Political Bias in the Social Sciences:

A Critical, Theoretical, and Empirical Review

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Reference:

Honeycutt, N., & Jussim, L. (2022). Political bias in the social sciences: A critical, theoretical, and empirical review. In Frisby, C.L., Redding, R.E., O'Donohue, W.T., & Lilienfeld, S.O. (Eds.), (2022). Ideological and political bias in psychology: Nature, scope and solutions. New York: Springer.

Author's notes: We thank Cory Clark, Bo Winegard, and Etienne LeBel for providing the data used for the analyses described in Tables 5 and 6.

"Everyone is biased," is a mostly vacuous truism. It may be literally true in some superficial sense, but this is entirely useless with respect to figuring which claims made by which person or scientist are valid or not. Clearly, some scientific claims are true, others are not. Sometimes, evidence is contradictory or muddled. Yet some scientific claims are obviously true, and some scientific claims may be true despite not being obvious. Thus, the truism "everyone is biased," does not necessarily mean that all conclusions reached by all people are biased, especially since some are better at overcoming their biases than others (Tetlock & Gardner, 2016). For truth-seeking enterprises, such as science, and truth-communicating enterprises, such as news and education, the stakes are unusually high. As Mark Twain probably never actually said (but is a good point nonetheless), "It ain't what you don't know that gets you into trouble; it's what you know for sure that just ain't so." Biased science can lead to counterproductive interventions, useless social programs, decades of wasted time and resources, and unnecessary social conflict by virtue of misleading people to believe false and derogatory things about those they view as their ideological opponents.

This chapter is a critical, theoretical, and empirical review of political bias. It is "critical" in that it roundly criticizes the manner in which the social sciences have allowed political biases to undercut the validity and credibility of their scholarship. It is a theoretical review because the chapter presents two complementary and synergistic models of academic bias (one about its manifestations, the other about its processes). It is empirical because the chapter then uses those models to review the now vast evidentiary case for political bias, and because this chapter presents new data providing further evidence of such biases. This chapter also highlights when proposed manifestations of political bias are plausible but not yet demonstrated – thereby also identifying potential directions for future empirical research.

Scientist's personal political biases, however, are not necessarily a problem under three conditions: (1). When there are plenty of scientists holding a range of ideological positions, so that, even though some individuals may be biased, the skeptical vetting that comes from having claims evaluated by political opponents insures that, over time, only the best and most valid claims–those most clearly supported by strong, rigorous evidence appropriately interpreted–come to be widely accepted as true (we

refer to this as “canonization”); (2). When the topic is apolitical; and (3). When the norms of, and practices of, scientists guarantee the winnowing of unjustified claims and the canonization of justified ones.

The first part of this chapter is organized around reviewing theory and evidence regarding those three conditions. Although the second condition is often met (there is a great deal of research on apolitical topics), we conclude that the evidence argues strongly against both the first and third conditions. Because political biases are a serious problem for social psychology and the social sciences, the second part of the paper presents theoretical models describing many of the ways those biases manifest, and reviews evidence regarding those manifestations.

The Massive Left Skew of Academia

Academia skews heavily left and the social sciences skew massively left (Langbert & Stevens, 2021). The skew is so extreme that, to those unfamiliar with the data, claims about the skew may sound like propaganda intended to delegitimize academia. In fact, some research has demonstrated that Americans–even those on the political right–actually underestimate just how massive the skew is (Marietta & Barker, 2019). But if extreme left skew constitutes justification for delegitimizing academia, then academia has delegitimized itself. Redding (this volume) hits many of the high points demonstrating the massive–and growing–left skew in the field of psychology. Specifically, one cited report indicates that upwards of 90% of social psychologists identify as liberal, and other cited studies indicate ratios of Democrats to Republicans range between 11.5 and 17 to one, with almost half of the psychology departments at the top 40 US universities not having a single Republican (Redding, this volume). The data, obtained from multiple independent researchers using a wide range of methodologies, all lead to the same conclusion: non-left scientists in psychology are an endangered species. Following we briefly describe data that demonstrates the same holds in the social sciences and humanities (and really, in the academy at-large).

Langbert & Stevens (2021) examined party registration of over 12000 faculty at “flagship” universities and colleges (i.e., ones highly ranked by U.S. News and World Report). In the social

sciences, the ratio of Democrats to Republicans ranged from a low of 3:1 in economics to a high of 42:1 in anthropology (in between was sociology, at 27:1). Similarly, Buss & von Hippel (2017) found that the social psychologists they surveyed voted for Obama over Romney by a ratio of 75:1. These findings, in sum, are consistent with those of many other studies of social science faculty politics (Gross & Simmons, 2014; Honeycutt & Freberg, 2017; Inbar & Lammers 2012; Kaufmann, 2021; Langbert, 2018; Peters et al., 2020). It is worth noting, though, that however extreme the skews are for self-reported ideology and party registration, they are probably *underestimates* when benchmarked against partisan *behavior.* For example, Langbert and Stevens (2020) found that more registered Republican faculty donated to Democratic political candidates than to Republican political candidates (6.0% v. 1.3%). Thus, it's likely that voter registration underestimates an already massive political skew.

Thus, the first condition is met for raising concern about political biases in the social sciences. Merton's (1973) norm of organized skepticism – one of the norms that supposedly justifies a privileged place in how a society goes about determining truth – is likely inherently impaired for politicized topics when those on the left outnumber the right by the magnitudes reported in the research we just reviewed. What is the political diversity threshold to ensure adequate Mertonian organized skepticism for politicized topics? We have no empirical answer to this question. However, we would speculate that the tipping point is somewhere around 3:1. We are not drawing a hard line at 25%; it is a speculative guess. Maybe the line is 15% or 20% or 30% or 35%. Maybe it varies from field to field depending on other specific field-related dynamics. Maybe it varies depending on topic, with topics in which people are more emotionally invested or which attract more activists, having a higher minority threshold in which biases will still heavily corrupt the field. Regardless of where the tipping point actually occurs, however, when it occurs, the types of biases reviewed in this chapter may be at dramatically heightened risk to corrupt some substantial portion of the field's scholarship on politicized topics. Furthermore, the skew in many social science fields is so extreme that one can be confident that, wherever the line is drawn, it has already been crossed.

Political Biases Are Irrelevant To Topics That Are Not Politicized

It should be obvious that biases only matter in domains about which one is biased. Just as gender biases should be irrelevant to estimates of ambient temperature, and ingroup biases should be irrelevant to estimates of ceramic tensile strength, political biases would be irrelevant to topics that are not politicized. There is abundant evidence of increasing political polarization in the U.S, increasing hatred of the other side, and strengthening of ideological "bubbles" where people primarily consume information from their own side, and even ascribe increased credibility to experts on one's own side on completely non-politicized topics (e.g., Drummond & Fischhoff, 2017; Finkel et al. 2020; Marks, Copland, Loh, Sunstein & Sharot, 2019; Pew, 2014; Twenge et al., 2016).

Thus, many topics are politicized, and it seems likely that these have increased over the years. For example, between 1994 and 2014, Democrats and Republicans increasingly diverged in their views on government waste and regulation, the personal responsibilities of the poor for being poor, whether immigrants are a boon or burden, and whether stricter environmental laws were needed, as well as on a slew of other issues. Party differences on these topics grew from about 20% in 1994 to about 40% in 2014 (Pew, 2014). Nonetheless, even in this context of increasing polarization and politicization, there are a great many topics that psychologists study that are (for now) completely devoid of political content. We are pretty sure one can study the neuroscience of smell, computational models of shape perception, or the effectiveness of cognitive behavioral therapy without triggering any sensitive political nerves. One might have an agenda when a psychologist studies issues such as these (a theoretical agenda, allegiance to a particular perspective or intervention, et cetera) and those agendas might operate in a manner similar to political ones, but that is beyond the scope of the present chapter. Even though biases may characterize almost any area of research, in many areas they will *not* be political biases.

Thus, even the potential for political biases to distort psychological science is limited to issues that are subjectively or implicitly politicized on the part of the researcher. But what about areas that *are* politicized? Do biases occur on politicized topics or do normal academic processes ensure political neutrality and scientific objectivity?

Arguments And Evidence That Normal Academic Processes Prevent Political Biases

From time to time, scholars have published defenses against the charge of political bias (e.g., Jost, 2011; Reinero, Wills, Brady, Mende-Siedlecki, Crawford, & Van Bavel, 2020; van Bavel, Reinero, Harris, Robertson & Parnamets, 2020). They generally argue that the personality dispositions of scientists or normal scientific processes ensure against social scientific research becoming compromised by political biases. We review these arguments next.

Personality and individual differences.

Some research has found that scientists score higher on personality measures of "openness to experience" (Lounsbury et al., 2012). And one paper reported that scientists require their beliefs to have more empirical consistency than do laypeople (Hogan & Maglienti, 2001). Presumably, the argument goes, these characteristics should render scientists sufficiently "open" to basing beliefs on scientific evidence, or revising their beliefs in response to disconfirming evidence, that political biases do not occur.

Peer review and the norms of science.

Furthermore, some argue that "... the norms of science attenuate the biases of individual scientists by institutionalizing vigorous debate and criticism (Merton, 1973)" (Reinero et al., 2021 p. 3). Indeed, Reinero et al. (2020) go further (also on p. 3) to argue that "...the peer review process is well designed to diminish groupthink because reviews are normally conducted in parallel by anonymous reviewers at arms length from the authors…" In fact, at least one social psychologist (Jost, 2011) has claimed that allegations of ideological bias are anti-scientific, in the sense that accusing scientists of political biases is merely an attempt to delegitimize rigorous science on purely partisan grounds.

Are left-leaning studies less replicable?

Reinero et al. (2020) proposed the following hypothesis: If political biases influence social science, then lower standards should be applied to left-leaning articles than to right-leaning articles. If this is the case, then left-leaning findings should prove less replicable than right-leaning findings. Reinero et al. (2020) conducted two sets of analyses, both finding no evidence that right-leaning articles were more

replicable than left-leaning ones. In one, doctoral students coded whether 194 replication attempts involved topics with a political slant; in a second, Mechanical Turk workers coded the same studies. Regardless of who did the coding, whether the replications succeeded or failed was unrelated to whether the articles were left- or right-leaning.

Arguments And Evidence That Normal Academic Processes *Fail* To Prevent Political Biases

Personality and Individual Differences

The idea that scientists are more "open to experience" may be true, but whether this leads to objectivity, validity, or credibility in producing science is an empirical question that has never been addressed. Even if true, we doubt its effect on validity is large, primarily because there are many other influences on the validity of research that can overwhelm a personality trait. Human behavior, including that of scientists, is influenced by far more than personality, including political attitudes, tribal/group affiliations, incentives, education, and social norms (Jussim, Krosnick, Stevens & Anglin, 2019).

Furthermore, one might also expect the highly educated and scientifically literate (such as academics) to generally be less polarized because, as this argument goes, such people would be more likely to base views on evidence. According to this view, if one has the training and expertise to understand the truth, political bias should get no traction.

Such a view surely sounds compelling. Unfortunately, evidence shows the opposite. Education and scientific literacy *increase* polarization on controversial science topics (Drummond & Fischoff, 2017). This probably occurs because the highly educated and scientifically literate are particularly good at enlisting arguments and evidence to bolster their preferred views. Inasmuch as academics tend to be highly educated and scientifically literate, this would tend to create greater rather than lesser tendencies towards political biases.

Indeed, contrary to the argument that academics' intelligence and commitment to evidence is sufficient to ensure against biases, we know for a fact that psychological research has been long plagued by many suboptimal methodological practices (see, e.g., Fraley & Vazire, 2014; Jussim et al., 2019; Simmons, Nelson & Simonsohn, 2011). These have produced what was once known as the Replication

Crisis (Open Science Framework, 2015). But, growing recognition of all sorts of dysfunctional and suboptimal practices go well beyond replication, and include measurement, interpretations, publication, citation, and canonization practices, all of which can and do undercut the validity of psychological science (see, e.g., Fraley & Vazire, 2014; Jussim et al., 2019; Flake & Fried, 2020; Yarkoni, 2020). Although these critiques do not directly address the issue of political biases per se, they constitute a strong refutation of claims that "the personalities of scientists immunize them from suboptimal scientific practices."

Failures of peer review.

The idea that peer review insures against invalid science is readily refuted by its many failures. Here are just a few:

- Psychology's Replication Crisis (see, e.g., Pashler & Wagenmakers, 2012)
- Registered reports produce fewer than half as many "statistically significant" findings as do conventional reports (Scheel, Schijen & Lakens, 2021)
- Papers published by researchers at prestigious institutions were nearly all rejected when they were subsequently resubmitted as new manuscripts by authors at low prestige institutions (Peters & Ceci, 1982)
- The Grievance Studies Sting, in which papers making wild claims were welcomed at peer reviewed journals, even when they made wild or vile claims, as long as those claims were framed as advancing social justice. These included, but are not limited to, advocating for men to (metaphorically) be leashed by dogs or White students to be chained to desks; rewriting excerpts from Mein Kampf as a treatise on feminist solidarity (Lindsay, Boghossian & Pluckrose, 2018).
- Over 90% of the literature touted the effectiveness of antidepressant medications, even though half the underlying studies failed to find they were effective (De Vries et al., 2019).

The pre-registered replication success rate in psychology is around 50% (Scheel et al., 2021), and no one

really knows which half of the studies that have not been subjected to replication attempts will hold up (for a discussion on what pre-registration is, see Nosek et al., 2018). Thus, contrary to the sanguine view that peer review is well-suited for eliminating bias (e.g., van Bavel et al., 2020), the inexorable conclusion from the evidence of rampant failures of peer review is that it does not insure against poor or biased science. Although reviewing the literature on the limitations and failings of peer review is beyond the scope of the present chapter, that literature is vast (for reviews, see Crane & Martin, 2018; Csiszar, 2016; Heesen & Bright, 2021).

Norms of Science

Some have argued that "norms of science" also function to limit political biases (e.g., Reinero et al., 2020; Van Bavel et al, 2020). There are, however, many problems with this idea. One is that the extent to which scientists embrace these norms, rather than *deploy them rhetorically to gain undeserved credibility* remains unclear. Some surveys have found that scientists *say* they embrace these norms (e.g., Anderson et al., 2010). On the other hand, some scholars have argued that this embrace is little more than a charade used by scientists to claim more credibility than they deserve (Mulkay, 1976), and that Merton's original norms have collapsed (Kellogg, 2006).

For example, consider Merton's disinterestedness norm. This is the idea that scientists should keep their personal interests and values out of science as much as possible. This norm is threatened and likely rejected (regardless of what scientists state on surveys) whenever social scientists endorse infusing their scholarship with activist goals (Becker, 1967; Gross & Simmons, 2014; Horowitz et al., 2018; Unger, 2001). One sees this whenever scientists accuse some academic publication of causing "harm" to some sort of activist agenda without regard for demonstrating that the findings or arguments are actually invalid (Dreger, 2016; Retraction Watch, 2020).

Consider also Merton's universalism norm: scientific truths should be evaluated for their validity based on impersonal standards such as quality of methodology and/or validity of the statistics rather than on the particular identities, statuses, or group memberships of the scientist making a truth claim. This norm is rejected writ large whenever academics privilege perspectives propounded by scientists from

particular identity groups, take "lived experience" as any more credible than conventional self-reports (limitations of which are legion in social psychology), or demand others incorporate such perspectives independent of a substantive reason for doing so. The extent to which psychologists adhere to Mertonian norms of science in their behavior–rather than rhetoric–is an open empirical question. There is currently little evidentiary basis for concluding that they constitute a guarantor against bias.

A Critical Evaluation Of Reinero et al. (2020): A Bad Hypothesis Badly Tested

In their *Scientific American* blog on their paper showing no replicability differences between right- and left-leaning findings, Reinero & van Bavel (2020) trumpeted, "... our study suggests that political bias may not plague psychological science to the extent that it dominates many other domains of society." Neither we nor they have any evidence comparing the extent of such biases among social psychologists versus "other domains of society." Indeed, people in other "domains of society" do not usually engage in scientific replication, so the comparison is odd. Nonetheless, our critical analysis of their study, which follows, suggests that it provides little useful information whatsoever about the extent of political biases in social psychology. Indeed, it is plausibly interpretable as evidence of political bias that a study (1) with this many flaws, and (2) which seems to vindicate the validity of research produced by (as we have demonstrated) overwhelmingly leftwing psychologists, was published in a prestigious psychology journal.

Bad hypothesis. The effort by Reinero et al. was misguided from the start. First, some full disclosure: One of us (Jussim) was invited by Crawford to collaborate on an earlier version of this project and turned down that opportunity for precisely the reasons identified here; that is, the study was *foreseeably* doomed to provide a terribly weak test of a badly-derived hypothesis from its conception.

The hypothesis can be characterized as badly-derived because: (1). There is an extensive literature on political biases in the social sciences (e.g., Crawford & Jussim, 2018; Duarte et al., 2015; Honeycutt & Freberg, 2017; Honeycutt & Jussim, 2020; Inbar & Lammers, 2012; Kaufmann, 2021; Martin, 2016; Redding, 2001, 2013; Tetlock, 1994; Zigerell, 2019); (2). That literature includes many hypotheses about manifestations of such biases (more on this later, but it includes biases in methods,

interpretations, citation, and canonization); (3). A simplistic claim that left leaning studies are less replicable is nowhere to be found in that literature. Point 3 may help explain why Reinero et al. resorted to citing a political pundit (Brooks, 2015) rather than any of the scholarly literature when generating it. They also cited two Dutch essays that "propelled the Dutch government to...study whether political bias affects research outcomes" (Reinero et al., 2020, p. 1311). Interestingly and amusingly, the two Dutch essays (Brugh, 2017a,b) also did not mention replication.

Bad hypothesis, badly tested. Reinero et al.'s findings were based on a sample of studies too small to detect any left/right differences in replication even if they exist. Their total pool was 194 studies. However, no one ever predicted political bias on apolitical studies, so all apolitical studies in their sample are irrelevant to the *political bias* hypothesis, except possibly as a neutral comparison. The critical ingredients for an appropriate test of their political bias hypothesis is a substantial sample of highly politicized articles. This proposition has two components, both critical. The first is "substantial sample." It is now well-established that studies based on small samples are notoriously unreliable (Fraley & Vazire, 2014). The second relates to how the concept of "strongly politicized" is operationalized. Even if the hypothesis that left-affirming studies are less replicable were true, such an effect would be weak-to-nonexistent for weakly political studies. The effect (if it exists at all) should most likely appear clearly for strongly politicized studies. Their research lacked both of these ingredients. This is, however, quite difficult for the superficial reader of their article to discern from their report because of both spin and obfuscation.

Spin and obfuscation of weaknesses. Their report includes some examples of what is plausibly considered "spin" that may serve to make the work appear stronger than it really was. It also included a distinct lack of clarity regarding some central aspects of the results critical for evaluating the strength of the evidence. "Obfuscation" refers to the act of making something unclear, and we make no claims about whether this was intentional.

Any reader of Reinero et al. (2020) can readily determine that each of the following statements are true about their report:

- The abstract refers to the 194 replications and the total number of human participants in these studies (over a million). It does not report the much smaller number of studies with political lean (24, at most), which is the sample size most critical with respect to testing whether replicability of studies relates to political lean.
- Nowhere in either the main report or supplementary materials is there a simple statement of the precise number of left or right-leaning studies.
- They framed their report as "two" studies, even though they tested the "differences in replicability" hypothesis on a single sample of 194 studies. They "justified" this because they performed the test for replicability differences twice, once with each of two different groups of participants coding for political lean. However, having different coders judge a single sample of studies is a robustness check; it is not two different studies. *It's the same set of 194 studies in both of Reinero et al's (2020) "studies."*

De Vries et al. (2018) referred to "spin" as occurring when researchers emphasize supportive secondary results and downplay unsupportive primary results; it is plausible to also consider "spin" as researchers' emphasizing features of their samples, design, or analyses that sound more impressive than what was actually relevant to test their hypotheses. Spin and obfuscation occur in additional ways throughout the paper as described next.

The tiny sample of studies with political lean. The number of studies with liberal/conservative political lean is also obfuscated. Reinero et al. (2020) never *state* how many studies were determined to have "political lean" by their two sets of coders (for some of their provided examples of articles with political lean, see below). Nonetheless, it is possible to *infer* those numbers from the results they did provide (although in the absence of a clear report, it is impossible to be certain that such inferences are correct). In footnote 13, they state "... the distribution of political-slant ratings from Study 2 includes all abstracts in the database (N = 194), whereas Study 1's distribution stems from the subset of articles deemed politically relevant on the basis of the doctoral coder's ratings (n = 101)." Because the *number* of studies with political lean is reported nowhere, it must be *imputed*. We did this by multiplying the

percentages they did report by 101 and 194 for "Studies" 1 and 2, respectively.

In "Study 1," they reported 20% and 4% respectively, for left- and right-leaning studies (p. 1315). In "Study" 2, they reported 3% and 2%, respectively, for left- and right-leaning studies (out of a total of 194; p. 1315). This produces the following table (Table 1) of sample sizes for their "studies." If, as per their footnote 13, "Study 1" had 101 studies, and "Study 2" had 194 studies, those percentages produce the following frequencies:

Table 1

	"Study 1" (based on 101 studies)	"Study 2" (based on 194 studies)
Number of Left-leaning studies	20%=20	3%=6
Number of Right-leaning studies	4%=4	2%=4

Number of left- and right-leaning studies in Reinero et al's (2020) "two studies." Study number is in quotes, because they had two samples of raters code the studies, but it was the same set of studies tested for replicability differences in both of Reinero et al.'s (2020) "studies," which we consider a robustness check rather than two different studies. Reinero et al reported the percentage but not the actual number of studies with political lean. This table translates their percentages to number of studies.

The numbers of studies with political lean are so trivially small that they are incapable of providing a strong and clear test of the "replicability differences" hypothesis. Consistent with the pattern of spin and obfuscation, the abstract did not mention the trivial number of left and right leaning studies.

Political amateurs? Political scientists, political journalists, and political party officials are experts on politics; graduate students and Mechanical Turk workers are generally not. Although Reinero et al. (2020) provided some training to the graduate student coders in their "first study," whether such political amateurs had the expertise to make these judgments with any validity was not tested and therefore remains unknown. In the absence of either using such experts or providing validity evidence for the coders they did use, confidence in the validity of the coding is limited.

Weak criteria for "politically relevant" and "political lean"? One can also see how badly this study failed to capture politicized research simply by looking at articles characterized as either liberal or

conservative, which was provided in their supplementary materials. Here are the first titles listed for each category (available in their supplementary materials):

- Liberal: "Reading literary fiction improves theories of mind."
- Conservative: "Influence of popular erotica on judgments of strangers and mates."

Although reading fiction does not strike us as a particularly hot issue on the left, many of the other studies listed in the supplementary materials coded as liberal-leaning seemed quite reasonable, and included phenomena such as stereotype-threat and the psychological justifications for inequality. Studies highlighted in their supplementary materials as "conservative-leaning," however, were less obviously appropriate. Many were about sex or romantic relationships. These topics pale in politicization compared to many modern culture war issues, such as the alleged prevalence of white supremacy, racial and gender inequality and discrimination, transgender issues, immigration, abortion, colonialism, and climate change. No one we know of who has ever addressed political biases (see references) has argued that they would manifest on tepid topics such as romantic relationships.

Reinero et al. (2020): Conclusions. Reinero et al. (2020) failed to find evidence for a manifestation of political bias which had never been proposed in the extensive scholarship on political bias. Whether it should be taken at face value is deeply unclear. The study suffers from many limitations and flaws, and it constitutes an exceedingly weak basis of support for the simplistic hypothesis that there would be replicability differences between left-leaning and right-leaning studies (we test a considerably more sophisticated hypothesis about political bias and replicability later in this chapter).

We believe its main conclusion – no replicability differences between left- or right-leaning studies – is probably valid, but not because of the evidence Reinero et al. (2020) provided. Our view is that there is so much noise in the peer review process, and room for so many other biases (biases favoring statistical significance, biases favoring dramatic findings, prestige and fame biases, hot topic bias, et cetera), that the likelihood of political biases influencing replicability is very small. We believed it before they conducted their study, and we still believe it. This is why previous reviews of political bias dating back 30 years have not predicted that there would be such differences (e.g., Crawford & Jussim, 2018;

Duarte et al., 2015; Honeycutt & Jussim, 2020; Redding, 2001, 2013; Tetlock, 1994).

Models of Political Bias Manifestation

In this section, we present two complementary models of political biases in academia, and review the extent to which evidence supports each model. The first, The Pipeline Model, is a model of the processes by which the social sciences in particular, and possibly academia more generally, are self-radicalizing. The second model, The Wheel Model, focuses specifically on identifying how political biases manifest in ways that undercut the validity and credibility of social science.

The Pipeline Model

Figure 1 presents the Activist to Academia to Activism Pipeline Model of Academic Self-Radicalization. According to this model, radicals, activists, and extremists select into academia and then create a hostile work environment for those they see as their opponents. They then make it more difficult for opponents to be hired, promoted, publish and fund their work, denounce and ostracize their opponents, including alleged misbehavior in their jobs, and effectively capture "peer reviewed science." Of course, they simply *implement this strategy without announcing it,* so it is only apparent from their behavior. There is no central authority engaging in conspiracies or command and control; this behavior is *socially distributed*; it emerges when the grassroot members of various fields share an ideological commitment to certain values and accept denunciation, demonization, and ostracism of those they view as their opponents as an appropriate, even necessary, way to conduct their professional behaviors within their fields.

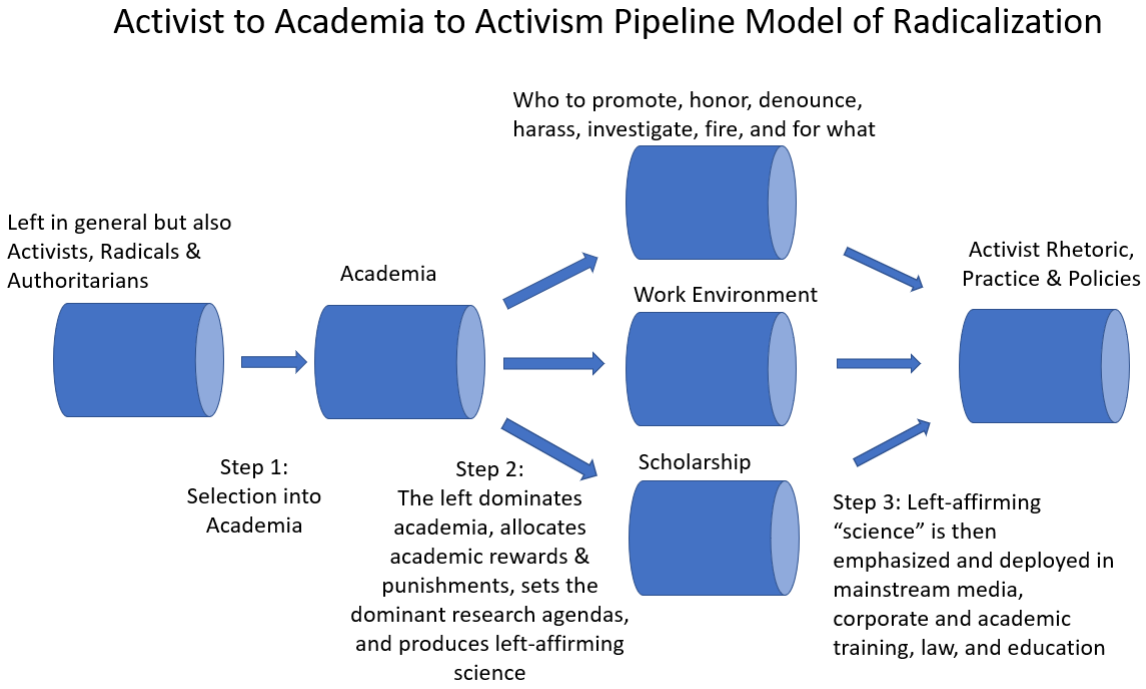

Figure 1: The Pipeline Model of Academic Self-Radicalization

In the final stage of The Pipeline Model, after purging rather than refuting those with different views, and after publishing study after study vindicating their political worldview (no matter how methodologically unsound, misinterpreted, or misrepresented), they can then rhetorically claim that their worldview is justified by the "peer reviewed science." We next elaborate on each step, and distinguish between *proposed or hypothesized* phenomena versus phenomena for which there is ample evidence.

Step 1: A Political Purity Spiral

We have already established the presence of massive left *overrepresentation* in academia. It takes minimal numeracy to recognize that this will almost inevitably lead to massive overrepresentation of *far left activists and extremists.* Most surveys indicate that 4-15% of Americans are on the far left (Hawkins, Yudkin, Juan-Torres & Dixon, 2018; Pew, 2014; Twenge et al., 2016). So, heuristically, one might guess that the far left is overrepresented in academia because, with so few academics right of center, one could approximately double the estimates of the far left found in representative surveys of the general American public to arrive at an initial plausible estimate of the proportion of the far left to be found in academia.

However, several factors probably exacerbate far left overrepresentation in academia beyond that initial estimate. First, once academia developed a reputation for being a bastion of leftwing views, it likely became more attractive to left extremists. Second, once a critical mass of extremists is reached in some university or department, they can easily hire comrades-in-arms and/or insure against hiring opponents. Although we are aware of no evidence documenting this process in full detail, research showing substantial minorities of academics *endorse* discriminating against their political opponents (e.g., Honeycutt & Freberg, 2017; Inbar & Lammers, 2012) is consistent with such a process. Both factors (academia's reputation as a bastion of left activism; political discrimination) could lead to a political purity spiral of ever greater overrepresentation of extremists and activists, at least until either saturation is reached or some new process intervenes to disrupt the spiral.

If something like these processes occur, then, in the social sciences, there would be far more than double the number left extremists as are in the general population. HERI data (Stolzenberg et al., 2019) showing about 13% of faculty identify as far left is consistent with the "mere doubling of the far left representation in academia" hypothesis (though 13% is for faculty overall–we are unable to ascertain percentages specific to the social sciences from their report). However, consistent with this "purity spiral" hypothesis, rather than the 8-30% representation of far left extremists one would obtain by simply doubling national survey estimates, about 40% of the faculty in the social sciences and humanities self-describe as radicals, activists, or other types of extremists (such as Marxists) (Gross & Simmons, 2014; Kaufmann, 2021). This strongly supports Step 1 of the Pipeline Model for the social sciences and humanities.

Step 2: Rewards, Punishment, Work Environment, Scholarship

Step 2 describes three related but separable phenomena: rewards and punishments (promotions, denunciations, et cetera); work environment; and scholarship. We next review the evidence bearing on each in turn, as Steps 2a, 2b, and 2c.

Step 2a: Who to reward and punish. Has the extreme left skew of the social sciences, complete with large cadres of radicals and extremists, influenced who the field promotes, honors, denounces,

harasses, investigates, and/or fires? We have no data on promotions or honors. Indeed, there are so few non-left faculty remaining in the social sciences that the probability of non-left faculty receiving major honors or awards is, by the scant base-rate alone, likely to be very low.

There is, however, evidence that bears on the general proposition that faculty rewards are channeled disproportionately to those on the left. Specifically, even after controlling for achievement (primarily publications), the more faculty held left attitudes towards social issues, the more likely they were to be found in positions at higher status institutions (Rothman & Lichter, 2009). Disproportionate rewards go to left faculty.

What about punishments? The Foundation for Individual Rights in Education (FIRE) recently (2021) published a Scholars Under Fire database (as of this writing, database last updated March 2022). A scholar is deemed to be "under fire" when there is "...a campus controversy involving efforts to investigate, penalize or otherwise *professionally* sanction a scholar for engaging in constitutionally protected forms of speech." In 2021 alone (counts obtained on 3/23/22), FIRE tracked 80 such targeting incidents from the left (transgressions included things like criticizing Martin Luther King or exposing a University Senate resolution condemning criticisms of critical race theory) and 34 from the right (transgressions included criticizing Trump or Republicans and publicly protesting a professor's use of a misogynistic slur).

However, the Figure 1 Pipeline Model refers to professors within academia; it is not a model of radicalization generally. Therefore, what is most relevant to Step 2a (who to punish) is not the overall number of attempts to sanction professors; most relevant are targeting incidents from within, not outside of, academia. Those numbers present a more stark contrast. When restricted to incidents initiated by scholars and graduate students, there were 42 from the left and 7 from the right. This pattern holds for every year included in FIRE's database, which goes back to 2015. For example, in 2015, eight faculty were targeted for sanctions by their left colleagues or graduate students, and zero were targeted by academics from the right.

Step 2b: Hostile work environment and leftwing authoritarianism. The numbers in FIRE's

database are quite small in the grand scheme of a country with many thousands of social science professors. However, we know for a fact that FIRE's database *underestimates* the numbers of faculty who have targeted by leftwing mobs, because we know of events that do not rise to FIRE's attention or meet their selection criteria, some of which can be found in Stevens et al. (2018) and others can be found in Shields and Dunn (2016). It is a near-certainty that far more incidents of shunning and ostracism, not to mention reputation-smearing whisper campaigns, interviews-never-received, promotions never granted, and jobs never offered, and the like have occurred than will ever be recorded. Consistent with the idea that FIRE's database is just the tip of a very large iceberg, many faculty surveys find large percentages of non-left faculty reporting that they experience a hostile climate (Honeycutt & Freberg, 2017; Inbar & Lammers, 2012; Kaufmann, 2021). Furthermore, it probably does not take many such incidents for people to get the message and self-censor. Put differently, non-left faculty generally know to keep quiet (Shields & Dunn, 2016) and these sorts of incidents help explain why.

Indeed, many academics openly declare blatant hostility to conservatives. Depending on what one uses as the measure, and depending on the precise cutoff, 20-80% of academics across several surveys *explicitly state* that they would discriminate against conservative viewpoints and individuals (Honeycutt & Freberg, 2017; Inbar & Lammers, 2012; Peters et al., 2020). This also likely underestimates actual discrimination against conservatives because self-report is biased by social desirability. One recent survey found that, in the social sciences and humanities, about a quarter of American professors and nearly half of all graduate students support not merely discriminating against, but *ousting* faculty members who express one or more of certain conventionally conservative viewpoints (Kaufmann, 2021).

Thus, when conservative professors express the belief that leaking their politics risks harming their careers (Shields & Dunn, 2016) they are likely correct. To cope with this fear of professional consequences, most stay "in the closet and under the radar" -- i.e., they avoid revealing their politics either directly or even through their scholarship, which they studiously keep as apolitical as possible (Shields & Dunn, 2016). This of course *biases* the existing literature towards topics favored by liberals (who can be relatively uninhibited in studying political topics and against topics favored by conservatives

(who fear being exposed and punished).

The recent blossoming of work on leftwing authoritarianism ("LWA"; Conway et al., 2018; Costello et al., 2021; Costello, this volume) might be useful for understanding the self-radicalizing nature of the social sciences. This work demonstrates that LWA has three key psychological characteristics: intolerance, censorship, and aggression, all directed at one's political opponents. Leftwing authoritarian aggression can manifest as social vigilantism (attempts to impose one's moral views on others; Costello et al., 2021), a phenomena that may help explain the rise of academic outrage mobs seeking to retract papers and ostracize professors for wrongthink (Stevens, Jussim, & Honeycutt, 2020).

We propose the hypothesis that LWA is common in the academy on several grounds. First, even though high scores on LWA are relatively rare in the general population, the social sciences have massive left overrepresentation. It is likely, therefore, that there are far more people in academia high in LWA than in the general population. Furthermore, behavioral manifestations of LWA -- such as aggression, censorship, and punishment -- are readily apparent in the rise of retraction-by-academic-outrage-mob and a range of academic attempts to get people fired or punished for violating left sacred values (Stevens, Jussim & Honeycutt, 2020). To better understand the radicalization of academia, it would be useful for future faculty surveys to include a measure of leftwing authoritarianism.

Step 2c, Scholarship: Academia is a conformity-rewarding social-reputational system. Success in academia hinges on approval from others (Honeycutt & Jussim, 2022). Admission to graduate school? A glowing letter from a famous person in the field is priceless; a damning letter from such a person is the kiss of professional death. Letters from famous scientists are even more important for obtaining an academic job, tenure, and promotions. The currency of success in psychology is peer reviewed journal articles and grants, and peer review constitutes others' evaluations of one's work. If others prefer left-enhancing findings, then vast literatures on politicized topics may be biased in a leftward direction in the following ways: (1). More may be conducted; (2). It may be more likely to be funded by grant panels composed of academics; (3). It may be more likely to be published in more prestigious, higher impact journals; and (4). It may be more likely to be cited and canonized (i.e., widely accepted as true; and to

note, “leftward studies are less likely to be replicated” is not one of our proposed manifestations).

Our general perspective here suggests that most research-active psychologists realize that they will face headwinds if they challenge leftist orthodoxies, and that they can reap the benefits of tailwinds if they promote those orthodoxies. Testing this empirically would be a natural direction for future research on political bias. Nonetheless, it often only takes a minority of negative reviews to block a grant from being funded or paper submitted to a prestigious journal from being accepted. Therefore, we hypothesize that many academics may well decide, “truth is not worth the risk of damage to my career” and “there are plenty of things to study that do not involve tacking *against* political headwinds.” This dynamic means that many probably abandon areas that risk alienating one’s colleagues, and seek out areas that one’s colleagues are likely to approve and support. Although no research has directly tested for this process, surveys and interviews show that conservative professors in particular, but also, more generally, professors whose work contests cherished left narratives in general (regardless of their personal politics) often learn to keep quiet out of fear of being ostracized by their colleagues (Dunn & Shields, 2016; Kaufmann, 2021).

A field dominated by activists and authoritarians on the left is likely to have impaired and corrupted quality control mechanisms for research on politicized topics. Rather than a field in which researchers’ opposing biases operate as effective checks on one another’s unjustified politicized claims, we have fields filled with political cheerleaders (“peer reviewers”) in which *there are few opposing political biases.* We have a political echo chamber, in which rewards, both social approval and tangible (publications, talk invitations, grants, and jobs) will likely go disproportionately to those whose scholarship on politicized topics affirms the left’s shibboleths. However, we hold off on reviewing the evidence that bears on these hypotheses here, because the role of political biases in disrupting and distorting scholarship is the primary focus of the second model we present later in this chapter (The Wheel Model).

Step 3: Activist Rhetoric and Policies

The final stage of The Pipeline involves the feeding of this left-distorted “scientific” literature

into applications, practice, policies and rhetoric. The logic for doing so is simple, straightforward, and deeply flawed. It is simple because it seems so eminently reasonable, even obvious, to base policies and practices on the "scientific" literature. And in the natural sciences, this works. If a new vaccine reduces infections, and does not have many serious unintended negative side effects, proposing to make it widely available to the public makes tons of sense. Advocacy built on a foundation of "sound science" often can make sense.

But the concept of "sound science" is doing a lot of work there. The entire point of our analysis of the social science on politicized topics is that, often, the science may *appear* far more sound than it really is. There may be scores, even hundreds of articles, seeming to support some claim (as is the case, e.g., with the notion of "implicit bias"). Those articles have experiments, sophisticated methods, and "statistically significant" results, so they are readily interpreted as justifying social interventions that take their conclusions as valid.

Unfortunately, the erosion of Mertonian Norms described herein means that, even when there are hundreds of studies reaching some conclusion, those conclusions may not be justified. Again, implicit bias is a perfect illustration of this problem (additionally, see later section on canonization for additional discussion of implicit bias). Implicit bias trainings, ostensibly built on the "science," can now be found everywhere, and politicians up to and including recent Presidential and Vice Presidential candidates have referred to it as if it is an established fact. Nonetheless, everything about implicit bias remains scientifically contested (e.g., Blanton & Jaccard, 2008; 2020; Jussim, Careem, Goldberg, Honeycutt & Stevens, in press; Schimmack, 2021). One review recently referred to it as "delusive" (Corneille & Hutter, 2020), and there is currently almost no evidence that implicit bias trainings do much to reduce bias or discrimination (Forscher et al., 2019).

The problems associated with activists and social scientists bringing "science" into the real world in Stage 3, however, goes well beyond the limitations to work with implicit bias (though it is an exquisite example). Left activist science has produced cottage industries of either similarly weakly vetted claims or widespread acceptance of a broad range of equalitarian perspectives (equalitarianism refers to

perspectives denying the reality of group differences, except for those produced by discrimination) despite the presence of trenchant criticisms that are simply ignored rather than refuted. For unrefuted criticisms of stereotype inaccuracy, see Jussim et al. (2009, 2016). For unrefuted criticisms of the idea that gender differences are generally small, see Del Giudice, Booth and Irwing (2012). For failed replications of stereotype threat that have been largely ignored, see Finnegan and Corker (2016) and Flore, Mulder and Wicherts (2019).

This is the stuff of equalitarian mythmaking. But once the myths are made, activists designing interventions can and do point to the "peer reviewed science!" as justification for requiring armies of employees to waste their time in anti-racism, implicit bias, and diversity trainings that have never been demonstrated to accomplish much (al-Gharbi, 2020). They are probably colossal wastes of time and resources for everything except possibly indoctrination into a very particular and peculiar form of "social justice." We write "peculiar" because the idea that useless trainings advance social justice (however one defines it) seems strange indeed and has never been empirically demonstrated.

The Wheel Model

Honeycutt and Jussim (2020) presented a preliminary theoretical model, The Wheel Model (Figure 2) for specific ways in which political bias manifests in the scientific enterprise to undercut validity and credibility. Here we review the evidence that bears on several of the manifestations of political bias The Wheel Model proposes (which appear as the spokes in Figure 2). Inasmuch as our prior paper (Honeycutt & Jussim, 2020) reviewed the evidence on discrimination and role models, we do not repeat that here. Instead, we focus in on articulating how political bias can manifest in and impact the questions researchers ask, measurement of topics studied, the interpretation and evaluation of research findings, what gets cited (or ignored), suppression of ideas and findings, and the canonization of research findings.

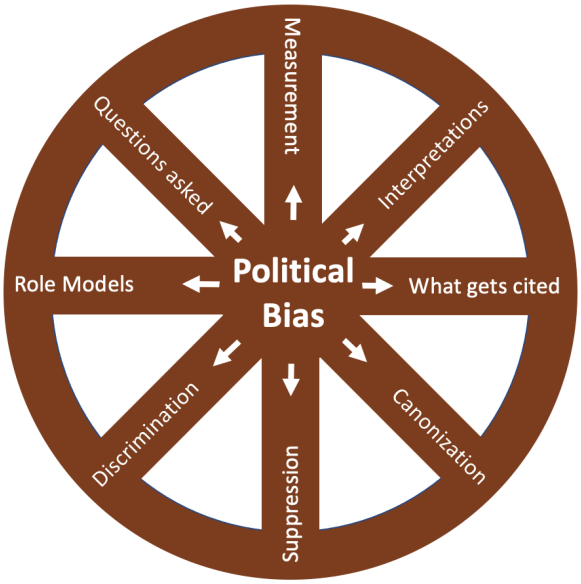

Figure 2. The Wheel Model; preliminary theoretical model for manifestations of political bias

Questions Asked

Political bias can impact the day-to-day conducting and operation of basic scientific research. Specifically, it can manifest in the questions academics/researchers ask (or desire to study/investigate; Jussim, Stevens, & Honeycutt, 2018). Certain research questions have become anathema and their investigation discouraged or avoided, while others that are consistent with and conform to leftist ideas and positions are subjected to no such repression (Redding, 2001). It has even been suggested by some prominent scholars (e.g., Jost, 2011) that it's anti-scientific to consider the possibility that ideological bias is an issue.

Thus, simply asking the questions "is political bias an issue?" and "how, if at all, does political bias manifest in the academy?", may, in a deliciously ironic twist, lead directly to the manifestations of political bias that are under investigation to begin with. We go even further here: We hypothesize that political biases are more likely to be found in the work of scientists most who most aggressively deny them than among those who readily acknowledge the possibility of politics undercutting the validity of their work. Future work could test this hypothesis by identifying scholars who deny versus acknowledge the potential problem and then comparing the extent to which the types of biases proposed by The Wheel

Model appear in their work.

For example, for decades there was premature scientific foreclosure on the conclusion that conservatives were asymmetrically more prejudiced and/or biased than liberals (e.g., Jost et al., 2003). Asymmetry findings were purported to be robust, and were quite flattering to liberals. Thus, there was little need (or incentive) for the conclusions to be challenged, or for researchers to ask whether there were conditions under which symmetry for political prejudice and bias might emerge–let alone under what conditions liberals might actually be more prejudiced or biased. Yet when some of these questions finally were asked, researchers found that the various consensuses were erroneous, as prejudice and bias were generally quite symmetrical (e.g., Brandt et al., 2014; Ditto et al., 2019).

Premature scientific foreclosure, and avoiding asking certain questions, also crippled the study of authoritarianism for nearly 70 years. A focus on rightwing authoritarianism quickly emerged from early investigations of authoritarianism (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950), a foothold which was solidified by RWA measures created by Altemeyer (e.g., Altemeyer, 1981). Essentially non-existent till 2018 was a systematic effort within social psychology to study leftwing authoritarianism ("LWA"). LWA was dismissed as a "myth" (Stone, 1980) and analogized to the "Loch Ness monster" (Altemeyer, 1996), in part because, as described by Altemeyer (1996, p. 229) "if there ever were any [authoritarians on the left], most of them have dried up and blown away and 'nobody makes them anymore.'" Therefore, asking whether LWA existed, and seeking to find leftwing authoritarians, was considered a waste of time and resources. Altemeyer (1996) did attempt to investigate LWA, but he concluded that while rightwing authoritarians were plentiful, "if you want a living, breathing, scientifically certifiable authoritarian on the left, I have found not a single one" (p. 229-230). Regardless, the recent blossoming of work on LWA (see Conway et al., 2018; Costello et al., 2021) has shown that authoritarianism is about as common on the left as on the right, and manifests as dogmatism, support for censorship, and aggression against one's ideological opponents.

Measurement

Measurement provides further opportunity for political bias to manifest (Reyna, 2018; Lilienfeld,

2015). Political assumptions, if researchers aren't careful, can be imported to the topics studied. It should be no surprise that left-leaning academics may use "science" to ascribe psychological defects to political opponents, working from the assumption that left-leaning views are correct, ethical, fair, open-minded, and scientific. And what better way to "scientifically" do so than to pre-load tests and measures with constructs or items that privilege "ideologically correct" views?

The saga of Stanovich's discovery and acknowledgement of his own political measurement bias. Not all political bias in measurement is intentional. And, to the benefit of science and truth-seeking, self-correction sometimes does occur. In one such instance, two decades after introducing a well-respected and highly cited measure for actively open-minded thinking ("AOT"), the researchers discovered they had inadvertently introduced bias against religious, and to a lesser degree socially conservative, individuals in their scale (Stanovich & Toplak, 2019). At the heart of the discovery was the realization by some of the researchers that they had been interpreting "beliefs" to be secular, empirically verifiable understandings of the world, while religious participants were interpreting the same items to reflect spiritual understandings that aren't easily altered by evidence. This was, by the admission of Stanovich, likely a byproduct of the previous labs and research teams being overwhelmingly secular. Thus, when asked, for example, if "certain beliefs are just too important to abandon no matter how good a case can be made against them (reverse scored)," religious participants were unfairly penalized on the AOT dimension of belief revision.

Stanovich and colleagues came to recognize that political assumptions and inaccurate interpretations were skewing the AOT scale because "[their] own political/worldview conceptions leaked into these items in subtle ways" (p. 163). Upon discovery of how political bias had impacted both measurement and interpretation, the AOT was revised. This dramatically reduced ostensible left/right differences in AOT. As such, it constitutes a rare example of scientific correction operating as it should.

Conservatism is often built into measures of moral failures. Scientific correction isn't always the norm when the impact of political bias on measurement is uncovered. There is an entire family of measures, currently called "racial resentment" scales, which include or are built upon older measures such

as Modern Racism and Symbolic Racism (e.g., Henry & Sears, 2002; McConahay, 1986). Such measures are frequently used and interpreted as evidence of prejudice. At the same time, these measures have been criticized since their inception for confounding politics with prejudice (see Cramer, 2020, for a thorough and balanced review of this controversy). Of course, no one refers to these measures as "measures of moral depravity," but if being a racist is deplorable, then attributing racism to people is to demonize them as depraved. Of course, if the conclusion is clearly based on sound science, then it stands no matter who it demonizes. The problem with racial resentment scales is that their status as measures of prejudice is scientifically controversial rather than definitively established.

Racial resentment scales often include questions worded like this: "many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors" and "Over the last few years, blacks have gotten less than they deserve." One problem with such scales is that the measures plausibly confound beliefs about what constitutes justified government safety nets with racism. A person who hates Black people will oppose government spending on Black people. A person who hates government programs will also oppose government spending on almost anyone, including Black people. Such questions cannot distinguish between ideology and racism.

Conservatives routinely score higher on these measures than do liberals. Furthermore, "racial resentment" often strongly correlates with racial policy preferences, such as affirmative action and government spending on programs to assist Black people (Carmines, Sniderman & Easter, 2011). However, they correlated *so highly* (often in the r=.80 range) that Carmines et al. (2011) concluded that "racial resentment" was, in essence, little more than a policy preference scale. Although racial policy preferences *may* be caused by racism, they may also be caused by many other beliefs and values. In short, racial resentment scales are not a clean measure of racism.

Similarly, a veritable cottage industry of studies was produced after the 2016 American Presidential election finding that racism (usually using some form of the racial resentment scales) predicted support for President Trump (e.g., Abramowitz & McCoy, 2019; Pettigrew, 2017). Into this mix stepped Carney and Enos (2017) who confirmed the relationship – people who scored higher on racial

resentment were more likely to support Trump. However, they also found that Trump supporters scored higher in resentment against all sorts of groups, including White groups such as Albanians and Lithuanians. As they put it, "Because resentment against other groups is actually higher on average than anti-Black resentment, these results suggest that modern racism questions are poorly suited for capturing attitudes specific to Blacks" (p. 20).

Other measures. We speculate that similar problems occur with many other scales that assess "isms" or "phobias" including sexism, authoritarianism, homophobia, transphobia, and other forms of prejudice. The psychometric problems of the implicit association test are legion (e.g., Blanton et al., 2015a,b), but the psychometric properties of other implicit measures are often even worse (van Dessel, Cummins & Kasran, 2020). Microaggression scales suffer from numerous unacknowledged problems, such as requiring mindreading (Cantu & Jussim, 2021; Lilienfeld, 2017). This may cause reasonable observers to ask how many other measures addressing politicized topics suffer similar limitations and weaknesses.

Interpretations and Evaluations

There are few standards in the social sciences dictating how to properly interpret empirical findings. There are, perhaps, vague norms indicating that interpretations should be grounded in the data. But this is so broad and general that it leaves great latitude for political bias to taint the process.

Bias in peer review. Abramowitz et al. (1975) had psychologists rate the appropriateness of either of two manuscripts for publication. Methods and analyses were identical for both papers. The result was experimentally varied such that it showed either that leftist political activists were mentally healthier or that they were less healthy than a comparison group of nonactivists. When the paper concluded that leftist activists were healthier, the more liberal reviewers rated the manuscript as more publishable, and the statistical analyses as more adequate, than when the otherwise identical manuscript reported that the activists were less mentally healthy. The less liberal reviewers showed no such bias. Abramowitz et al. did not identify any conservative reviewers. This study is now almost 50 years old and replication is long overdue to evaluate whether the pattern holds true today, and for different topics.

Bias in evaluations and explanations. Eitan et al. (2018) had 934 laypeople (Mechanical Turk workers) rate 306 conference abstracts that pre-testing had established address the characteristics of liberals and conservatives on two dimensions: Evaluations and explanations. Evaluations refer to whether liberals or conservatives were evaluated more positively in the article. Explanations referred to whether liberals or conservatives were the primary focus of what needed to be explained. They argued this constituted a form of political bias because groups viewed as deviating from the norm are often the focus of explanations. Result provided clear evidence of political biases. Conservatives were both evaluated more negatively and were more frequently the focus of explanations. However, these biases were not related to subsequent likelihood of the research being published in a peer reviewed journal.

Bias in acceptance of evolutionary psychology. Buss and von Hippel (2018) examined the relationship between social psychologists' ideology and belief in evolutionary psychology. Although they surveyed over 300 psychologists, this research is inherently hampered by the ideological homogeneity of the field. With almost everyone in psychology left of center (their survey found that 301 psychologists voted for Obama in 2012 and four voted for Romney), ideology suffers a severe restriction of range problem that will artificially limit its potential to correlate with other variables.

Nonetheless, the "leftness" of the ideology correlated about r=.20 with each of three questions about sex differences: whether sex-differentiated hormones play a major role in attitudes and behavior; whether well-known sex differences might be primarily genetic; and whether it might be more difficult for men than women to remain sexually faithful. We write "leftness" because the near-total lack of conservatives meant that ideology ranged from center to far left.

In addition to the correlation with ideology, Buss and von Hippel (2018) found that many social psychologists doubt each of the following: evolution influenced human attitudes and preferences; there are universal standards of physical attractiveness; sex differences in psychology are primarily genetic; and men evolved to have more difficulty being sexually faithful than have women. Even larger numbers reported viewing it as "bad" to report one or more of these conclusions.

Other evidence and limitations. It is often quite easy to uncover unjustified left-affirming

interpretations in the literature, such as claims that stereotypes are inaccurate, 90% of Americans are unconscious racists, sex differences on many variables are trivially small, and eliminating stereotype threat eliminates race differences in standardized test scores (for articles chronicling and debunking each of these claims, see Blanton et al., 2015; Del Guidice et al., 2012; Jussim et al., 2016; Sacket et al., 2004). However, showing that *one particular* study has been misinterpreted, or that *one particular claim is left-affirming but unjustified*, however strongly it shows bias, does not show a *pattern of biased interpretations across the discipline.* It is hypothetically possible that there are just as many unjustified right-affirming claims afflicting the discipline.

This is where the empirical studies of interpretations and evaluations are useful (Abramowitz et al., 1975; Buss & von Hippel, 20018; Eitan et al., 2018). Although all point in the same direction – of left biases dominating over right biases – all of the studies found modest, rather than large, effects. So can Team Left celebrate? Can they declare, "The hard scientific evidence is sparse and produces weak to modest effects! None of these studies have been replicated! One is decades old! Charges of political bias are wildly overstated!"

They can, and they probably will. But keep in mind that all of these studies produced *some as opposed to no* evidence of political bias, and that this is only one mechanism by which political biases can taint the field. The purity spiral that has led to the field being completely dominated by people on the left means that it is almost impossible to conduct a complete test of the extent to which interpretations and explanations are biased. Restriction of range is well-known to attenuate correlations, so if the observed correlation between politics and interpretations is, e.g., r=.20, this is likely to be far lower than it would be if the field was not almost exclusively people on the left.

This can be readily seen in an extreme hypothetical: Let's say a field is made up entirely of Marxists who interpret all results as "We must smash the bourgeoisie." This is 100% political bias. In this hypothetical, the correlation of researcher ideology and interpretation will be effectively zero. Although social psychology is, fortunately, not this extreme, the extreme attenuation of the range of political identities and values renders almost any observed estimate of political bias an underestimate.

Citations

Citation biases occur when studies producing a particular finding are cited at higher rates than are comparably methodologically sound (or unsound) studies producing opposing findings. Citation biases can occur for many reasons including but not restricted to political biases (theoretical biases, fame biases, significant result biases, etc.). In some situations, papers reporting unreplicable findings are cited at exponentially greater rates than the research that has failed to replicate the desired findings (Jussim et al., 2016). In a field in which political biases influence *that which is studied*, this has the effect of producing political citation biases. If more studies are produced with left-affirming findings (as found by Reinero et al., 2020), and if citation biases favor original studies, then, even when those studies are debunked as irreplicable, debunked left-affirming original studies will continue to be cited as if they are true. If false or dubious left-affirming work is routinely cited as true, the overall state of the literature will convey the impression the science affirms left beliefs more than it actually does. As shown in Table 2, this can be seen in research on stereotype bias, stereotype threat, the effects of stereotypes on behavior, interpersonal expectations, and gender bias.

For example, Finnigan and Corker (2016) attempted to replicate Spencer et al.'s (1999) classic study on stereotype threat. In a pre-registered, highly powered study, Finnigan and Corker failed to obtain a significant main effect for stereotype threat, or any interaction effect found in the original work. Finnigan and Corker's work was recognized by the Association for Research in Personality as the best paper in 2016, which by some counts may be the first time a failed replication paper was recognized with an award by a major psychological organization. Yet since 2017 (the year after their paper was published), their paper has only been cited 52 times, while Spencer at al. (1999) has been cited 1712 times over the same time period. This is a staggering disparity, particularly in light of how the main findings of Spencer et al. (1999) have essentially been disconfirmed.

Table 2. Papers finding opposite patterns, or failing to replicate the original findings

Publication	**Narrative**	**Key Aspect of Methods**	**Citations, Total**	**Citations, since year after failed replication**
Darley and Gross (1983)	Stereotypes lead to their own confirmation; stereotype bias in the presence but not absence of individuating information	People judge targets with vs. without relevant individuating information. Single experiment. N = 59–68, depending on analysis	1951	Since 1996, 1600
Baron, Albright, and Malloy (1995)	Failed replication of Darley and Gross (1983). Positive results in opposite direction: stereotype bias in the absence of individuating information; individuating information eliminated stereotype bias	Close replication (and extension) of Darley and Gross (1983). Two experiments. Total N = 161.	107	Since 1996, 103
Spencer et al. (1999)	Stereotype threat for women and math; apprehension of being judged by the negative stereotype leads to poorer math performance	Three experiments. Total N = 177.	4824	Since 2017, 1712
Finnigan and Corker (2016)	Failed replication of the stereotype threat effect in Chalabaev, Major, Sarrazin, and Curry (2012), modeled closely on Spencer et al. (1999). No	Pre-registered. Close replication of Chalabaev et al. (2012), and extension from Spencer et al. (1999). Single experiment. Total N =	55	Since 2017, 52

	significant main effect or interaction effect for threat or performance avoidance goals	590		
Bargh, Chen, and Burrows (1996)	Automatic effects of stereotypes on behavior	Two experiments. Total N = 60	5955	Since 2013, 3010
Doyen, Klein, Pichon, and Cleeremans (2012)	Failed replication of Bargh et al. (1996). No effects of stereotypes on behavior except when experimenters were not blind to condition	Two close replication and extension experiments. Total N = 170	763	Since 2013, 729
Snyder and Swann (1978)	People seek to confirm their interpersonal expectations	Four experiments. Total N = 198. People chose among confirmatory or disconfirmatory leading questions (no option was provided for asking diagnostic questions)	1512	Since 1984, 1410
Trope and Bassok (1983)	People rarely seek to confirm their interpersonal expectations. Instead, they seek diagnostic information	Three experiments. Conceptual replication. Total N = 342. People could seek information varying in the extent to which it was diagnostic vs. confirmatory	211	Since 1984, 206
Note: Citation counts collected November 22, 2021				

A similar pattern can be found in citations to articles showing or failing to show gender biases in

peer review of articles and grants (Honeycutt & Jussim, 2020). We first identified every paper we could find examining gender bias in peer review, then excluded papers that were either too recent (because there was not enough time for citations to mount) or whose findings were muddled (rather than clearly showing biases favoring men versus women versus no bias; details can be found in Jussim, 2019). The results are reported in Table 3.

Two patterns stand out. First, papers finding biases against women were cited at over five times the rate of papers showing no biases against women. Second, it shows that the sample sizes of the studies showing no biases against women were vastly higher than the sample sizes of the studies showing biases against women. Although sample size is not the only indicator of methodological quality, it is an extremely important one (Fraley & Vazire, 2014). Thus, despite having a major marker of lower scientific quality, the papers showing gender bias were cited far more frequently.

Table 3. Citations to Papers Based on Whether or not They Found Gender Bias Favoring Men

	Found Biases Favoring Men (Four Papers)	Found Unbiased Responding or Biases Favoring Women (Six Papers)
Total Citations	3982	890
Median Sample Size	182.5	2311.5
Citations per year	51.5	9.00

Note: Citation counts collected and calculated June 22, 2019

The problem in many situations isn't that failed replications or studies that contest left narratives don't get published. The problem is that these papers go largely ignored. We refer to this as the *fundamental publication error* (Jussim, 2017)—the mistaken belief that just because something has been published correcting past scientific errors, the scientific record has thus been corrected. If scientific work correcting past errors is not cited and is instead ignored, scientific correction has not taken place. Every time there are citation patterns like those described in Tables 2 and 3, 80-90% of the literature is saying

"X is true" when, at best, it is unclear whether X is true, and "X is false" is plausibly the more valid conclusion. This is the stuff of political mythmaking masquerading as science.

Suppression

The politics of social psychology can also influence what types of findings and ideas are suppressed. In scholarship and science there is a difference between suppression and rejection. Rejection occurs when an idea has been explored and has been found to be clearly unjustified. Suppression occurs when social norms and processes prevent ideas from being explored or communicated. Although nonscientific actors (government, activists, et cetera) may seek to suppress social science of which they disapprove, this chapter's focus is on the political biases *of* social psychologists and other social scientists, so we do not address *external* attempts at suppression further. There are two main types of scholarship suppression: suppression by others and self-suppression, and we discuss each next.

Suppression by others. The most direct route to scholarship suppression in academia is to attempt to punish people for their ideas (suppression by others). The most obvious modern manifestations of idea punishment include: firing, loss of position (e.g., a dean is removed though may remain on the faculty), de-platforming, and retraction of published papers for anything other than fraud, misconduct, or flagrant and frequent data errors. If successful, the ideas being promulgated will be suppressed (e.g., see Warne & Frisby, this volume). A retracted paper is no longer in the literature; a deplatformed speaker has lost a platform, a fired scholar may never return to academia or publishing. However, even if the attempt to punish fails, the work may still get suppressed because the scholar targeted has to spend time and effort defending against the attempt, and this is time not spent conducting or disseminating scholarship. Stevens et al. (2020) review a slew of real-world cases in which academic outrage mobs sought punishment of other academics, usually for violating left-activist equalitarian values.

Self-suppression. However, one of the most powerful effects of such punishment attempts is to inspire waves of self-suppression. Self-suppression occurs when people do not pursue certain ideas or avoid trying to publish certain findings because they fear punishment or prefer that the findings do not see the light of day (Noelle-Neumann, 1974). Self-suppression is notoriously difficult to empirically assess

because there is mostly an absence of evidence (if the idea is suppressed, it cannot usually be found).

Nonetheless, if our analysis is true at all, then findings that counter left or equalitarian narratives should be likely to be buried, hidden, or downplayed. Zigerell (2018) reports results consistent with this analysis. He first identified TESS studies of racial bias. TESS is a National Science Foundation-sponsored program that supports the conducting of experiments as part of nationally representative surveys. Zigerell (2018) identified 17 such studies, one finding that White respondents were biased against Black targets, the other 16 finding no bias, or biases favoring Black targets. Thirteen of these findings (1 showing anti-Black bias, 9 showing no bias, and 3 showing anti-White bias) were never published. Zigerell (2018) also found that, among these 17 studies, Black participants consistently favored Black targets, significantly so in 7 studies and in an overall meta-analysis, and only two of these findings were published.

Although we do not know whether the researchers producing these findings tried-but-could-not get them published, or simply did not try, the upshot is the same: the published scientific literature in social psychology will *overestimate* racist biases because a large number of high quality studies based on nationally representative samples *are simply not in the literature.* Although one might object that, because Zigerell (2018) is now published, it *is* in the literature, this argument fails for two reasons. First, it would not have been in the literature without Zigerell's forensic efforts. Second, Zigerell (2018) has been cited a grand total of 19 times (as of 3/23/22), meaning that, for most practical purposes, it has been ignored.

The Zigerell (2018) study also raises an important question: How many *more* studies out there fail to find evidence of racism that have gone unpublished? In fact, it raises an *even bigger question:* How many studies *failing* to find evidence supporting left-affirming narratives have gone unpublished? We may never know the answer to this question.

Canonization

Canonization is the process by which research findings and conclusions are incorporated into a field's accepted base of knowledge (Jussim, Krosnick, et al., 2019). We characterize work as canonized when claims and findings are incorporated into journals of record, Annual Review and Handbook

chapters, foundational textbooks, and similar outlets. Validity and robustness of findings would constitute grounds for concluding that a line of research is actually true, and thus should be canonized. Table 4 captures why canonization matters. The ideal and intended operation of the scientific process is for valid findings to be canonized and for invalid findings to be ignored.

Table 4. Why Canonization Matters

Published Research is:	**Ignored**	**Canonized**
Invalid	**IRRELEVANT**: No major harm, scientific process operating as intended	**REIGN OF ERROR**: Misunderstanding, misrepresentation, bad theory, ineffective and possibly counterproductive applications; time and resources wasted
Valid	**LOSS**: Understanding, theory, and practical application deprived of relevant knowledge	**IDEAL**: Understanding, theory, and practical application enhanced by relevant, robust, and extensively tested and validated knowledge

Adapted from Honeycutt & Jussim, 2020

The canonization of implicit bias. Many of the manifestations of political biases have come together to prematurely canonize the notion of implicit bias and, especially, its workhorse measure, the implicit association test (IAT). First, has the notion of "implicit bias" been canonized? If the standard is "thousands of articles employ the concept; it appears in outlets of record such as Annual Review chapters (multiple) and textbooks (multiple), and articles extolling its virtues and importance have been cited thousands of times" then the answer is a resounding "yes" (e.g., the study announcing the IAT, Greenwald, McGhee, & Schwartz, 1998 has been cited almost 15000 times according to Google Scholar). Triumphal reviews have declared that "the existence of implicit bias is beyond reasonable doubt" (title of Jost, Rudman, Blair, Carney, Dasgupta, Glaser & Hardin, 2009) and that we are in the midst of an "implicit revolution" (title of Greenwald & Banaji, 2017).

De Ridder (2021) recently published a philosophy of science article that helps understand: (1)-

Why implicit bias has been canonized, even though (2)- Almost everything about work on implicit bias justifies *not* treating any of the conclusions that have emerged from this area as actually true. De Ridder (2021) reviews a great deal of the meta-scientific work show many of the ways work in the social and biomedical sciences produces invalid claims, and he concludes with this (p. 16):

> Recent meta-research shows that at least large swathes of the biomedical and social sciences are, on average, not very reliable. Even influential papers in high-profile outlets frequently cannot be replicated. For any individual published article, chapter, or monograph, the odds are thus against its central claims being objectively well-justified and likely true. Moreover, science doesn't reliably clean up the publication record. Finally, fraud and lesser misconduct is neither quickly discovered nor heavily penalised. Even though various reform movements to improve scientific practice have been greeted with initial enthusiasm, their adoption and implementation in the biomedical and social sciences is slow and things are improving very gradually at best.

Thus de Ridder's (2021) answer to the question, "Is trust in scientists epistemically justified?" (interpreted to mean, "can we trust scientists to be reliable informants?") is an emphatic "no."

What, then, is going on (in science generally, though we focus in this section on implicit bias and the IAT)? De Ridder (2021) provides a second interpretation of the "Is trust in scientists epistemically justified?" question. If this question is interpreted to mean, "can we trust scientists to reach conclusions by the methods common in their disciplines?" then, yes, trust is "epistemically justified" in this sense. Certainly, work on implicit bias and the IAT fits this latter interpretation. It is filled with experiments, statistics, correlations, and theories. These are the workhorse methods and statistics in psychology and related disciplines.

However, our view is that this latter sense is trivial and no one should care very much whether scientists reach conclusions that are justified by methods common in their disciplines. If the accepted method among physiologists and psychologists in the 19th century for determining a person's strengths and weaknesses was assessing the size, shape, and bumps a person had on their skull (phrenology), this is helpful for understanding why these scholars would believe one another. But, such methods and understandings did not produce an accurate understanding of a person's psychological strengths and weaknesses. Similarly, if one examines the methods used by the foremost proponents of implicit bias and the IAT, whether one can believe the most common conclusions becomes dubious indeed.

Readers interested in doing a deep dive into the flaws, limitations, and unjustified conclusions reached on the basis of IAT research should consult the over 30 articles one of us (Jussim) posted in an open access repository at OSF (Jussim, Thulin, Fish, & Wright, 2021). A sense of those articles can be gleaned from some of the titles of articles that can be found there:

More Error than Attitude in Implicit Association Tests

Unconscious Racism: A Concept in Pursuit of a Measure

Implicit? What Do You Mean? A Comprehensive Review of the Delusive Implicitness Construct in Attitude Research

Sexy but Often Unreliable: The Impact of Unreliability on the Replicability of Experimental Findings with Implicit Measures

Unconscious Gender Bias in the Academy: Scarcity of Evidence

Invalid Claims about the Validity of Implicit Association Tests by Prisoners of the Implicit Social-Cognition Paradigm

Here, we briefly review and describe the substance of key limitations, flaws, and criticisms of the IAT and implicit bias. All of these points (as well as review of the relevant literatures) can be found in Jussim et al. (in press).

1. Claims based on the IAT were wildly oversold when it was first developed, including completely unjustified claims to the effect that 90% of Americans were supposedly found to be unconscious racists. Mitchell and Tetlock (2017) recount the history of how it was oversold immediately after publication of the first IAT article (Greenwald et al., 1998). Such claims are always inadvisable and imprudent, because the validity of any new method or measure cannot be established by any set of preliminary studies. Instead, doing so requires years, sometimes decades, of skeptical scrutiny by independent scientists before validity, if any, can be scientifically established with any certainty (Jussim, Stevens, Honeycutt, Anglin, & Fox 2019). Mitchell and Tetlock (2017) also review the historical record to show that there was a rush to influence policy and the law – a pattern

consistent with one of the main themes of the present chapter. Specifically, the entire history is consistent with the claim that ideological and activist agendas often insinuate themselves into scholarship claiming to be scientific in ways that undermine validity and then this dubious work is deployed by activists who can claim a veneer of scientific respectability to advance political goals. The next set of conceptual and methodological criticisms explain why such claims constituted leaping to an unjustified conclusion with respect to the IAT and implicit bias..

2. There is no widely accepted definition of implicit bias. A review of those definitions revealed that many papers that use the term do not even define it; implicitly (so to speak), they assume that implicit bias is whatever is being measured by their preferred measure, usually the IAT. Among those that do define it, the definitions are almost completely unconnected to one another. For example, it has been defined as behavior, cultural stereotypes, decision-making, and mental associations. These are entirely different constructs. Thus, it is impossible to even know whether researchers are discussing the same phenomenon when they use the same term, or even the same measure.
3. There is one variant that Greenwald (2017) claimed constituted a common working definition for most of the prior 20 years: “Introspectively unidentified (or inaccurately identified) effects of past experience that mediate discriminatory behavior.” Unfortunately, however, those who actually used this definition were soon to discover its assumptions were either logically or empirically unjustified. IAT scores are not “introspectively unidentified” (people are quite good at predicting what they will be). Furthermore, the IAT assesses neither behavior (in any meaningful sense beyond “reaction times to the IAT”) nor mediation (see Jussim et al., in press, for a review).
4. The IAT is a reaction time measure. To claim reaction times constitute any sort of bias is to import a conclusion by fiat rather than evidence.
5. At best, the IAT measures strength of association of concepts in memory, which is not

any type of bias or prejudice. And that is at best; a slew of statistical issues and methodological artifacts mean that the IAT is not even a clean measure of strength of association.

6. Critiques of the IAT have concluded that it contains more error than attitude (Chequer & Quinn, 2022), may capture cultural stereotypes (beliefs about what other people believe) as much as or more than own beliefs and attitudes (Arkes & Tetlock, 2004) or actual knowledge about actual group differences and conditions (Jussim et al., in press; Payne, Vuletich & Lundberg, 2017); and that IAT scores reflect four separate phenomena, of which attitude is just one (Conrey, Sherman, Gawronski, Hugeberg & Groom, 2005).
7. The IAT, as used and reported, has a potpourri of methodological and statistical oddities. These undercut simple interpretations of results using IATs (all of these are reviewed in Jussim et al., in press). Its test-retest reliability is usually low, about $r = .4$. Additionally, IAT scores are difference scores, which complexifies interpretation (relationships with other variables could result from relationships with only one of the variables involved in computing the difference, both, or their difference). As computed, the IAT is an effect size, yet, rather than simply reporting the mean IAT score as an effect size, its adherents often compute a Cohen's d from the IAT D-scores; this doubly-computed effect size usually functions to exaggerate IAT effects (Jussim et al., in press). Any IAT difference will converge on a very high IAT D-score of 2.0, when within-trial variance goes to zero, meaning that the entire computational scheme creates the impression of larger than actual attitudinal differences.
8. Although recent work comparing scores on different IATs has been interpreted as vindicating the "true zero" interpretation of zero (i.e., as no bias; Cvencek et al, 2020), the only research that has ever attempted to validate IAT scores against external standards has found that scores well above zero (typically ranging from IAT D-scores of .3 to .6, depending on the study) correspond to egalitarianism (Blanton et al., 2015b).

If IAT scores greater than zero correspond to egalitarianism, then almost every claim about the number of people who display "implicit" or "unconscious" racism based on the IAT is exaggerated. In addition, even if the zero really is the true point of egalitarianism, measurement and sampling errors should lead the egalitarianism point to fluctuate a great deal from sample to sample (Blanton et al., 2015, a,b).

9. Many of the studies that use IAT scores to predict behavior find little or no anti-Black discrimination.
10. Whether IAT scores predict behavioral manifestations of bias beyond self-report prejudice scales is unclear, with some studies finding they do and others finding they do not.
11. Claims that small bias effects are "socially important" (Greenwald, Banaji, & Nosek, 2015) have yet to provide any evidence demonstrating such social importance. Instead, they are based on the presumption that small effects accumulate, which is an empirical question and should not be a reified truth absent evidence. A similar claim was once made about small self-fulfilling prophecies being socially important if they accumulate, but once the evidence started rolling in, accumulation was rare. Instead, the already-small effects tended to dissipate (Jussim & Harber, 2005).
12. Procedures that change IAT scores have failed to produce changes in discriminatory behavior (Forscher et al., 2019).
13. There is currently no evidence that implicit bias trainings accomplish anything other than teaching people about the research on implicit bias. That is, there is no evidence that IAT trainings reduce prejudice or inequalities. In their thorough review of the literature on prejudice reduction, Paluck, Porat, Clark and Green (2021, p. 549) conclude: "Thus, a fair assessment of our data on implicit prejudice reduction is that the evidence is thin. Together with the lack of evidence for diversity training, these studies do not justify the enthusiasm with which implicit prejudice reduction trainings have been received in the

world over the past decade." We speculate, however, that, in addition to teaching about the research, they likely do have another effect: to create an organizational culture of conformity, groupthink, and self-censorship around progressive ideological views regarding prejudice, discrimination, and inequality.

14. A recent review of how the IAT is presented and taught to students in introductory psychology courses indicates that critiques and discussions of the limitations or weaknesses of the IAT are almost entirely ignored (Bartels & Schoenrade, 2021). Bartels and Schoenrade argue that this biased presentation of the IAT may lead to confusion and misunderstanding, both of the IAT as a test, and about one's (potential) personal implicit biases.

Other examples of unjustified canonization. Although this is not a comprehensive review of unjustified canonization, some other examples consistent with left narratives include stereotype inaccuracy (Jussim et al., 2009, 2016), stereotype threat (Finnegan & Corker, 2016), social priming (Doyen et al., 2012), the power of stereotype and expectancy biases (Jussim, 2012), the power of microaggressions (Lilienfeld, 2017), the supposedly trivial size of most gender differences (Del Guidice et al. 2012), the supposed nonexistence of leftwing authoritarianism in the democratic West (Conway et al., 2018; Costello et al., 2021) and the supposedly greater propensity of conservatives to engage in biased processing of social and political information (Ditto et al., 2018).

Empirical test of the role of political bias in premature canonization: Is there a disproportionately high number of replication failures of highly touted left-affirming studies? Clark & Winegard (2020) recently hypothesized that equalitarian-friendly findings would be overrepresented among psychology's replication failures of highly touted studies. Although their phrase was "highly touted," we view it as approximately synonymous with "canonized" – both usages refer to work that is widely celebrated and accepted as true. Note, however, that this is *not* a simple political bias/replicability hypothesis wherein left-affirming studies will be less replicable. They must also be "highly touted." Although they did not provide a definition of "most-touted," the case of "unreplicable" that they

discussed was stereotype threat, the initial reports of which have been cited thousands of times and which routinely appears in textbooks and diversity interventions.

Contra Reinero et al. (2020), Clark & Winegard (2020) did not propose a general replicability difference between left- and right-affirming studies. Indeed, their hypothesis (p. 12) is restricted to *failures to replicate:* "...many of the most touted effects that fail to replicate and/or that are found to be relatively small, perhaps even trivial, in systematic analyses will likely be equalitarian-friendly findings." We propose here that "highly touted" is the "special sauce" that, contrary to the approach taken by Reinero et al. (2020), can actually predict replicability. This may work in at least two very different ways (which are not mutually exclusive).

First, studies can become highly touted for good reasons, such as when strong methods produce insights into broad patterns of human behavior. However, they can also become highly touted for bad reasons – such as when they seem to vindicate deeply held political beliefs and attitudes. The opportunity to use such findings for rhetorical or activist purposes may short-circuit the type of critical thinking necessary to first skeptically vet such studies to be sure their findings are actually credible. Note that this is *not* a general "replicability differences" between left- and right-affirming studies. It only predicts a replicability difference for the small subset of studies that hit a sufficiently strong political nerve to become highly touted.

Second, there may be bias in *what gets touted.* If psychologists place special value on results that they can rhetorically exploit for political purposes, they may be more likely to tout left-affirming studies *in general* (both strong and weak). Therefore, left-affirming studies on hot button issues may be overrepresented among all highly touted studies. If they are overrepresented among highly touted studies *in general,* they are likely to be overrepresented among failed replications of highly touted studies. Here, the bias is not located in a failure of researchers to be sufficiently critical of left-affirming studies; it is, instead, located in their tendency to favor ("tout") left-affirming studies.

To test the Clark & Winegard (2020) hypothesis, one needs to: (1). Identify a population of highly-touted studies that failed to replicate; and (2). Evaluate how many support or oppose

equalitarianism. We do so here, though we also test whether this pattern holds for *any* left-affirming highly touted study, and not just equalitarian ones.

For this analysis, we focused exclusively on failed replications that helped trigger the Replication Crisis (published 2012-2016, see Table 5). We operationalized "highly touted" as papers having at least 1000 citations. We identified 18 such papers that were subject to replication failures in this time period. Six were equalitarian and one was on the liberal hot button issue of environmentalism. One (Vohs et al., 2006) was borderline. Vohs et al. (2006) was not equalitarian and was not quite on a liberal hot button issue. It is, however, plausibly viewed as anti-capitalist and anti-individualism – both of which *have* been condemned by social justice activists as "harmful" and even "malignant" contributors to racism and inequality (American Medical Association, 2021; Kendi, 2019).

Similarly, we were not sure what to do with the Hagger et al. (2016) failure to replicate on ego-depletion. The original ego-depletion study (Baumeister, Bratslavsky, Muraven & Tice, 1998) has been cited over 6600 times. However, Hagger et al. (2016) was a direct replication of Spirada, Kessler & Jonides (2014), which did address ego-depletion, but used a somewhat different method (computer based rather than in person).

Therefore, we had three separate choice-points: (1). Do we include Vohs et al. (2006) as a left-serving or not? (2). Do we include the pro-environmental study (Goldstein et al., 2008) as left-serving or not? (3). Do we include Baumeister et al. (1998) as a paper subject to failed replication? We answered all questions with a "yes, we will do both." Specifically, we performed a multiverse analysis (Steegen, Tuerlinckx, Gelman & Vanpaemel, 2016). Given that there were several different ways to analyze these data and all were defensible, we performed all of them. One set of analyses were just for equalitarian studies; another were for all left hotbutton studies. One set of analyses included the borderline study (Vohs et al., 2006) as equalitarian (and left leaning); and one set did not. And one set included Baumeister et al. (1998) and one did not.

This produced a 2 (How left? Just equalitarian or all hotbutton articles) x 2 (with/without the one borderline article) x 2 (with/without Baumeister et al., 1998) x 2 (20% v. 3% base rate of left leaning

articles) table of 16 separate analyses. All results reported in Table 6 are the probabilities that emerged from binomial tests, which were used to determine the likelihood that the observed number of failed replications of highly touted studies left-serving studies, or more, would occur if the base rate of replication attempts of highly touted (more than 1000 citations) left leaning studies equaled the base rate of left-leaning studies reported in Reinero et al. (2020). Put simply, how likely is it that, given Reinero et al.'s (2020) base rates, six, seven, or eight (or more) out of 17 or 18 highly touted studies that have failed to replicate would have provided equalitarian or left-affirming findings?

One could argue that our criticisms of Reinero et al. (2020) render it dubious to use any of its results as a benchmark. However, we do so for three reasons. First, as a paper published in one of the outlets of record in psychology, we suspect that many of our colleagues give it more credibility than we do. If so, then despite *our* reservations, by the standards used by those who consider it credible, this is an appropriate standard. Second, despite our reservations, we have estimated the proportion of politicized studies to be about the same as that found by Reinero et al. (2020) (see Stevens et al., 2018).

Table 5. Did Failed Replications that Triggered the Replication Crisis (2010-2016) Disproportionately Support Hotbutton Left Beliefs and Values?

Original Paper	**Citations**[1]	**Key Finding of Original Paper**	**Does this support or oppose equalitarianism, or other left hotbutton issues (environmentalism, abortion) or is it irrelevant to politics?**	**Why?**	**Failed Replication**
Bargh et al. (2001)	2884	Goals can be unconsciously primed	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Harris, Coburn, Rohrer, & Pashler (2013)
Bargh, Chen & Burrow (1996)	5959	Priming the elderly stereotype led people to walk more slowly	Supports Equalitarianism	Priming stereotypes causes people to confirm them.	Doyen, Klein, Pichon, & Cleeremans (2012)

Baumeister, Bratslavsky, Muraven & Tice (1998)	6632	Capacity for volition, self-regulation and self-control is a limited resource	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Hagger et al. (2016)
Chen, M., & Bargh, J. A. (1999).	1842	Pervasive tendency to automatically categorize stimuli as good or bad	Supports equalitarianism	Explains implicit, unconscious or automatic prejudice and stereotyping	Rotteveel et al. (2015)
Demerouti, Mostert, & Bakker (2010)	1064	Nature of burnout	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Choi (2013)
Carney, Cuddy & Yap (2010)	1386	Powerposing "embodies power."	Supports Equalitarianism	Women underperform because they have been socialized to lack confidence[2]	Ranehill, Dreber, Johannesson, Leiberg, Sul, & Weber (2015)
Finkel, Rusbult, Kumashiro & Hannon (2002)	1119	Relationship Commitment relates to forgiveness	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Cheung et al. (2016)
Galinsky, Magee, Inesi, & Gruenfeld (2006)	1488	The powerful are less likely to understand others' perspectives	Supports Equalitarianism	The powerful are bad	Ebersole et al. (2016)
Glenberg & Kaschak (2002)	2744	Language comprehension is related to action	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Papesh (2015)
Goldstein, Cialdini, & Griskevicius (2008)	3244	Appeals to "provincial norms" increase hotel towel reuse more than other appeals	Supports environmentalism	Towel reuse conserves resources	Bohner & Schuter (2014)
Haley & Fessler (2005)	1571	Subtle cues affect generosity	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Jolij, J., & de Haan, T. (2014)
Monin & Miller (2001)	1236	People are more willing to express prejudice if they have previously	Supports Equalitarianism	Prejudice is everywhere, even among the supposedly unprejudiced	Ebersole et al. (2016)

		shown they are not prejudiced			
Ophir, Nass & Wagner (2009)	2105	Multi-tasking produces distraction	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Alzahabi & Becker, (2013)
Schnall, Haidt, Clore & Jordan (2008)	1832	Disgust influences moral judgments	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Johnson et al. (2016)
Spencer, Steele & Quinn (1999)	4832	Removing stereotype threat eliminates sex differences in math achievement	Supports Equalitarianism	But for stereotypes, women would achieve as highly in math as do men	Finnegan & Corker (2016)
Strack, Martin, Stepper (1988)	2551	Facial feedback influences humor response	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Wagenmakers, Beek, & Dijkhoff (2016)
Vohs, Meade & Goode (2006)	1717	Money priming increases individualism and reduces communalism	Borderline	Results *might* be viewed as reducing support for groups that have fared poorly under capitalism	Klein et al. (2014)
Walker, Brakefield, Hobson & Stickgold (2003)	1267	Evidence for three stages of memory processing	Irrelevant	Has nothing to do with prejudice, discrimination, stereotypes or explaining group differences or other hotbutton issues	Hardwicke, Taqi & Shanks (2016)

[1] Citation counts obtained from Google Scholar between 12/2 and 12/10 2021.
[2]See Cuddy advance this interpretation in this 2017 interview: https://www.youtube.com/watch?v=qKENoimrXbY
Bem (2010) fits the criteria for the wrong reasons. It is highly cited in large part because it is infamous, not as evidence that what it found is really true. Therefore, it is not included in the table.

All 16 binomial tests (Table 6) indicated that it was unlikely that the base rate of replications reported in Reinero et al. (2020) would produce the observed pattern of failed replications of highly touted studies. Despite the fact that the small sample of 18 works against finding "statistical significance," 12 of the 16 analyses reached the conventional standard of "$p<.05$," and two others were at $p=.051$. These results confirm Clark and Winegard's (2020) hypothesis that failures to replicate would occur disproportionately for highly touted equalitarian studies, and support our slightly broader hypothesis that

failures to replicate would occur disproportionately for highly touted studies seeming to support liberal positions on hotbutton topics.

Table 6. Statistical Tests Of The Hypothesis That Failures To Replicate Highly Touted Studies Triggered The Replication Crisis (2012-2016) Were Disproportionately Left-Affirming

	20% base rate With Vohs et al. 2006 as equalitarian	20% base rate without Vohs et al. 2006 as equalitarian	3% base rate With Vohs et al. 2006 as equalitarian	3% base rate without Vohs et al. 2006 as equalitarian
Equalitarian	7, .04, .051	6, .11, .13	7, <.001, <.001,	6, <.001, <.001
Left HotButton	8, .01, .02	7, .04, .051	8, <.001, <.001	7, <.001, <.001

Base rate refers to the proportion of studies leaning left as per Reinero et al.'s (2020) two sets of analyses. First number in each cell is the number of equalitarian studies (in the first row) and the number of left hotbutton studies (in the second row). The second number in each cell is the p-value, which is the probability of obtaining that many, or more, equalitarian or left hotbutton failures to replicate out of 17 total failures to replicate (not including Baumeister et al. (1998) identified as being highly touted between 2012 and 2016 (see text for explanation), given the base rate of left leaning studies. The third number in each cell is almost the same as the second, except that it is the p-value obtained out of 18 failures to replicate, including Baumeister et al., 1998.

Conclusion: Can Anything Be Done?

We are not optimistic about whether anything can be done anytime soon to substantially limit social psychology and the social sciences' political biases. Our view is that many of the main professional psychology organizations (e.g., APA, APS, SPSP, SPSSI) have fully embraced activism and advocacy, and are complicit in the canonization of work riddled with political biases. It is clear that, when most social psychological and social science organizations and officials refer to "diversity" and "inclusion" they are *not* talking about *diversity of political perspectives or identities* (Redding, 2001) and they are not talking about "including" nonleftists. When American social psychologists refer to "underrepresented" groups, they are restricting their reference to social justice, racial reckonings, being "on the right side of history" and the like.[1] They are not referring to the fact that conservatives are one of the most underrepresented groups in all of social psychology (SPSP, 2019). Their use of the term

1 We do not have enough information about social psychologists in, e.g., Nigeria, Bhutan, Iran, or Albania to have opinions about the representation of conservatives in other country's social psychological organizations. Thus, our claim here is restricted to the U.S.

"underrepresented" is primarily defined by how much the group is perceived to have been historically oppressed in America.

The widespread acceptance of the sort of linguistic legerdemain by which "underrepresented" becomes synonymous with "oppressed" renders it difficult to use "underrepresented" in such a manner that ensures that modern social scientists will understand what we mean by it. Therefore, we define it here. We use the term "underrepresented" to refer to this ratio, which reflects the proportion of representation:

$$\frac{\text{Proportion of Group in Social Psychology}}{\text{Proportion of Group in U.S.A.}}$$

If our colleagues meant "underrepresented" in the "proportion of representation" sense, and if they valued ensuring adequate representation of underrepresented groups (as defined here), then they would embrace policies and efforts to increase non-left representation in social psychology and the social sciences, and to reduce hostility toward, and derogation of, nonleftists. Of course, this is in no way antithetical to *also* seeking to increase representation of underrepresented demographic groups. But, not only are they doing nothing to increase representation of underrepresented mainstream political groups, they are instead continuing to press the purity spiral described earlier. That is, social psychology, as a field, is actively embracing and participating in the activist to activism pipeline (Figure 1).

This is not to say that all social psychologists (or social scientists) are activists. Plenty of social psychologists study non-politicized topics and, as we have stated repeatedly, political biases do not influence the study of apolitical topics. Nonetheless, these non-activist scholars are sitting on the sidelines, and are almost entirely silent as activists press the purity spiral and political biases even further. If more scholars had the courage to speak out this could potentially start to help limit political biases. But thus far few have demonstrated the fortitude or willingness to do so.

What about adversarial collaboration? (See Tetlock, this volume) Adversarial collaborations are often touted as a solution to social psychology's potential problems with political biases (e.g., Duarte et al., 2015). However, we now believe that the much-touted practice of adversarial collaboration has

become largely precluded as a solution to the study of politicized topics. Why? Because there are almost no non-leftists remaining or entering social psychology, so a political adversarial collaboration is all-but-impossible, at least within the field. Of course, one can still engage in a theoretical adversarial collaboration, and one might even be able to engage in a political one if one's collaborators come from outside of academia.

One might argue for special efforts to attract non-left students into the field so that, down the road, there are more non-left faculty than at present. However, given that the field has created a hostile work environment for people who do not subscribe to leftist orthodoxies, it's not clear that attracting non-left students is either ethical or possible, at least with respect to careers in the academic social sciences.

One could seek to embrace Mertonian norms, but Merton's (1942/1973) core claim was that these work because they are *norms* that are widely accepted. You, gentle reader, if you are seeking advice, cannot single-handedly change a field's norms, no matter how much you seek to adhere to them personally.

Other possibilities are more grim. As the public learns that the social sciences have become vehicles for progressive and woke activism, public support will likely erode. Some of this we are already starting to see, for example, with substantial percentages of Americans saying that colleges/universities are having a negative impact on the way things are going in the country (Pew Research Center, 2017, 2021). Perhaps legislators who eschew progressive/woke politics will organize to defund highly political/politicized research, researchers, and institutions. Though even if such draconian policies were adopted, because of tenure, the current generation cannot possibly be replaced anytime soon. Because of academic freedom protections, mass firings seem unlikely as well, and are undesirable. Our view is that, as bad as it is in academia, government dictating what academics can and cannot study is a solution vastly worse than the crime of political bias. But this is not to say that it wouldn't be in the interest of academics to try to gain back credibility lost among non-leftist legislators (for a discussion of this, see Inbar & Lammers, 2016). Nor is it to say that legislative or policy solutions to academia's extreme skew cannot be found, but a consideration of such solutions is beyond the scope of the present chapter.

A somewhat less grim possibility, though we do not view it as much of a "solution," is to create new organizations and institutions within the field that prioritize truth-seeking over activism, and welcome scholars from across the political spectrum. When the professional environment turns hostile, one solution may be to leave, and create an entirely new one. This has been a route taken by those excluded from clubs, professions, and guilds for centuries. "If, as a culture, you thrust people out, you run the risk of those same people realizing they like it better on the outside" (Mach, 2019, p.19). Such organizations might preserve truth-seeking on politicized topics, though it will likely take some time before such groups actually start to change the way the social sciences or the field of psychology operates.

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