

Loss Aversion, Intellectual Inertia, and a Call for a More Contrarian Science: A Reply to Simonson & Kivetz and Higgins & Liberman

David Gal
University of Illinois at Chicago

Derek D. Rucker
Northwestern University

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Higgins and Liberman (2018) and Simonson and Kivetz (2018) offer scholarly and stimulating perspectives on loss aversion and the implications for the sociology of science of its acceptance as a virtual law of nature. In our view, Higgins and Liberman (2018) largely complement our conclusion that the empirical evidence does not support loss aversion. Moreover, in alignment with our call for a contextualized perspective, they provide an excellent discourse on how a more nuanced view of reference points and consumers' regulatory focus enriches our understanding of the psychological impact of losses and gains. Simonson and Kivetz (2018) approached our perspective with skepticism, and, while they retain some skepticism, they express agreement on the larger point that loss aversion has been accepted too uncritically. Both commentaries point to a need for a critical reevaluation of prevailing paradigms. Here, we build on these perspectives, as well as our experience working on the topic of loss aversion, to call for structural changes to facilitate scholarly debate on science's status quo.

Keywords Loss aversion; Sociology of science; Contrarian science

Four exceptional scholars—all who have made outstanding contributions to the study of decision making and psychology—provided two thought-provoking commentaries to our article (Gal & Rucker, 2018). We appreciated the commentary by Higgins and Liberman (2018) (hereafter H&L) on several fronts. First, they provided a rich discussion of reference points within the framework of regulatory focus theory; H&L elucidate how different regulatory strategies lead to the adoption of different reference points which, in turn, lead to distinct outcomes. Second, while we entertained the idea of elaborating on the implications of regulatory focus theory for loss aversion in our target article, we could not have done it nearly as well as H&L. As such, we particularly appreciated their perspective and insights. Third, H&L supplement our review of the literature by identifying a number of additional contexts where evidence for loss aversion is notable by its absence.

We also valued the commentary by Simonson and Kivetz (2018) (hereafter S&K). First, we appreciated the skepticism that they brought to our argument; we anticipate skepticism will be the most common reaction among decision-making researchers. Indeed, we do not believe challenges to existing ideas should

be accepted blindly; rather, we believe they should be approached in the critical but open-minded manner demonstrated by S&K. Second, we understand some of the skepticism offered by S&K, and we view this as perfectly suited toward further scholarly dialog. Finally, we found S&K's discussion of the implications for our science of the uncritical acceptance of paradigms highly insightful.

In this rejoinder, we offer a brief discussion of our points of agreement followed by points of disagreement with the commentaries. We conclude with an extension of the theme of our target article and of the commentaries. We do this by calling for scientists to be wary of intellectual inertia and to maintain a critical but open mind to contrarian ideas as a means to advance the search for scientific truth. We also illuminate potential directions to extend this discussion to structural changes that might stimulate debate of competing ideas and thereby advance science.

Points of Agreement

As stated in our target article (Gal & Rucker, 2018), we believe a strong need exists to take a contextualized approach to the study of the psychological

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Correspondence concerning this article should be addressed to David Gal, University of Illinois at Chicago, 601 S. Morgan St., Chicago, IL 60607, USA. Electronic mail may be sent to davidgal@uic.edu.

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impact of losses versus gains. Loss aversion represents an alternative perspective because it suggests an innate and general tendency—*ceteris paribus*—of losses to outweigh gains. Intentional or not, we view acceptance of loss aversion as an obstacle to a more systematic, contextualized, and informative study of losses and gains.

H&L's discussion of reference points and regulatory focus offers an excellent instantiation of the type of contextualized theories we believe are required. We concur with H&L that an important contribution of prospect theory was to highlight that people evaluate outcomes, in part, in terms of the change from a reference point. We further agree with H&L that much of the decision-making literature has assumed that the status quo is the only relevant reference point. As a consequence, past research has done little to develop systematic theories of how people adopt reference points, how they integrate multiple reference points, and how the motives and strategies they bring to bear on a decision influence the reference points used. H&L's description of reference points as "locations on the value axis that receive attention" resonated with us and we hope their discussion of reference points is widely read and extended.

H&L also highlighted several domains, beyond those we identified, where evidence for loss aversion is notable by its absence. They report that upward counterfactuals do not appear to exert more influence than downward counterfactuals, upward social comparisons do not appear to exert more influence than downward social comparisons, and using the end-state of goal pursuit as a reference point appears no more motivating than using the starting-point of goal pursuit as a reference point. They also note that "loss aversion characterizes better (but, even then, not fully) people in a prevention focus who are trying to restore the status quo when they are below it, but it does not characterize people in a promotion focus." These observations further buttress the case against both the strong and weak versions of loss aversion.

S&K also express agreement with our argument that loss aversion has been accepted too uncritically, and that it is less robust and more contextual than typically assumed. Although we disagree with S&K on whether the evidence supports losses to be more impactful than gains on average—a point we will return to—upon reflection, we view this as secondary to what we see as the larger issue at hand. Specifically, regardless of whether one could meaningfully demonstrate that losses loom larger than gains on average or vice versa, a contextual perspective suggests that the more interesting and

important endeavor concerns the development of psychological theories of *when* and *why* losses versus gains are more impactful. In this vein, and in strong agreement with our views, S&K call for "greater attention to moderators, boundary conditions, and psychological mechanisms."

Points of Disagreement

Because the commentary by H&L largely complemented our arguments or extended them in a meaningful way, we have no critical points of disagreement that merit discussion. In contrast, S&K made several points from which we diverge.

First, S&K view our delineation of a strong version of loss aversion as a rhetorical strawman. They write, "we are unaware of any empirical phenomenon in the social sciences that *always* holds, and presenting it as a viable option may create the wrong impression that the 'weaker version' they advocate represents a compromise view." This criticism merits two points of clarification. First, our designation of "stronger" and "weaker" are simply descriptive devices; the weaker version is not meant to imply that such a perspective is unimportant or invaluable; rather, it is simply "weaker" in form. Indeed, were we to view the weaker version as trivial, we would not have gone to great lengths to challenge such a perspective. Second, and more importantly, our *strong version* of loss aversion is not the idea that losses will always be observed to be more impactful than gains, but that the *psychological mechanism* that leads losses to fundamentally loom larger than gains always operates. To clarify, in the target article we write, "This strong version does not require that losses must outweigh gains in all circumstances, as factors such as measurement error and boundary conditions might obscure or reduce the fundamental propensity for losses to be weighted more than equivalent gains."

Far from a rhetorical straw man, we maintain our assertion that the strong version is an accurate representation of what many researchers believe about loss aversion and that it is substantively distinct from the weak version we delineate. To elaborate, the weak version states that sometimes losses loom larger than gains, sometimes gains loom larger than losses, and sometimes there is no difference, but, *on average*, the first (losses loom larger than gains) ultimately predominates. However, as S&K note, many researchers seem to hold beliefs much more aligned with the stronger version. Specifically, in S&K's own words, "loss aversion has been elevated to something

akin to a law of nature that might be hard-wired from birth and may hold also for some other species. It represents a fundamental and all-encompassing driver of judgments and decisions." We believe this quote accords well with what we term the strong version of loss aversion. To illustrate by way of an analogy, the statement "Earth's gravity always exerts a pull on objects in the direction of the Earth" is similar to our representation of the strong version of loss aversion. It does not mean that objects will always be observed to fall toward the earth. For example, when a person exerts muscular force to jump upwards or when a rocket reaches escape velocity, the constant force of gravity is overcome by other forces.

In contrast to our assessment of the evidence, S&K view the evidence as supporting the weaker form of loss aversion whereby losses, on average, loom larger than gains. In their discussion, they state that "some of the evidence proffered by G&R appears to actually support loss aversion; for example, G&R cite their finding that people enjoy gaining things that are lost (e.g., finding a lost flashlight) more than they enjoy gaining new things. . . which suggests that losses do loom larger than gains." However, our interpretation is that such an inference conflates a *specific context* where a loss might be more impactful than a gain with a *general tendency* of losses to loom larger than gains. Our contextualized perspective does not deny that contexts exist where losses loom larger than gains—indeed it embraces such outcomes by its very nature. Individual instantiations where losses exceed gains are not evidence for a general tendency of loss aversion any more than individual instantiations where gains exceed losses are evidence for a general tendency of gain seeking (e.g., our finding that for mundane objects, individuals report greater positive feelings from acquiring them than they report negative feelings from losing them). H&L's commentary presents an excellent example of this perspective with regard to different outcomes associated with promotion versus prevention.

S&K also view evidence from risky choice contexts as supportive of loss aversion. They write, "G&R unconvincingly and superficially try to explain away the strongest illustration of loss aversion, namely, the typical extreme risk aversion exhibited in choices between a certain gain and a mixed, high expected value gamble." We will not review the evidence again here, but we stand by the conclusion of our initial review (Gal & Rucker, 2018). When acceptance of a risky gamble is not framed as a change to the status quo, in the context of low to moderate stakes, little evidence is found that people are loss averse, as previously reviewed

by Ert and Erev (2013) and Yechiam and Hochman (2013). At high stakes, the evidence suggests people prefer a safer over a riskier option, but, as discussed in our initial review, this is perfectly rational and can be explained by expected utility theory. Moreover, even in the case of fairly high stakes, the observed level of risk aversion tends to be far from "extreme."

Finally, S&K view some of the new experimental paradigms we have introduced to detect the presence of loss aversion in riskless choice as relying on "what appears to be unrealistic experimental manipulations that are susceptible to confounds and give rise to simple alternative explanations." Because no specific confounds and alternatives are identified, we cannot address such criticism directly. However, it is possible that our paradigms might have had some hidden confound that inhibited our ability to demonstrate loss aversion. Nonetheless, beyond the thought given to eliminate potential confounds, we used a large number of operationalizations and stimuli. The advantage of a varied approach is that it reduces the likelihood that a persistent confound is likely to account for our effects. In addition, our conclusion that little evidence for loss aversion exists rests on the totality of the evidence, of which our new procedures represent only a part. As noted, H&L add to this evidence with examples from other domains. Nonetheless, we believe challenges to any theory should be scrutinized carefully and not merely accepted. And we welcome further dialog with S&K, perhaps even a mutual collaboration, to design even more rigorous paradigms.

Intellectual Inertia and A Call for a More Contrarian Science

S&K offer a powerful and cogent articulation of the disincentives for scholars to challenge reigning paradigms. Most prominent is gatekeepers' tendency to favor work that builds on established paradigms. S&K also articulate the costs associated with favoring the incumbent paradigm: "the bias in favor of incumbent assumptions can impede scientific progress, bar new ideas from the literature, and reinforce well-established but contingent notions that may apply under some conditions but not others." H&L likewise point to uncritical acceptance of established ideas as problematic, and call for a critical reevaluation of such notions, for example, the "bad is stronger than good" assertion. Indeed, a way to understand the totality of these

concerns is that science falls prey to a form of intellectual inertia that favors the status quo even when equivalent—or stronger—evidence challenges it.

Here, we extend this discussion to call for a greater acceptance, tolerance, and open-mindedness toward contrarian science. Our point is not that scientists should be contrarian for the sake of being contrarian, nor is it to say that contrarian science should be held to lower standards. Rather, we focus on how intellectual inertia may lead to protection of the status quo in a manner that hinders science. Specifically, we first contest the notion that it is logical to hold competing ideas to *higher* standards of evidence and logic than accepted ideas. Second, we make the case that challenging accepted ideas is often a critical route to advancing the search for scientific truth. Finally, we offer recommendations to encourage a more open-minded discussion of challenges to established ideas.

Higher Standards of Evidence to Challenge Established Ideas

The case of loss aversion serves, we believe, as a sharp illustration of a more general phenomenon. Namely, established ideas become so entrenched that they take root and persist in spite of irreconcilable and even contrary evidence. As noted in our target article and by S&K, this outcome might reflect the fact that the threshold for publication and acceptance among scientists tends to be much higher for ideas that contrast with existing theory compared to those that buttress or extend established theories. As Galileo might have attested, this is not unique to our field or time. Nassir Ghaemi (2017), a psychiatrist, recently wrote an open letter of advice to medical students: “You can make it your passion to try to raise psychiatry up and move it forward when all the influence of the status quo holds it back. Are you willing to spend your entire career fighting the powers that be? You may become a hero for future generations, if you succeed in the process of change in the long run, but that posthumous adulation will do nothing for your personal happiness in this life. . . the awards go to those who maintain the status quo, not to those who change it”.

On the one hand, a case can be made on epistemological grounds that work designed to challenge established paradigms should face more stringent criteria than work that builds on existing paradigms. Existing paradigms are likely to have been established through the defeat of inferior rival accounts over time. Moreover, the scholars that

develop and work within existing paradigms have the most relevant expertise, and therefore their support of these paradigms is a testament to their validity. In other words, a reasonable case can be made that established paradigms are more likely to be true than upstart rivals. As such, a conservative bias against new accounts can be viewed as completely appropriate. However, this seems far from certain in our field. Ioannidis (2012), for example, argues, “In several fields of investigation, including many areas of psychological science, perpetuated and unchallenged fallacies may comprise the majority of the circulating evidence.”

In particular, a tendency toward intellectual inertia and conformity might arise because it is a challenge to update one’s worldview. It can be difficult to adapt to new skills and paradigms that differ from those one has been trained in and on which one’s reputation has been built. An unintended byproduct of intellectual inertia is that people might seek strong proof of challenges to extant theory, but ultimately favor established paradigms even when the established evidence is no greater than the contrarian evidence. To provide a telling anecdote, in the discussion of the new procedures we developed to test for loss aversion, we have heard concerns expressed with respect to the artificial nature of the scenarios we employ (e.g., paying to retain a possession). Yet, a large portion of the evidence cited to support loss aversion, both historically and contemporarily, relies upon just as artificial scenarios (e.g., a consumer selling a possession that an experimenter has just given them). We view this as an excellent example of when the evidence demanded to challenge an idea is qualitatively different from the evidence demanded to support it. Whereas one set of standards is viewed as acceptable for the status quo, a different set is applied to challenges.

Of greater concern, in the absence of a culture of critiquing accepted ideas, it is unclear how we can know if our theories and paradigms are leading us astray. The U.S. Navy admiral and strategist William Sims wrote, “it may be stated in general terms that most arguments in favor of fundamentally new weapons have failed except those that resulted in shedding the blood of the unbelievers; that defeat alone has been accepted as a final demonstration” (Sims, 1921). Yet, unlike in the military domain, flawed, incorrect, or overgeneralized psychological theories cannot be laid bare through defeat on the battlefield. A building erected on a flawed foundation will collapse, but again, this seems less true for psychological theories, which can be expanded

upon indefinitely, however, flawed they might be. Dissertations based on the theory can continue to be published, accolades can continue to be awarded, and no decisive climax or ultimate reckoning awaits.

Contrarian Ideas and the Advancement of Science

The Hierarchy of the Sciences hypothesis states that as fields of inquiry move from the study of the low-level and general (e.g., physical chemistry) to the high-level and specific (e.g., human decision making), the ability of scholars to reach theoretical consensus diminishes (Comte, 1835; Fanelli & Glänzel, 2013). Indeed, when inscrutable evidence is difficult to obtain, social proof, such as the opinions of other scholars, particularly prominent ones, serves as a substitute basis for beliefs (Simonton, 2004).

It is common for scientists debating competing theories to use the phrase, “it’s an empirical question.” But, in looking retrospectively, we believe that it is rarely as simple as an empirical question. As we believe the case of loss aversion suggests, few strongly held beliefs in our field are changed solely by data. A large number of findings suggest, at a minimum, that losses are not always more impactful than gains; in fact, this evidence has been accessible for decades. Yet, rather than leading to the generation of more complex and nuanced models, it seems that most evidence continues to be interpreted in line with a loss aversion account or, alternatively, is dismissed and ignored. It seems, in line with Kuhn (1962), that scientific truth is slippery and data alone, no matter how accurate, is fundamentally filtered through the lens of established paradigms that have been fortified by the social proof of acceptance. That the data cannot speak for themselves underscores the need for open discourse around contrarian theories as a means to facilitate critical consideration of conflicting interpretations.

Of note, even the publication of “wrong” ideas, to the extent they challenge established ideas, can ultimately be beneficial from the standpoint of advancing science. For example, even though we believe Zajonc (1980) was correct in his assertion that affect need not be dependent on cognitive inference, a tremendous amount was learned from his debate with Lazarus (1982), who advocated the contrary viewpoint. It forced Zajonc to better synthesize the evidence, articulate his logic, and refine his perspective (Zajonc, 1984). Although the established theory in this example could be viewed as

ultimately more correct than the contrarian viewpoint, we learned a great deal from the publication of the contrarian article.

As two additional, more recent examples of contrarian articles, consider the debate stimulated by Frederick, Lee, and Baskin (2014) that challenged the attraction effect and the debate stimulated by Scheibehenne, Greifeneder, and Todd (2010) on the choice overload effect. Right or wrong, an open dialog on the contrarian view offers insight into these topics. Yet, to our knowledge, both of these papers had a fairly painful road to publication, much more so than a typical paradigm-confirming paper might have had. Indeed, an author of one of these papers told one of us that the publication process was “not worth it.” Moreover, all three reviewers, experts on the topic, recommended rejecting the other of these papers before it was ultimately published.

A productive disagreement can force those who are closer to the truth to strengthen their logic and arguments and to illuminate unclear assumptions. Conversely, being overly cautious to publish “correct” articles often provides little new insight and can lead to the perpetuation of flawed paradigms. Thus, perhaps surprisingly, even when it is flawed, contrarian science can ultimately serve to advance science more than confirmatory science.

Recommendations

To be clear, we are not advocating publishing contrarian ideas merely because they are contrarian, nor are we eschewing concerns about evidence and logic. Rather, we believe scientists are biased in favor of accepted ideas (Greenwald, Pratkanis, Leippe, & Baumgardner, 1986; Kuhn, 1962). As such, we advocate greater balance and greater attention to the ability of contrarian ideas to advance science. Part of this recommendation to be more open-minded toward contrarian research is directed to researchers and gatekeepers. However, we believe most scientists are already open-minded in principle, and this is likely insufficient to encourage contrarian science.

For example, although we privately (and now publicly) lament the lack of value accorded to ideas that challenge the status quo in our field, we find ourselves as reviewers, letter writers, hiring committee members, and even as authors in danger of perpetuating the status quo. Put simply, ideas that fit the purview of existing knowledge seem to be easier to match up with existing standards and norms than ideas that do not. We believe such behavior reflects the fact that a tendency to

perpetuate the status quo is difficult to escape as we are socialized by our field to act according to its norms (Ellison, 2002; Lehmann, McAlister, & Staelin, 2011). Thus, our view is that if contrarian paradigms are to receive a fairer hearing, structural reforms are needed.

First, we suggest that conferences and journals in our field develop tracks for particularly contrarian, controversial, or otherwise paradigm-upending research.¹ Ideally, papers accepted through this track would receive a special designation, so that schools and letter writers who value such work can more easily give it the appropriate weight. Indeed, the current Research Dialogues section of the *Journal of Consumer Psychology*, in which this discussion appears, provides an excellent context for such an opportunity. In addition, the NIH, cognizant of the inherent bias against paradigm-upending research, has recently taken several steps to help promote such research. For example, the NIH seeks to promote transformative research through a grant portfolio specific to “research projects that have the potential to create or overturn fundamental paradigms” (Transformative R01, 2017).

Second, because the publication lag in our field is long and uncertain, and likely particularly so for contrarian research, we suggest that contrarian papers can benefit from an evaluation process that is both more rapid and more heavily weighted toward conceptual importance than is typical. By conceptual importance we refer to an emphasis on conceptual arguments, novelty, challenge to accepted ideas, and ability to stimulate scientific debate. One option is a two-tiered review process, in which the first tier is almost entirely concept driven and the turnaround time is fast, before the work is sent out to reviewers. Indeed, such a process would mimic and build upon the approach by some journals to make initial decisions based on a first reading by the editor and associate editor prior to review.

Third, a component of the NIH transformative research grant initiative is that reviewers are recognized authorities in their field, but are *not* experts on the specific topic that they are tasked to evaluate. The purpose is to facilitate the advancement of paradigm-upending research and to protect proposals from being rejected for lack of conformity with existing methods or theories. Similarly, we suggest that contrarian research papers be assigned at least some reviewers who are not experts in the particular research subject. Again, this does not mean that poor research should be accepted for contrarian

purposes; rather, it should be given a fairer and more impartial trial.

Will such reforms work, leading to greater publication of contrarian research and to the advancement of science? Ultimately, it might (or might not) be an empirical question.

Endnote

¹Though more targeted toward innovation in methods or modeling than toward challenging existing theories, Marketing Science has recently adopted a similar initiative (Sudhir, 2018; <https://pubsonline.informs.org/doi/10.1287/mksc.2018.1091>).

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