

Let's Not Mention That in the Report

I was finishing my senior year in college—a small mid-Western, liberal arts college—and was working in Dr. Smith's lab. Now, this college did not receive many grants, but Dr. Smith was recognized throughout the school (and frankly envied by a lot of his colleagues) as a real rainmaker. Despite the rather humble resources of the college, Smith was always getting money to run and grow his lab, and he turned out a number of students over the years who went on to have significant careers in science.

I was doing some extra-credit work in his lab, frankly hoping to be able to add material to my resume as I was applying to veterinary school. I was finishing a preliminary project and had gotten some very preliminary, but very interesting data. With graduation looming, however, I was discouraged that I couldn't replicate them. Naturally, I reported this to Dr. Smith, and we worked on it some. But to no avail: we simply could not replicate the original findings.

I then had to write a final report on this project which we would send to the funding agency for grant continuation. I duly noted the nature of the experiments, the preliminary data, and the fact that repeated attempts to replicate the data failed. I turned the report into Dr. Smith, but when he gave me the final copy that he was sending to the funding agency, I noticed that he had deleted the sentences about the data replication failures.

I asked him why, and he said that it was abundantly clear in the report that this data was very preliminary and was not at all being described as definitive. Second, he remarked that my project was one of three others that he was reporting on, and that these projects were much farther along and more important to the grantor. He felt that my findings were relatively insignificant in comparison to the others so there wasn't any point in belaboring my current failure to replicate my results. Third, he pointed out that it might still be possible to replicate the data. He speculated that perhaps my samples had gotten contaminated and that if we had a few more months to work on it, we'd confirm my original results.

And that was it. I graduated and moved on. But Dr. Smith's omitting mention of my replication failures has always stuck in my memory. Was it wrong or was he justified?

Expert Opinion

So, the dilemma contributor tells us that she got some interesting, but very preliminary data per an experiment she was performing. She then, after repeated attempts, utterly failed to replicate the original findings. In editing the dilemma contributor's final report, Dr. Smith—the dilemma contributor's advisor and lab supervisor—deletes mention of the replication failures and only mentions the positive findings. When questioned, Dr. Smith offers three reasons:

- 1) The experimental data are very preliminary, so they might be confirmed or disconfirmed at some future time. In Smith's mind, this preliminaryity morally justifies the selective omission of the replication failures in the report.
- 2) Because other projects in the lab are farther along, the focus of the report must be on them. This seems to justify a brief, but nevertheless positive mention of the original findings without a need to belabor the details of the replication failures.
- 3) Perhaps the replication failures are the result of contamination of the student's samples, such that the original findings will ultimately turn out to be true.

This case scenario is an interesting one as it invites a discussion of the important phenomenon of "motivated reasoning."

Motivated reasoning has attracted scholarly attention for nearly twenty years and draws on even older theories, especially dissonance and attribution theory.¹ Taken together, the core premises of motivated reasoning are that people usually will be 1) motivated to reduce the unpleasantness of

conflicting or dissonant beliefs, by 2) evaluating such dissonant information on the basis of how well it coincides with their own deeply held values, or about issues in which they have a serious, personal stake, or according to their self-understanding as worthwhile, competent, adequate, decent human beings.²⁻⁴ In a word, motivated reasoning holds that individuals will predictably reach conclusions that nicely corroborate or are synchronous with the beliefs and values they already hold very dear and which often sustain or are significant aspects of their self-identity.

Consequently, when it comes to cognitive materials that strike at issues about which we have strong, *antecedently formed* notions, we will “reason” in reverse. Consciously or unconsciously, one “knows” the conclusion one wants to reach before the data are presented. As that data are being presented, he or she rather automatically rejects, (re)arranges, saliences, “massages,” or simply ignores certain ones such that the conclusion that is secured is the one that he or she prefers.

One does not have to be a moral psychologist to grant the persuasiveness of motivated reasoning theory. What kind of “objective evidence” would it take to turn a die-hard liberal like Teddy Kennedy into a die-hard conservative like Rush Limbaugh and vice versa? What would it take to turn a rabid anti-abortionist into a staunch supporter of a woman’s right to choose? What kind of evidence would be required to persuade the Pope that God doesn’t exist or turn a staunch gun-control advocate into a card carrying member of the NRA? The motivated reasoning theorist would say that virtually any intellectual effort at ideological conversion in these cases will fail. Each individual would hold onto his beliefs unshakably and not be deterred by even the most factually compelling, logically powerful arguments the other side offers. Each would counterargue by ignoring evidence contrary to his cause or reinterpret or rewrite that evidence such that it fits his or her ideology. As noted above, the motivated reasoner already knows the conclusion he or she will reach. One simply selectively chooses and arranges the premises, reasons, or evidence to infer it.

And so we return to Dr. Smith. He is the school’s rainmaker and has turned out a large number of students over the years who have gone on to have significant careers in science. Doubtlessly, he holds himself, his lab, and his lab’s deliverables in high esteem (or, at least, he is anxious about maintaining that public reputation). To him, the public admission of repeated failures to replicate certain initially interesting, provocative findings is very distasteful and implies defeat. It doesn’t comport with his vision of his lab’s deliverables and, of course, he needs no reminding of the apparent bias in scientific publications for positive findings.⁵ Not surprisingly, the reasons he gives above are motivated to provide just the conclusion he wants: omit mentioning the replication failures. So let us review Dr. Smith’s (motivated) reasoning.

If, as the first reason suggests, the data are very preliminary, then it is hard to understand why that preliminary somehow favors publishing the positive results but doesn’t equally apply to acknowledging the replication failures. If one is going to only publish half the data, why favor the positive half? Indeed, given the repeated but failed attempts to replicate the positive findings, one might argue that the negative data are less preliminary and more robust than the positive findings.

The second argument—that because other projects are much farther along, we won’t dwell on yours—is odd: “We will just briefly highlight the positive findings of your preliminary experiment, and then quickly move on to the more serious material.” This thinking attempts to shift attention away from the distress of considering the replication failures and one’s moral obligation to present them. It is an excellent example of motivated reasoning by way of causing an attentional shift from unpleasant considerations to things that are much more comfortable to ponder.

The third argument—that if we had more time, we might be able to replicate those positive findings after all—is wishful thinking masquerading as rational argument. If there is no current evidence that the experimental samples are contaminated, why give that speculation so much credibility? The answer is because the speculation corroborates the desired or preferred course of action, which is to note the positive findings and omit mention of the replication failures.

Believing as true only the data I want to is antithetical to science. While the above scenario is only a report to a funding agency for grant continuation, one might argue that the purposeful deletion of relevant material is just as ethically indefensible as submitting massaged, incomplete, or otherwise untruthful data for publication. With grant money increasingly insufficient to fund the growing number of competitive, highly ranked proposals, deceit in a grant continuation report (or an application that cites previous work) can have downstream consequences just as damaging to the advancement of science as a faulty publication.

Sooner or later, of course, replicability is supposed to win out. In the meantime, partial data reporting only slows down the scientific engine and can create a great deal of needless effort among other scientists. Further and as noted in other expert opinions on this website, the publication of negative findings can sometimes be much more valuable than publishing positive ones. Perhaps the scientific community will support stricter requirements for posting negative lab and trial results, and penalties for failure to do so. Registering clinical trials, where all data must be reported, is certainly a step in the right direction.⁵ Dr. Smith should have appreciated all this more. He should have had his student publish both the positive and negative findings; have her speculate on reasons for the replication failures; and not have presented her with an instance of motivated reasoning.

Readings:

1. Kunda Z. The case for motivated reasoning. *Psychological Bulletin*, 1990;108(3):480-498.
2. Haidt J. The emotional dog and its rational tail: a social intuitionist approach to moral judgment. *Psychological Review*, 2001;108:814-834.
3. Westen D, Blagov P. A clinical-empirical model of emotion regulation: From defense and motivated reasoning to emotional constraint satisfaction. In *Handbook of Emotion Regulation*. J. Gross (ed), New York: Guilford, 2006: 373-392.
4. Bechara A, Damasio H, Damasio A. Emotion, decision making and the orbitofrontal cortex. *Cerebral Cortex*, 2000;10:295-307.
5. Johnson RT, Dickersin K. Publication bias against negative results from clinical trials: three of the seven deadly sins. *Nature Clinical Practice Neurology*, 2007;3(11):590-591.