SOME OF MY FAVORITE EXCERPTS FROM GELMAN’S “WINDS HAVE CHANGED” BLOG

We have now reached the “emperor has no clothes” phase

the research incumbency rule . . . once an article is published in some approved venue, it should be taken as truth. I’ve written [elsewhere](http://andrewgelman.com/2016/02/01/peer-review-make-no-damn-sense/) on my problems with this attitude

find-statistical-signficance-any-way-you-can-and-declare-victory paradigm

huge, obvious multiple comparisons problems

I see a [connection](http://andrewgelman.com/2016/02/09/28905/) between scientific fraud, sloppiness, and plain old incompetence

massive uncontrolled researcher degrees of freedom

What Fiske [i.e., Podos and collaborators] should really do is cut her losses, admit that she and her colleagues were making a lot of mistakes, and move on. She’s got tenure

And that’s why the authors’ [claim](https://pubpeer.com/publications/E043DD982C3CC7F4B2CB4980522684#fb51945) that fixing the errors “does not change the conclusion of the paper” is both ridiculous and all too true. It’s ridiculous because one of the key claims is entirely based on a statistically significant p-value that is no longer there. But the claim is true because the real “conclusion of the paper” doesn’t depend on any of its details—all that matters is that there’s *something*, somewhere, that has p less than .05, because that’s enough to make publishable, promotable claims about . . . whatever . . . they want to publish that day. . . . When the authors protest that none of the errors really matter, it makes you realize that, in these projects, the data hardly matter at all.

the paradigm of the open-ended theory, of publication in top journals and promotion in the popular and business press, based on “p less than .05” results obtained using abundant researcher degrees of freedom . . . is “more vampirical than empirical—unable to be killed by mere data.”

collaborators and former students also seem to show similar research styles, favoring flexible hypotheses, proof-by-statistical-significance, and an unserious attitude toward criticism

For Cuddy, Norton, and Fiske to step back and think that maybe almost everything they’ve been doing for years is all a mistake . . . that’s a big jump to take. Indeed, they’ll probably never take it. All the incentives fall in the other direction.

I bring this up not in the spirit of gotcha, but rather to emphasize what a difficult position Fiske is in. She’s seeing her professional world collapsing . . . her work and the work of her friends and colleagues is being questioned

To put it another way, Fiske and her friends and students followed a certain path which has given them fame, fortune, and acclaim. Question the path, and you question the legitimacy of all that came from it.

Journals and authors often apply massive resistance to bury criticisms

it’s a problem for other researchers who want to do careful work but find it difficult to compete in a busy publishing environment with the authors of flashy, sloppy exercises in noise mining

We learn from our mistakes, but only if we recognize that they *are* mistakes. Debugging is a collaborative process. If you approve some code and I find a bug in it, I’m not an adversary, I’m a collaborator. If you try to paint me as an “adversary” in order to avoid having to correct the bug, that’s *your* problem.