

EDITORIAL

I suppose most of us have wondered at one time or another whether a person's apparently defining character traits are fixed, or whether people are capable of meaningful and deep change. Perhaps the issue arises most frequently in the context of intimate personal relationships, but it seems also to be a recurring topic of practical or professional concern to those studying scientific anomalies.

I subscribe to several email listserves, one of which is a forum for psi researchers, and this topic routinely forms the background for discussion. It usually happens in the following way. First, some intransigent skeptic (or, perhaps more accurately, passionate psi-denier) publishes something outrageous (if not libelous) about a serious and diligent member of the parapsychological research community. It might be a severe blast of ad hominem attacks lacking even the appearance of empirical support. Or, it might be an ostensibly evidence-supported critique which only the well-informed would know to be inexcusably one-sided if not blatantly dishonest. The former happens frequently to Rupert Sheldrake. The latter is a specialty of career skeptics who posture as careful researchers themselves but who sedulously avoid discussing evidence that's most difficult to explain away.¹ When a new such attack surfaces, listserv members usually respond with a furious flurry of postings about how best to respond and whether to respond at all.

A related manifestation of skeptical intransigence is the manner in which Wikipedia biographies of parapsychologists, LENR researchers, and others have been hijacked by a collection of Wikipedia insiders who have rewritten the biographies, replaced accurate information with falsehoods, and thwarted all attempts to correct the misrepresentations. Here, too, victims of this treatment wonder how—or whether there's any point in even trying—to counter the damage.

Of course, many *JSE* readers are familiar with the manifestations of egregious and obdurate skepticism in areas of frontier science. As far as parapsychology is concerned, one need only examine the writings and public pronouncements of Richard Wiseman, Michael Shermer, James Alcock, and others, many of which are documented on the recent successor to Sheldrake's Skeptical Investigations website <http://www.skepticalaboutskeptics.org/>

Additional and plentiful examples of recalcitrant skepticism also deface the literature in a controversial area of research mentioned only infrequently in this journal—namely, dissociative disorders generally

and dissociative identity disorder² in particular. Consider, for example, the writings of Elizabeth Loftus, Richard Ofshe, and others who attacked mental health professionals treating dissociative disorders by appealing to a condition they dubbed *false memory syndrome*. The problem is not simply that this so-called condition has never been recognized in either the DSM (*Diagnostic and Statistical Manual of Mental Health Disorders*) or ICD (*International Classification of Diseases*). What many don't seem to realize is that in medicine, naming a syndrome is easy; virtually anyone can do it. And unlike the classification of *disease*, it requires no broad professional consensus. In fact, the designation of false memory syndrome has no greater antecedent credibility or authority than capricious syndromes one can easily and whimsically concoct at the drop of a hat. I've done this myself. Consider, for example, *premature seatbelt release syndrome*, describing the behavior of impatient arriving airline passengers, or *delusions of invisibility syndrome*, to characterize drivers of cars who think others can't see them while they pick their nose, or *golden voice syndrome*, characterizing those who, when singing in the shower, greatly overestimate their vocal talent.

I realize that a false memory syndrome, if real, would have more serious social consequences than those I invented (although I suppose one could argue that golden voice syndrome helps account for numerous abominations promulgated by the recording industry). What matters here, though, is that my whimsical "syndromes" probably rest on a firmer empirical foundation than false memory syndrome. As Ken Pope (among others) has noted (Pope 1996, 1997), those who allege that there's a false memory syndrome have never indicated how it was determined that there's a widespread false memory problem (much less an "epidemic" of false memory reports, as some like to claim). By contrast, there's plenty of evidence that people engage in the seat-belt unfastening, nose-picking, and shower-singing behavior referred to above. But the only way to determine whether there's a false memory syndrome is, first, to determine whether certain memory reports are false. However, there's no clear or reliable procedure to follow here, even for police investigators. And one can be sure that the vocal skeptics in question have neither conducted nor sponsored anything as thorough as police work in connection with the memory reports of therapy patients.

To support the claim that there's an excess (or epidemic) of false memory reports in therapy, one must conduct detailed, sensitive, in-depth, and sweeping studies of a kind that simply have not been carried out. In fact, those who claim that there's a false memory syndrome have not even met, much less interviewed, the majority of those who apparently recovered memories in therapy. So I'd suggest that when someone alleges that there's a false memory syndrome, a proper response is to ask how one can detect

the presence of that syndrome without ever meeting the people allegedly suffering from it. Another is to ask by what process they determined that the testimony of those denying the memory reports is more credible and less prone to motivated distortion than the testimony of therapy patients.

At any rate, the hubbub over false memory syndrome was a big deal a couple of decades ago. But despite obviously sensible criticisms from several quarters, one still finds glib skeptical critics appealing to false memory syndrome (for a critical review of one recent example, see Cardeña 2014). And for further examples of the wretched quality of skeptical reasoning surrounding the study of dissociation, see Braude (1995, 1998, 2014). In fact, there are strikingly close parallels between intransigent skepticism of psi research and skepticism of research into dissociative disorders (Braude 2014).

Now when the targets of shoddy skeptical attacks discuss response strategies, what they often ponder is whether there's any point in responding to the attacks, not just because the efforts would distract from more constructive research activity, but because there's no reason to think that anyone's opinion—or at least that of the attacker—could conceivably change, even in the face of compelling rational argument. And even more cynically, some speculate that, even if a critic's *beliefs* can be influenced by rational argument, his/her *behavior* may remain the same. That is, some critics may simply yield to overriding professional and social pressures to maintain their skeptical reputation, no matter what they believe personally. I know that many SSE members (including myself) have often noted how academicians may denounce some area of frontier science in public but confess in private to quite different points of view.

In any case, whatever psychological struggles might be occurring in the minds of hardcore skeptics, the issue here is: How should one deal with the continued barrage of poorly reasoned (if not simply dishonest) attacks on one's research, especially when there's ample evidence that the critics would never change their minds or behavior, no matter how sensibly one replies to them? After all, many anomalies researchers have noted the almost evangelical fervor of some of the criticism often directed against their work. Similarly, critics sometimes state their disagreements, their differences of opinion, as dogma, usually seasoned with a generous helping of contempt or sarcasm, and they do this with a certitude totally disproportionate to their knowledge of the facts. One would think that the transparent and intense hostility of such criticism is simply inappropriate to an objective scientific inquiry. In these cases it seems that anomalists aren't simply confronting opposing theoretical positions when they're under such emotionally charged and intellectually dishonest attack. Instead, they're apparently in conflict

with articles of faith, and engaged in something closer to a religious battle than an honest and dispassionate appraisal of rival hypotheses. But in that case, is there any point in trying to mount a rational defense and respond with data and arguments? Wouldn't that just be a futile attempt to reason people out of positions they hadn't been reasoned into?

I can't pretend to know the answer to this, but there are certainly grounds for wondering whether it's worth making a serious effort to respond to those we know are entrenched in their skepticism and not likely to play fair in a public dialogue. Consider, for example, the case of the late Charles Honorton, who spent years debating critics (primarily Ray Hyman) over the merits of ganzfeld experiments, time he could have devoted productively to further experimentation or (say) the search for superstar subjects. I seriously question whether this was time well-spent. Hyman never identified problems with the ganzfeld experiments that could be linked to those experiments' positive results. After much wrangling, Honorton and Hyman eventually jointly endorsed a revised, autoganzfeld (i.e. computerized) experimental design that was supposed to avoid the methodological "flaws"—including the risk of sensory leakage—Hyman claimed to have identified in the earlier series. But (presumably contrary to what Hyman expected) the new tests achieved scores at virtually the same level of significance as that yielded by the earlier series. Nevertheless, Hyman maintained an unwavering skepticism about the evidence for ESP, and he continued to argue that orthodox science needn't pay any attention to the work of psi researchers (Hyman 1989). So my take on the overall result of Honorton's efforts to engage Hyman in a real and honorable dialogue is that there was no positive outcome even remotely commensurate with the effort.³ There's no reason to think that many (if any) skeptical opinions about ESP were revised as a consequence, and there's no reason to think that Honorton or anyone else learned anything new or important about ESP from all that work.

We're often reminded that patience is a virtue, but I'm not sure that even patience-virtuosi could successfully engage career skeptics in a profitable dialogue. However, I also think that anomalists have no choice but to do the best work they can and wait for the chorus of irrational critics to disappear. Hopefully, that development is something they'll live to see. But if history is any guide, this change won't happen entirely and it won't happen soon. After all, the shoddy dialectical strategies favored by opportunistic career skeptics haven't changed in more than a century (Braude 1997, 2014). Still, I have faith in the steady (if not unimpeded) progress of science, and I believe the truth will out, at least eventually. At any rate, my hope—and possibly naïve expectation—is that dishonest skepticism has no more

chance of succeeding than Mussolini's notorious and futile attempt to outlaw handshakes.⁴

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I would like, once again, to conclude my end-of-year Editorial with sincere thanks to my hardworking team of Associate Editors and our still regrettably small but trusty stable of referees. Despite my repeated demands on their valuable time, I count on them to help maintain the high standards of the *JSE*, and they do a really splendid job. And of course, kudos (as usual) to Managing Editor Kathleen Erickson, who keeps the whole machine running smoothly and thereby somehow succeeds at the thankless and heroic task of preserving the illusion of my editorial competence.

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Notes

- ¹ For an expose of one such treatise, see Braude (1985).
- ² Formerly Multiple Personality Disorder.
- ³ For details, see Bem (1994), Bem and Honorton (1994), and Bem, Palmer, and Broughton (2001).
- ⁴ For those unaware of this, Mussolini thought Italians needed some toughening up, and so he decreed that handshakes were henceforth illegal and were to be replaced by the Roman salute (see Kertzer 2014).

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