally related to the result), why the result is in one direction rather than another, and why some subjects (but not others) score well. If the mere existence of a feedback channel, or the mere presence of a subject near the feedback display, were all that mattered, then one would think that *anybody* appropriately situated would be a psi source. Millar seems to recognize this, and therefore he later appeals to psi-source *strength* and "internal programs." But these last-minute maneuvers are a tacit concession that the original definition of PK was inadequate, that PK *cannot* be understood in simple operational terms, and that it must instead be understood in *causal* terms, with respect to conditions that enable us to

distinguish PKers from non-PKers, positive scorers from negative scorers, and virtuosos from hacks.

I am also perplexed by Millar's statement that "the very existence of PK effects is defined in terms of comparison of results with different psi circuits" (p. 258). The problems here go beyond the inadequacies of his operational definition of PK. Apparently, Millar has in mind the difference between his figures 3b and 3c. But according to Millar, 3b represents a situation with the psi source "disconnected" (p. 258). But if that's the case, then why say any psi circuit exists at all? It's not clear that anything deserving to be called a circuit exists in this case, unless (counter-intuitively, and obfuscatingly) a disconnected or broken circuit counts as a circuit. (That would be analogous to calling an incomplete sentence a sentence or a foiled robbery a robbery.) But even if 3b represented a complete circuit, with no excess of 1s or 0s, it is unclear why it should count as a *psi* circuit. One would think that only situations of the type represented by 3c would be psi circuits. By using the term "psi circuit" for cases where there is no appropriate or significant excess of signals, one can only wonder what a *nonpsi* circuit would be. Following Millar's terminology, it appears that any circuit can be a psi circuit, no matter what happens.

A more serious problem concerns Millar's distinction between primary and secondary causation. Millar claims that "setting up different psi circuits causes different results" (p. 259). (We may ignore for now the aforementioned difficulties with the term "psi circuit.") This is what Millar calls *primary* causation; and, as he observes, it makes no appeal to retrocausation and resulting causal loops. Millar contends that my original criticism mistakenly focused on secondary causation, which (I gather) would concern putative causal processes occurring *between* the setting up of psi circuits and the occurrence of the PK results. Millar charges that my approach is "like attempting to explain the properties of a telephone system in terms of the messages it transmits" (p. 259).

But Millar's criticism here seems off the mark. Indeed, Millar's analogy only highlights the weakness of his position. Millar apparently believes that the OTs want merely to describe something analogous to the properties of a telephone system, never mind what messages the system is used to convey. The OTs aim only at a "specification of the apparatus (the [experimental] preparation)" (p. 259), not the experiment itself. Ironically, however, Millar has not even given us that much. What he tells us about psi circuits is far less than what we would need to know about the properties of telephone systems. Now of course the OTs want, among other things, to specify the conditions necessary for actual PK events to occur. And Millar believes that one of those necessary conditions is the existence of a feedback channel. But even if that were true, the mere existence of a feedback channel would not *explain* PK in any interesting sense. And, *at best*, it is not very illuminating to be told that setting up a feedback channel is the (primary) cause of PK. In fact, it is probably false to say this in most or all contexts.

Millar's telephone analogy illustrates the point clearly. Placing a tele-

phone (even a working telephone) within a person's reach does not, generally speaking, cause a message to be transmitted. Although it may be a necessary causal condition of telephone transmission to have a phone near at hand, other conditions are at least as vital—in particular, certain crucial *activities* of the person near the phone, such as dialing a number and then speaking. And these latter conditions may remain unsatisfied even if the former condition is fulfilled. Similarly, we do not *explain* the phenomenon of telephone transmission simply by asserting the existence of a telephone connection between two individuals. Unless we have only the most minimal need to understand events—far less than what conventional scientific theories attempt to satisfy—our explanation must go much further. It must describe *how* a telephone connection works (i.e., *how* it handles actual signals) and what a person must do in order to use a telephone. It doesn't matter whether our explanation specifies how a *particular* message—say, "Aunt Jean sends her love to the kids" (p. 259)—is transmitted. (Millar misleadingly suggests that my position is that this *does* matter.) But we don't understand how a telephone works unless we understand what it would do with some signal or other, never mind what particular signal it might be.

It should be clear, then, why Millar's primary causation is an almost totally ineffectual explanatory tool. It doesn't explain what the relevant properties of the psi circuit are—that is, how the psi circuit does anything and what the crucial contribution of the agent is (analogous to dialing a telephone and then speaking). It merely asserts the causal relevance of setting up a psi circuit, the workings of which are left to our imagination. But since the OTs purport to be explanatory theories, they must do more than simply assert the existence of psi connections (circuits) in the world. Yet that is what Millar takes to be the fundamental tenet of the OTs; once again, he seems to assert no more than that PK requires a PKer. Ironically, that is too little even for someone like myself, who believes that no conventional *theory* of PK is even possible (Braude, 1986, Chap. 4). Hence Millar's attempt to rescue the OTs has an oddly (and presumably unintentional) anti-theoretical flavor (or perhaps, odor). To those who believe that science should be able to explain something about how or why PK works (and I would imagine that that class includes the majority of observational theorists), Millar's version of the OTs will be unacceptably austere. It explains PK in the useless way a person's birth is explained by reference merely to a lustful gleam 9 months earlier in the eyes of two sexually functional adults.

I mentioned earlier that Millar's operational definition of PK afforded him no way of distinguishing, among other things, PK virtuosi from PK hacks, and that Millar later introduces the concept of *psi-source strength* for this purpose. But it's quite absurd to think that *psi-source strength* will handle the distinction, especially when we consider refined macro-PK (e.g., of the sort surveyed in Braude, 1986). That would be analogous to thinking that the difference between my tennis playing and that of Ivan

Lendl is a purely quantitative measure of something akin to intensity or amplitude.

Millar seems to dodge the question of whether the OTs can be extended to cover anything beyond the severely limited types of phenomena obtained with random event generators. Apart from his brief footnote (no. 4) suggesting that such an endeavor might have promise, Millar makes only a quick, but contentious, remark in the text. He says that large-scale PK would do "violence to both preconceptions and laboratory experience of PK" (p. 263). But these are the weakest of reasons for rejecting the possibility of large-scale PK. First, our preconceptions are notoriously fallible and have evolved dramatically over the course of intellectual history. And second, laboratory evidence is never a guide to what the *limits* of human capacities are—that is, to what humans can do outside the artificial environment of an experiment (see Braude, 1986, 1987).

Moreover, one can hardly be optimistic about the possibility of extending the conceptual apparatus of the OTs beyond a severely restricted domain of experimental results, especially when numerous powerful anti-mechanistic arguments suggest otherwise (Braude, 1979a, 1986). Defining PK in the operational terms appropriate to a Schmidt-type experiment (or any quantitative laboratory PK test) is reminiscent of the regrettably still fashionable error of defining the nonformal, semantic concept of *information* in terms of the technical, formal, and syntactic concept developed in information theory (see Braude, 1979a, 1979c, for a discussion of that particular error). It is analogous to defining "curiosity" as "the capacity of an object to be attracted to a magnet," and then using that restricted quantitative concept to apply to all varieties of human curiosity (this example is inspired by a similar analogy used by Bursen, 1978). I argued earlier that the OTs, at best, fail to explain the phenomena that inspired them, and that, at worst, they are deeply unintelligible. And now it appears that even if these criticisms turned out to be unjustified, the OTs would still have no application to the richer psychokinetic phenomena that drove parapsychologists into the lab a long time ago.

Despite my criticisms, I must commend Millar for having provided the most thoughtful and sophisticated defense yet of the OTs. Of course, in the long run, it is of little moment that a particular theory succeeds or fails. What matters more is that we develop the broader conceptual tools to discriminate promising from unpromising theoretical approaches. To that end, Millar has performed a valuable service.

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