

## **RESEARCH WITH BUILT-IN REPLICATION: COMMENT AND FURTHER SUGGESTIONS FOR REPLICATION RESEARCH**

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## **RESEARCH WITH BUILT-IN REPLICATION: COMMENT AND FURTHER SUGGESTIONS FOR REPLICATION RESEARCH**

In this brief commentary on the paper *Designing Research with In-Built Differentiated Replication*, we expand on concerns about a lack of replication research raised by the authors by focusing on three key questions of continuous importance: Why should more replication research be conducted? Why do we find so few replications studies? What can be done about it? We identify barriers preventing replication related to the scientific system, the replication researcher, and the initial research. Suggestions are made that *all papers* should be published electronically along with reviews, authors should take steps to encourage replications of their work, and editors should invite replications of important papers. Moreover, the scientific community should establish a replication index as a measure of output quality.

## INTRODUCTION

Many authors have called for more replication research in management science, with apparently little impact on research practice (e.g., Evanschitzky et al. 2007). Therefore, we applaud contributions to increase the occurrence of replication research.

One such attempt has been made by the authors of the paper *Designing Research with In-Built Differentiated Replication*. One key focus of this paper is to re-state the importance of replication as an integral component of the initial research design. In doing so, the authors highlight the importance of differentiated replication and the use of multiple sets of data in establishing empirical generalizations.

Before discussing how to replicate, it is important to understand why replications should be conducted and more importantly, what discourages researchers from doing so more frequently.

## WHY REPLICATE?

Researchers generally agree on the importance of replication research (e.g., Hubbard and Vetter, 1996; Hunter, 2001; Madden, Easley, and Dunn, 1995; Singh, Ang, and Leong, 2003; Tsang and Kwan, 1999; Wells, 2001). This is partly due to the large percentage of failed replications (Evanschitzky and Armstrong, 2010; Evanschitzky et al. 2007; Hubbard and Armstrong, 1994).

Many proposals have been made to encourage replications. These include editors inviting replications of important papers, accepting replications based on evaluating proposals that outline the replication attempt, appointing replications editors, and finding ways to publish all replications.

Scientific findings rest upon replication. As things stand now, many findings in the management sciences have not been successfully replicated. Given this, suggestions have been made that practitioners should be skeptical about changing their decision-making based on findings reported in journals. One might think of an analogous situation in medicine where researchers test many treatments and occasionally some may prove useful by chance. Moreover, teachers should be wary of including the findings of one-off studies in their curricula, and researchers should recognize that such findings rest on a weak foundation.

Many journals have responded to the challenge of publishing replications. These include Winer's (1998) revival of the "Research Notes and Communications" section of the *Journal of Marketing Research*, and Mick's (2001) introduction of a "Re-Inquiries" section in the *Journal of Consumer Research*. A practical solution to make replications possible has been provided by the *Journal of Money, Credit and Banking* as authors must deposit the data and code used for papers they publish. In an attempt to ease replication of papers published in their journal, the editorial team of the *International Journal of Forecasting* has for the past few years had a policy of requesting data and details on the methods used in order to encourage replications. It also has recently instituted a systematic procedure to obtain data and methods from authors prior to publication. A similar editorial policy has been applied by the *Journal of Conflict Resolution*. Additional emphasis can be provided by appointing a replications editor, as has been done, for example, by the *Journal of Applied Econometrics*.

### **WHY SO FEW REPLICATIONS?**

Despite positive steps by some journals, the rate of published replications is still declining (Evanschitzky et al., 2007). We are pessimistic about the short run and propose explanations on

why this occurs. Potential explanations can be grouped into barriers related to the scientific system, the replication researcher, and the initial research (Baumgarth and Evanschitzky, 2005).

### ***Scientific Review System***

Research has shown that despite the generally held belief about the importance of replication, editorials (e.g., Monroe, 1992a & b), and other appeals for more replication research, there seems to be a bias against publishing replication research (Bornstein, 1990; Easley, Madden, and Dunn 2000; Neuliep and Crandall 1990). As a case in point, Kerr, Tolliver, and Petree (1977) found that 52 % of reviewers indicate that direct replications would be directly rejected. Similarly, Roney and Zensiek (1980) found that 34 % of reviewers have serious concerns with replications, causing them to reject any direct replication attempt.

Adding to this problem, Bornstein (1990) identified a replication paradox: In case research successfully replicates previous findings, reviewers and editors find it hard to see this as an important contribution as it is (falsely) considered as nothing new, something which merely confirms previous findings. Failure to replicate initial findings does not increase the chance of being published: either, findings are insignificant or researchers have trouble explaining why replicating results has failed (Roney and Zensiek, 1980).

### ***Replication Researcher***

Reid, Soley, and Wimmer (1981) suggest that conducting a replication can be as time-consuming and laborious as doing original research. Hence, why bother replicating in light of a low likelihood of being published? The current reward system is based on the number of publications in the “right” journals. Fairness in promotions at business schools is more important than whether the researcher discovered anything important. This philosophy carries over into the journal review process. Papers are published when the reviewers vote in favor.

The increasing emphasis on quantity leads to “the iron law of important papers” (Holub, Tappeiner, and Eberharter, 1991). The iron law is that the number of important papers rises linearly while the total number of papers rises exponentially. As a result, important papers are a smaller percentage of total papers published. Pay for papers and one gets papers, not scientific progress. Said another way, the advancement of scientists is becoming more important than the advancement of science.

Another barrier preventing replications involves a lack of knowledge on how to conduct replications, and more generally, misinterpretation of empirical findings. Research has shown that researchers themselves (falsely) believe that the tests of statistical significance provide good information about the likelihood that the findings could be successfully replicated. Oakes (1986) showed that 42 of 70 (60%) experienced academic psychologists falsely believed that an experimental outcome that is significant at the 0.01 level has a 0.99 probability of being statistically significant if the study were replicated.

### ***Initial Research***

A final barrier preventing replication research touches upon the more general issues of academic practice and importance of research findings. Madden, Franz, and Mittelstaedt (1979) for instance concluded that only 2 out of 60 papers published in proceedings of leading marketing conferences could be replicated based on the information in the paper. Furthermore, Reid, Rotfeld, and Wimmer (1982) found out that only about 50% of authors of leading marketing journals were willing to share necessary materials to allow their work to be replicated. Madden, Franz, and Mittelstaedt (1979) and Dewald, Thursby, and Anderson (1986) came to similar conclusions in their studies. Apparently, the way in which researchers present their findings might prevent replication.

Replication of an academic study is only likely to create additional insights if the original study has important findings. There are, of course, exceptions such as when a substantial number of researchers pursue an area that shows little promise (game theory and Box-Jenkins spring to mind for us, but readers can supply their own favorites). However, we as academics have to seriously question the importance of our findings. Various estimates suggest that only 3% to 20% of published papers are important (e.g., Armstrong 2004; Armstrong, Brodie, and Parsons, 2001; Churchill, 1988; Simon, 1986). Papers with controversial empirical findings seem to be especially difficult to publish (Armstrong and Hubbard, 1982). This complements the previously mentioned “iron law of important papers” (Holub, Tappeiner, and Eberharter, 1991). Apparently, a key issue in increasing replication research is making the initial research important, or put differently: make it worth replicating.

### **WHAT TO DO?**

Authors with important and well-supported papers should take steps to encourage replications. This might involve extra efforts, but it will pay off for the authors eventually. Ioannidis (2005) found that replication studies were conducted for about 3/4 of highly cited papers in medicine (in a sample from 1990 through 2003). Hence, being replicated can be seen as a sign of importance of the initial study. In fact, one might consider successful replications of initial research as one way of judging the value of a research paper. Maybe it is time to introduce a “replication index” as a measure of output quality.

Journal editors could identify important papers in the field that should be replicated/extended, and then invite designated researchers to publish such replications. If invitations were restricted to important problems, they would be more likely to gain interest by

those invited and cooperation from the authors of the original study. Furthermore, researchers are much more likely to undertake a replication of an important study, especially when it is an invited paper.

Given the reward system, we are pessimistic about the short term; however, the long-term is bright, thanks to technology. We now have the capability of publishing all papers submitted to a journal at no marginal cost. In fact, it would be less expensive for editors who spend time trying to justify rejections, and especially for authors who go through endless rounds of revisions, often for trivial changes. How would this occur? All papers would be published electronically. Because anyone can publish, it would be senseless to reward people based on the number of publications. The effect would be to reduce the number of papers submitted.

Thus, only those who have something important to say would bother to publish. Those with unimportant or useless papers would be ignored. As a result, researchers would be judged on the importance of their findings not on a count of publications. They would be judged on how well their findings hold up in replication attempts. This would move management sciences closer to physical sciences where researchers often pay to publish their papers, acceptance rates are very high, and replications are common for important papers.

We hope that our suggestions as well as suggestions put forward by the paper *Designing Research with In-Built Differentiated Replication* would help to advance management science in such a way in the future.

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