A Response to a Nonresponse to Criticisms of a Nonstudy

One Humorous and One Serious Rejoinder to Slater

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Lauren Slater is a creative and imaginative writer. We were initially uncertain how best to respond to her witty reaction to our paper questioning the reported results of what she now describes as a nonstudy. Ultimately, we elected to begin our rejoinder with a bit of tongue-in-cheek humor of our own.

A TERRIBLE MISUNDERSTANDING

It appears that there has been a terrible misunderstanding! Now that Dr. Slater has provided a full explanation of why she did not respond to our requests for documentation of her nonstudy (although her attorney was kind enough to respond to these requests by warning us promptly of potential legal action), we now feel obliged to apologize publicly to her. She seems to be correct after all: there *was* no study and, therefore, no evidence to document one.

What led us astray was that the events reported in her chapter certainly *looked* like a study: there was an important research question, a method for collecting data, a description of the results, and a set of conclusions. What further confused us were Slater's own words from her chapter:

"'I'm going to try it,'" I say [to my husband]. "'Repeat the experiment exactly as Rosenhan and his confederates did it and see if I get admitted.'" (p. 81)

[To Spitzer] "'So what do you predict would happen if a researcher were to repeat the Rosenhan experiment in this day and age?'" I ask. (p. 89)

"'Okay,'" I say [to Robert Spitzer]. "'Let me tell you, I tried this experiment. I actually did it.'" (p. 89)

These quotations suggest that Slater did conduct a study—specifically, a replication of Rosenhan's (1973) well known pseudopatient study. She also explained to Spitzer the implications of her results: ". . . the zeal to prescribe drives diagnosis in our day, much like the zeal to pathologize drove diagnosis in Rosenhan's day, but either way, it does seem to be more a product of fashion or fad" (p. 90). We hope that we and other readers can be forgiven for regarding her work as a study.

Slater's response suggests another question: did she even perform her nonstudy in the first place? She does not provide readers with evidence that it ever took place. By "nonstudy," does she mean only that her hospital observations were unsystematic or unscientific? Or does she also mean that the events she described were fictional?

Finally, we hope we can be forgiven for mistaking Slater for a serious science writer rather than, as she maintains, almost exclusively a writer for fashion magazines. In fact, she has written nine articles on the science of psychology for the *New York Times*, was an MIT Science Journalism Fellow, and described her book as an exposition of "great psychological experiments" (Slater, 2004, p. 2).

ON A MORE SERIOUS NOTE

Slater's comment on our paper raises several deeply troubling questions. First, she seems to want to have things both ways. In her book (Slater, 2004), she explicitly describes an attempt to "repeat the experiment" (p. 81, 89) that Rosenhan conducted. Then, on the basis of her results, she concludes that Rosenhan's central claim—that psychiatric labels "rise and fall depending on public perception" (p. 91) and lie more in the eye of the beholder than in patients themselves—still stands after 3 decades. Yet when criticized, she backpedals and now insists that her "study" was not really a study. If so, how can she draw conclusions from it? Like *Saturday Night Live*'s Miss Emily Litella (portrayed by the late Gilda Radner), who famously said, "Never mind," whenever she was corrected, Slater's defense appears to be, "Never mind. It wasn't a study after all. Scientists have no right to criticize it." Yet she does not renounce her all-encompassing conclusions regarding the implications of her findings for psychiatric diagnosis. How can Slater claim not to have performed a study and then proceed to base conclusions on it? She may be correct that a postmodern game is being played here, but she is wrong about the player.

Second, Slater claims erroneously that our findings actually corroborate hers. She writes, "More than half of the respondents to Spitzer et al. gave the character in the vignette a diagnostic DSM code for some sort of psychosis, NOS or otherwise. I would say that finding goes a long way toward validating the original Rosenhan study and casting credibility on my chapter." Not at all. Whereas she claims that she was "not given a deferred diagnosis" and that "almost every time I am given a diagnosis of psychotic depression," the psychiatrists in our study avoided a specific diagnosis in 80% of the

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cases and diagnosed psychotic depression in only 6% of the cases. In fact, the substantial majority of physicians in our study offered diagnoses of psychotic disorder not otherwise specified, a residual category that clinicians typically use to defer a diagnosis on patients whose psychotic symptoms necessitate more thorough follow-up assessment. Surprisingly, Slater neglects to mention that our findings—which revealed that only 34% of clinicians suggested medication prescriptions, none of which were antidepressants (as Slater claims she was given)—run counter to her overarching conclusion that diagnostic decisions are driven by a "zeal to prescribe."

Third, Slater argues that our study neglects to take into account findings such as those of Milgram (1974), who reported that a group of Yale University psychiatrists massively underestimated the level of subjects' obedience to authority in his classic shock study. Slater's invocation of Milgram in this context is a striking non sequitur. Milgram's survey findings are a potent illustration of the fundamental attribution error (Ross, 1977): the fact that individuals tend to underestimate the impact of situational factors on others' behavior. The relevance of these findings to a vignette study in which overpowering situational pressures (e.g., a white-coated authority figure commanding a subject to administer electric shocks) are largely or entirely absent is dubious at best. Slater is of course correct that our vignette study does not fully reproduce the diagnostic decision-making processes that occur in real-world settings (a point which, contrary to her assertion, we acknowledged explicitly). A fundamental problem is that without any documentation information (e.g., hospital record evidence demonstrating that she received the diagnoses and medicines she claimed to have received)—which Slater declines to produce—there is no way to ascertain whether this is a difference that makes a difference.

Fourth, Slater neglects to note that this is not the first time that others have raised serious questions concerning the accuracy of her reporting and her blurring of the lines between fact and fantasy. For example, in a review of Slater's book, "Lying: A Metaphorical Memoir," *New York Times* critic Rebecca Mead (2000) conducted some detective work and discovered that the "Professor of Philosophy at USC, Dr. Hayward Krieger," who presumably wrote a lavish introduction to the book, never existed and was a fabrication of Slater's imagination. Mead notes how Slater informed her editor of her intention ". . . to ponder the blurry line between novels and memoirs. Everyone knows that a lot of memoirs have made-up scenes; it's obvious. And everyone knows that half the time at least fictions contain literal autobiographical truths. So how do we decide what's what, and does it even matter?" There is of course no way to know for certain whether Slater ever conducted the present nonstudy, but her nonresponse to our requests for corroborating documentation is hardly reassuring.

Inexplicably, Slater neglects to inform readers of *The Journal of Nervous and Mental Disease* of the genuine reasons for the outcry that followed the publication of her book. In particular, she neglects to mention that a chorus of prominent critics, including psychologists Elizabeth Loftus and Jerome Kagan, psychiatrist Robert Spitzer, and Deborah Skinner Buzan (B. F. Skinner's daughter), all insisted that they were seriously misquoted or misportrayed in her book (Lee, 2004; see also Wade and Tavris, 2006). Slater portrays herself as the innocent victim of a vigilante mob of angry academics. In fact, the true victims are Slater's readers, many of whom may have been duped into taking the findings and conclusions of her study, which she only now informs us was not actually a study, seriously.

CONCLUSION

In conclusion, if anything constructive has come of this strange interchange between Slater and ourselves, it is that readers may now safely disregard the nonfindings of Slater's widely read nonstudy. Rosenhan's (1973) poorly supported conclusion that most or all psychiatric diagnoses are invalid products of the social context can again be safely put out to pasture (Ruscio, 2004; Spitzer, 1975). We can therefore sum up our bottom-line conclusion regarding Slater's chapter and its implications for psychiatric diagnosis in two words: "Never mind."

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