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The Impact of Changing Norms on Creativity in Psychological Science

Jonathan Wai[1] and Diane F. Halpern[2,3]
[1]Department of Education Reform, University of Arkansas; [2]Department of Psychology, Claremont McKenna College; and [3]Minerva Schools at KGI

Abstract
The open science or credibility revolution has divided psychologists on whether and how the "policy" change of preregistration and similar requirements will affect the quality and creativity of future research. We provide a brief history of how norms have rapidly changed and how news and social media are beginning to "disrupt" academic science. We note a variety of benefits, including more confidence in research findings, but there are possible costs as well, including a reduction in the number of studies conducted because of an increased workload required by new policies. We begin to craft a study to evaluate the short- and long-term impacts of these changing norms on creativity in psychological science, run into some possible roadblocks, and hope others will build on this idea. This policy change can be evaluated in the short term but will ultimately need to be evaluated decades from now. Long-term evaluations are rare, yet this is the ultimate measure of creative scientific advance. Our conclusion supports the goals and procedures for creating a more open science.

Keywords
policy change, creativity, evaluation, cumulative science, future generations

This symposium is about how open science and the credibility revolution, which are starting to become a part of the normative process in psychological science, may affect creativity and other factors that are important for high-quality psychological research and the future of our discipline. From the outset, we note that "creativity" in psychological science can mean different things to different people (e.g., Simonton, 2004, 2017), which is reflected in the diversity of ideas on this topic expressed in this symposium. The contributors are distinguished scholars, each of whom has valuable perspectives to share. We begin with a brief overview of the proximal causes of the crisis in credibility, comment on each author's contribution, and end with some thoughts of our own, including an initial proposal to study this "policy" change.

Understanding the Crisis: A Brief and Recent History

First, it is important to look back to consider how this all happened. Though scholars have long been concerned with methods and practices in psychological research (Cronbach, 1957; Dunnette, 1966; Meehl, 1978), only recently has there been a cultural change in the research norms of psychology. The beginnings of this cultural change could be seen in articles debating the quality of research on ESP, priming, and discussions of "false-positive psychology" (e.g., Doyen, Klein, Pichon, & Cleeremans, 2012; Simmons, Nelson, & Simonsohn, 2011; Wagenmakers, Wetzels, Borsboom, & van der Maas, 2011; for a history of these issues, see Chambers, 2017; Nelson, Simmons, & Simonsohn, 2018). However, this cultural change reached a tipping point when *Science* published the Open Science Collaboration's (2015) "Estimating the Reproducibility of Psychological Science," which was widely covered in the news and on social media. This demonstrates that quality research in psychology is being assessed not just by peer-reviewed publication in prestigious outlets, but also by how much "buzz" it generates.

Corresponding Author:
Jonathan Wai, Department of Education Reform, 201 Graduate Education Building, University of Arkansas, Fayetteville, AR 72701
E-mail: waijon@hotmail.com

There are takeaway messages: Psychological science (and other areas) is in trouble, and the responses of scholars in other fields and the general public may now be important indicators of influential research in psychology. Whether we like it or not, news and social media are starting to have an impact on the way that academic science is practiced. The open-science revolution has rapidly evolved and may eventually become the norm in psychology and perhaps more broadly across the sciences (Nosek, Ebersole, DeHaven, & Mellor, 2018).

In our view, the open-science revolution is essentially a major cultural shift in the way that psychological science is practiced. Thus, it is a "policy" change in our field. Understandably, many scholars have been hesitant and even strongly resistant to such a change given that these shifting norms can have consequences to individual careers—both established and beginning—as well as the collective process of scientific advancement. As the Open Science Collaboration (2015) article evidenced, this change has "disrupted" the patterns in which academic fields typically operate (e.g., Gelman, 2016).

Distinguished Scientists Weigh In

The contributors to the symposium are distinguished scientists whose careers were successful under the "old" system in which researchers, alone or in teams, designed and conducted research, analyzed data, inferred meaning from the data, and then wrote manuscripts designed to persuade editors and reviewers that the process met a prevailing standard on all of these aspects. They all succeeded and were rewarded with promotions, tenure, reputations for high-quality work, and, in most cases, grants to do more of the same. Of course, some of them may have been approaching their research using practices that are part of the open-science movement. Even the most and least enthusiastic cheerleaders for change recognize that the open-science movement, especially preregistration of research plans (PR) or registered reports (RR), may offer benefits and detriments. PR is a research plan with a date that you can point to after conducting your study to show you are testing a predicted relationship. RR is a research plan that is peer reviewed, before the results are known, on the basis of the importance of the research question and the quality of the proposed methods to answer the research question; if accepted, the eventual manuscript will get published no matter the outcome. The topics addressed in this forum are less important for senior scientists who can choose to adopt the new procedures or continue as they have by using publication outlets that do not require PR or RR. The real importance of the ideas debated here is their effect on the next generation of psychological scientists and how this policy change might affect those younger scholars. As Frankenhuis and Nettle (2018; this issue) suggest, "They might leave science or forego entering it, resulting in a loss of human capital" (p. 443).

Alternatively, it is also possible that the open-science revolution will inspire young scientists to join psychological science, thus resulting in a gain of human capital. Thus, our most important audience is students and psychologists who are in the early years of their career or considering a career in psychological science. Most of the contributors to this symposium commented on the tensions between fostering creative research and following a protocol that is designed to increase reliability and confidence in the findings. Fiedler (2018; this issue) called this the "dialectics of loosening and tightening." (p. 433). Broadly, the contributors tend to fall into two camps: The first group of contributors—Brainerd and Reyna (2018), Fiedler (2018), and Kaufman and Glăveanu (2018)—are more concerned that creativity may be hindered by open science. The second group of contributors—Frankenhuis and Nettle (2018), Grand, Rogelberg, Banks, Landis, and Tonidandel (2018), Vazire (2018), and Wagenmakers, Dutilh, and Sarafoglou (2018)—broadly take the view that creativity will not be hindered and will likely be enhanced by this policy change. Obviously individual contributors in each group have more nuanced perspectives.

Group 1: The cons, or why psychology's response to the credibility crisis could harm psychological science

Brainerd and Reyna (2018) are concerned with unintended consequences that include the additional workload required to complete registration forms and the "career-destroying reputational assaults" (p. 429) that may result from failures to replicate. They argue that predictions about research outcomes are deduced from theories, and thus the probability that people will change predictions after seeing their data is not as high as proponents of PR (or RR) believe it is. We agree that failures to replicate findings from published studies could have negative effects on the reputations and careers of the original study authors, although in large part that depends on how the authors respond to criticism, and it may be less harmful if failure to replicate findings becomes the norm (a scary possibility for the entire field). Of course, if our aim is to advance the collective goal of psychological science, whether findings are real (i.e., widely replicable) or not is the ultimate criterion. It is also important to ensure we treat all scholars at every stage with respect.

Fiedler (2018) raises a different set of fears. He believes that stricter compliance rules, planned analyses, exact *p* values, arbitrary sample sizes, and so on are targeted in PR and RR, whereas the real creative endeavors, such as theory construction and the more creative aspects of research, are not given as much attention. As planned, the new regulations neglect creative theorizing, which he believes is the heart of quality research. We counter this concern with the other possibility, which is that researchers are not constrained in the theory-construction aspects of research by the need to adhere to certain accepted practices about the way research is executed. We think focusing on theory can also restrict creative scientific approaches that are not driven by theory.

Kaufman and Glăveanu (2018) understand the rationale for PR and RR but take issue with the way a generally good idea is being implemented. They note that registration procedures will not solve all problems and may only reduce the size and number of some problems. For example, it is difficult to know whether the requirements before data collection will remove or reduce outright deceit. News about psychologists who fabricated entire data sets or engaged in the practice of eliminating data that do not support a favored hypothesis have created an honesty crisis that may not be fixed with PR or RR. Perhaps, more critically, these practices will enhance "little-C" creativity, in which creative steps are small increments to a dominant theory, whereas "big-C" creativity that leaps across dominant theories and starts new directions in research will be less likely to occur. We note the alternative perspective that when large leaps in new directions are not evaluated properly, they may not be large leaps in scientific advancement. It can be argued that PR and RR have also initiated large leaps in new directions by making us reevaluate the evidentiary basis for major theoretical claims. From this perspective, even a small but robust and well-replicated effect may in fact be a very important advance for the field.

Group 2: The pros, or why critical changes in the process of psychological science will result in a stronger psychology

Frankenhuis and Nettle (2018) argue that creativity in research will not be hampered by PR and RR requirements because researchers are still free to explore their data and report unexpected findings, as long as they clearly indicate that these were post hoc actions and thus should be subject to greater scrutiny than preplanned outcomes. They believe that creativity is enhanced when there are some constraints and thus may be enhanced with PR and RR requirements. Because people habituate to new procedures over time, objections to doing open science will dissipate, and open science may eventually become the accepted standard. These authors make the point that PR and RR change the incentive structures for researchers. Because publications are (essentially) guaranteed in advance of data collection, there will be no HARKing (hypothesizing after the results are known) or suppression of unfavorable results.

Unlike Frankenhuis and Nettle, we are less confident that the incentives for finding statistical significance are changed, or in practice may take a long time to change. It is unlikely that a string of publications with null results or failed replications will help the authors of these registered studies navigate the choppy waters of retention and promotion at their university or that they will increase the likelihood of grant success. It is hard to imagine a researcher building a reputation on null findings, so the incentive for statistical significance (with large effect sizes, of course) will remain strong even with PR and RR unless we can change the entire social structure of rewards in research. From the pure perspective of scientific advance, we note that knowing what is not well replicated may be just as important if not more important than "discovering" new relationships, but that shifting the incentives will take time. We note that some aspects are already changing in the field, such as faculty job advertisements asking all applicants to address how they incorporate open science practices into their work.

Grand et al. (2018) make many of the same points that others have mentioned, including the ability to explore one's data by labeling post hoc exploration as such, and they also argue that even with PR and RR, it is still possible to game the system. We acknowledge this concern but add a point that is likely to get us in trouble with contemporary notions of what is "kosher" in statistical analyses. Except for replications, few studies are identical to ones that have already been published, which means our best estimate of an expected effect size is exactly that—an estimate. In the old days, it was common to run a few more subjects than originally planned to cross the magical line of statistical significance. This practice (verboten now) was justified by explaining that estimates were not precise and that science and society might lose something when a potentially important finding never makes it to print. This is another example of the tightening-loosening tug of war. The failure to replicate findings in a majority of studies has shifted research away from concerns about Type II errors, but the concern for "findings that could have been" deserves some respect even in the new science. Of course, a valid counterargument is that some of these "missed findings" are not solid findings

and that once something becomes "popular" but is incorrect, it may take much research effort from other scholars to show that such a finding rests on a weak evidence base (e.g., see Marcus & Oransky, 2018). And no matter what the policy and incentive structure is in psychological science, gaming the system will occur because the competition for jobs is fierce and has probably increased in recent years (e.g., Pennycook & Thompson, 2018).

Wagenmakers, Dutilh, and Sarafoglou (2018) make the important point that scientists are humans, and we have cognitive and emotional biases. PR, RR, and other practices of open science may allow us to be go beyond our own myopia and self-interest and free us up to be more confident in the findings produced, even if those are incremental advances rather than giant leaps. This can lead to increased credibility of the field. Like other authors in the symposium, Wagenmakers and colleagues recognize that creativity and verification are not competing forces in a zero-sum game. We agree that PR and RR may make us more aware of our biases and take steps to counteract them. However, individual reputations still soar with sexy and surprising findings that are statistically significant, large in size, and relevant to addressing a social issue. Perhaps, over time, incentives could change if the new generation of psychological scientists who get tenure-track jobs (and tenure) decide to shift that culture as they rise in the ranks and educate future generations.

There were several common themes across most of the articles. One of the main benefits of the open-science movement is increased transparency in research. It would be hard to argue against transparency because everyone wins if readers can see how the research was planned and executed. Another proposal under the open-science rubric is the goal of having researchers share their data sets. Having more data sets available would be a great benefit to all researchers, although we expect that when data collection was time-consuming, difficult, and/or expensive, there will be some hesitancy to "give it all away," especially if additional analyses are possible using the same data set. We expect that over time, as sharing data sets becomes normative, researchers will feel the pressure of reciprocity and be more willing to share the results of their labors.

Evaluating This Policy Change

As Vazire (2018) notes, "These questions can and should be studied empirically" (p. 411). We agree completely with this important point. The authors in this symposium suggest a wide range of possible outcomes, including a drop in the number of studies conducted because of the increased bureaucratic workload needed for registration, more confidence in research outcomes, and even destroyed careers for researchers whose studies are not replicated, to name a few. As the open-science revolution becomes increasingly adopted by psychological scientists (Nelson et al., 2018; Nosek et al., 2018), whether and in what ways creativity is affected can be evaluated. Some journals are already evaluating the short-term impact of such policy changes (e.g., Hardwicke et al., 2018). We turned to "Research Preregistration 101" by Lindsay, Simons, and Lilienfeld (2016) to help us attempt to sketch out a potential study to test empirically whether and how creativity in psychological science might be affected. There are many possible empirical outcomes from this policy change, and the following proposal is just an initial concept and starting point that we hope others might improve, given that these evaluations are likely to occur in the future (for this specific policy change or for others). So we call this is a "pre-preregistration" (PPR), where we hope to crowdsource ideas from scholars to improve it. The point of this PPR is simply to get scholars to think about how to evaluate policies in both the short and long term and how policy changes can affect a field in many ways (intended and unintended). Of course, our ideas about how to study this policy change may be different from others'—and may have their own set of weaknesses—and so we encourage others to surpass our ideas and create a diversity of approaches to study this issue. Hopefully future research on the effects of open science will inform data-based decisions.

A simulated pre-preregistration of a study of the impact of the open-science revolution on creativity

The first problem is that we do not have a strong (or even a weak) theory to make a priori predictions. We want to know if PR (or RR) has increased, decreased, or had no effect on "creativity" in psychological research. Reasoning to support each of these possible outcomes has been presented by the authors in this symposium. Are we ineligible for PR (or RR) from the start? Perhaps we can simply state that we do not have any predictions and that we plan to do exploratory analyses to evaluate this policy change from many different angles. We could draw from the already existing fields of "science of science" (e.g., Fortunato et al., 2018), the sociology of science (e.g., Merton, 1979), the study of scientific creativity in psychology (e.g., Simonton, 2004; Stumpf, 1995), and research policy, among other areas.

PR (or RR) requires a reasonable estimate of effect size to determine sample size. We have no similar studies to use for effect-size estimates, and even if we did, we do not know how many studies with and without

PR (or RR) will be published in the next few years. But given that our study is likely to include evaluation of many individual studies, as we note below, perhaps sample size will not be a large issue; however, the larger pattern of findings might be (e.g., Steen, 1988).

We first propose to broadly examine the creativity of research in psychology in three periods: (a) before the policy change, (b) during the policy change, and (c) after the policy change. Of course, the policy change is not guaranteed to become uniform throughout the field. Many journals may adopt these policies, but perhaps not all. Policies may also adapt and change to accommodate unforeseen circumstances and concerns, or new directions might take shape. The period before the policy change might be defined as that before any journal adopted PR or RR. The period during the policy change might be defined as that after the first journal adopted PR or RR up through the point when the policy change is no longer present and/or up through the point when a sufficient number of journals have adopted PR or RR (e.g., some percentage of psychology journals). For example, for the period during the policy change, many short-term evaluations could take place (and some already are) comparing journals that have adopted PR or RR with journals that have not yet adopted PR or RR. The period after the policy change might be defined as that after the point when the policy change is no longer present and/or up through the point when a sufficient number of journals have adopted PR or RR (e.g., a sufficiently high percentage of psychology journals). To examine a large sample of studies in each period, we would ideally decide on roughly equivalent periods of time before, during, and after the policy change. However, it may also be useful to examine the creativity of studies in psychology going back many decades, as well as examining the creativity of studies in psychology well into the future (e.g., it may take decades to understand what findings in psychology empirically stand the test of time). For each of the periods studied, articles might be selected as a function of publication or journal tier (e.g., top, middle, lower, which would be defined), with an equal number of articles in each group.

Measurements of creativity. A panel of experts in various fields might be asked to rate three (or some other reasonable number) masked articles each, assessing the number of creative ideas and how creative each idea is. Because we do not know how large our sample will be, we cannot specify the number of experts we hope will donate their time for this study.

Typical research productivity metrics, such as number of publications, journal of publication, journal impact factor, and other quantifiable metrics used in the "science of science" and other relevant areas discussed earlier, can also be used as measurements of creativity (e.g., Fortunato et al., 2018). The main dependent variables are the variety of "ideas" generated during the periods and how "creative" they are. We will need to break down articles by the topical area in psychology. It may be that PR (or RR) will have different effects on creativity for perception studies than for social psychology, but we have no theoretical basis for this distinction. We might also study the policy change in fields outside psychology to determine whether such findings are field specific or not.

Ultimate creative criteria. We also aim to test whether the creative ideas generated before, during, and after the policy change survive the test of time in terms of empirical support, the ultimate measure of scientific advance. This could take decades or more to fully understand, given that many popular or creative ideas are not always new and/or empirically supported.

Other data collected will include the typical metrics in the science of science (e.g., various productivity indices and researcher perceptions of the policy change). In addition, we would want to know about the types of researchers hired (and not hired) in each of the periods at elite and standard universities. We might also attempt to try to examine any unintended consequences of the policy change, or even intended consequences, such as determining whether reproducibility alone is improved in 10 or 20 years by conducting a study similar to that conducted by the Open Science Collaboration (2015).

Some core limitations of this approach of studying time periods before, during, and after a policy change include not being able to pinpoint the exact causes of creativity in different time periods given history, cohort effects, or other potential intervening factors. It might also be difficult to determine which aspects of the open-science revolution and related policies are the exact cause(s) of a shift in creativity. However, we think it would be informative to conduct such a study to understand research creativity in psychology in recent history and into the future, and we encourage many other approaches that build on and/or are completely different from our ideas.

Going through this process of even attempting a "pre-preregistration" was an instructive learning experience for us and showed us how challenging it will be to determine norms and policies for the wide diversity of preregistered research plans. One thing this simulation shows us is that some research does not fit well into the standard PR or RR mold. PR or RR has primarily been developed and used to address concerns regarding research conducted using the experimental paradigm

or method. We should make a concerted effort, as other scholars have discussed, to ensure that we encourage all kinds of approaches—especially outside the experimental paradigm, such as the initial study we have proposed here—to improve the creativity and robustness of the field.

Looking to the Future of Psychological Science

Daniel J. Boorstin noted that "the greatest obstacle to discovery is not ignorance—it is the illusion of knowledge" (Krucoff, 1984). It is a scary possibility for our field, but also a humbling one: Not all of our "established" findings may be as strong as we wish, and some or even many of them may not be solid findings. Creativity in science may be just as much about removing what is incorrect or labeling findings honestly (Pashler & De Ruiter, 2017) as it is about ensuring that a process is in place to help psychology build a cumulative science (Cronbach, 1975).

Feynman (1974), when speaking about social science practiced many years ago, said, "I call these things Cargo Cult Science, because they follow all the apparent precepts and forms of scientific investigation, but they're missing something essential, because the planes don't land" (para. 11). Even under the credibility revolution, the planes may not always land. There may be unforeseeable negative and positive unintended consequences, as Kaufman and Glăveanu (2018) note, and we may need to address them. The next generations of psychological scientists may find new ways to "disrupt" and innovate in our field. Our field is still growing, and that is a good thing.

Academia is notoriously hard to change. The fact that this change is happening and that it is happening rapidly is actually quite remarkable. In any policy change this large, we will certainly lose some things and gain some things. However, given that the way science has been practiced in the past seems to not be working too well, not just in psychology but also in other fields, perhaps it is worth a shot to shake things up and see what happens. We can evaluate the policy change and figure out if it works. The next generation of psychological scientists, the students reading this symposium, will experience the consequences. Of course, our hope is that these policies will evolve on the basis of evidence, and that psychological science not only will be practiced with rigor, with the goal of moving toward a collective building of empirical truth, but also will shift toward an incentive structure that rewards young scientists for their creativity. Hopefully the new era of open science will encourage all kinds of minds to choose to enter under this "new" system, make their homes in our discipline, and continue to rebuild it for future generations.

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References

Brainerd, C. J., & Reyna, V. F. (2018). Replication, registration, and scientific creativity. *Perspectives on Psychological Science*, *13*, 428–432. doi:10.1177/1745691617739421

Chambers, C. (2017). *The seven deadly sins of psychology: A manifesto for reforming the culture of scientific practice*. Princeton, NJ: Princeton University Press.

Cronbach, L. J. (1957). The two disciplines of scientific psychology. *American Psychologist*, *12*, 671–684. doi:10.1037/h0043943

Cronbach, L. J. (1975). Beyond the two disciplines of scientific psychology. *American Psychologist*, *30*, 116–127. doi:10.1037/h0076829

Doyen, S., Klein, O., Pichon, C.-L., & Cleeremans, A. (2012). Behavioral priming: It's all in the mind, but whose mind? *PLOS ONE*, *7*(1), Article e29081. doi:10.1371/journal.pone.0029081

Dunnette, M. D. (1966). Fads, fashions, and folderol in psychology. *American Psychologist*, *21*, 343–352. doi:10.1037/h0023535

Feynman, R. P. (1974). *Cargo cult science: Some remarks on science, pseudoscience, and learning how to not fool yourself*. Retrieved from http://calteches.library.caltech.edu/51/2/CargoCult.htm

Fiedler, K. (2018). The creative cycle and the growth of psychological science. *Perspectives on Psychological Science*, *13*, 433–438. doi:10.1177/1745691617745651

Fortunato, S., Bergstrom, C. T., Borner, K., Evans, J. A., Helbing, D., Milojevic, S., . . . Barabasi, A.-L. (2018). Science of science. *Science*, *359*, Article eaao0185. doi:10.1126/science.aao0185

Frankenhuis, W. E., & Nettle, D. (2018). Open science is liberating and can foster creativity. *Perspectives on Psychological Science*, *13*, 439–447. doi:10.1177/1745691618767878

Gelman, A. (2016, September 21). *What has happened down here is the winds have changed* [Blog post]. Retrieved from http://andrewgelman.com/2016/09/21/what-has-happened-down-here-is-the-winds-have-changed/

Grand, J. A., Rogelberg, S. G., Banks, G. C., Landis, R. S., & Tonidandel, S. (2018). From outcome to process focus: Fostering a more robust psychological science through registered reports and results-blind reviewing. *Perspectives on Psychological Science*, *13*, 448–456. doi:10.1177/1745691618767883

Hardwicke, T., Mathur, M., MacDonald, K., Nilsonne, G., Banks, G., Kidwell, M., . . . Frank, M. (2018). Data availability, reusability, and analytic reproducibility: Evaluating the impact of a mandatory open data policy at the journal *Cognition*. Retrieved from https://osf.io/preprints/bitss/39cfb/

Kaufman, J. C., & Glăveanu, V. P. (2018) The road to uncreative science is paved with good intentions: Ideas, implementations, and uneasy balances. *Perspectives on Psychological Science*, *13*, 457–465. doi:10.1177/1745691618753947

Krucoff, C. (1984, January 29). The 6 o'clock scholar. *The Washington Post*. Retrieved from https://www.washingtonpost.com/archive/lifestyle/1984/01/29/the-6-oclock-scholar/eed58de4-2dcb-47d2-8947-b0817a18d8fe

Lindsay, D. S., Simons, D. J., & Lilienfeld, S. O. (2016, December). Research preregistration 101. *APS Observer*. Retrieved from https://www.psychologicalscience.org/observer/research-preregistration-101

Marcus, A., & Oransky, I. (2018, February 14). Meet the "data thugs" out to expose shoddy and questionable research. *Science*. Retrieved from http://www.sciencemag.org/news/2018/02/meet-data-thugs-out-expose-shoddy-and-questionable-research

Meehl, P. E. (1978). Theoretical risks and tabular asterisks: Sir Karl, Sir Ronald, and the slow progress of soft psychology. *Journal of Counseling and Clinical Psychology*, *46*, 806–834. doi:10.1037/0022-006X.46.4.806

Merton, R. K. (1979). *The sociology of science: Theoretical and empirical investigations*. Chicago, IL: University of Chicago Press.

Nelson, L. D., Simmons, J., & Simonsohn, U. (2018). Psychology's renaissance. *Annual Review of Psychology*, *69*, 511–534. doi:10.1146/annurev-psych-122216-011836

Nosek, B. A., Ebersole, C. R., DeHaven, A. C., & Mellor, D. T. (2018). The preregistration revolution. *Proceedings of the National Academy of Sciences, USA*, *115*, 2600–2606. doi:10.1073/pnas.1708274114

Open Science Collaboration. (2015). Estimating the reproducibility of psychological science. *Science*, *349*, aac4716. doi:10.1126/science.aac4716

Pashler, H., & De Ruiter, J. P. (2017, September). Taking responsibility for our field's reputation. *APS Observer*. Retrieved from https://www.psychologicalscience.org/observer/taking-responsibility-for-our-fields-reputation

Pennycook, G., & Thompson, V. (2018). An analysis of the Canadian cognitive psychology job market (2006–2016). *Canadian Journal of Experimental Psychology*. Advance online publication. Retrieved from https://psyarxiv.com/mxa35

Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant. *Psychological Science*, *22*, 1359–1366. doi:10.1177/0956797611417632

Simonton, D. K. (2004). *Creativity in science: Chance, logic, genius, and zeitgeist*. Cambridge, England: Cambridge University Press.

Simonton, D. K. (2017). Varieties of (scientific) creativity: A hierarchical model of domain-specific disposition, development, and achievement. *Perspectives on Psychological Science*, *4*, 441–452. doi:10.1111/j.1745-6924.2009.01152.x

Steen, L. A. (1988). The science of patterns. *Science*, *240*, 611–616. doi:10.1126/science.240.4852.611

Stumpf, H. (1995). Scientific creativity: A short overview. *Educational Psychology Review*, *7*, 225–241. doi:10.1007/BF02213372

Vazire, S. (2018). Implications of the credibility revolution for productivity, creativity, and progress. *Perspectives on Psychological Science*, *13*, 411–417. doi:10.1177/1745691618751884

Wagenmakers, E.-J., Dutilh, G., & Sarafoglou, A. (2018). The creativity-verification cycle in psychological science: New methods to combat old idols. *Perspectives on Psychological Science*, *13*, 418–427. doi:10.1177/1745691618771357

Wagenmakers, E.-J., Wetzels, R., Borsboom, D., & van der Maas, H. L. J. (2011). Why psychologists must change the way they analyze their data: The case of psi: Comment on Bem (2011). *Journal of Personality and Social Psychology*, *100*, 426–432. doi:10.1037/a0022790