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Undeserved Recognition

Bill McGuire, my colleague at Yale for 15 years, would mail his reprints in response to requests with some version of this message scrawled on letterhead: "For those of you you read JPSP, consider wearing sunglasses when you read this to whose lips move when shield yourself from the glare of the brilliance of these words." It was never fully clear whether he was poking fun at himself while skewering a novice (like me), or whether he was subtly signaling that his work had not received the recognition it deserved.

Since 1980, the year I started graduate school, I have sat in an intellectual rotunda filled with remarkable colleagues in every direction. Nobody could question that they each had received enormous recognition, and in many cases, complete adoration as well. But even gods feel underappreciated, I learned, and being a keen observer of them, I have, for the past three decades, waited for the moment when I too would get in a huff and dash off a note about the dull pupillary reflexes of some poor reader. But alas, that occasion hasn't presented itself I do not deserve to be in this book. Moreover, the reception to my work has corresponded pretty well to my own sense of it. The work I've regarded to be relatively more shiny has also been more recognized than the work I myself have regarded to be less radiant. Not only has nothing been forgotten or unappreciated, there has been a sufficiently reassuring correspondence in self-other perception of the work. So, as I said, I don't deserve to be in this book.

Infact, the contrary has been true. Some papers that I thought would have gathered dust have actually received more attention than they have deserved. On one occasion, a long time ago, I wrote a paper with my colleague Robert Crowder stating that the "practical aspects of memory" movement had made the mistake of equating the use to which basic research is put with research that employs seemingly realistic methods (Banaji & Crowder, 1989). Maybe it was the tone of the paper that created a ruckus; it started this way:

> Once upon a time, when chemistry was young, questions of ecological validity were earnestly raised by well-respected chemists, and were debated at scientific meetings and in scholarly journals. We understand from a colleague (who is a distinguished historian of science but modestly asked not to be named) that partisans of one point of view called themselves the "Everyday Chemistry Movement." They pointed out that the world offered many vivid examples of chemical principles at work in our daily lives-the rising of pastry dough, the curdling of sauces (the great chef Brillat-Savarin was then laying the foundation for the principles of applied chemistry called thereafter French cuisine), the smelting of metal alloys, the rusting of armor, and the combustion of gunpowder. Why not, they asked, study chemical principles in these ecologically faithful settings rather than in tiresome laboratories with their unnatural test tubes, burners, and finicky rules of measurement? The normal world around us, they said, has no end of interesting and virtually unstudied manifestations of chemistry. (p. 1185)

Tongue in cheek, we narrated the obvious oddity of arguing that a science should strive to make itself look ecologically valid to naive observers. Surely no biologist would argue that we should set aside the use of C. *elegans,* a worm that lives for 2 to 3 weeks, as the preparation to understand development, genetics, aging, and disease because C. *elegans* doesn't look much like us. In this paper on the study of memory we said nothing terribly profound; just the sort of thing one would say to coach a high school debate team preparing to make a case about the value of the scientific method and the role of basic research in solving practical problems. What nonsense all this practical methods stuff was in the context of understanding human memory, we said, and ended with a harrumph. And then we went to bed like any other day. When we awoke, a tea-party-like mob had formed on both sides of the Atlantic and an issue of *American Psychologist* had been devoted to responses to that paper, mainly critical, with one exception by Roddy Roediger, who stuck his neck out for us. I had only completed my second year as assistant professor at the time, but I had to duck with the swiftness of President Bush dodging a shoe at the next Psychonornics meeting I attended. Fame was one thing, but not at the expense of taking a few stitches in the head.

Many years later, another colleague at Yale described my apparently perceptive use of the early years to write theoretical rather than empirical papers because Yale's

ıbject pool was so small I couldn't do much research. I'm pretty sure that no such lvance planning went into the decision to write that paper. It just seemed like the ght thing to do, not to mention fun, but I had no idea it would get the undeserved tention it did. Itbrought me many reprint requests, most of them from the many nazing teachers of psychology at non-research institutions who told me horror ories of the difficulty of getting people to understand the value of basic research. nd it brought from those who felt attacked a strong and personal sense of being ırmed that I did not understand. (One very famous psychologist who I admired 'eatly told me he would not shake my hand at that Psychonomics !)

I learned a lot from the response to that paper. I learned that although I had no omach for interpersonal conflict, I was unflappable when disagreements con- ɛrned intellectual matters, no matter how severe. The experience of writing that ıper gave me the opportunity to spar. I would leave meetings bloodied, metaphor- al sword still in hand, but jubilant that somebody had engaged with the ideas. I so learned that people didn't expect tough words to emerge from the body of a nallish, brown-skinned woman who seemed reasonably nice when you met her. A ɛnder stereotype was being disconfirmed in a small way, and how could I not be in ɛaven over that too.

Another occasion on which I experienced undeserved attention wasn't in the ɔntext of a single paper but rather in the response to a body of work on implicit ɔcial cognition. My colleagues and I said what everybody else before us had already id: *Thinking and feeling can operate without conscious awareness. Therefore, mental ıtes have consequences that are not intended. You, not just those "other" subjects in some xtbook psychology experiment, may be prone to this*. That's it. Perhaps because we ɜed black Americans and not green peas as attitude objects, the shoes came flying ɡain. We were asked what hubris had led us to make a website and invite anybody ıd everybody to sample what we had learned about our own unconscious biases in e areas of social group attitudes. By now, I had rheumatoid arthritis and couldn't ıck as fast as President Bush when the shoes came flying. But again, the experi- ıce was nothing short of exhilarating. Some pretty remarkable people put aside e primary work of their own careers to devote time to challenging our point of ew. What more can one ask? I had certainly not done the same for them. I seem to ıve gained enormously, yet again, from this undeserved recognition.

EFERENCE

anaji, M. R., & Crowder, R. G. (1989). The bankruptcy of everyday *memory. American Psychologist, 44,* 1185-1193.