

13

Crossing the Line: Pseudonyms and Snark in Post-Publication Peer Review

Paul S. Brookes

To discuss the role of snarkiness in scientific criticism, I will start out with an example. It is fun to think of Johnny Cochran, the lawyer from the O. J. Simpson trial, and his famous quotation, “*If the glove doesn’t fit, you must acquit.*” I think we can adapt this for science, and say “*If the data is whack, you must retract.*”

In addition to snarkiness, I will discuss pseudonyms and anonymity, and the fine line between what we can say to criticize each others’ science, without getting into slander/libel and *ad hominem* attacks. This line is not well defined for scientists (for example, the difference between saying “*I think these figures are too sufficiently similar as to have occurred by pure coincidence*” versus “*these figures are identical*”). The blurriness of that line may be a factor that causes many scientific commenters to remain anonymous.

Prologue—How I Got into This Mess

I entered the whole misconduct field when looking at some papers from a competitor’s lab. They published a paper (Pu et al., 2008) reporting on novel mitochondrial splice variants of potassium channels, and then a year later they published another paper (Ye et al., 2009) using the same custom antibody with the same tissue prep and magically an extra band appeared on the western blot at 55 kilodaltons where there was not one before. This made us suspicious.

Around the same time, I received a grant to review for the American Heart Association, which came from a postdoctoral researcher in that group. The grant was actually about these novel splice variant potassium channels. They were exposing mouse hearts to different treatments—*ischemia* or *hypertrophy*—and then using the custom antibody to probe a western blot for the potassium channel. Now, when we do this type of experiment, we have to ensure that we load the same amount of protein

from the sample. So we do what is called a “loading control” for the blot. Anyway, it was obvious that the same loading control was used on the western blots for two completely unrelated experiments.

Once you find one example of this, the first thing you do is dig deeper, and so I started probing into the prior publications of this individual from his graduate student days and discovered a number of instances in which he was using the same images for different experiments; “*blot splicing and dicing*” is what we call it. I put all of this information in a PowerPoint presentation and sent it off to the ORI (the US Federal Office of Research Integrity), and then waited and waited. A few months later, I got a very nice email back from the mentor of the person in question, saying “*thank you for bringing these issues to my attention.*”

How did he find out? I asked the ORI and they replied that standard protocol is to simply pass on the allegations to the institution, non-blinded, nonanonymized. If you want anonymity you have to specifically ask for it! That scares me—the standard protocol of the ORI is just to pass this stuff on to wherever the problem is. That was one of the defining moments for me—I decided if I am going to do more in this area, I have to do it anonymously. While there are various arguments for and against anonymity, as a scientist, getting an email from your direct scientific competitor, who knows it was you who ratted out his lab to the ORI, can create real problems.

Moving forward, in 2011 there emerged a number of “*snarky*” blogs, and the genre of using witty titles and making jokes and puns on papers with suspicious data. An early example from the blog Abnormal Science was “*PNAS called, they want their gels back*” (<https://web.archive.org/web/20120313083119/http://abnormalscienceblog.wordpress.com/2011/12/12/pnas-called-want-their-gels-back/>). Juichii Jigen was another blogger, who ran about fifteen different sites, each devoted to suspect papers from an individual scientist (<https://www.blogger.com/profile/03513633746083109180>). One of Jigen’s blogs featured half a dozen papers by Bharat Aggarwal, a prominent cancer researcher from MD Anderson in Houston. Aggarwal is a PhD, not an MD, and so has no short clinical case reports to boost his publication numbers, but nevertheless he was publishing about forty-five scientific research papers a year—roughly a paper a week! In December 2011, I took the first two hundred entries from Aggarwal on PubMed, and managed to pull ninety-two PDFs. I found fifty-two had suspect data, and sent them to Jigen who posted them on his blog. We eventually found about eighty-five papers with problems and reported them to the ORI.

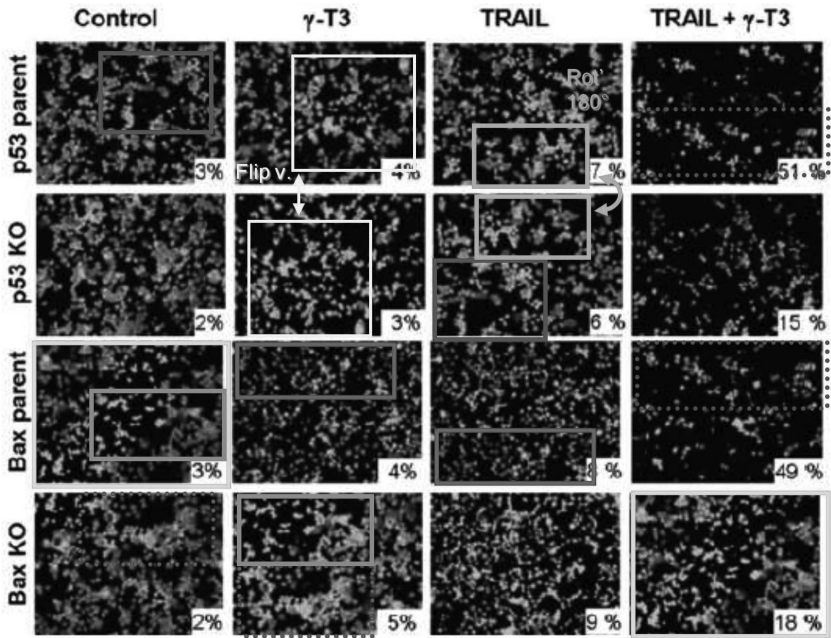


Figure 13.1

An example is shown in figure 13.1 (from Kannappan et al., 2010). These are microscope images of cells that are allegedly dying, and each panel is supposed to represent a different experimental condition or treatment. What you soon realize is that some of the images look similar, and as shown here with the overlay, everything in the same colored box is cloned. Some images have been flipped, some rotated, and others cropped differently. What's remarkable is this study is still out there in the literature, unannotated. The journal has been written to, but the editors did not respond. To the unsuspecting reader, these data are perfectly legitimate, even though you can see they represent the epitome of the term “data fabrication.” I would like to be able to report that this paper is an isolated incident, but cannot. From the eighty-five papers of Aggarwal that were flagged, there have to date been only six corrections and no retractions. In the mean time, he published forty-seven new papers since the ORI was notified about these problems. One has to question where is the ORI in all this? It's been four years now, and there has been no action whatsoever, and no sign that any action is forthcoming.

The Blog

When faced with that level of recalcitrance in correcting the literature, my response was to start a blog. I called it <http://www.science-fraud.org>, which I now realize was a rather naïve thing to do, because apparently people get upset having their name associated with the word fraud! Since I'm English and we swear a lot, I also decided the blog would use snarky language and be obnoxious about things. All of this was done under the pseudonym “Francis de Triusce,” which is an anagram of “Science Fraudster.” Note that, despite the coincidental name, I am not Clare Francis, although I have received a lot of emails accusing me of being that person.

Regarding the choice to do this anonymously, in addition to the reasons already cited, I am a strong believer that in science the message itself should be the focus, not the identity of the messenger. I have seen numerous examples on PubPeer (e.g., <https://pubpeer.com/publications/D2A46528724F9B59FD58693CA41560>) where the focus has been not on the actual scientific content, but rather the qualifications of the commenter—whether they are worthy enough to comment. This is wrong. If a grade-school student identifies a genuine problem in a science data set, their opinion should be just as valuable as that of a Nobel Prize winner.

Here are a few examples of the types of posts made on the blog, and my rather lame witticisms related to the persons or science involved:

- A) In the case of an electrophysiology paper (Rottlaender et al., 2010), I called it “*An Electrifying Case of Image Manipulation.*” As seen in figure 13.2, every one of the patch clamp recordings in the same colored boxes is replicated. This led to a post-doc in the lead author's lab being investigated by the DFG (the German Research Funding Agency) and found guilty of misconduct.
- B) Another example was from a prolific cancer researcher, Michael Karin, who had many papers featured on one of Juichii Jigen's blogs (<http://karinlab-et-al.blogspot.com/>). After so many posts, I simply went with the title “*I'm Past Karin.*”
- C) Keeping with the puerile tradition, I posted on a number of papers from an inflammation researcher at the University of Glasgow by the name of Foo Y. Liew, with the post entitled “*Fooled You.*”
- D) I featured a number of posts on sirtuin biology, because my lab has an interest in that area. One of the people featured most heavily was Gizem Donmez, from the lab of Leonard Guarente at MIT, and the

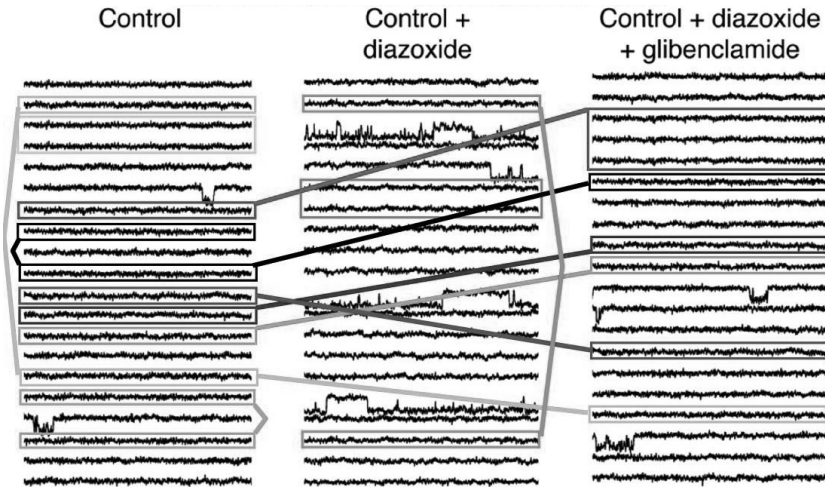


Figure 13.2

title “*Don’ Mez with the Sirtuins*” was too good to pass up. Donmez was subsequently fired for misconduct.

The Legal Threats

The blog ran for about six months (July to December 2012) before legal threats started coming in. I first received letters from lawyers representing Rui Curi (a prominent Brazilian scientist with hundreds of publications), then Rakesh Kumar (of George Washington University, who also attempted to sue his employer for wrongful dismissal prompted by the fallout from these allegations; <http://retractionwatch.com/2015/02/11/rw-cited-scientists-8-million-suit-university/>). I also received a legal threat from Sam W. Lee (of Massachusetts General Hospital), and then from Gizem Donmez.

What really caught my attention was that Lee and Donmez were both represented by Normand Smith, who was the defense attorney from the David Baltimore case.¹ At that point, I decided to not mess with this anymore. There are obviously a number of ways to respond to legal threats, and perhaps this is easier if you are a journalist rather than a scientist running a lab, and I have to consider how an extended legal battle might affect my actual science career. The fecal matter hit the rotational cooling device in January 2013 when somebody was able to obtain the proxied WHOIS information from my website, and decided to email everybody that I’d ever blogged about, plus several people within my own university, telling them

I run a “hate site” and urging them to sue me. I had a nice little chat with my boss, who asked about my priorities, and that was the end of the blog.

The Aftermath

Although I am tenured, and I think criticizing the scientific literature is firmly within my job description, the university declined to provide any legal support. I hired an attorney at my own expense, and successfully rebutted all the legal threats.

During late 2013, I began to question whether something good could come out of this. I realized that I had a set of papers that constituted a unique data set. There were 274 papers I had blogged about, but I had another set of approximately 220 papers, which were all received around the same time frame. They all went through the same vetting process, and in fact many of the cases were written up and ready to blog about before the site was shut down. The question arose, what happened to those papers? Were the ones that were blogged about treated differently to the ones that never made it into the public eye? Were they corrected or retracted at different rates?

In the resulting paper (Brookes, 2014), the keynote result was that the blogged papers were corrected and retracted seven-fold more than the ones that stayed private. I think that says a lot about the role that publicity has to play in correcting the scientific literature.

One thing I’ve done recently is to go back and ask: is the result still true? One of the nagging doubts about this study was that although all the papers were received by me in roughly the same seven-month time frame, the papers that I never blogged about were received ever-so-slightly later than the blogged ones (November 2012 to January 2013 vs. June to December 2012, respectively). Given the increasing availability of social media tools such as PubPeer and PubMed Commons, I questioned whether the nonblogged papers would eventually catch up. In fact, in the time since my study was published, the blogged papers continued to accrue retractions and corrections at a rate seven-fold faster than the nonblogged papers. There has been no catch up, even though I know that many of the papers I held back have now made it into public view on sites such as PubPeer.

The take home message is not simply that criticizing science in public gets results. Rather, it is that a particular type of criticism—the snarky variety—gets better results. There is an additional boost, where writing about science by telling a story using colorful language yields more action than simply tagging a paper on a polite forum. I realize that as scientists

we are supposed to behave professionally, but when a few swear words can yield a seven-fold increase in suspect papers being dealt with, that is not a small effect size. Another key point is that real scientists do not need lawyers. There is an appropriate response to being approached about your data, and it's not to respond with legal threats. If the data speak for themselves, then lawyers are not a necessary accompaniment to the scientific process.

Notes

1. https://en.wikipedia.org/wiki/David_Baltimore#Controversies.

References

- Brookes, P. S. 2014. Internet Publicity of Data Problems in the Bioscience Literature Correlates with Enhanced Corrective Action." *PeerJ*. 2:e313.
- Kannappan, R., J. Ravindran, S. Prasad, B. Sung, V. R. Yadav, S. Reuter, M. M. Chaturvedi, and B. B. Aggarwal. 2010. Gamma-Tocotrienol Promotes TRAIL-Induced Apoptosis Through Reactive Oxygen Species/Extracellular Signal-Regulated Kinase/p53-Mediated Upregulation of Death Receptors. *Mol. Cancer Ther.* 9:2196–2207.
- Pu, J. L., B. Ye, S. L. Kroboth, E. M. McNally, J. C. Makielski, and N. Q. Shi. 2008. Cardiac Sulfonylurea Receptor Short Form-Based Channels Confer a Glibenclamide-Insensitive K_{ATP} Activity. *J. Mol. Cell. Cardiol.* 44:188–200.
- Rottlaender, D., K. Boengler, M. Wolny, G. Michels, J. Endres-Becker, L. J. Motloch, A. Schwaiger, A. Buechert, R. Schulz, G. Heusch, and U. C. Hoppe. 2010. Connexin 43 Acts as a Cytoprotective Mediator of Signal Transduction by Stimulating Mitochondrial K_{ATP} Channels in Mouse Cardiomyocytes. *J. Clin. Invest.* 120:1441–1453.
- Ye, B., S. L. Kroboth, J. L. Pu, J. J. Sims, N. T. Aggarwal, E. M. McNally, J. C. Makielski, and N. Q. Shi. 2009. Molecular Identification and Functional Characterization of a Mitochondrial Sulfonylurea Receptor 2 Splice Variant Generated by Intraxonic Splicing. *Circ. Res.* 105:1083–1093.

This is a section of [doi:10.7551/mitpress/11087.001.0001](https://doi.org/10.7551/mitpress/11087.001.0001)

Gaming the Metrics

Misconduct and Manipulation in Academic Research

Edited by: Mario Biagioli, Alexandra Lippman

Citation:

Gaming the Metrics: Misconduct and Manipulation in Academic Research

Edited by: Mario Biagioli, Alexandra Lippman

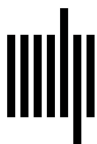
DOI: 10.7551/mitpress/11087.001.0001

ISBN (electronic): 9780262356565

Publisher: The MIT Press

Published: 2020

This title is freely available as an open access edition thanks to the TOME initiative and the generous support of the University of California, Davis. Learn more at openmonographs.org



The MIT Press

© 2020 Massachusetts Institute of Technology

This work is subject to a Creative Commons CC BY-NC-ND license.



Subject to such license, all rights are reserved.

This title is freely available as an open access edition thanks to the TOME initiative and the generous support of the University of California, Davis. Learn more at openmonographs.org.

This book was set in Sabon by Westchester Publishing Services.

Library of Congress Cataloging-in-Publication Data

Names: Biagioli, Mario, 1946- editor. | Lippman, Alexandra, editor.

Title: Gaming the metrics : misconduct and manipulation in academic research / edited by Mario Biagioli and Alexandra Lippman.

Description: Cambridge, MA : MIT Press, [2020] | Series: Infrastructures | Includes bibliographical references and index.

Identifiers: LCCN 2019010150 | ISBN 9780262537933 (pbk. : alk. paper)

Subjects: LCSH: Scholarly publishing—Corrupt practices. | Learning and scholarship—Corrupt practices. | Research—Corrupt practices. |

Communication in learning and scholarship—Moral and ethical aspects.

Classification: LCC Z286.S37 G36 2020 | DDC 070.5—dc23

LC record available at <https://lccn.loc.gov/2019010150>

10 9 8 7 6 5 4 3 2 1