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**Preregistration Is Neither Sufficient nor Necessary for Good Science**

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Author Note

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# PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

## Abstract

To address widespread perceptions of a reproducibility crisis in the social sciences, a growing number of scholars recommend the systematic preregistration of empirical studies. The purpose of this essay is to contribute to an epistemological dialogue on the value of preregistration in consumer research by identifying the limitations, drawbacks, and potential adverse effects of a preregistration system. After a brief review of some of the implementation challenges that commonly arise with preregistration, we raise three levels of issues with a system of preregistration. First, we identify its limitations as a means of advancing consumer knowledge, thus questioning the sufficiency of preregistration in promoting good consumer science. Second, we elaborate on why consumer science can progress even in the absence of preregistration, thereby also questioning the necessity of preregistration in promoting good consumer science. Third, we discuss serious potential adverse effects of preregistration, both at the individual researcher level and at the level of the field as a whole. We conclude by offering a broader perspective on the narrower role that preregistration can play within the general pursuit of building robust and useful knowledge about consumers.

*Keywords:* preregistration, open science, reproducibility crisis, consumer research

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

Over the past few years, several events have raised the specter of a reproducibility crisis across the social sciences, affecting fields as varied as psychology, economics, and political science. These events include well-publicized reports of large-scale failures to replicate previous empirical findings (Camerer et al., 2018; Open Science Collaboration, 2015); reported evidence of widespread use of “questionable research practices” (QRPs; John, Loewenstein, & Prelec, 2012); dramatic demonstrations of the impact of researchers’ undisclosed degrees of freedom on the possibility of reporting false-positive results (Simmons, Nelson, & Simonsohn, 2011); and multiple revelations of shocking scientific fraud (e.g., Bhattacharjee, 2013; Singal, 2015). Similar concerns have affected other scientific fields including medicine, biology, and even physics (National Academies of Sciences, Engineering, & Medicine, 2019). As an applied discipline that is closely linked to psychology and other social sciences, consumer research and consumer psychology have not been immune to this crisis. Failures to replicate consumer research findings are receiving increased attention (see Simmons & Nelson, 2019); there is suspicion of widespread “p-hacking” among consumer researchers (Meyvis & van Osselaer, 2018; Simonsohn, Nelson, and Simmons, 2014); and in recent years, a handful of articles in our major journals had to be retracted due to scientific malpractice.

In light of this crisis, a variety of proposals have been advanced to promote research that is more transparent, more reliable, and (presumably) more veridical. These proposals include the required sharing of comprehensive methodological details for journal submission (a path followed by the *Journal of Consumer Psychology* as early as fall 2013, during the first author’s term as President of the Society for Consumer Psychology), the promotion of open access to research data, the facilitation and incentivization of replications, encouragement for multi-lab collaborations, revamping of doctoral training, and structural changes in academic reward

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

systems (Ioannidis, 2014; National Academies of Sciences, Engineering, & Medicine, 2019; Nosek et al., 2015). Some researchers have also proposed new methodologies for detecting low reproducibility and the use of QRPs in reported research (Brown & Heathers, 2017; Brunner & Schimmack, 2020; Simonsohn, Nelson, & Simmons, 2014; but see McShane, Böckenholt, & Hansen, 2016, 2020, for a critique).

One particular proposal in the quest for a greater reproducibility of social science research is the preregistration of studies. In preregistration, researchers archive critical aspects of their studies—such as the hypotheses, design, procedure, and analysis plan—*before* the studies are actually conducted (van't Veer & Giner-Sorolla, 2016). The registration is typically done through a time-stamped document uploaded on an external repository such as the American Economic Association Registry, the Open Science Framework (OSF), or aspredicted.org. The registered document eventually becomes part of the publicly available scientific record, thus potentially enabling a comparison between the original study plan and subsequent reports of the study. Preregistration creates a stronger distinction between prediction and postdiction, and between exploratory and confirmatory analyses (Nosek et al., 2018; Wagenmakers et al., 2012). In theory, it should (a) discourage the selective reporting of results, (b) forestall the nontransparent use of QRPs, and (c) prevent the presentation of post hoc hypotheses informed by the results as though they were a priori hypotheses—a practice known as HARKing (“Hypothesizing After the Results are Known,” Kerr, 1998). As a result, from a science perspective, preregistration should logically foster a more accurate and reliable scientific record by reducing the file-drawer problem, improving the interpretability of statistical tests, and enabling a more precise assessment of the proposed theorizing. In addition, the process of preregistration is thought to benefit the researchers themselves by prompting them to clarify their

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

theoretical predictions in advance and develop a more thorough research plan (Olken, 2015; van't Veer & Giner-Sorolla, 2016). Another added value of preregistration for researchers is the presumably greater credibility of preregistered studies and results (Toth et al., 2020). Moreover, in the case of research sponsored by a party with a vested interest in the results of the study (e.g., a funding agency, a corporate sponsor), preregistration provides the researchers some protection against undue pressure from the sponsoring party (Olken, 2015).

The principle of preregistration as a standard practice for empirical social sciences is rapidly gaining considerable support. One of the most frequently cited recent articles in psychology is one that champions the use of preregistration (Nosek et al., 2018). Across various social sciences, dozens of other articles, chapters, editorials, and working papers have similarly advocated the use of preregistration (Chambers et al., 2014; Cockburn, Gutwin, & Dix, 2018; Monogan, 2015; Moore, 2016; van Veer't & Giner-Sorrola, 2016; Wagenmakers et al., 2012; West, 2020; Yamada, 2018). A recent search on the OSF registry yielded more than 50,000 registrations. To incentivize and reward the preregistration of studies, various journals, including *Psychological Science*, now issue special recognitions in the form of "Open Science Badges" to published articles involving preregistered studies. Proponents of the Open Science Movement suggest that journals should even consider mandating the preregistration of studies submitted for publication (Nosek et al., 2015).

Given other disciplines' growing enthusiasm for preregistration as a key remedy for the reported reproducibility crisis in the social sciences, an important question for our field is to what extent preregistration should be embraced as a standard scientific practice for consumer research. This essay addresses this question by offering a considered alternative viewpoint about the merits of preregistration in advancing the science of the consumer. There is no question that

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

transparency and general reproducibility are strong imperatives for consumer science. We largely agree with the broader goals of the Open Science Movement. However, before preregistration can be recommended—if not mandated—as a normative scientific practice within our field, it is critical to have an informed discussion of not only the promises of preregistration (Nosek et al., 2018; van't Veer & Giner-Sorolla, 2016; Simmons, Nelson, & Simonsohn, this issue) but also its limitations and potential drawbacks, which have thus far received less attention in the literature.

In this essay, we start by briefly reviewing some of the implementation challenges that commonly arise with preregistration, before turning to what we see are more substantive concerns. First, we discuss the epistemological limits of preregistration as a means of advancing consumer science, thus questioning the sufficiency of preregistration in promoting good consumer science. Next, we discuss why consumer science can meaningfully progress even in the absence of preregistration, thereby also questioning the necessity of preregistration in promoting good consumer science. We then discuss potential adverse effects of preregistration, both at the individual researcher level and at the level of the field as a whole. We conclude by offering a broader perspective on the role that preregistration can play within the general pursuit of building robust and useful knowledge about consumers.

### **Implementation Challenges**

While registries such as OSF or aspredicted.org attempt to make the process of preregistration as costless and seamless as possible (see Simmons et al, in this issue), preregistration is not without friction. Although on their own these frictions are not sufficient reason to oppose the principle of preregistration, they should be kept in mind in our field's assessment of the costs and benefits of preregistration. One of these frictions is the overall

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

administrative cost that a preregistration system entails in terms of (a) researchers' time for preparing and submitting the preregistration documents; (b) gatekeepers' (editors and reviewers) time for evaluating the alignment between submitted manuscripts and corresponding preregistrations; and (c) the substantial financial cost of building and maintaining an infrastructure such as OSF that is capable of supporting preregistration on a large scale. In addition, it can be challenging for a researcher interested in investigating preregistered studies to properly interpret preregistration records with the current registration infrastructure. For example, published study numbers (e.g., "study 4") do not necessarily correspond to their numbering on the preregistration site; search of a particular study protocol often retrieves multiple versions of similar protocols; or preregistration records may lack sufficient details (Claesen et al., 2019; Ofosu & Posner, 2020).

A more substantive challenge in implementing preregistration as a normative principle is that the pre-specification of theoretical predictions and analytical plans is much easier for simple bivariate models (e.g., effect of a manipulation of defaults on the probability of renewing a subscription; correlation between income and willingness to donate one's time to a social cause) than it is for more complex conceptual models. For example, typical consumer studies involve not only a cause-and-effect prediction (e.g., strong message claims increase persuasion) but also theoretical propositions about hypothesized mediating mechanisms (e.g., more favorable cognitive responses and few counterarguments) and moderating factors (e.g., involvement and domain expertise). Full specification of an analytical plan becomes rapidly more complex as the number of variables and constructs increases in a theoretical model (Olken, 2015). As an illustration, the latest version of Hayes's popular PROCESS macro for mediation analysis now includes no fewer than 80 different statistical models.



## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

Another challenge is the fact that a substantial proportion of consumer knowledge is generated through the paradigm of Consumer Culture Theory (CCT), which emphasizes the contextual, symbolic, and experiential dimensions of consumption along its entire cycle, from acquisition to disposition (Arnould & Thompson, 2005). By nature, CCT research is inherently inductive, constructive, and interpretative, and thus less amenable to preregistration. Given the important role that CCT research plays in the generation of knowledge and identity of our field, which prides itself for its multidisciplinary orientation (MacInnis & Folkes, 2010), it is problematic to envision preregistration as a scientific norm if it would exclude such a substantial portion of the field.

Another source of friction is that even if a legitimate analysis plan was previously registered, during the review process editors and reviewers may still request that authors perform additional (unregistered) analyses of the data. Such requests would seem to logically defeat one of the main purposes of preregistration, which is a pre-commitment to a particular analytical plan (Ofosu & Posner, 2019). A related issue arises when the study rests on analyses of preexisting data (e.g., a study of Amazon reviews, a study of Airbnb listings, results of older, published surveys or experiments). With such studies, it is difficult for researchers to convince outsiders that any preregistered hypotheses or analyses were actually generated without examining the data first (Christensen & Miguel, 2018; Olken, 2015).

While we do not regard these various types of friction as dispositive with respect to the merits of preregistration, they need to be acknowledged. However, we are more concerned about other, more fundamental issues raised by a policy of preregistration, discussed next.

### **Is Preregistration Sufficient for Good Science?**

Assuming that preregistration could be frictionless—that is, if preregistration were relatively costless, easily accommodating of more complex models, and respected by scholarly gatekeepers—would the resulting research products necessarily be stronger scientific contributions? We have reasons to doubt it.

#### **The Illusion of Transparency**

A major shortcoming of preregistration as a normative standard is that the increased transparency it provides may be more illusory than real. Under present systems of preregistration, there is still substantial room for selective reporting and researchers' degrees of freedom (see Ikeda et al., 2019). For example, researchers may select which particular study or hypothesis to preregister; they may preregister a study only after running a long series of calibration tests that are not reported; they may preregister multiple studies or versions of the same study, then only report a selection; they may intentionally or unintentionally specify the hypotheses to be tested and analyses to be performed in loose terms, hence buying themselves flexibility in subsequent testing and analyses. Moreover, it is not uncommon for published research that was formally preregistered to deviate in material ways from the preregistration (e.g., using different exclusion criteria or a different statistical model than what was preregistered), without proper disclosure (Claesen et al., 2019; Ofosu & Posner, 2019; Vassar et al., 2019). There is even a danger that some unscrupulous researcher would “preregister” a study or an analysis *after* it has been conducted or performed—a form of fraud known as “PARKing” (Preregistering After Results are Known; Yamada, 2018).

The potential discrepancy between the appearance of transparency and foresight that preregistration provides and the actual transparency of reported preregistered research points to

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

the necessity of complementary systems of monitoring and enforcement for the goals of preregistration to be fully realized (Laitin, 2013; Ofose & Posner, 2019). Implementing such systems properly would undoubtedly be costly.

### **The Illusion of Robustness**

In everyday discussions of scientific findings it is not uncommon to use the terms “reproducibility/replicability” and “robustness” interchangeably: A result is deemed “robust” if it is empirically replicable. To the extent that preregistration of a reported finding entails a presumption of greater replicability, it is tempting to regard preregistered findings as probably more robust than similar findings that were not preregistered. We believe that this is a misconception. According to the Merriam-Webster dictionary, the word “robust” means “capable of performing without failure *under a wide range of conditions*” (italic added). The true mark of a “robust” finding, therefore, is not its mere replicability under specified conditions but rather the ability to reproduce it across a reasonable range of conditions (Baribault et al., 2018). Hence, the fact that a particular finding was anticipated under a preregistered protocol does not mean that one can reasonably expect to observe it again and again; it only means that one is likely to observe it *under the specific conditions* of the preregistered protocol. Therefore, preregistration does not necessarily guarantee more robust findings, it only increases our confidence that the specific set of conditions spelled out in the preregistration—the type of participants, design, procedure, stimuli, measures, exclusion criteria, and so on—will likely reproduce the result observed in the preregistered study. To cultivate a more robust body of knowledge about the consumer, other scientific practices such as systematic replications across stimuli and contexts, field testing, and meta-analyses are needed (Christensen & Miguel, 2018; Lewandowski & Oberauer, 2020).

### **Other Inference Fallacies**

In a preregistration system, more credence is typically given to a statistical analysis if it was pre-specified before data collection than if it was adopted after the data have been collected, considering that post-data collection analyses are open to researcher degrees of freedom. This heuristic can be misleading. From a science standpoint, what should logically matter is not the point in time at which a particular analysis is selected but whether the analysis is appropriate given the nature of the data and the nature of the prediction being tested (Oberauer & Lewandowski, 2019). From a logical viewpoint, the mere fact that a particular analysis was preregistered does not necessarily elevate it above other possible analyses. One could conceivably preregister analysis plans that are fundamentally flawed. Stated differently, there are often multiple reasonable ways to analyze given data to test predictions. The fact that one particular approach was preselected and registered by the researchers is no guarantee that it is the best approach. A similar statement can be made about theoretical hypotheses (Baron, 2018; Szollosi et al., 2020). In assessing their merits, one should not be overly swayed by whether they were made a priori (and preregistered) or post hoc (see Brush, 1989). One should also weigh whether these hypotheses are theoretically sensible (Oberauer & Lewandowsky, 2019). Imagine that a researcher preregisters a nonsensical hypothesis, along with a proposed experimental plan and set of procedures. Then, by sheer luck, the results of the study turn out to be consistent with this hypothesis. Should the fact that the hypothesis was preregistered increase our confidence in its truth value? Probably not. The fact that empirical results seems to be consistent with a previously formulated hypothesis does not guarantee that the hypothesis is valid. Therefore, one should be wary of placing too much heuristic value on the fact that particular hypotheses and analysis plans are preregistered. In fact, there is even a danger that preregistered studies and

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

results are accepted with a lower level of scrutiny than equivalent studies that were not preregistered, thus raising the risk of false-positive results from preregistered studies (e.g., reviewers and readers indiscriminately believing the results of a severely confounded study because it was preregistered).

### **Reproducible but Pointless Science**

A final reason for doubting that preregistration would necessarily raise the quality of consumer science is that the reproducibility of empirical findings does not guarantee that these findings are actually useful or contribute significantly to our knowledge. As one of us argued a few years ago, an ample proportion of consumer research—possibly as high as 70 percent according to citation data—is rather pointless (Pham, 2013). Regardless of their reproducibility (or lack thereof), too many consumer studies fail to make genuine scientific contributions because of (a) a narrow conception of the substance of consumer behavior, (b) limited theoretical perspectives, (c) a predilection for research convenience, (d) a lack of generalizability, and (e) weak applicability to genuine business and policy problems. Therefore, to elevate the quality of consumer science, it is not sufficient to encourage the preregistration of consumer studies as a means to enhance their reproducibility. Other dimensions of scientific merit, besides reproducibility, need to be considered as well.

### **Is Preregistration Necessary for Good Science?**

The preceding section explains why preregistration policies may not be sufficient for good consumer science. In this section, we explore whether such policies are even necessary for advancing consumer knowledge. In other words, can good consumer science emerge without preregistration? We believe it can.

### **The Replicability Crisis in Perspective**

As previously mentioned, the issue of preregistration (and the broader Open Science Movement) was originally motivated by widespread perceptions of a replicability crisis in the social sciences. These perceptions are consequential and definitely need to be addressed. However, an informed discussion of the pros and cons of preregistration as a proposed remedy for the replicability crisis needs to acknowledge the existence of other evidence suggesting that the crisis may not be as severe as implied by studies typically cited in Open Science research. For example, whereas some failures to replicate psychological findings have received a great deal of publicity (Open Science Collaboration, 2015), other studies suggest a greater rate of replicability (Gilbert et al., 2016; Klein et al., 2014). Similarly, whereas one highly cited study indicates widespread use of QRPs (John et al., 2012), a follow-up study suggests that such uses may be more limited (Fiedler & Schwarz, 2015). Finally, whereas Simmons and colleagues' (2011) highly influential computer simulations point to massive distortions of test statistics when QRPs are used, recent empirical estimates of the actual impact of self-serving analyses suggest more modest degrees of distortion of reported test statistics in recent consumer studies (see Krefeld-Schwalb & Scheibehenne, 2020; see also Brodeur et al., 2016 and Olken, 2015, for related findings in economics). Therefore, while concerns about the reproducibility of social sciences must be addressed, one should be careful not to overprescribe certain policies out of a possibly miscalibrated diagnosis of the actual severity of the crisis.

### **The Distinction between Exploratory and Confirmatory Research.**

Much of the argument for preregistration as a normative practice rests on a sharp distinction between exploratory and confirmatory research (Wagenmakers et al., 2012). In exploratory research, data may be explored extensively, analysis methods selected as data are

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

explored, and hypotheses formulated as analysis results emerge. Hence, conventional tests of statistical significance are not really meaningful. In contrast, in confirmatory research, clear hypotheses are formulated upfront, independent of the data used to test the hypotheses, and specific analyses are determined beforehand, thus enabling a proper interpretation of the resulting statistical tests (de Groot, 1956/2014). Exploratory research is commonly seen as an initial step for confirmatory tests, which are generally regarded as having higher evidentiary status.

We believe, however, that within the realm of consumer research—and indeed much of social science research—the extent to which exploratory and confirmatory research can be distinguished is debatable (Alba, 2011). In fact, most consumer research should probably be regarded as exploratory for multiple reasons. First, most “theories” in consumer research can be characterized as “low-level” in that they pertain to relations that are very local, specific, and context-dependent (e.g., effects of specific emotion X on willingness to donate to charity). Considering the almost infinite set of research questions that could possibly be investigated at this level of specificity (e.g., effects of emotions Y, W, Z, etc., on willingness to pay for luxuries, necessities, national brands, etc., or on consumption of fattening food, sugary drinks, social media, etc.), the very selection of these narrowly defined questions exposes the fundamentally exploratory nature of most consumer-research inquiries. Second, and relatedly, in typical empirical consumer studies, the predictive power of the proposed hypotheses is fairly low, accounting for about five percent or less of the explainable variance (Krefeld-Schwab & Scheibehenne, 2020; Peterson, Albaum, & Beltramini, 1985). Therefore, individual findings provide at best a very limited exploration of the phenomena of interest. Third, the hypotheses typically tested in consumer research are at most directional (e.g., “Y will be higher in condition

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

A than in condition B”), rather than more fully parameterized (e.g., “a \$1,500 subsidy of electric car purchases will increase nationwide purchases of such vehicles by 5 percent within the next 12 months”). Given the mere directionality of most consumer-behavior hypotheses, tests of these hypotheses do not have the same confirmatory value as those of the more fully parameterized predictions commonly made in the natural sciences. Finally, although consumer research is often simplistically portrayed as a largely hypothetico-deductive enterprise (see Lynch et al., 2012, for a discussion), in reality much of consumer research is highly inductive in nature, and reasonably so (Alba, 2011; Holbrook & O’Shaughnessy, 1988).

Therefore, rather than advocating preregistration as a means to foster more falsification-oriented, confirmatory research, it may be more realistic and productive to simply acknowledge that most consumer research is largely exploratory, thus limiting the epistemological value of traditional falsificationism. If that is the case, one should put less emphasis on and faith in the “significance” of the reported statistical tests and instead treat observed test statistics as no more than rough indicators of goodness of fit of the data. From this perspective, most consumer research should be regarded as hypothesis-generation endeavors, rather than genuine theory tests. Under this more humble but realistic view of what most consumer research is, the usefulness of preregistration is limited, except for certain types of studies identified later in this essay.

### **Exploration and Serendipity in Scientific Progress**

That most consumer research is largely exploratory does not mean that it is not scientific and conducive to advances in our understanding of consumers (see Holbrook & O’Shaughnessy, 1988). We should remember that exploration plays a critical role in scientific progress across all disciplines. Within our field, some of the most significant contributions to our understanding of consumer behavior emerged from studies that could be regarded as mostly exploratory, including



## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

Iyengar and Lepper's (2000) studies of the choice-overload phenomenon, Aaker's (1995) studies of brand personality, and Goldstein, Cialdini, and Griskevicius's (2008) field studies of the effectiveness of various recycling appeals. These papers have been very influential not because of their conclusive nature, but because they each advanced important theoretical hypotheses that prompted considerable subsequent investigations of a more confirmatory nature.

Although it is often argued that preregistration does not preclude exploratory analyses, one of its key drawbacks, if not its main drawback, is that it discourages exploration by insisting on a sharp distinction between exploratory and confirmatory findings and granting evidentiary status only to confirmatory results. Notwithstanding a professed tolerance for exploration, a preregistration regime effectively suppresses exploration in two ways. First, it makes researchers apprehensive to undertake analyses of the data that have not been preregistered or to report any post hoc conceptualization of the observed data, even if such additional analyses and post hoc theorizing would be informative despite their exploratory nature. Second, by placing much higher evidentiary status on confirmatory as opposed to exploratory research, a preregistration regime undermines researchers' incentive to undertake and report exploratory investigations—a type of research that is critical for scientific progress. As Olken (2015, p. 72) observed in the field of economics, “if journal editors were to restrict themselves to publishing studies based on the limited, pre-specified, confirmatory parts of analysis, and relegating exploratory analysis to second-tier status, a substantial amount of knowledge would be lost.” One should not forget that scientific progress does not rest solely on the reproducibility of individual findings but also on the collective generation of a large volume of tentative findings, a small subset of which will eventually be found to be useful and withstand replication and generalization (Shiffrin, Borner, and Stigler, 2018).

### **Other Paths for Reproducibility**

A final reason why preregistration may not be necessary for scientific progress is that even if we assume that mere reproducibility is paramount, preregistration is not the only means to foster transparency and reproducibility. As proponents of Open Science have noted (Ioannidis, 2014; Nosek et al., 2015), a variety of methods can improve the transparency and reproducibility of research findings, including (a) better research training; (b) incentives for replications (especially independent replications); (c) sharing of detailed study protocols and materials; (d) open-access to study data and associated codes; (e) proper disclosure and containment of conflicts of interest; and (f) greater standardization of methodologies (procedures and analyses). A different type of proposal (g), which focuses on the transparency of the statistical analyses, is to encourage “multiverse analyses” of study data (Steenen et al., 2016). In a multiverse analysis, instead of reporting a single, preferred analysis of the data—which typically involves multiple reasonable choices in terms of variable coding, data transformation, and exclusion criteria—the researchers report all analyses across an entire set of plausible options for each choice made (which may entail dozens if not hundreds of combinations). The reported summary results show the frequency distribution of the key test statistics across all combinations of plausible analytical choices, thereby giving a more accurate picture of the robustness of the findings.

Within our field, transparency efforts have primarily focused on (c), with the requirement of extensive methodological appendices for journal submission. In addition, there is a movement toward providing greater access to study data, at least during the review process, which is consistent with (d). For example, under the latest policies of the *Journal of Consumer Research*, revised in July 2020, submitting authors are required to provide the Journal access to the data underlying their research and preserve the Journal’s access to these data up to seven years after

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

publication of the research. We also believe that greater collective awareness of matters of reproducibility has resulted in improved training (a) and greater standardization of methodological practices (f). Replication efforts and incentives (b) are still greatly lacking, and multiverse analyses remain virtually unseen. Nevertheless, in assessing the merits of preregistration in fostering more robust consumer science, one should consider whether some of these other methods could achieve the same goals more efficiently and effectively. In addition, one should evaluate whether a regime of preregistration might introduce some unintended adverse effects, as discussed next.

### **The Risk of Adverse Effects of Preregistration**

The preceding sections suggest that while in theory preregistration offers several benefits, in reality it may be neither sufficient nor necessary for good consumer science. In this section, we discuss several potential adverse consequences that a preregistration regime may inadvertently produce, thus inviting further caution about the use of preregistration as a scientific norm.

#### **Under-exploration, Loss of Flexibility, and Higher Risk of Type-II Error**

As noted above, one of the major drawbacks that we see in preregistration is that it effectively discourages conceptual exploration, methodological flexibility, and data probing (despite claims to the contrary). With respect to the latter, we believe that sound scientific thinking entails an openness to learn from observed data beyond a mechanical testing of whether the data support some a priori prediction. Any consumer researcher who has conducted an empirical study can attest that the resulting data are often richer and more complex than originally anticipated. It would be a mistake not to attempt to understand what the data teach us,

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

both empirically and conceptually. As Gelman (2014) noted, many of his “most important applied results were interactions that my colleagues and I noticed only after spending a lot of time with our data.” A related problem is that strict abidance to a preregistered study protocol can result in some unfortunate methodological inflexibility. For example, in a recent registered report (a preregistered study that a journal committed to publishing regardless of the results) on the effect of preregistration on trust in the reported research (Field et al., 2020), almost 70 percent of the participants failed the manipulation check and had to be excluded per the authors’ preregistered exclusion rules, leaving as few as 12 participants per cell, thus yielding results that were largely inconclusive. Had this study not been preregistered, the authors would most likely have chosen to simply abort it and revise their manipulations to obtain a more diagnostic test of their hypothesis. An inability or unwillingness to deviate from a preregistered protocol, even in the face of a foreseeable study fiasco, is especially problematic when the study is costly (e.g., a large experiment among paid physicians), cannot easily be repeated (e.g., a field experiment with a large retailer), or cannot be aborted and redone after updating the registered protocol (e.g., participants have already been invited and compensated). If a study is particularly costly, refusing to deviate from a preregistered protocol in spite of new information indicating that the protocol may be fatally flawed seems unreasonably wasteful.

Overall, such reductions in conceptual and empirical exploration, and inflexibility in methodology, while presumably reducing the probability of false-positive results (type-I error), tend to increase the total cost of science and likely increase the probability of genuine findings not being discovered (type-II error; Fiedler, Kutzner, & Krueger, 2012; Laitin, 2013). As Shiffrin and colleagues (2018) recently argued, science advances not just by controlling type-I error, but by fostering conditions that are conducive to the generation of a large number of findings, a

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

small subset of which—the “right tail” of the distribution—will prove to be impactful (see Fiedler, 2018, for related arguments).

### **Undesirable Changes in Types of Research Conducted**

A second foreseeable adverse effect of preregistration is a qualitative change in the type of studies that researchers elect to pursue. By design, a preregistration system encourages the pursuit of studies that have a higher probability of confirming the researcher’s expectations. This incentivizes empirical research for which the effect is expected to be either relatively large (in the case of studies designed to support a given hypothesis) or particularly small (in the case of studies designed to disprove a certain hypothesis). In either of these cases, the selected research inquiry tends to be one that is “safer” and “low risk” in that it conforms to strong a priori expectations. Examples would be research testing hypotheses that are trivial or intuitively obvious (e.g., trust increases customer loyalty; consumers from interdependent cultures are more sensitive to social norms), or research testing phenomena that have already been extensively documented (e.g., different negative emotions have different effects on judgment; see Han, Lerner, & Keltner, 2007; Raghunathan & Pham, 1999; Raghunathan, Pham, & Corfman, 2006). Paradoxically, the incentive to pursue only studies with a strong likelihood of confirming the researcher’s a priori expectations encourages research with lesser diagnostic value from a Bayesian’s perspective, in that such studies are implicitly designed to produce results that are unlikely to cause a revision of prior beliefs.

Another likely consequence of a preregistration regime is a gradual shift in methodology toward research that is more easily preregistered and that is inexpensive to redo if necessary (to accommodate repeated registrations, which tends to defeat the very purpose of preregistration). This favors (a) studies with simple designs and predictions, (b) studies with inexpensive

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

respondents or participants (e.g., Mechanical Turks, unpaid student pools), (c) hypothetical, vignette-type studies that can be easily administered online, and (d) effects-oriented studies with limited concern for underlying processes and mechanism. Thus, preregistration may compound the “research-by-convenience” bias that Ferber (1977) originally decried. As one of us pointed out several years ago (Pham, 2013), it is a serious problem for our field if the choice of research questions becomes primarily dictated by methodological convenience rather than by more substantive and theoretical considerations.

Finally, there is a risk that by constantly compelling researchers to think of their work in specific operational terms that can be registered, a regime of preregistration may gradually impoverish the level of conceptualization within our field. Instead of being formulated at a general construct level (e.g., effects of relaxation on monetary valuation; Pham, Hung, and Gorn, 2011), conceptualization may increasingly be formulated at a more operational level (e.g., effects of 10 mins of exposure to a nature-rich video on willingness to pay for products A, B, and C). Even worse, researchers may be encouraged to advance extremely narrow conceptualizations that only pertain to the specifics of their preregistered studies, with little to no relation to the real world—a predicament that we denounce as “theories of studies” (Pham, 2013).

### **Politicization of the Research Enterprise**

A final type of negative externality that a preregistration regime may create is an unhealthy set of power dynamics that would be damaging to our scientific community. First, to the extent that preregistration is interpreted as a heuristic for research integrity and reproducibility, there is a risk that it will be overused as a means of “virtue-signaling.” If preregistration were an error-free signal of research transparency and reproducibility, this would not be a concern. However, as discussed before, there is a serious risk of self-interested distortion

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

of this signal through various means (e.g., multiple parallel registrations, undisclosed deviations from registered protocols). Moreover, one should keep in mind that certain types of studies such as CCT research are inherently less amenable to preregistration. This makes any value judgment of research (and researchers) based solely on preregistration potentially prejudiced. Clearly, not all research that was preregistered should be accepted as valid, and not all research that was not preregistered should be discounted as likely “p-hacked” or unscientific.

As a second concern, to the extent that preregistration increases the cost of doing research—whether legitimately (e.g., spending time to craft a properly detailed protocol, running several pretests to refine a to-be-registered manipulation) or for questionable reasons (e.g., running multiple preregistered variations of the same experiment to report only the ones that “worked”)—such a system may exacerbate disparities between researchers from institutions that are resource-rich (e.g., generous research budgets, large subject pool, good lab facilities) and researchers from institutions where resources are more constrained. Ideally, researchers’ ability to contribute to the scientific record should be determined by the objective quality of their research (e.g., originality, soundness of the theory, rigor of the methodology) rather than by their sheer access to ample research resources.

A third concern is that a regime of preregistration may result in uneven demands for evidence. During the review process, one can envision a scenario in which reviewers and editors elect to set a higher bar in terms of preregistration for certain manuscripts than for others. If the setting of the bar is not genuinely related to the true merit of the research but to some implicit biases, uneven demands for preregistration may introduce inadvertent political biases in the review process. For example, editorial requests for the preregistration of additional studies may be selectively issued if the research challenges the editor’s pet theory, if the findings question a

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

popular ideology (see Kupferschmidt, 2018), or if the researchers are relatively junior or affiliated with less prestigious institutions. Conversely, demands of preregistration might not be raised as much if the research conforms with popular dogmas not actually rooted in scientific evidence (e.g., certain religious beliefs and political ideologies), or if the manuscript is authored by a famous researcher. Similar biases may also operate after publication if readers' assessment of whether some of the studies should or should not have been preregistered is tacitly influenced by considerations that are unrelated to the true nature or objective merit of the research (e.g., author reputation, ideological implications of the findings).

Note that from a science perspective, it does make sense to set a higher bar for research that challenges established scientific knowledge (e.g., research that challenges classic notions such as loss aversion, consideration effects, or affect-congruent evaluation). A problem arises, however, if a preregistration bar is selectively set higher for research that challenges beliefs that are ideological rather than actually rooted in scientific evidence, including religious beliefs, political beliefs, and radical activist dogmas of all types. Therefore, we should beware of the use of preregistration requirements as a disguised means to politically advance pseudo-scientific ideologies.

In summary, not only do legitimate questions exist as to the necessity and sufficiency of preregistration as a means to elevate consumer behavior research, there is a nontrivial risk that a blind embrace of preregistration as a norm may have unintended adverse effects on the quality of future consumer science.



**Conclusion: Looking Forward**

There is no question that transparency and reproducibility are critical for scientific progress. Nor is there any doubt about the general desirability of a more “open” science, which we applaud. What really needs to be determined are the best means to achieve such transparency and reproducibility as well as to elevate the quality of the science conducted within our field and the usefulness of the knowledge generated. To this end, it is important not only to appreciate the promises of preregistration—more trustworthy test statistics; more transparent methods; mitigation of the file-drawer problem; clearer separation of exploratory versus confirmatory analyses and of a priori versus post hoc theorizing; more carefully considered research plans—but also to consider its limitations, drawbacks, and potential adverse effects, not just from the perspective of transparency and reproducibility but for the ultimate goal of advancing consumer science. This essay contributes to this dialectic by highlighting and synthesizing the main challenges of preregistration as a scientific norm within our field.

Table

*Hypothetical State of Empirical Consumer Research*

	<b>Truth Value of the Finding</b>		<b>Row Total</b>
	<b>True</b>	<b>False</b>	
<b>Finding is interesting/useful</b>	12% [a]	18% [b]	30%
<b>Findings is <u>not</u> interesting/useful</b>	28% [c]	42% [d]	70%
<b>Column Total</b>	40%	60%	

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

To put this issue in perspective it is useful to consider an imaginary  $2 \times 2$  table in which the columns separate empirical findings that are “true” and those that are “false” (type-I errors); and the rows separate findings that are interesting, relevant, and useful from those that are not [see Table]. Let’s assume— based, for instance, on various replication rates observed in psychology—that in consumer research, 40% of the published findings are “true” and 60% are “false.” Let’s further assume that of all published findings in consumer research, 30% are interesting, relevant, and useful, and 70% are not (Pham, 2013), and, for the sake of simplicity, that the interestingness of the findings is independent of their truth value. There would be four classes of studies: [a] those that are true and interesting; [b] those that are interesting but false-positives; [c] those that are true but not interesting; and [d] those that are false and not interesting. One could argue that what really matters for advancing consumer science is the *absolute* (not relative) size of class [a]: the greater the number of observed consumer findings that are both true and useful, the greater progress in our knowledge of the consumer (see Shiffrin et al., 2018). Viewed from this perspective, one problem with an overemphasis on preregistration is that it focuses almost exclusively on reducing the number of false-positive results ( $[b] + [d]$ ) as a means to improve the ratio of true-positive to false-positive results ( $([a]+[c])/([b] + [d])$ ). From a total-cost-of-science perspective, such a system is very inefficient in that a great deal of resources are wasted in filtering the large proportion of studies (70%) that are uninteresting and irrelevant, and therefore unlikely to have any material impact on the scientific record. In this respect, a more robust system of replication would be more efficient in that it would focus only on assessing the truth value of the smaller set of findings that are deemed to be interesting enough to warrant further verification (Lewandowski & Oberauer, 2020). In addition, as discussed previously, a system of preregistration—that discourages exploration, reduces

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

flexibility, and increases the total cost of research—may reduce the overall rate of true discovery (the absolute size of cell [a]) by increasing the probability of type-II errors and slowing down the pace of scientific production.

As a complement to our reflections on this issue, we conducted a brief anonymous survey of all members of the current editorial boards of the *Journal of Consumer Research* and the *Journal of Consumer Psychology*, who can be considered the main scientific gatekeepers of our field. Of the 289 editorial board members contacted for the survey, 181 (62.6%) responded (more details in the methodological web appendix). After a brief introduction of the survey clarifying the definition of preregistration, respondents were asked to report their “honest opinion about preregistration for consumer research studies” on a -3 (strongly opposed) to +3 (strongly in favor) 7-point scale. As shown in the Figure, reported opinions about preregistration were clearly mixed, with a nontrivial segment of the editorial board members moderately to strongly opposed (24%), and a similar-sized segment moderately to strongly in favor (25%), and 51% somewhat ambivalent on this issue. When asked whether preregistration should “be mandated for empirical consumer research (whether experimental, survey-based, ethnographic, web-scraping, etc.),” the vast majority of respondents, 90.6%, answered “no,” and only 9.4% answered “yes.” A review of the open-ended comments that respondents provided to explain their position echoed many of the arguments synthesized in this essay. The most frequently mentioned reservation against a mandated regime of preregistration is that such a system can be easily gamed (what we call the “illusion of transparency”), a concern raised by 30% of the respondents. Other major arguments against preregistration include the importance of exploration (mentioned in 17% of the responses), a loss of flexibility (13% of the responses), and the availability of other paths for achieving transparency and reproducibility (13% of the

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

responses). A nontrivial number of respondents (12%) also lamented that a preregistration regime fosters an unhealthy culture of mistrust and policing in the field. Overall, the results of this survey are consistent with the view that our field should be cautious about widely adopting preregistration as a norm.

Moving forward, we offer the following cautions and recommendations:

1. Preregistration can play a useful role in scientific progress but should not be regarded as the primary solution for issues of transparency and reproducibility. We believe that other solutions such as the full sharing of methodological details, more open access to study data, and the incentivization of self and independent replication would be more productive for the field. Multiverse analyses of data (Steegeen et al., 2016) can also help address problems of researchers' degrees of freedom.
2. In evaluating the quality of research, we should not just focus on the transparency and apparent reproducibility of reported findings but also their interestingness, usefulness, and relevance. Correspondingly, incentives in our field should reward not just "Open Science" but useful, meaningful, and impactful science (Pham, 2013). For example, there should be less emphasis on the sheer counting of number of "A-level" publications that is so popular in our field and more emphasis on indicators of genuine lasting contribution. At any rate, preregistration should not be used as a lazy heuristic for research quality.
3. One context in which we see greater value for preregistration is in application-oriented, "clinical-type" research that is meant to directly inform managerial and policy decisions (e.g., effects of type of promotion X on probability of purchase; effects of intervention Y on mask-wearing during the COVID-19 pandemic). For such research,

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

accurate estimates of effect-size and a detailed understanding of the stimulus- and context-sensitivity of the predicted effects are critical. Another setting in which preregistration would be helpful is in testing hypotheses that challenge well-established scientific beliefs (not to be confused with ideological dogmas, which demand vigilant scrutiny). In such settings, classic falsificationism makes sense. Moreover, from a Bayesian perspective, greater diagnosticity of the data is needed to overcome conflicting theoretical priors.

4. More generally, we think that the field would be better served by a lower emphasis on significance testing (Amrhein, Greenland, & McShane 2019), and, as Gelman (2017) proposes, more continuous assessment of research contributions, both pre-publication (e.g., conference presentations, working paper posting) and post-publication (e.g., data and code sharing). Indeed, many of the transparency and reproducibility problems that preregistration is meant to address arise because our current system mostly incentivizes researchers based on discrete, all-or-none outcomes such as “significant/not significant,” “replicated/not replicated,” “accepted/rejected at a major journal,” etc.
5. Finally, as repeatedly stated by several journal editors (Kirmani, 2015; Pechmann, 2014), to reduce authors’ incentive for data “embellishments,” reviewers should be more tolerant of “imperfections” in reported data, which are necessarily noisy, and be more attuned to the entire *pattern* of empirical and conceptual evidence presented and its most plausible interpretation. A balanced consideration of both type-I and type-II errors is obviously desirable.

To conclude, preregistration has its merits within the broader set of tools of Open Science for advancing transparency and reproducibility. However, before recommending it as a desirable

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

scientific norm for our field, it is important to recognize its limitations, drawbacks, and potential adverse effects. While preregistration can be valuable under specific circumstances, it is neither sufficient, nor necessary for good consumer science. An indiscriminate and overly dogmatic embrace of preregistration may ultimately harm the quality of science that the practice is meant to promote. Other Open Science practices such as more open data access, self- and independent replications, and multiverse analyses would generally be more useful.

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METHODOLOGICAL APPENDIX

**Survey of Editorial Board Members of JCR and JCP on Preregistration**

The survey was conducted online (through Qualtrics) between March 19-March 27, 2020. All members of the current editorial boards of the *Journal of Consumer Research* and *Journal of Consumer Psychology* whose email addresses could be found online were contacted and invited to participate in a short survey. Current Editors and Associate Editors of the two journals were invited as well. Survey responses were anonymous (IP address collection was disabled), although several respondents chose to identify themselves.

Of the 289 editorial board members contacted, 181 (62.6%) responded. Of these, 13% received their PhD less than 10 years ago, 44% received their PhDs 10-19 years ago, 22% received their PhD 20-29 years ago, and 17% received their PhD 30-plus years ago (4 percent did not report this information). When asked to classify their primary research orientation, 50% answered “Information Processing/Social Psychology,” 23% answered “Judgment and Decision Making/BDT,” 10% answered “Marketing in general,” 7% answered “Consumer Culture Theory,” 1% answered “Quantitative Modeling, and 4% answered “Other” (4 percent did not answer this question).

Respondents first received the following introductory frame:

*We are interested in your honest opinion on the issue of preregistration.*

*Broadly, preregistration is defined as "publicly registering research plans (including but not limited to: participant size and pool, exclusion criteria, study design and purpose, procedures and measures, hypotheses/predictions, etc.) on an online repository prior to collecting data."*

They were then asked to answer three substantive questions about the issue of preregistration, the third of which was open-ended.

1. *What is your honest opinion about preregistration for consumer research studies?*

<i>Strongly opposed</i>							<i>Strongly in favor</i>
-3	-2	-1	0	+1	+2		+3

## PREREGISTRATION NEITHER SUFFICIENT NOR NECESSARY

2. *Should preregistration be mandated for empirical consumer research (whether experimental, survey-based, ethnographic, web-scraping, etc.)?*

*Yes*

*No*

3. *Please explain why or why not. What are your primary arguments in favor or against mandating the preregistration of consumer studies?*

[Open textbox]

4. (A fourth question, unrelated to this project, asked respondents to identify consumer researchers whose scholarly work they admire.)

Finally, respondents were asked the following two background questions:

5. *For classification purpose, how long ago did you receive your PhD?*

*Less than 10 years*

*10 to 19 years*

*20 to 29 years*

*30 years or more*

6. *Which of the following subfields best characterizes your primary research orientation? (Check one only)*

- *Consumer Culture Theory*
- *Information Processing / Social Psychology*
- *Judgment and Decision Making / BDT*
- *Marketing in general*
- *Quantitative Modeling*
- *Other*

Respondents spent a median time of 3.99 minutes answering the three preregistration questions. (The mean response time was distorted by a few respondents who appeared to have completed the survey in more than one sitting and left their browser open.) Respondents' open-ended explanations for favoring or opposing the mandatory preregistration of consumer studies included an average of 91.6 words (SD = 106.4).