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Welcome Back Theory!

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Abstract

The three articles appearing in this special section are constructive and optimistic, and they will encourage new research in the behavioral priming domain. They also show that the recent criticism of behavioral priming is largely overblown. The few widely publicized nonreplications have functioned as a welcome wake-up call, but they should not suggest initial findings to be false positives. Instead, they should inspire research on yet-to-be-identified moderators. I also argue that other criticism on behavioral priming, such as on the supposedly large effect sizes of behavioral priming experiments, is based on the disregard of theory in combination with premature or faulty logic.

Keywords

unconscious, unconscious thought, decision making

The articles on behavioral priming in this special section of *Perspectives on Psychological Science* could not have been more timely. These days, every discussion on behavioral priming revolves around replication, and although the special section discusses replication too—it is unavoidable, and indeed, this commentary is also about replication—it is done against the background of something we had almost forgotten: theory! Stroebe and Strack (2014, this issue) argue that focusing on the replication of a phenomenon without any reference to underlying theoretical mechanisms is uninformative. Cesario (2014, this issue) draws the conclusion that although behavioral priming researchers could show more methodological rigor, the relative infancy of the theory is the main reason the field faces a problem. And, as if it were coordinated, Klatzky and Creswell (2014, this issue) present a new theoretical article in which they use insights from cognitive science to explain behavioral priming effects. Together with other recent theoretical contributions (Bargh, in press; Loersch & Payne, 2011; Schröder & Thagard, 2013), their article fills an important void.

In past years, an astounding amount of attention has been paid to a few published nonreplications from the behavioral priming domain (Doyen, Klein, Pichon, & Cleeremans, 2012; Pashler, Coburn, & Harris, 2012; Shanks et al., 2013). The three articles published in this section, in combination with the other theoretical papers listed above, may help us to assess the value of these nonreplications. Clearly, there is no denying that these nonreplications were insightful: We thought, implicitly or explicitly, that some phenomena were more robust than they really are, and hence, we should pay more attention to the systematic investigation of boundary conditions and to more precise theorizing. The nonreplications functioned as a welcome, though rather loud, wake-up call.

That being said, the evidence for behavioral priming is overwhelming. There are new papers appearing each month, and depending on your definition, there are between 200 and 400 empirical behavioral priming papers by now. Behavioral priming effects have been obtained with children as well as with elderly participants (Ambady, Shih, Kim, & Pittinsky, 2001; Over & Carpenter, 2009; Levy, 1996) and with a range of stimuli, including odors (Castiello, Zucco, Parma, Anuini, & Tirindelli, 2008; see also Klatzky & Creswell, 2014). Some research areas, such as those on social power and on terror-management theory, almost completely rely on behavioral priming paradigms. In the past years, behavioral priming has been successfully applied in all kinds of naturalistic settings (Berger, Meredith, & Wheeler, 2008; Latham & Piccolo, 2012; Papies, Potjes, Keesman, Schwinghammer, & van Koningsbruggen, 2013). Bargh (in press) reviewed the area for an upcoming special issue on priming effects published by *Social Cognition*.

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Behavioral priming has also captured the welcome attention of neuroscientists. Bengtsson, Dolan, and Passingham (2011), in a replication of the Dijksterhuis and van Knippenberg (1998) intelligence-priming experiments, showed that priming participants to be either clever or stupid led to increased (for clever) or decreased (for stupid) activation of the anterior cingulate cortex and to changes in participants' performance on a working memory task. Inagaki and Eisenberger (in press) extended Williams and Bargh's (2008) "warmth" studies and compared brain activation of participants reading socially warm versus socially neutral messages with brain activation of participants holding physically warm versus neutral temperature objects. Inagaki and Eisenberger observed an overlap in experience as a result of social and physical warmth: Participants felt more connected to close others after holding the physically warm object (relative to holding the neutral object), and participants felt physically warmer after reading the warm social messages (rather than the neutral messages). They also found overlap in brain activation caused by the social and physical warmth manipulations.

It is interesting that the few published nonreplications have led some to suggest that behavioral priming may not exist. However, there are good reasons to believe that the fear that psychology is infested with false positives is largely unnecessary (Dalton, Aguinis, Dalton, Bosco, & Pierce, 2012; Murayama, Pekrun, & Fiedler, in press); in the case of behavioral priming, the hundreds of papers cannot be erased by the mere flick of a skeptic magic wand, no matter how hard you try. On the other hand, behavioral priming researchers cannot make the nonreplications disappear, so what are the implications of the three new articles on behavioral priming?

Perhaps the biggest problem is that exact replication is an illusion. All three articles make this point rather explicitly. As Cesario argues, one can try to copy an old procedure as adequately as possible (see Brandt et al., in press, for a recipe on "close" replication), but in the end, there are always variables that cannot be copied perfectly. This can be seen as a weakness of the field—and to some extent it is—but it is also the reality of our subject. Whether we like it or not, many (social) psychological phenomena are affected by people's mood, by atmospherics, time of day, fatigue, motivation, and even the weather. It is interesting to note that the new framework by Klatzky and Creswell can, to some extent, accommodate such moderators by, for instance, explaining why fatigue can affect the impact of elderly priming or why the weather can affect the impact of a warm or cold coffee cup. For now, the lesson to be learned is that unless one can control such variables, the quest for a paradigm that replicates everywhere and every time before "believing" behavior priming is like saying you believe in the existence of the concept of a climate only after having experienced perfectly stable weather for 4 weeks in the Scottish Highlands.

Skeptics have tried to discredit behavioral priming in other ways. For example, it has been asked how there could be many boundary conditions with effects that are so big? Astronomers sometimes wait decades for all the boundary conditions to be right for a certain phenomenon to occur, but are these phenomena small? Weather forecasts are based on dozens of factors that always affect the weather, but the variance some of these individual factors explain is minute. The size of an effect and its ubiquity can be correlated in a certain domain, but until you have evidence for such a correlation, it stands to reason that one should assume they are orthogonal. This example also emphasizes why theory is so important. The claim that the strength of an effect is necessarily correlated with its ubiquity may sound fine on a superficial level, but it does not follow from any of the four recently published theoretical frameworks referred to above. In fact, they can all explain why behavioral priming has so many potential moderators and why this does not yet mean the effects are small.

Another objection is that it is suspicious that behavioral priming effects are bigger than semantic priming effects. But is this a problem? First of all, the size of priming effects is partly dependent on the strength of the priming manipulation, and behavioral priming experiments usually use much bolder primes than semantic priming experiments. Second, although both effects are likely mediated by the accessibility of the primed construct (though this is more complicated for embodied primes), and though one may assume that a reaction time measure is a better proxy for accessibility than overt behavior, we also all know that reaction times can be noisy. Third, and perhaps most important, evolution works on real human behavior, not on button presses. The fact that an accessible construct or an embodied prime has a bigger effect on real behavior than on artificial behavior makes perfect sense. Finally, social psychologists often use stimulus materials that are more motivating for participants than cognitive psychologists. People are more interested in other people than in Chinese ideographs or irregularly shaped figures, and they care more about the relation between professors and intelligence than the relation between bread and butter. If you teach children arithmetic by using only equations, you'll have a more difficult time than when you use real examples ("Train A leaves station X at…"). However, if semantic priming effects were bigger than behavior priming effects? *That* would be cause for concern.

As all three contributions point out, nonreplications of findings that have been replicated in many labs are much more likely caused by yet-to-be-identified moderators and

boundary conditions (see also Dijksterhuis, van Knippenberg, & Holland, in press). At first, it seemed that Doyen and colleagues (2012) had replicated the seminal experiment by Bargh, Chen, and Burrows (1996) more or less exactly, but on closer inspection it became clear that Doyen and colleagues, unlike Bargh et al. (1996), focused participants on the dependent variable by giving explicit instructions. Their findings thereby confirmed earlier work showing that priming effects are attenuated or even reversed when people suspect they are being primed. The Doyen et al. approach, whereby a nonreplication does not lead to immediate speculations about false positives but is primarily seen as an encouragement to further study a phenomenon, sets a good example., In their attempts to replicate intelligence-priming work, Shanks et al. (2013) mimicked earlier experiments closely, but their questionnaire measuring intellectual performance was more difficult than those used in the original studies and in almost all other replications. Given that effects of intelligence priming are mediated by self-efficacy (Hansen & Wänke, 2009), it is plausible that the effects do not occur for extremely difficult tasks.

Cesario rightly points out that the fact that we do not know enough to accommodate surprising findings is something we should take to heart. On the other hand, I am not quite as negative about the current state of affairs. Half a dozen moderators of behavioral priming effects have been studied extensively in recent years (and it is unfortunate that these well-known moderators have not been taken into account in most replication efforts; see also Bargh, in press; Fay & Maner, in press). Also, the theoretical papers that are appearing now give rise to optimism. It can lead researchers to investigate moderators more systematically so that we may be able to theoretically accommodate all findings—positive effects as well as null effects.

A better understanding of the moderators is also important from an applied perspective, and the new theories will likely be very helpful here as well. Currently, behavioral priming is applied to increase fund raising, to reduce biases in elections, to improve eating behavior, and to help people to quit smoking and fight other addictions, to name just a few examples. Such laudable initiatives should not be jeopardized by too much skepticism, but there is no denying that a better understanding of the boundary conditions of behavioral priming will make applications more efficient.

Finally, I do not want to pretend there is nothing wrong. It is plausible that the liberal use of researchers' degrees of freedom in the past has led to inflated effect sizes and the odd false positive. However, a few nonreplications in combination with loosely applied reasoning will not solve this problem. Only a change of habits can: Researchers should conduct theory-driven research and follow the advice of Simmons, Nelson, and Simonsohn (2011) to stop (ab)using researchers' degrees of freedom. Indeed, this special section can help priming researchers to build the next generation of priming research with an emphasis on theory-guided mechanisms and boundary conditions.

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