High Quality Direct Replications Matter: Response to Williams (2014)

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Abstract

We respond to Williams' (2014) comments on our three failures to replicate of Study 2 from Williams and Bargh (2008). We clarify our conclusions on this topic, making clear that although the results of our studies cast doubt on the specific effect reported in Williams and Bargh (i.e., that instant hot and cold packs influence choice of reward for self or friend), a more complete understanding of the embodiment hypothesis in question requires consideration of relevant conceptual replications. Accordingly, we consider the strength of the evidence in the conceptual replications that Williams identifies and find that small samples appear to be the norm. We conclude that in order for researchers to move forward, future studies must take seriously issues of power, researcher degrees of freedom, and file drawer problems. Doing so will ensure that future studies are more informative tests of this hypothesis.

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We thank Dr. Williams for his thoughtful comments on our three replication attempts of Williams and Bargh (2008), Study 2. We also want to acknowledge that Dr. Williams participated fully in the pre-registration process of our studies, and we appreciate that he shared the original data and clarified aspects of the original procedure for us. Our efforts were strengthened by his input and graciousness. Indeed, as he notes in his reply, we are surely “allies pursuing the same goal” of truth (p. 6). There are but a few issues we wish to clarify in response to his concerns.

Making Our Conclusions More Precise

First, Williams (2014) rightly notes that a single failure to replicate does not provide definitive evidence that an effect is not real. The results from failed replication attempts should not be privileged, just as results from original studies should not be given more weight just because they were published first. We regret that we were not clearer in our abstract when we wrote that “there is not evidence that brief exposure to warm therapeutic packs induces greater prosocial responding.” We should have been more careful to emphasize that this conclusion referred only to the results of our three studies. It is certainly possible that despite our failure to find evidence for this phenomenon, the effect does exist.

However, it is the case that as more and more evidence accumulates, the weight of that evidence may begin to suggest that the original finding was dependent on the precise contextual features of the original study or that the hypothesis underlying the effect was not correct. We reported three independent failures to replicate the original results, each with sample sizes that were over four times larger than the original study. Taking all of this evidence into account, the cumulative evidence for *this specific effect* (i.e., the ability of instant hot and cold packs to

influence the choice of a treat for self vs. friend) is not especially strong.

Scientific Judgment and the Importance of Direct v. Conceptual Replications

In his response to our paper, Williams (2014) appeals to the need for strong theory to guide research on behavioral priming studies like those we attempted to replicate. He makes a compelling case that the theory that led to this study would predict the very result he (and we) set out to examine. However, he also appears to argue that the strong theoretical basis for this prediction somehow lessens the importance of our repeated failures to replicate the original result. We find this position to be somewhat surprising – if this finding is so clearly predicted by theory, then the failure to find such an effect in multiple, large-sample replications should be especially problematic for that theory. Thus, we suggest that direct replications inform theories more so than the other way around. Indeed, direct replications are generative to the extent that they motivate future revisions to a theory and generate additional experimental tests involving different ways of operationalizing variables.

In discussing the theoretical context for these results, Williams (2014) also highlights several studies that serve as conceptual replications of the original Williams and Bargh (2008) finding. Conceptual replications “try to operationalize the underlying theoretical variables using different manipulations and/or measures” (Stroebe & Strack, 2014, p. 60). We acknowledge that there is debate within the field about the relative merits of conceptual versus direct replications. However, we strongly disagree with the position espoused by Stroebe and Strack (which Williams seems to endorse) that conceptual replication serves the same goals or can take the place of direct replication. The problem is that in designing conceptual replications additional decisional flexibility can make false positives more likely (Simmons, Nelson, & Simonsohn, 2011). Further, when reviewing bodies of evidence, researchers must decide what “counts” and

what doesn't count as a conceptual replication, adding "researcher degrees of freedom" to the research process (Simmons, Nelson, & Simonsohn, 2011). A conceptual replication that does not support a theory may be ignored in a review, or it may be tucked away in the file drawer where it can't be considered by other researchers (LeBel & Peters, 2011).

Weighing the Evidence

Regardless, let us consider these conceptual replications together with the current direct replications. Table 1 displays the effect size estimates (converted to Cohen's d) for Williams and Bargh (2008), the current replication studies, and relevant conceptual replications identified by Williams. The table contains only studies that link temperature manipulations to prosocial or related behavior¹. We also included one additional study not mentioned by Williams (Kang et al., 2011), which tests the effect of hot and cold instant packs on trust.

One clear message from Table 1 is that the average sample size in these conceptual replications (omitting the current replications) is extremely small, with an average of just 36 participants ($SD = 10.72$)². Our direct replication studies are *each* 6.5 to 8 times larger than this average. Sample sizes in the range of 176 are typically required to have adequate power (80%) to detect the average $d = .43$ effect size in social psychology (see Richard et al., 2003). The effect sizes in Table 1 are much larger than this average. However, publication bias and the file-drawer effect make it likely that the actual effect is much smaller than these estimates. Thus, the literature cited by Williams (2014) is largely based on underpowered samples. Future tests of this hypothesis – both conceptual and direct – should be conducted with an eye towards achieving adequate power.

In the end, we hope that there is process in psychological science that would lead researchers to abandon theories that lack robust empirical support, and we believe direct

replications play an important role in this process. Nonetheless, we realize that psychological theories do not live and die by the results of a single study or set of studies. We agree with Williams (2014) that more work is needed to better understand how temperature priming impacts thoughts, feelings, and behaviors. Our hope is that advocates of temperature priming research do not dismiss strong failures to replicate theoretically important results by focusing on conceptual replications that confirm their original beliefs. Instead, we urge researchers to conduct future tests of the theory with large samples so that results are informative.

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Footnotes

¹ Studies that tested the opposite hypothesis of behavior impacting judgments of temperature are excluded (IJzerman & Semin, 2010; Szymkow et al., 2013; Zhong & Leonardelli, 2008). All of these studies failed to meet the basic requirement of a conceptual replication in that they did not test the same hypothesis as Williams and Bargh (2008). However, these studies evinced roughly similar effect and sample sizes to the studies in the table. Also excluded from the table are Hale et al. (2011; a study of rats with $N = 24$) and Lowry, Lightman, and Nutt (2009; a review paper).

² Including the remaining studies with human participants (IJzerman & Semin, 2010; Szymkow et al., 2013; Zhong & Leonardelli, 2008) increases the average sample size to $N = 51$ ($SD = 30.24$).

Table 1

Effects Sizes for Temperature on Prosocial Outcomes

Study	Analyzed <i>N</i>	<i>d</i>	95% CI
Williams & Bargh (2008), S2	50	0.69 ⁺	[0.03, 1.36]
Lynott, Corker, et al. (2014)	237	-0.05 ⁺	[-0.32, 0.23]
Lynott, Corker, et al. (2014)	294	-0.27 ⁺	[-0.53, -0.01]
Lynott, Corker, et al. (2014)	282	-0.14 ⁺	[-0.42, 0.13]
Williams & Bargh (2008), S1	41	0.65 [^]	[0.02, 1.31]
Inagaki & Eisenberger (2013)	18	0.78 [#]	[0.28, 1.30]
IJzerman & Semin (2009), S1	33	0.78	[0.07, 1.53]
IJzerman & Semin (2009), S2	52	0.43	[-0.12, 1.00]
IJzerman & Semin (2009), S3a	39	0.75	[0.09, 1.44]
IJzerman & Semin (2009), S3b	39	1.17	[0.50, 1.90]
IJzerman et al. (2013), Secure Kids	37	0.72	[0.06, 1.43]
IJzerman et al. (2013), Insecure Kids	22	-0.63	[-1.56, 0.23]
Kang et al. (2011), S1	30	1.22	[0.45, 2.06]
Kang et al. (2011), S2	23	0.06	[-0.79, 0.92]

Note. S = Study. ⁺ Converted from odds ratio. [^] Converted from *F* statistic. [#] Converted from paired-*t* statistic. Remaining *ds* and 95% CIs were calculated from reported *Ms*, *SDs*, and *Ns* (if available). IJzerman and Semin (2009), Study 3, reported results on two dependent variables, listed here as S3a and S3b.