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Discrepant Data and Improbable Results: An Examination of Vohs, Mead, and Goode (2006)

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ABSTRACT

A highly cited article by Vohs, Mead, and Goode (2006) reported several experiments in which an incidental reminder of money produced large effects on subsequent behaviors unrelated to money. We attempted 2 high-powered direct replications of the first experiment, which found that money-primed subjects worked on a puzzle nearly twice as long as controls before quitting. The replication studies showed no evidence of money priming. Moreover, 25% of the subjects in our studies solved the puzzle correctly or incorrectly, whereas none reportedly did so in the original study. We also list anomalies in the reported results of the original study.

Vohs, Mead, and Goode (2006) reported nine simple experiments with dramatic results. In each study, one group of subjects saw a reminder of money that a control group did not see, and this money prime caused marked changes in subsequent behavior, including large changes in behaviors seemingly unrelated to money. For example, in several experiments, subjects first descrambled words to create a phrase (THE SWEATER IS GREEN), and some of the phrases seen by the money prime group had meanings related to money (WE CAN AFFORD IT). Later, the money-primed subjects worked 66% longer than controls on a puzzle before quitting (Experiment 1), and they volunteered only half as much time to a confederate needing help (Experiments 3 and 4). In two other experiments, subjects who merely sat near a computer monitor with a screen saver showing money later placed their chair 60% farther away from their partner than did controls (Experiment 7), and they chose to work alone (rather than with a partner) 3 times as often as controls (Experiment 9). In total, the authors reported 14 findings, and each was statistically significant in the predicted direction. Moreover, the effects were larger than most known psychological effects, as Cohen's *d* values ranged from 0.59 to 1.49. (The authors reported 12 of the *d* values, and we computed the other two.)

The article appeared in the journal *Science*, and the results have been highly influential. The findings were discussed extensively in popular media. A prestigious journal invited the authors to recount their results in more detail (Vohs, Mead, & Goode, 2008). Nobel Prize winner Daniel Kahneman (2011) wrote in his best-selling book *Thinking, Fast and Slow* that the research is "remarkable" and that the experiments are "profound" (p. 56). The findings also inspired a large literature of money-priming studies using a variety of manipulations and measures. To date, Google Scholar lists more than 1,300 citations of the article—the majority in the past 5 years.

In the past few years, however, the Vohs et al. (2006) findings have drawn some skepticism from a number of researchers with expertise in priming and methodology. For instance, Meyer (2014) pointed out that money-priming effects are entirely unlike the well-established semantic priming effects that he codiscovered (e.g., Meyer & Schvaneveldt, 1971), which are typically small and fleeting even when the prime and dependent measure have strong semantic links (e.g., CAT and DOG). Moreover, multiple teams of researchers have noted that the Vohs et al. findings, in aggregate, exhibit meta-analytic warning signs about the credibility of the data, as evidenced by funnel plot (Vadillo, Hardwicke, & Shanks, 2016), R-index (Schimmack, Heene, & Kesavan, 2017), and the Test of Excess Significance (Francis, Tanzman, & Matthews, 2014; Vadillo et al., 2016). It is also quite clear that the Vohs et al. findings cannot sensibly be dismissed as mere statistical flukes, because the authors found a strong effect in each of nine

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experiments. With nine studies and an alpha level of .05 (as used by Vohs et al.), the chance of finding just six or more false positives would equal about one in a million.

Here we describe an examination of Experiment 1 in Vohs et al. (2006). We found anomalies in the reported findings, and we conducted two high-powered direct replication studies. We believe this work is the first report of an attempt to directly replicate any of the studies reported by Vohs et al.

Attempts to replicate money-priming effects have instead focused on a later set of studies reported by Caruso, Vohs, Baxter, and Waytz (2013). These authors reported a money-priming effect in each of five experiments using either the aforementioned phrase descramble task or money screen saver used by Vohs et al. (2006), although the Caruso et al. dependent measures differed from the ones used by Vohs et al. Caruso shared his data and methodology with researchers seeking to replicate the effects, and the replication attempts wholly failed to confirm the findings. In the most notable of these attempts, a collaboration of 36 laboratories known as the Many Labs Replication Project sought to replicate the largest effect found by Caruso et al. ($d = 0.80$ in Experiment 1) but instead found an average effect size of $d = -0.02$ (Klein et al., 2014). Only one of the 36 labs found a significant effect, and this rate of success (1/36) was less than the alpha level ($.05 = 1/20$). In addition, we conducted six high-powered replications of the first four studies in Caruso et al., and none of the studies showed any evidence of priming (Rohrer, Pashler, & Harris, 2015). After this string of null effects, Caruso himself spearheaded three additional large-scale attempts to replicate his money-priming effects, and these also failed to confirm the original results (Caruso, Shapira, & Landy, 2017). In short, the available evidence appears to decisively discredit the money-priming effects reported by Caruso et al. (2013).

Yet, as just noted, the earliest and most highly cited money-priming effects—those reported by Vohs et al. (2006)—have not undergone empirical scrutiny. Direct replications of these studies pose more of a challenge because much of the information needed to conduct a direct replication was not provided in either the article or the Supporting Online Materials (SOM). For this reason, we requested the necessary information from the contact author, K. Vohs. Our inquiries complied with published guidelines for researchers who seek to replicate published findings (e.g., Kahneman, 2014; Lewandowsky & Bishop, 2016). For instance, we informed K. Vohs of our plans to replicate some of her studies, and we asked her about certain oddities in the reported results (described next) before reporting these oddities. We also fulfilled each of her multiple requests for raw data from our own studies (K. Vohs, personal communications, March 31, 2015; April 3, 2015; April 17, 2015).

As we sought information to help us conduct our replication studies, K. Vohs responded to some of our queries, and she provided us with useful but limited information about one experiment only. She sent us the sample sizes for each subject group in Experiment 1 (personal communication, May 13, 2015), but she declined to send us the deidentified data, indicating that sharing data was not permitted by her university's Institutional Review Board (personal communication, May 6, 2015). She also added a revised figure to the article's online erratum after we asked her about oddities in the reported results of Experiment 1, as detailed later in this article. Finally, after repeated requests, she sent us the task instructions for Experiment 1 (personal communication, July 20, 2015). We later wrote her two more times to ask about a second oddity in the Experiment 1 data, as detailed later in this article, but we received no response. We also wrote K. Vohs three times in 2016 to ask only for the group sizes in Experiments 2 through 9, but we again received no reply. Thus, we have written K. Vohs five times since we last received any communication from her. In summary, we received some cooperation from K. Vohs during our efforts to understand and replicate Experiment 1, but we were unable to learn more about the findings in any of the other experiments. Thus, the present article is about Experiment 1 only.[1]

The original experiment

Experiment 1 in Vohs et al. (2006) assessed whether a money prime would increase subjects' persistence on a difficult puzzle. All subjects first completed a phrase descramble task in which they saw sets of five words (SKY WENT GRAY THE IS) and created a four-word phase from each (THE SKY IS GRAY). By random assignment, subjects performed this task in one of three ways. In the money phrase group, half of the phrases were related to money (I CASHED A CHECK). The control group descrambled only neutral phrases, and the play money group descrambled neutral phrases while sitting near a stack of play money. Later, all subjects were asked to solve a puzzle and

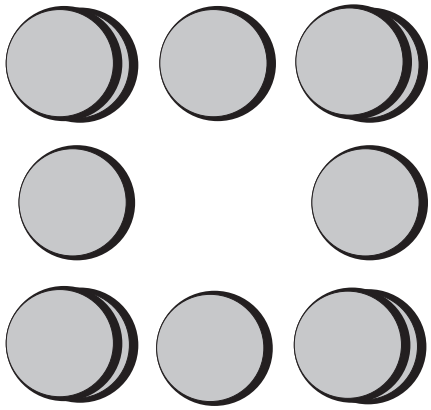


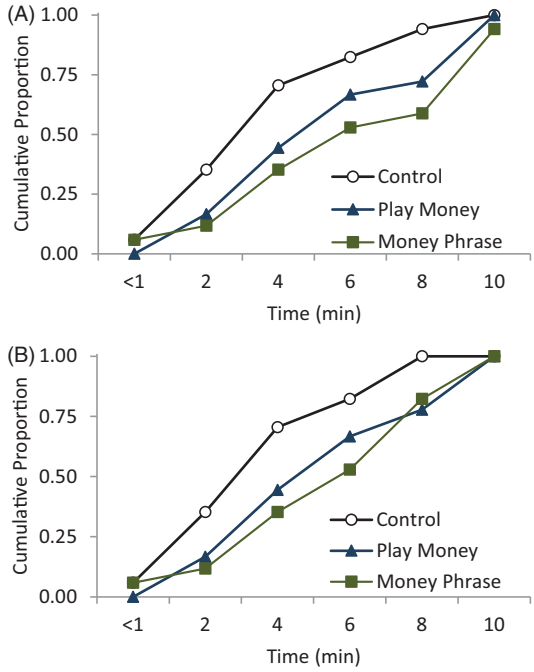
Figure 1. Puzzle solution. *Note.* Subjects were asked to arrange 12 disks as a square with five disks on each side. The puzzle is solved by stacking two disks at each corner. Subjects were given 10 min to find the solution.

told that they could ask the experimenter for a hint. The puzzle is shown in Figure 1.

The primary dependent measure was the amount of time that subjects worked on the puzzle before asking for a hint, and subjects who persisted for the allotted 10 min were stopped and assigned a time of 10 min. Money priming sharply increased subjects' persistence. On average, both the money phrase group (314 s) and the play money group (305 s) worked more than 66% longer than the control group (186 s). Cohen's *d* equaled 0.86 and 0.84, respectively.

Yet the reported findings included discrepancies. Their article included a figure showing the cumulative frequencies for each group, and we noticed that the data points were inconsistent with the reported means (Figure 2A). We wrote K. Vohs to ask about these oddities (personal communication, May 8, 2015). She sent us a revised figure (personal communication, July 3, 2015), which now appears in the paper's online erratum (Vohs, Mead, & Goode, 2019). The revised figure included changes to the data for each of the three groups (Figure 2B). Of course, errors are not uncommon in scientific papers, which is one key reason why authors' sharing of data is helpful.

Of further interest, the reported results give no indication than any of the subjects solved the puzzle, either correctly or incorrectly. The SOM specifies that 52 subjects participated in the study, and the revised figure indicates that all 52 subjects either sought help or persisted for the allotted 10 min (Figure 2B). Thinking that perhaps the authors excluded subjects who found a solution, we asked K. Vohs whether the experiment included any subjects other than these 52 subjects (as part of our aforementioned e-mail about the figure in her article). She replied that the study



	<1	2 - 4	4 - 6	6 - 8	8 - 10	10
Control	1	6	12	14	~~16~~ 17	17
Play Money	0	3	8	12	~~13~~ 14	18
Money Phrase	1	2	6	9	~~10~~ 14	~~16~~ 17

Figure 2. Experiment 1 in Vohs et al. (2006). *Note.* (A) Original figure. The money phrase plot is missing one subject. Also, the reported means for the money phrase and play money groups differed by only 9 s, yet the figure suggests a greater difference. (B) Revised figure and table. After we asked K. Vohs about the discrepancies in the original data points, she added a revised figure to the article's erratum and sent us a frequency table. The table excludes the time interval between 1 and 2 min, which might be an inadvertent error. The correct time intervals cannot be inferred from either figure, because the axis labels are ambiguous.

included only one other subject and that this individual quit the study before seeing the puzzle task (personal communication, May 26, 2015). Several months later, we twice wrote K. Vohs and specifically asked whether any of the subjects solved the puzzle, but we received no reply.

Although the puzzle is difficult, we find it strange that none of the subjects solved the puzzle or even believed they had solved the puzzle. The subjects were undergraduate students at the University of Minnesota, Twin Cities, which is the state's flagship university, and undergraduate admission is selective. This subject sample presumably included many students who performed well on tasks requiring motivation and insight. In light of this oddity and the marked effects of money priming, we attempted to replicate the original study.

Replication studies

We conducted two high-powered direct replications of Experiment 1 in Vohs et al. (2006). Here we report all of our dependent measures, and these two studies are the only unpublished money-priming studies that any of us has conducted. We preregistered the second study (AsPredicted.org/nbc8m.pdf). The students who ran the second study created a 5-min video of the procedure (doi.org/10.6084/m9.figshare.3409786.v1). We posted the deidentified data from Experiment 1 (doi.org/10.6084/m9.figshare.8134820.v1) and Experiment 2 (doi.org/10.6084/m9.figshare.3409744.v1).

Method

Each subject was randomly assigned to one condition. Replication 1 included a money phrase group and a control group but not a play money group, but this exclusion could not have affected the observed results because of the random assignment of subjects to groups. Replication 2 included all three groups. Each replication study included more than 3 times as many subjects per group as in the original study.

Subjects

We tested undergraduate students enrolled in psychology courses at the University of California, San Diego. They received course credit in return for their participation. Replication 1 included 131 subjects (65 in money phrase, 66 in control). Replication 2 included 180 subjects (67 in money phrase, 57 in play money, and 56 in control). The original study included 52 subjects from the University of Minnesota, Twin Cities (17 in money phrase, 18 in play money, and 17 in control). The power of each replication study exceeded .99 when assuming a two-tailed test, an alpha level of .05, and the effect sizes observed in the original study. In Replication 1, the sample size was large enough to ensure that the observed difference between means had a precision of 0.34 (in standard deviation units), assuming an alpha of .05, as given by the *a priori procedure* (e.g., Trafimow, Wang, & Wang, in press). The precision values for Replication 2 equaled 0.36 (money phrase vs. control) and 0.39 (play money vs. control).

Procedure

Each subject was tested alone in a small room. All subjects first completed the phrase descramble task. Subjects received a booklet with 30 sets of five words (IS OUTSIDE COLD DESK IT) and handwrote a four-word phrase for each set (IT IS COLD OUTSIDE). Half of the phrases descrambled by the money phrase group related to money (HE HAS THE CAPITAL). None of the phrases seen by the play money and control groups were related to money, but the play money group descrambled the phrases while sitting near a stack of Monopoly money that the experimenter demonstrably placed on the subject's desk just before the task began. Immediately after the descramble task, subjects completed the Positive and Negative Affect Schedule mood inventory (Watson, Clark, & Tellegen, 1988). We did not analyze these responses.

Subjects next began the puzzle (Figure 1). Each subject received 12 metal disks. We are not certain that disks were given to subjects in the original study, but Vohs et al. (2006) wrote that the puzzle required subjects to arrange "12 disks into a square with five disks per side" (p. 1154). Providing disks also ensured that the experimenter could accurately classify a subject's solution as correct or not, whereas a subject's hand-drawn solution or spoken explanation might be ambiguous. In Replication 1, the experimenter read aloud the following instructions:

> You will now complete a puzzle. You will have twelve disks [experimenter shows disks]. Please arrange these disks into a square. Each side of the square must be made of five disks. You will have ten minutes to solve the puzzle. Please open the door when you are done. If you want help, I am available.

The wording of these instructions differed from the wording used in the original study because we did not obtain the original study's instructions from K. Vohs until after we conducted Replication 1. In Replication 2, however, each subject received the written and oral instructions that we received from K. Vohs (personal communication, July 20, 2015). Specifically, subjects received a sheet of paper with the statement "Instructions: using 12 circles, create a square that has five circles along each side" and a diagram consisting of 12 circles (diameter = 1.6 cm) arranged as a square with four circles on each side. The experimenter said,

> Next, you will be completing this task. Let me know if you get to the point where you want a hint—just let me know; I will be out here. Knock on the door when you are done working or if you need me.

(Note: Subjects in the original study were asked to "ring a bell" rather than "knock on the door.") From this point forward, the two replication studies used the same procedure.

Once subjects began the puzzle, the experimenter waited outside the door. Subjects who knocked on the

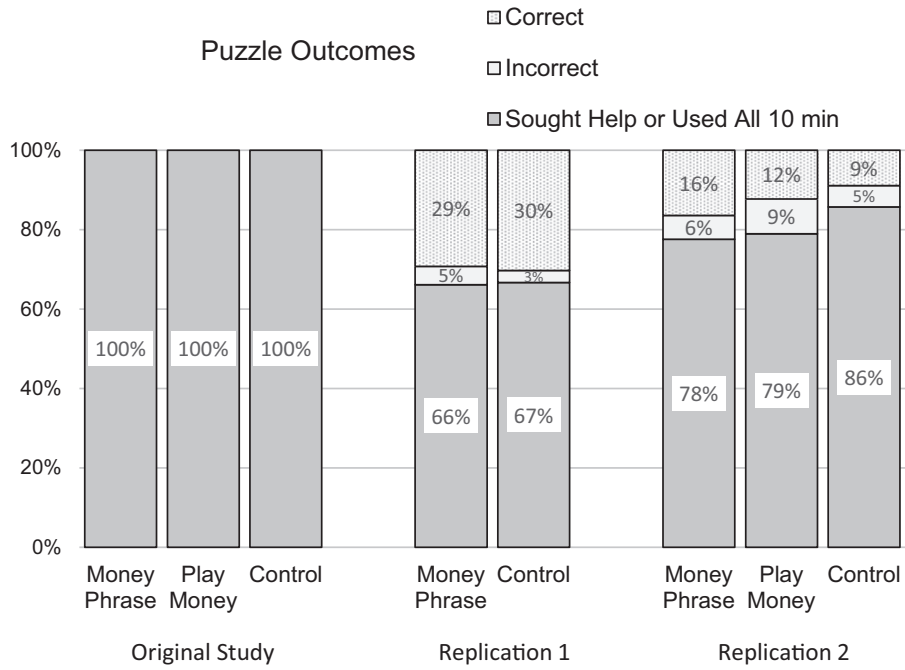


Figure 3. Puzzle outcomes. *Note*. The number of subjects in each group is listed in the Method section. The difference between the results of Replication 1 and Replication 2 might reflect a difference in the task instructions (see text).

door to ask for help were given a hint, but their subsequent performance was not recorded. Any subject who asked for a hint was classified as one who sought help, regardless of whether the subject later found a correct or incorrect solution. Subjects who did not knock on the door during the allotted 10 min were stopped and assigned a datum of 10 min. As in the original study, our analyses combined subjects who used all 10 min with subjects who asked for a hint.

Results

Neither replication study confirmed the results of the original study. Furthermore, dozens of subjects in each replication study informed the experimenter that they had found a solution, an outcome that reportedly never happened in the original study. In fact, each condition in both studies included subjects who found a correct solution as well as subjects who found an incorrect solution (Figure 3).

The two replication studies differed from each other in one key respect. Subjects were more likely to find a correct solution in Replication 1 (29% of subjects) than in Replication 2 (13%), whereas incorrect solutions were a bit *less* likely in Replication 1 (4%) than in Replication 2 (7%). This difference between the two replication studies might be due to subject variability, and it also might reflect the difference in the puzzle instructions used in the two studies. Most notably, subjects in Replication 2 were shown a diagram with 12 circles arranged as a square with four circles per side, and this diagram could have impeded subjects' chances of finding a correct solution (see the Method section). At any rate, many subjects in each replication study either solved the puzzle or believed that they had solved the puzzle.

We also examined whether the prevalence of solutions was affected by money priming. In Replication 1, the chance of a subject finding a solution (correct or not) was nearly the same in the money phrase group (22/65 = 34%) and the control group (22/66 = 33%). In Replication 2, solutions (correct or not) occurred more often in the money prime groups than in the control group: money phrase (15/67 = 22%), play money (12/57 = 21%), and control (8/56 = 14%). However, this association was small. After collapsing across the two money-priming conditions (which yields 27 solutions from 124 subjects), the correlation between money prime exposure (yes/no) and the finding of a solution (yes/no) produced a phi coefficient of .09. Thus, the money prime explained less than 1% of the variance.

As for subjects who did not report finding a solution, neither replication study showed an effect of money prime on persistence, unlike in the original study (Figure 4). In Replication 1, the money phrase group quit slightly *sooner*, on average, than did the control group—a difference in the opposite direction from the original finding. In Replication 2, the money phrase and play money groups persisted slightly

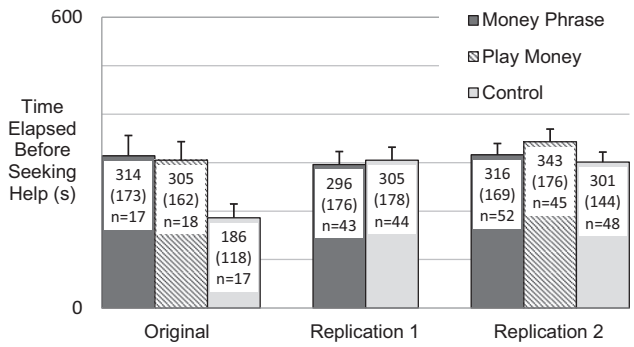


Figure 4. Subjects' persistence on puzzle. *Note.* Labels show mean, standard deviation, and group size. Error bars represent 1 standard error. Subjects who never asked for help were assigned the value of 600 s and included in this analysis, as in the original study. Analysis excludes subjects who solved the puzzle correctly or incorrectly.

longer, on average, than did the control group, and these differences were in the same direction as in the original study. Because Replication 2 found an effect in the predicted direction, we statistically analyzed both the money phrase versus control difference and the play money versus control difference. First, in keeping with our preregistration plan, we assessed both differences by null hypothesis testing and found that neither money-priming effect was statistically significant. Second, in keeping with the policy of this journal, we conducted a Bayesian analysis. We used online software crated by Rouder, Speckman, Sun, Morey, and Iverson (2009). This algorithm assumes a Jeffreys prior, and we set the scale r to the default value of .7071. The results favored the null hypothesis over the alterative hypothesis for both the money phrase versus control comparison and the play money versus control comparison (the Scaled-Information Bayes Factors equaled 3.30 and 1.85, respectively). In short, neither replication study found a money-priming effect.

Discussion

Our examination of Experiment 1 in Vohs et al. (2006) produced three main findings. First, we found discrepancies in the reported data (Figure 2). Second, two direct replications provided no support for the money-priming effects observed in the original study (Figure 4). Third, about 25% of the subjects in our studies solved the puzzle correctly or incorrectly, whereas none in the original study reportedly did so. This distinction between the *many* solutions found by our subjects and the *zero* solutions reportedly found in the original study is arguably a difference of kind rather than a difference of degree, and thus the two replication studies provide what we would describe as a qualitative as well as quantitative nonreplication. These findings, along with the red flags raised by the previously reported meta-analyses described in the Introduction, would seem to warrant serious doubts about the validity of the findings of the original study.

To be sure, a replication failure can never imply with certainty that the original effect is not real (e.g., Earp & Trafimow, 2015). That said, the usual list of reasons for caution do not appear pertinent to the present results. For instance, a failed replication might be a false negative (Type 2 error), but each of the two replication failures reported here had statistical power exceeding .99 (see the Method section). By this value, and given the original effect size, the chance that both replication studies would produce false positives is less than one in 10,000.

A real effect also might not replicate because of methodological differences between the original study and the replication attempt, yet attributing the present replication failures to hidden moderators seems hard to reconcile with the reported robustness of the original findings. After all, Vohs et al. (2006) reported large effects in each of nine studies using several kinds of priming manipulations (descrambled phrases, play money, poster, screen saver, vignette), a variety of dependent measures (e.g., persistence, helpfulness, preference to be alone), and different subject populations.[2] If such a diversity of methodologies produced uniformly large effects in the hand of the original experimenters, it is difficult to understand how two *direct* replications could fail to show the effect. An effect cannot be both astonishingly robust and mysteriously fragile at the same time.

The moderator argument also has been put forth to explain the many failures to replicate the money-priming studies reported by Caruso et al. (2013), as described in the Introduction. For instance, after the Many Labs Replication Project (Klein et al., 2014) failed to find the largest Caruso et al. money-priming effect at dozens of different universities, including both public and private, selective and not selective, small and large, and urban and rural, Vohs (2015) suggested that these replication attempts might have failed because none took place at the University of Chicago, where Caruso et al. conducted the original study. As Vohs (2015) argued, the undergraduate subjects at the University of Chicago might have responded uniquely to money primes because that university is known for an economics program that supports free markets (p. e87). Such a strict boundary condition is hard to reconcile with the uniformly large effects reported by Vohs et al. (2006), none of which

were found at the University of Chicago. In a similar vein, Schuler and Wänke (2016) attributed failures to replicate Caruso et al. to subject differences, and they reported two studies showing an interaction between money priming and subjects' socioeconomic status (but no main effect of priming). However, the interaction disappeared in two highly powered preregistered direct replications by Crawford, Fournier, and Ruscio (2019).

Other money-priming studies

The published literature includes many money-priming effects (see review by Vohs, 2015), though none appear to be direct replications of studies reported by Vohs et al. (2006). However, a few of these studies are similar to one of the Vohs et al. studies. In two studies that perhaps come closest to a direct replication, the handling of money led Polish children to spend more time on a paper-and-pencil maze or jigsaw puzzle (Gasiorowska, Chaplin, Zaleskiewicz, Wygrab, & Vohs, 2016). These two studies are obviously based on Experiment 1 in Vohs et al., which is the focus of the present article. Yet the two studies with Polish children are not direct replications because these studies used a different prime, a different task, and subjects who were neither adult nor American. Although such studies are sometimes described as "conceptual replications," the notion that such a study can validate a previous finding has been challenged by investigators in recent years (e.g., Doyen, Klein, Simons, & Cleeremans, 2014; Earp & Trafimow, 2015; Nosek, Spies, & Motyl, 2012; Pashler & Harris, 2012).

Furthermore, a literature with mostly positive findings can be misleading because of publication bias and inappropriate research practices (e.g., Bishop, 2019; Munafò et al., 2017; Nelson, Simmons, & Simonsohn, 2018; Nosek et al., 2012). In a recent meta-analysis, Lodder, Ong, Grasman, and Wicherts (2019) found that published money-priming effects were likely to be distorted by publication bias, whereas unpublished and preregistered money-priming studies, which showed little or no effect, exhibited no signs of bias. Similarly, Vadillo et al. (2016) conducted several meta-analyses of the money-priming effects cited in the Vohs (2015) review and concluded that the "effects are distorted by selection bias, reporting bias, or *p*-hacking" (p. 665). Indeed, two of the largest money-priming effects in the Vohs review were reported by Chatterjee, Rose, and Sinha (2013), and that article was retracted shortly after oddities in their data were brought to light by Pashler, Rohrer, Abramson, Wolfson, and Harris (2016).

Transparency

In response to the replicability crisis in psychology, numerous researchers have pointed out that authors need to make their data and methods available to other researchers (e.g., Kahneman, 2014; Lewandowsky & Bishop, 2016; Marsman, Ly, & Wagenmakers, 2016; Miguel et al., 2014; Munafò et al., 2017; Nelson et al., 2018; Nosek et al., 2012; Wicherts, 2011). As Kahneman (2014) wrote, "[Authors] are obligated to share the details of their procedures and the entire data of their study, and to do so promptly" (p. 310). Such transparency is a fundamental tenet of science, and the refusal to share data or methods violates the policies of numerous professional organizations (e.g., American Psychological Association) and funding agencies (e.g., the U.S. National Institutes of Health). The same policy holds for authors of articles in *Science*, where Vohs et al. (2006) was published, though the policy was put in place after the article was published.

Of course, one cannot expect that authors will always have access to the data and methods of every study they have published, especially studies conducted long ago. Yet if they do have this information, it seems clear that it should be shared with other researchers. Thus, we suggest that K. Vohs post the Vohs et al. (2006) data to a publicly available repository. Whether or not the data are shared, it seems essential that the group sizes and other basic facts about these studies need to be made available to other researchers. Indeed, the Vohs et al. article includes a number of studies that are arguably more astonishing than Experiment 1. For instance, in Experiments 7 through 9, merely sitting near a screen saver or wall poster depicting paper currency (instead of a different image) reportedly increased subjects' *subsequent* preference to be alone, and each of these experiments produced a Cohen's *d* value greater than 1. When findings like these are published, critical details about the findings need to be shared with other researchers.

Summary

Vohs et al. (2006) reported 14 strikingly large effects of money priming on a variety of dependent measures, only one of which was nominally related to money, and the findings drew widespread attention and greatly influenced important theoretical work. The effects are unlike conventional priming effects, which are small and transitory even when the prime and measure are semantically related, and several meta-analytic studies suggest that the effects are

improbable. We found discrepancies in the reported results of Experiment 1 (Figure 2), and we conducted two high-powered direct replications that provided no evidence for money priming (Figure 4). In addition, many subjects in each replication study solved the puzzle correctly or incorrectly, yet neither outcome reportedly happened even once in the original study. Finally, we propose that K. Vohs needs to share essential methodological information about Experiments 2 through 9 so that researchers can attempt to verify the findings of these studies, some of which produced results that are no less remarkable than those of Experiment 1.

Notes

1. We also wrote both coauthors to ask for information about Experiments 2 through 9, and we received a reply from only N. Mead, who referred us to the contact author, K. Vohs (personal communication, January 28, 2016). However, the coauthors are not listed as corresponding authors, and they might never have had the information we sought.
2. Although Experiment 1 was conducted at the University of Minnesota, the SOM indicates that most of the experiments were conducted at the University of British Columbia, where K. Vohs was once a faculty member.

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