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 correspondence in self-other perception of the work. So, as I said, I don’t deserve to be in this book.

In fact, 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.

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 undervalued, 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
subject pool was so small I couldn't do much research. I'm pretty sure that no such
lousy planning went into the decision to write that paper. It just seemed like the
good thing to do, not to mention fun, but I had no idea it would get the undeserved
attention it did. It brought me many reprint requests, most of them from the many
fascinating teachers of psychology at non-research institutions who told me horror
stories of the difficulty of getting people to understand the value of basic research.
It brought from those who felt attacked a strong and personal sense of being
wrong that I did not understand. (One very famous psychologist who I admired
early 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-
cerned intellectual matters, no matter how severe. The experience of writing that
paper gave me the opportunity to spar. I would leave meetings bloodied, metaphor-
ical 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
rather nondescript, brown-skinned woman who seemed reasonably nice when you met her. A
Gender stereotype was being disconfirmed in a small way, and how could I not be in
heaven over that too.

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

REFERENCES