

Reflection on the Professor-Priming Replication Report

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I read the Registered Replication Report (RRR) with great interest, and I am glad I was given the opportunity to briefly reflect on the reported findings.

In my view, the most remarkable and at the same time unfortunate finding was that 66% of the participants were suspicious about, or even aware of, the relation between the prime and the dependent variable. That is, some participants indeed thought that the priming manipulation and the trivia task were linked, and others indicated that the prime might have affected their performance on the trivia task. This makes the experiments difficult to compare with the original experiments reported in 1998, the replications published afterward (see Lakens, 2017, for an overview), and our own recent study (conducted by Ad van Knippenberg, Rob Holland, and me), which replicated the original findings, at least among male participants. The latter study, briefly mentioned in the RRR, was conducted in September 2014 among first-year undergraduates only. This strategy was chosen to minimize the chance that participants would be aware of the goal of the experiment (although admittedly I cannot say to what extent the strategy was successful). After all, soon after they start their major, many, if not all, psychology students learn about the effects of priming on behavior.

Worse still, the 66% may be an underestimation, as experience shows that some participants want to rush toward the end of an experiment. They say “no” to questions rather than “yes” because they know a “yes” answer will prompt an additional question asking for some kind of explanation. We did not expect 66% of the participants in this replication project to be aware of, or at least to be suspicious of, the goals of the study. If we had expected this, we would have chosen a different course of action, and we would have at least preregistered a plan for analyses in which the suspicious participants were excluded.

The term *priming* was coined by Lashley (1951), who published the first of a series of both theoretical and empirical publications on this topic (e.g., Segal & Cofer, 1960; see Bargh, 2014, for a review). The term was used

to refer to unconscious and unintended carryover effects from one task to the next. The first demonstrations of semantic priming (Meyer & Schvaneveldt, 1971) and social priming (Higgins, Rholes, & Jones, 1977) were also demonstrations of such unconscious or unintentional carryover effects. In light of these seminal studies, one could argue that an experiment in which people are aware that they have been primed is not even a priming experiment in the first place.

With this limitation in mind, what can one conclude from the findings? The main analysis of the RRR showed no effect of the priming manipulation, but excluding people on the basis of their presumed awareness of the goal of the experiment made the predicted priming effect emerge. This latter finding should be interpreted with caution, as it is clearly exploratory, and an analysis of all the data rather than the data from the labs that met the preregistered requirements made the effect smaller again. However, the finding that the priming effect was evident among “naive” participants supports earlier findings showing that priming effects disappear or even reverse among people who are aware of the potential effects of the prime (e.g., Lombardi, Higgins, & Bargh, 1987; Shih, Ambady, Richeson, Fujita, & Gray, 2002) and is also fully in line with the now dominant theory of behavior priming, the situated-inference model (Loersch & Payne, 2011). This model treats priming as a consequence of a misattribution process, and, of course, there is no room for misattribution once the correct attribution has already been made or at least assumed.

I do not think the current project allows for strong conclusions. Recently, behavior priming has been demonstrated with a procedure (Payne, Brown-Iannuzzi, & Loersch, 2016) that improves on the procedure in the

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seminal experiments of the 1990s. However, perhaps researchers should make additional changes in the way they conduct priming experiments, for instance, by trying to better obscure the goals of the experiments. In designing the protocol for the replication project, we clearly underestimated the probability that participants would become aware of the nature of the experiment. Moreover, priming as a mechanism is much more well known than in the past; this is definitely the case among students, but is most likely also true outside academic psychology. In most countries, packs of cigarettes contain images that people know are supposed to influence their behavior. The idea that merely being exposed to something may exert some kind of influence is not nearly as mystifying now as it was 20 years ago.

Furthermore, as psychologists, we may ask ourselves whether investing in a large-scale replication project such as the current one is the best and most economical way to investigate older findings. Forty labs were involved. A cumulative research program by one or two individual labs may yield much more diagnostic results. To begin with, such a research project could benefit from a more precise methodology. In the current replication project, every lab across the world used the same set of general-knowledge questions; despite all the efforts to maximize quality, this obviously made for a less sensitive dependent variable than would have been possible with a tailor-made set of questions pilot-tested by an individual lab. This is just an example, of course. The general point is that stimulus materials pilot-tested in the same lab as where they will be used will likely yield the strongest results. In addition, a cumulative approach allows one to learn. A first experiment showing that 66% of the participants were aware of the relation between the prime and dependent variable would obviously lead to procedural changes that would prevent, or at least discourage, awareness in a second experiment. More generally, we may also want to conclude that the hope that large-scale replication projects would provide quick answers as to the validity of old findings is way too optimistic (see also Wagenmakers et al., 2016). And perhaps that is for the better. After all, in the past few years, the field has learned that engaging in slow science rather than in quick science is the way to proceed.

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