

Rotten Reviews

September 30, 2015



Back in the early 1980s, the actress Dame Diana Rigg began asking colleagues in the theater and film industries — including some of the world’s most honored thespians — to share their worst-ever reviews. The responses turned into a collection, No Turn Unstoned, which eventually drew a cult following as she toured university campuses reading excerpts from the book.

In that spirit, we asked some distinguished APS members, all of whom are leaders in their areas of study, to share their own worst wounds from the critics (in this case, journal editors, peers, job recruiters, or even laypeople hearing about their studies). These researchers offered up some of the weirdest, harshest, or — in hindsight — amazingly off-base reviews they suffered. Some of the respondents simply shared direct quotes from the reviewers, while others provided full background stories. In many cases, these papers were submitted to other journals and became seminal pieces of work.

Here are some of the most memorably brutal critiques and reactions leveled against some of psychological science’s leading lights.



Toni C. Antonucci

University of Michigan

What to say after one reviewer didn’t think I understood the convoy model and told me that I should read some of Antonucci’s work to get a better feel for it?!



Timothy B. Baker

University of Wisconsin–Madison

When I was a fairly young investigator, I was invited to give a talk to an organization that was associated with the production and sales of alcoholic beverages. The organization paid for my travel to and my stay at a swank resort.

During the first morning meeting of the conference, I heard some very interesting talks given to the attendees. One talk was about how the association between alcohol use and crime was spurious because personality factors caused each. Another talk was a very scholarly examination of how alcohol expectancies could powerfully shape behaviors that followed alcohol consumption. As my talk was approaching, a copresenter leaned over to me and said, “You have it made. Once you are invited as a keynote speaker, you are then invited every year afterwards. Next year is in Australia!”

I then got up and began my talk, arguing that to some extent we researchers had focused overly much on dysphoria as a setting event. I presented data showing that positive affect was directly related to the use of cocaine, heroin, nicotine, and alcohol. I also spoke about how these data suggested that similar motivational mechanisms were involved in the dependence syndromes associated with these agents. At this point a rather elderly, distinguished-looking gentleman raised his hand to ask a question. (I later learned that he was a senior officer in the organization and very influential.) I called on him and he asked two questions: (1) So, you are discussing positive affects, not the positive effects of alcohol? And, (2) you are suggesting that in some way alcohol has something in common with cocaine and heroin?

I gave the rest of my talk to an audience that seemed strangely wooden and distant. They seemed to be averting their eyes as if they did not want to witness the inevitable unfolding of a humiliating event. At the end, which was eerily silent, I returned to my seat, where my fellow presenter leaned over and whispered to me, “I’ll send you a postcard from Australia.” No one spoke to me over the next 2 days of the meeting. Needless to say, there was no trip to Australia.



David H. Barlow

Boston University

“The study as presented fails at so many levels, I am disinclined to list all the specific individual problems.”



Linda M. Bartoshuk

University of Florida

I've never had a paper rejected for absurd reasons, but I've been forced to make changes that were absurd. I remember a study on children in 1991 done with a colleague, Jean Ann Anliker, which had to be completely rewritten after a reviewer refused to let us use the term "supertaster" because it had not previously been in the literature. We rewrote the paper and introduced the term in later papers.



BJ Casey

Weill Cornell Medical College

Our seminal developmental imaging paper (Galvan et al., 2006, *The Journal of Neuroscience*), which provided the empirical evidence for our theoretical imbalance model of adolescent brain development, was rejected by *Nature* in 2005. The letter stated, "We do not believe your manuscript represents a development of sufficient scientific impact to warrant publication in *Nature*."

But then this work was featured in a brief article on the teen brain by *Nature* the year following its publication in *The Journal of Neuroscience*. The paper and model also were featured heavily in the National Institute on Drug Abuse's strategic plan in 2010 and highlighted on its website.

The empirical paper has been cited more than 500 times in the past 5 years alone, and the theoretical model that emerged from it has been cited more than 1,100 times.

The best part is that if you look closely at the picture of the teenager being scanned in the *Nature* article, it's my son, Jonah. So he got published in *Nature* before his mom did!



Stephen J. Ceci

Cornell University

I think of all of the harsh comments, the one I best remember is from a review about a study I did with another assistant professor back in 1980: “I advise these young authors to consign this manuscript to their developmental juvenilia and not try to publish it.” *American Psychologist* rejected it, of course. It went on to be cited nearly 800 times in *Behavioral and Brain Sciences*.



Susan T. Fiske

Princeton University

There was the job interviewer who said, “If we were hiring the person we liked better, it would have been you.”



Morton Ann Gernsbacher

University of Wisconsin–Madison

A reviewer said that publishing my critique of mirror neurons would be “dangerous.” Dangerous? Seriously? I’m 5’1”, I always have manicured nails, and I hate the sight of blood. Dangerous?

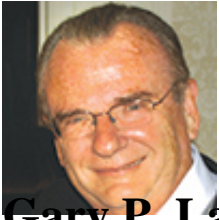
The irony is that other people have since published mirror neuron critiques, including an entire book dedicated to *The Myth of Mirror Neurons*. So dangerous!



Sam Glucksberg

Princeton University

My favorite bad review asserted that my research-grant proposal was “just another knee-jerk reaction-time study.” The other reviewers were highly positive, so I got the grant (NSF) anyway. This reviewer was right about the dependent variable (one of many), but what did she/he mean by “knee-jerk”?



Gary P. Latham

University of Toronto, Canada

“If I did not know Latham, I would not have read past the abstract. Unfortunately, I did; the paper was even worse.”



Elizabeth F. Loftus

University of California, Irvine

In 1983, a student (Wesley Marburger) and I published an article in *Memory & Cognition* with the unusual title, “Since the Eruption of Mt. St. Helens, Has Anyone Beaten You Up?: Improving the Accuracy of Retrospective Reports With Landmark Events.” We found that providing a landmark event (in this case, the 1980 volcano eruption in Washington State) for subjects who are trying to remember their past victimizations reduced the problem of misdating past events and resulted in more accurate reporting. As of February 2015, the paper was cited 324 times in Google Scholar. Citers might be surprised to learn that without some perseverance on the part of the senior author, no one might know about this paper: It was rejected by five journals. After the fifth, I read all the reviews and saw that the ones provided by the *Memory & Cognition* reviewers were least negative. I went back to that journal and made a case for publication.



Henry L. “Roddy” Roediger, III

Washington University in St. Louis

In 1975, Bob Crowder [now deceased; a Yale professor at the time] and I wrote a paper entitled “A Serial Position Effect in Recall of United States Presidents.” Like most authors, we were hopeful others would find it of interest and that reviewers and an editor would accept it. We submitted it to the *Journal of Verbal Learning and Verbal Behavior* (now the *Journal of Memory and Language*) edited by Ed Martin. In those days, the idea of triaging papers had not yet taken hold in psychology. Ed blazed a trail

in triage by sending our manuscript back to us by return mail. His action letter was two sentences long:

Dear Dr. Roediger:

Your manuscript with Robert G. Crowder, “A Serial Position Effect in Recall of United States Presidents,” is, of course, rejected. Check any source and you will see the presidents best recalled are most often cited in print, so all you have shown is that frequent items are better recalled.

Sincerely,

Ed Martin, Editor

The paper eventually appeared in the now defunct *Bulletin of the Psychonomic Society*, which had a rejection rate of 0% (for members of the society). We discussed Martin’s criticism there. Interestingly, the original data from that rejected paper recently appeared in *Science* in November 2014, as part of the data in a paper on “Forgetting the Presidents,” by me and K. Andrew DeSoto.