

# The Loss-of-Confidence Project



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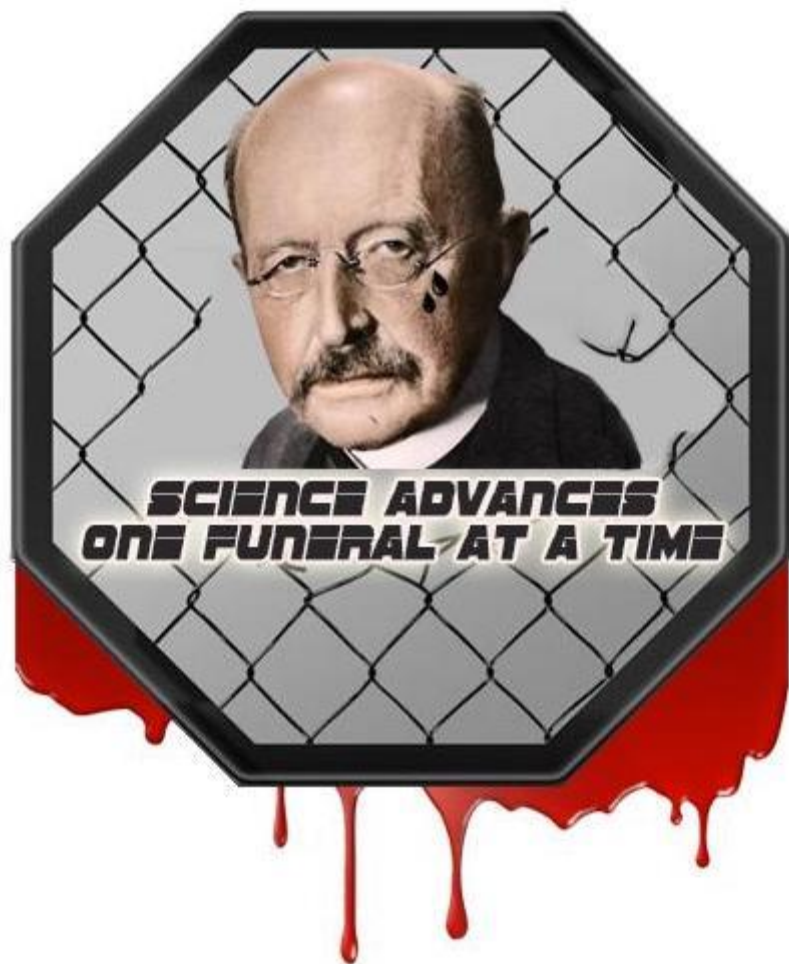
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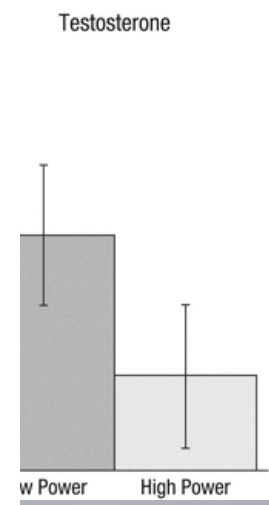
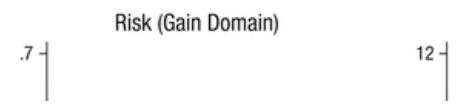
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“A new scientific truth does not generally triumph by persuading its opponents and getting them to admit their errors, but rather by its opponents gradually dying out and giving way to a new generation that is raised on it”





Does it need to be that  
way?



*APS Observer, in press*

## Mob Rule or Wisdom of Crowds?

*Introduction by APS President Susan Goldin-Meadow:* New forms of media are making it easier and easier for us to react to, and comment on, research within our community. Although free-flowing comments and criticisms can often push an argument or research program forward in a good direction, they can also derail, and perhaps even threaten, the process. I invited guest columnist Susan Fiske, a former APS president, to think about the impact that the new media are having not only on our science, but also on our scientists.

**Susan T. Fiske**  
APS Past President

Our field has always encouraged – required, really – peer critiques. But the new media (e.g., blogs, twitter, Facebook posts) are encouraging uncurated, unfiltered trash-talk. In the most extreme examples, online vigilantes are attacking individuals, their research programs, and their careers. Self-appointed data police are volunteering critiques of such personal ferocity and relentless frequency that they resemble a denial-of-service attack that crashes a website by sheer volume of traffic.

“methodological terrorism”

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professors afraid to come up for tenure, mid-career people wondering how to protect their labs, and senior faculty retiring early, all because of methodological terrorism. I am not naming names because ad hominem smear tactics are already damaging our field. Instead, I am describing a dangerous minority trend that has an outsized impact and a chilling effect on scientific discourse. I am not a primary target, but my goal is to give voice to others too sensible to object publicly.

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## My position on “Power Poses”

*Regarding: Carney, Cuddy & Yap (2010).*

Reasonable people, whom I respect, may disagree. However since early 2015 the evidence has been mounting suggesting there is unlikely any embodied effect of nonverbal expansiveness (vs. contractiveness)—i.e., “power poses” - on internal or psychological outcomes.

As evidence has come in over these past 2+ years, my views have updated to reflect the evidence. As such, **I do not believe that “power pose” effects are real.**

Any work done in my lab on the embodied effects of power poses was conducted long ago (while still at Columbia University from 2008-2011) – well before my views updated. And so while it may seem I continue to study the phenomenon, those papers (emerging in 2014 and 2015) were already published or were on the cusp of publication as the evidence against power poses began to convince me that power poses weren’t real. My lab is conducting no research on the embodied effects of power poses.

The “review and summary paper” published in 2015 (in response to Ranehill, Dreber, Johannesson, Leiberg, Sul, & Weber (2015) ) seemed reasonable, at the time, since there were a number of effects showing positive evidence and only 1 published that I was aware of showing no evidence. What I regret about writing that “summary” paper is that it suggested people do more work on the topic which I now think is a waste of time and resources. My sense at the time was to put all the pieces of evidence together in one place so we could see what we had on our hands. Ultimately, this summary paper served its intended purpose because it offered a reasonable set of studies for a p-curve analysis which demonstrated no effect (see Simmons & Simonsohn, *in press*). But it also spawned a little uptick in moderator-type work that I now regret suggesting.

I continue to be a reviewer on failed replications and re-analyses of the data -- signing my reviews as I did in the Ranehill et al. (2015) case -- almost always in favor of publication (I was strongly in favor in the Ranehill case). More failed replications are making their way through the publication process. We will see them soon. The evidence against the existence of power poses is undeniable.

There are a number of methodological comments regarding Carney, Cuddy & Yap (2010) paper that I would like to articulate here.

### Here are some facts

1. There is a dataset posted on dataverse that was posted by Nathan Fosse. It is posted as a replication but it is, in



Encouraging researchers to  
be open about their past  
research

# The Loss-of-Confidence Project

- » website inviting psychologists to describe their loss of confidence
- » rules
  - » author has lost confidence in primary/central finding
  - » because of theoretical or methodological problems
  - » for which they take the primary responsibility
- » all submitters become co-authors of the resulting manuscript



# The Loss-of-Confidence Project

- » initial public reaction: very positive
- » but only a handful of submissions
- » preprint, then invite more submissions

# Example: Statement by Tal Yarkoni

I now think most of the conclusions drawn in this article were absurd on their face. My understanding of statistics has improved a bit since writing the article, and it is now abundantly clear to me that (a) I *p*-hacked to a considerable degree and that (b) because of the “winner’s curse,” statistically significant effect sizes from underpowered studies cannot be taken at face value.



# 13 Loss-of-Confidence Statements

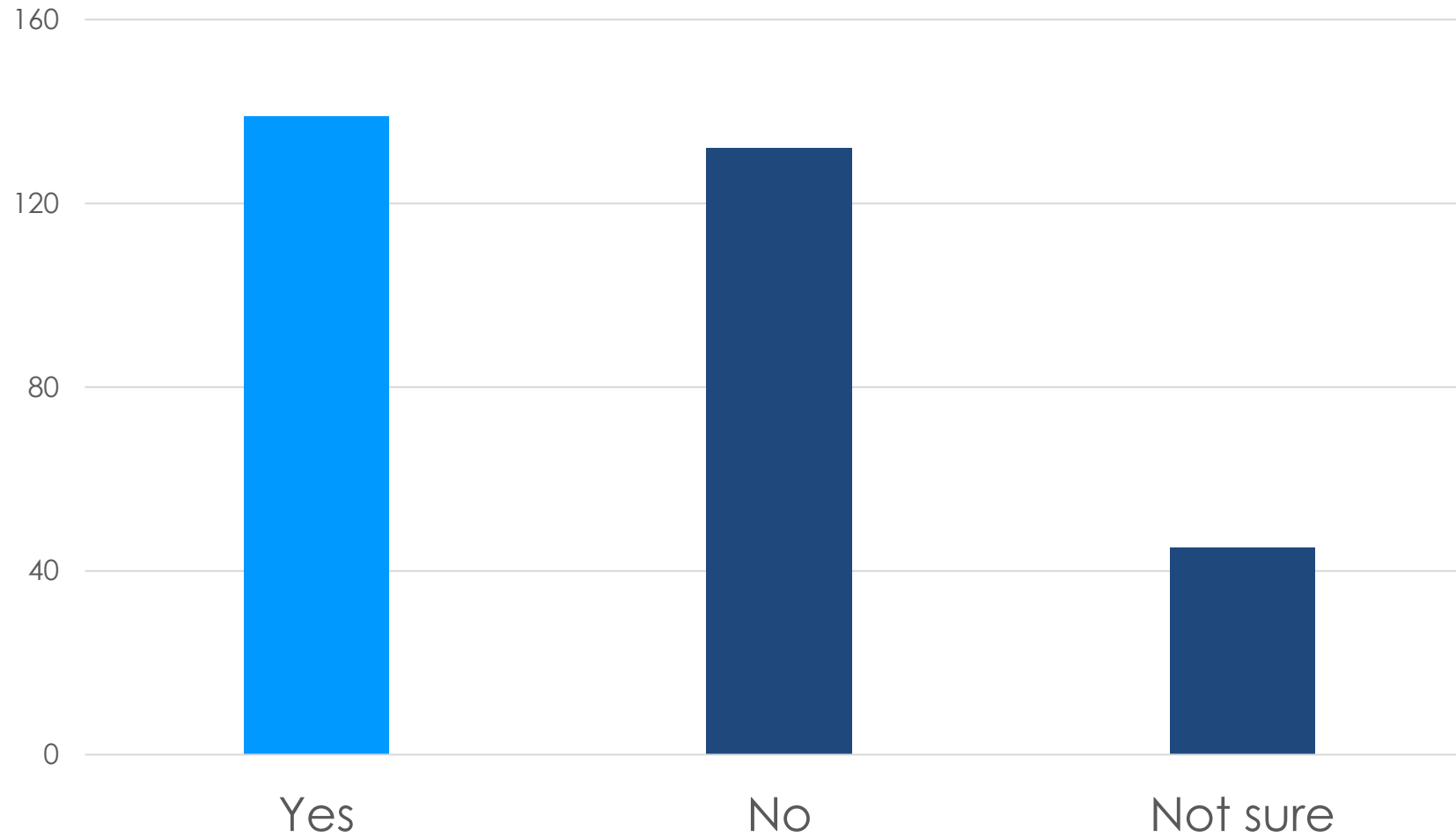
- » from a broad variety of psychological fields
  - » neuro, social, evolutionary, experimental, personality,...
- » broad variety of issues
  - » p-hacking, model misspecification, invalid inference,...

How often does something  
like this happen?

# Loss-of-Confidence Survey

- » non-representative online survey
- » open to researchers from all fields
- »  $N = 316$

Have you ever lost confidence in one of your own published research articles?



# Why did researchers lose confidence?

» mostly questionable research practices, but again broad variety of reasons

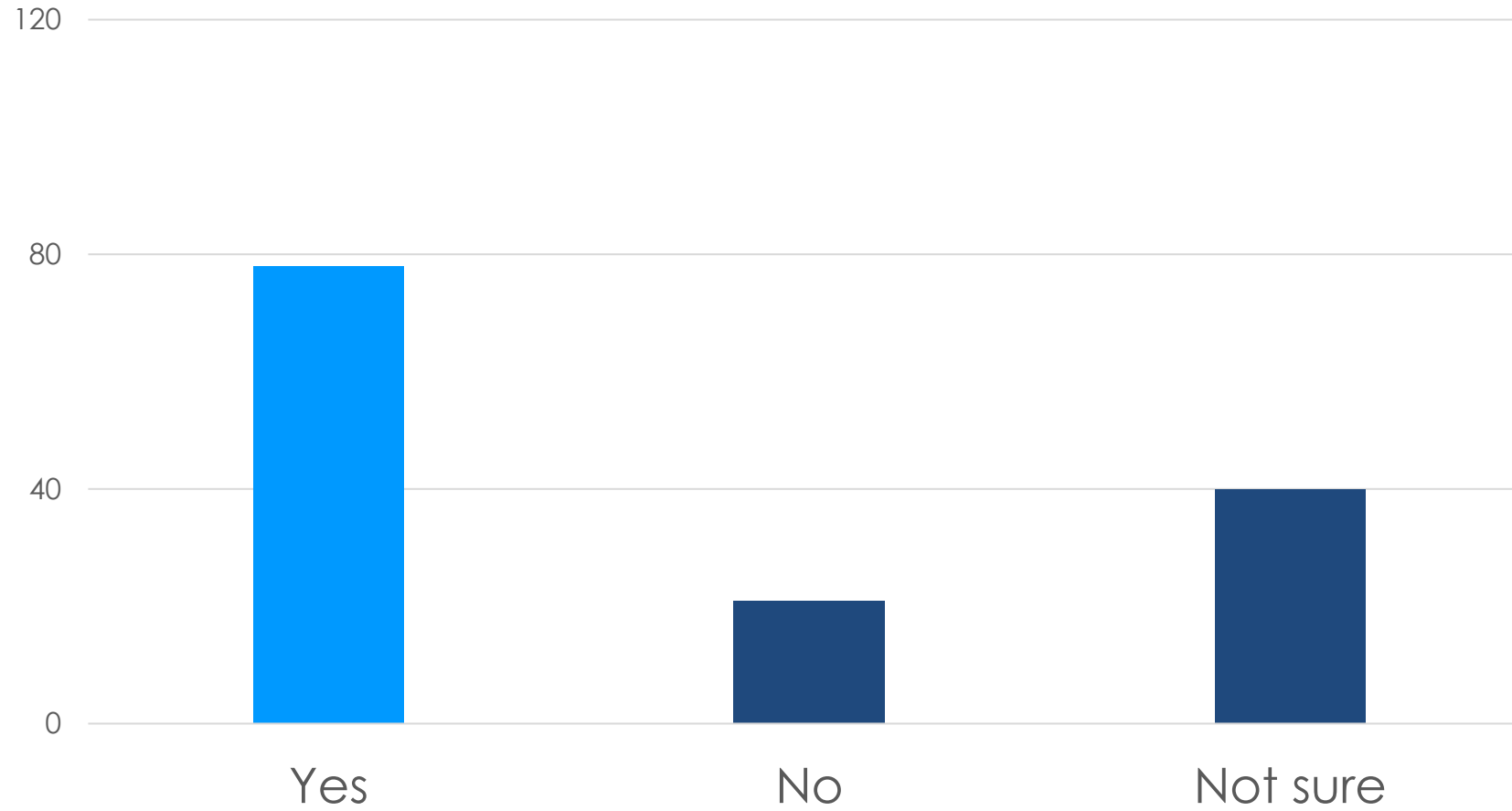
» e.g.

» “I was a junior co-author who collected and cleaned data and I felt some of the results were HARKed or hacked, but did not do very much about it”

» “I think there was a mistake in the analysis script that I didn't double-check because the results were in the direction of our hypotheses”

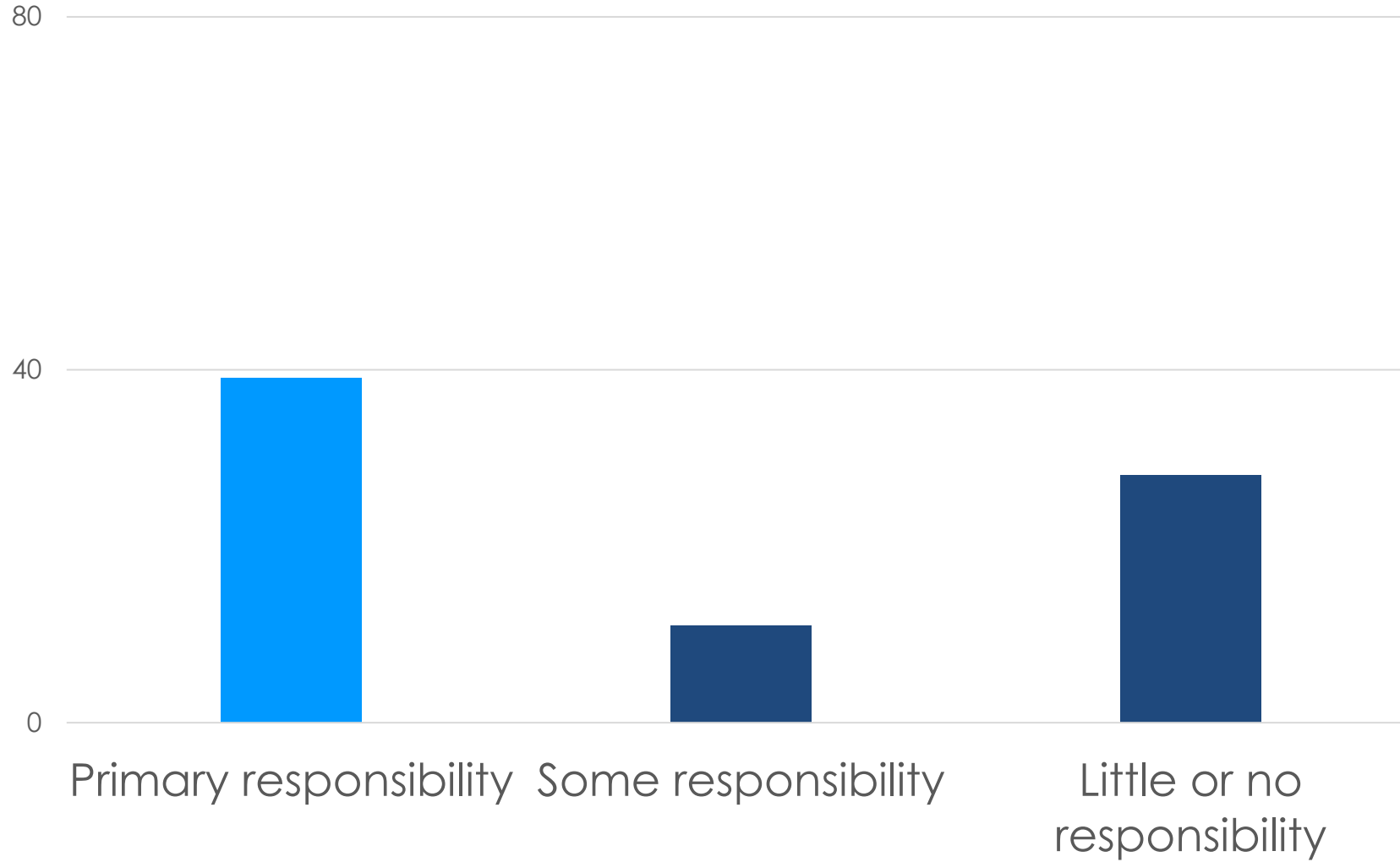
» “poor understanding of causal inference”

Was the loss of confidence due to a mistake or  
shortcoming in judgment on the part of the  
researcher?

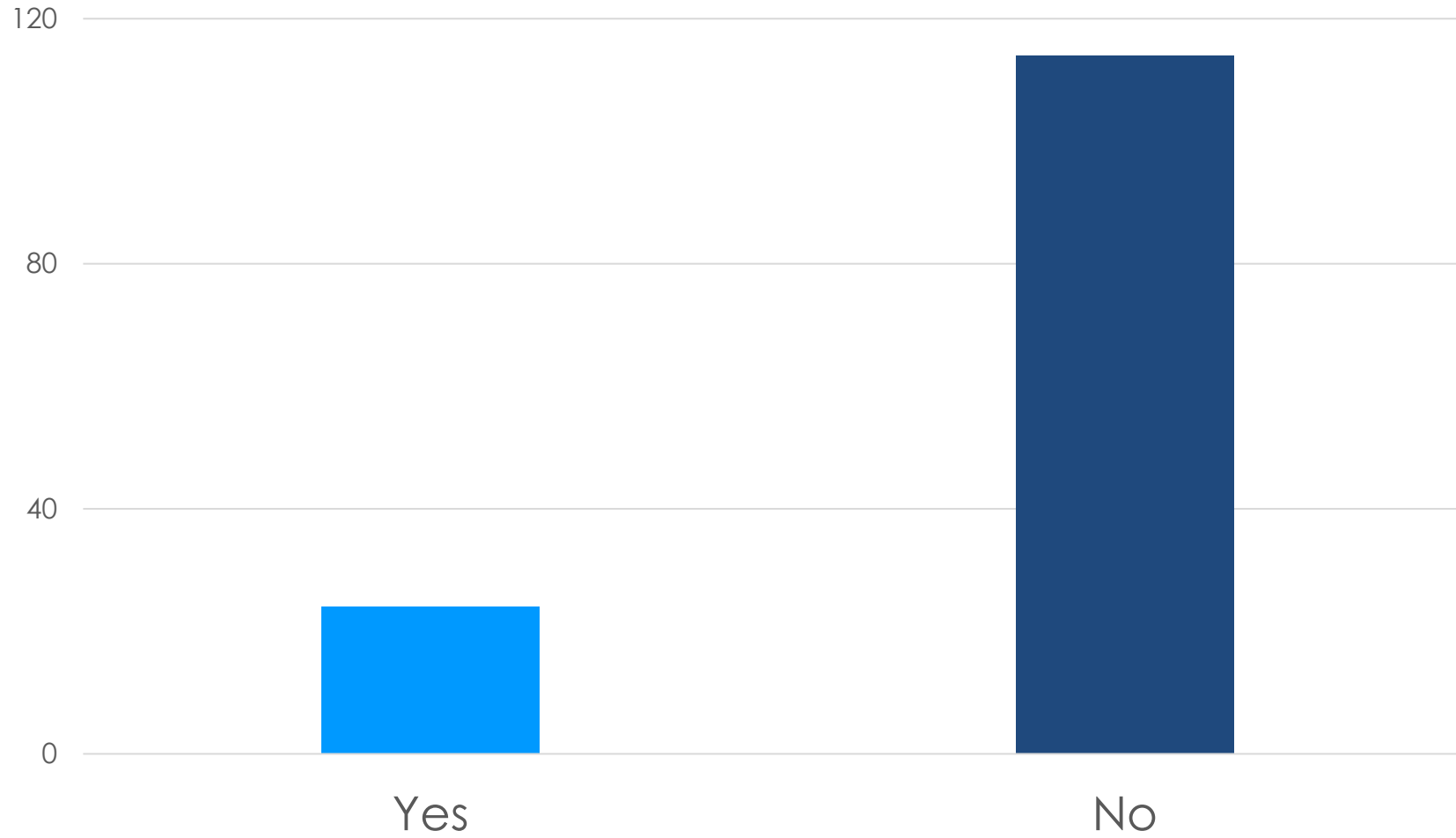




# To what extent do you take personal responsibility?



Is your loss of confidence a matter of public record  
in some way?



# Why not public?

» not sufficiently sure, not necessary, co-authors feelings, not sure how to communicate, worry about perception...

» e.g.

» “I haven't had time to try and redo the analyses that I know are wrong.”

» “Would hurt my career plans”

» “I need published papers to get my phd. Supervisor basically pressured to apply QRPs for publication, otherwise story wouldnt be sexy enough. Honestly, i stopped to care, all in all, i'll leave science anyway.”

# What can we learn from the survey?

- » substantial number of researchers have lost confidence in one of their findings
- » few of them make their loss of confidence public
- » host of reason keeps researchers from correcting their own claims
  - » concerns about own reputation and career
  - » concerns about co-authors, doctoral students
  - » information not important, nobody would care
  - » lack of protocol for how to deal with situation, lack of venue

# Putting the Self in Self-Correction

» prescriptive vs. descriptive norm

» fixing the formalities:

» lower threshold for correction, retraction (under different label?)

» make self-correction discoverable

» more dynamic models of publishing

# Putting the Self in Self-Correction

- » problem: perception that self-correcting is not worth the hassle/a sign of weakness/career killer
  - » Researchers who actually retracted one of their own papers report that concerns about suffering reputational damage turned out to be unfounded (Hosseini et al., 2018)
- » Need for a general shift towards more openness about the whole research process



**Frances Arnold**

@francesarnold



It is painful to admit, but important to do so. I apologize to all. I was a bit busy when this was submitted, and did not do my job well.

[Tweet übersetzen](#)



**Frances Arnold** @francesarnold · 2. Jan.

For my first work-related tweet of 2020, I am totally bummed to announce that we have retracted last year's paper on enzymatic synthesis of beta-lactams. The work has not been reproducible. [science.sciencemag.org/content/364/64...](https://science.sciencemag.org/content/364/64...)

7:02 nachm. · 2. Jan. 2020 · [Twitter Web App](#)

**658** Retweets **5.188** „Gefällt mir“-Angaben



**Anmol Kulkarni** @3\_anmol · 2. Jan.

Antwort an [@francesarnold](#)

Seeing a Nobel laureate tweet about a paper retraction teaches how important it is for scientist to be honest about their data. For someone like me who is just starting out in the field of research, your act teaches an important lesson.



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[1 weitere Antwort](#)

Thank you for your  
attention!