A Comprehensive Meta-Analysis of Money Priming

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Research on money priming typically investigates whether exposure to money-related stimuli can affect people's thoughts, feelings, motivations, and behaviors (for a review, see Vohs, 2015). Our study answers the call for a comprehensive meta-analysis examining the available evidence on money priming (Vadillo, Hardwicke, & Shanks, 2016). By conducting a systematic search of published and unpublished literature on money priming, we sought to achieve three key goals. First, we aimed to assess the presence of biases in the available published literature (e.g., publication bias). Second, in the case of such biases, we sought to derive a more accurate estimate of the effect size after correcting for these biases. Third, we aimed to investigate whether design factors such as prime type and study setting moderated the money priming effects. Our overall meta-analysis included 246 suitable experiments and showed a significant overall effect size estimate (Hedges' $g = .31$, 95% CI [0.26, 0.36]). However, publication bias and related biases are likely given the asymmetric funnel plots, Egger's test and two other tests for publication bias. Moderator analyses offered insight into the variation of the money priming effect, suggesting for various types of study designs whether the effect was present, absent, or biased. We found the largest money priming effect in lab studies investigating a behavioral dependent measure using a priming technique in which participants actively handled money. Future research should use sufficiently powerful preregistered studies to replicate these findings.

Keywords: meta-analysis, money, priming, publication bias

Money plays an important role in our modern society. In the past 10 years, psychologists have started to investigate its influence on human behavior. A prominent article suggests that because money enables goal attainment, exposure to money-related stimuli (i.e., money priming) would bring about a self-sufficient orientation (Vohs, Mead, & Goode, 2006). This self-sufficient orientation can, in turn, have behavioral consequences if it decreases the willing-ness to help others and increases in the preference to work alone. Following the pioneering work by Vohs and colleagues (2006), a large body of work has not only provided evidence supporting these psychological and behavioral effects, but also uncovered other effects, such as how money priming can bolster support for existing socioeconomic systems (Caruso, Vohs, Baxter, & Waytz, 2013).

However, recent large-scale replication projects failed to replicate the effect of money priming on the endorsement of socioeconomic systems (Caruso, Shapira, & Landy, 2017; Klein et al., 2014; Rohrer, Pashler, & Harris, 2015). Whereas most replication projects focused on a specific kind of money prim-ing study, Caruso and colleagues (2017) varied their experi-ments over different money primes, dependent measures, and moderators. They concluded that none of the five studied ma-nipulations consistently influenced the dependent measures. These findings echo other failed replications in social priming research (Pashler, Coburn, & Harris, 2012; Shanks et al., 2015; Van Elk & Lodder, 2018).

Although successful replication attempts can increase our confidence in the reliability of research findings, unsuccessful replication attempts can spark debates and controversies. In response to unsuccessful money priming replication attempts, Vohs (2015) suggested two possible reasons for these failed replications. First, existing theories do not offer a clear predic-tion regarding the effects of money priming on the endorsement

of socioeconomic systems. Although money priming may increase the endorsement of existing socioeconomic systems by increasing the saliency of these systems, it could also reduce the defensive need to endorse these systems by stimulating a self-sufficient orientation. Hence, the inconsistency between the original and replication studies may be attributable to the interplay between these two forces. Second, the unsuccessful replications could have been caused by differences between participants across study samples (e.g., in the perceived meaning of money). These two reasons imply that the effects of money priming may be contingent on (hidden) moderators such as the type of dependent variable, study design, or participant characteristics.

In addition, Vohs (2015) listed 63 experiments which support the effects of money priming (and counted 102 more). These experiments purportedly demonstrated that money priming had a reliable effect, especially on performance-related and interpersonal outcome measures. However, Vadillo et al. (2016) argued that such a "vote counting" strategy is inappropriate (see Hedges & Olkin, 1980) as it fails to take into account actual effect sizes, potential biases caused by how data are analyzed, and the selective mechanisms in the reporting of results (publication bias). They also found that the studies listed in Vohs (2015) contained an excess of significant findings, which hinted at such biases. For example, although 85% of the results listed in Table 1 of Vohs (2015) were statistically significant, the observed power was only .70. Further, Vadillo et al. conducted a meta-analysis on the experiments listed by Vohs (2015) and found that this set of studies likely suffered from publication/selection bias. Nonetheless, as the experiments included in the meta-analysis were not based on a systematic search, Vadillo and colleagues (2016) highlighted the need for a comprehensive meta-analysis on money priming.

The Current Research

Our current study answers the call for a comprehensive meta-analysis that examines available evidence on the effects of money priming (Vadillo et al., 2016). By conducting a systematic search of published and unpublished literature, we sought to achieve three key goals. First, we aimed to assess the presence of biases (e.g., publication bias) within this body of work, thereby allowing us to better evaluate the reliability of money priming effects. To do so, we used three techniques: p-uniform, selection models, and Egger's test for funnel plot asymmetry. Second, if biases were indeed present, we sought to derive a more accurate estimate of the (mean) effect size in several subsets of money priming studies after correcting for these biases. Our third goal was to examine whether effect sizes were moderated by several experiment characteristics. Namely, we assessed whether effect sizes differed across the types of dependent variable, methods of money priming, and the settings in which the experiment was conducted. These findings would offer insights into the variation of the effect, which can potentially help guide theory formulation, direct future replication efforts, and the planning of registered studies that experimentally investigate potential moderators.

Method

Search Procedure and Inclusion Criteria

First, we conducted a search for published articles via PsycINFO and ISI Web of Science, with the search terms "(*currency* OR *money*) AND (*priming* OR *prime**)."[1] Additional published studies were obtained from Tables 1 and 2 in Vohs (2015) and from inspecting reference lists of included studies. Unpublished studies were obtained through personal communication. Specifically, we e-mailed the authors of articles that met our inclusion criteria and asked them for published and unpublished data and reports suitable for our meta-analysis.[2,3] We also published calls for (un)published money priming results on the ListServ of the Association for Consumer Research (ACR: July 17th 2015) and on the forum of the Society for Personality and Social Psychology (SPSP: January 22nd 2018). Furthermore, we have searched online lists of conference abstracts (i.e., we searched the 2013–2018 lists for the annual conferences for the Association for Psychological Science [APS] and the 2003–2018 lists for the Society for Personality and Social Psychology [SPSP] for the word 'money') and have contacted all authors of abstracts on money priming experiments. Taken together, these search methods allowed us to make our literature search as comprehensive as possible. We are therefore confident that our sample of studies is representative for the collection of studies on money priming.

In the money priming field, researchers have used a wide variety of experimental manipulations, some of which, such as counting bank notes, are not considered primes in the classical sense. Because of this, one could argue that some studies included in our meta-analysis do not prime money but merely activate the idea of money. We follow Janiszewski and Wyer (2014) in defining priming as an "experimental framework in which the processing of an initially encountered stimulus is shown to influence a response to subsequently encountered stimulus" (p. 97). As such, we do not limit our analysis to specific kinds of priming and therefore include all studies with experimental manipulations aimed at activating the idea of money in the mind of participants. For reasons of simplicity we will refer to all such experimental manipulations as money priming.

In a typical money priming study, researchers randomly assign participants into two conditions. In the money priming condition, participants are primed with money (e.g., they are shown images of bank notes), whereas participants in the control group receive a similar prime that is however not related to money (e.g., they are shown images of blank paper). In a subsequent task, subjects in both conditions respond to the dependent measure(s), typically

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[1] Search conducted on February 15th 2018.

[2] Although we aimed to find as much studies as possible, we are aware of the file-drawer effect (Rosenthal, 1979) so we realize that there might still be some relevant studies that we did not include in our analysis. We therefore invite researchers to contact us if they have any additional material that meets our inclusion criteria but was not included in our meta-analysis. Based on this additional data, we will provide a periodic update of our meta-analysis on its OSF page (see Lodder, Ong, Grasman, & Wicherts, 2019).

[3] We are thankful to Kathleen Vohs—a lead scholar in the money priming field—who assisted us in requesting unpublished results from a large number of authors.

reflecting construct(s) hypothesized to be related to money (e.g., charitableness). Subsequently, the difference between conditions on this dependent measure represents the money priming effect.

In our meta-analysis, we included studies that met the following inclusion criteria. First, only *empirical* studies investigating a money priming effect (on any dependent measure) were included (i.e., reviews and commentaries were excluded). Hence, we excluded studies that prime concepts related to money, such as materialism. Second, studies employing between-subjects designs must have randomly assigned participants to one of the conditions. Third, studies needed to compare at least one money priming condition with a nonmoney prime comparison control condition (note that this also includes within-subject designs). Finally, studies had to be reported in English, leading us to exclude one study published in Chinese.[4]

Effect Size Computation

For each included study, we calculated the necessary meta-analytics statistics (i.e., estimates of the effect-size and its sampling variance). When necessary, we e-mailed the researcher(s) requesting more detailed statistics.[5] We used Hedges' *g* as the primary effect size in our meta-analysis. This effect size represents the standardized mean difference between the money priming- and the control condition (Hedges' *g* is a small sample bias corrected estimate of Cohen's *d*; Hedges & Olkin, 2014). If possible, we directly calculated Hedges' *g* from the means, standard deviations, and sample sizes reported for the money priming- and control conditions. Whenever necessary, we transformed other effect sizes such as Cohen's *d'*s, (log) odds ratios, *F* ratios, or zero-order correlation coefficients to Hedges *g* using the guidelines reported by Borenstein, Hedges, Higgins, and Rothstein (2009). While extracting the relevant statistics from the included studies we used the following conventions:

1. If cell sizes were not reported, we first tried to reconstruct them based on available information (e.g., degrees of freedom) and if that was impossible we assumed that the overall sample size was equally divided across conditions.

2. If a study investigated a between-subjects interaction, we computed the simple main effect of money priming at each level of the second crossed factor. For instance, when a study investigated the interaction between money priming and gender, we computed a money priming effect for males and females separately, and included them as two separate rows in our dataset.[6,7] We subsequently coded which of these rows should show the largest effect size according to the authors' predictions. Although this is not an ideal approach, we used it to allow inclusion of studies that hypothesized that the money priming effect on a dependent measure was moderated by a third variable. Not taking this moderating variable into account by including the main effect of money priming in the meta-analysis might result in an underestimated money priming effect. For instance, consider an experiment investigating a between-subjects interaction effect of money priming and socioeconomic status on system justification. Suppose that the authors of

that study claimed that a money priming effect would *only* show up in people with high socioeconomic status. Based on this assumption, one could argue that the inclusion of the subset of participants with *low* socioeconomic status would deflate the money priming estimate in the meta-analysis. We could tackle this problem by including socioeconomic status as a moderator in a metaregression. However, besides socioeconomic status, researchers have proposed many other moderating factors, most of which have only been studied a few times or only once, making it difficult to include these factors as moderators in a metaregression. To solve this problem, we included each level of the interaction as a separate row (independent sample) in our dataset. This enabled us to analyze both the complete dataset, as well a subset that included only those rows of the interaction designs that were (a priori) hypothesized to show the largest money priming effect. Focusing on the rows of the dataset that were expected to show the largest effect implies a liberal stance, while taking a conservative stance involves also including the levels of the interaction hypothesized to show a smaller money priming effect. In our results section, we report the results from a liberal stance, whereas the Appendix includes the results from a conservative stance.

3. If a study investigated the money priming effect on multiple dependent measures within the same sample of participants, then we first checked whether the authors predicted one of those measures to show a larger effect than the other(s). If such a prediction existed, we included the dependent measure with the strongest predicted effect in our meta-analysis. If the authors did not clearly explicate such a prediction, we derived an aggregated effect size (including appropriate SEs) based on all (reported) dependent measures. For instance, Capaldi and Zelenski (2016) examined the effects of priming money on (a) sustainable willingness, (b) social value orienta-

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[4] Three experiments by Gasiorowska (2013), originally published in Polish were still included as its author provided us with a brief English description of the study.

[5] For each included study, we requested an effect size estimate; a variance of the effect size estimate (or the statistics necessary to compute an effect size and its variance) and information to test for moderators in a metaregression (e.g. study setting, money prime type, type of dependent measure). If the experiment had a within-subjects design or assessed multiple dependent measures on the same sample, we also requested a correlation between the dependent measures.

[6] Using separate rows for studies on the same sample of participant introduces dependency in the data. We have investigated this dependency by introducing a shared random effect estimate for rows involving the same sample of participants. The random effects structure of this multilevel meta-analysis can be specified by using the rma.mv function in the R-package metafor (Viechtbauer, 2010). As the overall effect size estimates of this multilevel meta-analysis differed only slightly from those of the regular random effects model, we decided to only report the results of the latter model.

[7] If the second factor was a continuous variable we conducted a median split and computed the money priming effect for both sides of the median split. According to this convention, we coded each level of the interaction as a separate row in our dataset.

tion, and (c) social connectedness, yet they did not provide any explicit predictions regarding the relative magnitudes of these effects so we therefore decided to aggregate these effect sizes. We realize that aggregated effect sizes are difficult to interpret. However, as some studies lack a clear theory regarding the relationship between different dependent measures we decided to aggregate those measures, but only when they were based on the same sample of participants.

4. If a study included more than one control or money priming condition, we first checked whether the authors predicted one of the similar conditions to show the largest effect. If such a prediction existed, we included this particular effect in our meta-analysis. If the authors did not provide such an explicit prediction, we aggregated the means and standard deviations of the similar conditions before computing the money priming effect. For instance, we aggregated the means and standard deviations of the two neutral conditions (i.e., fish screensaver and no screensaver) in an experiment by Vohs et al. (2006; Experiment 7).

5. If a study involved a within-subject design, we converted the within-subject effect size to a between subject effect size according to the formulas reported by Borenstein et al. (2009), including the appropriate standard errors (*SE*s).

6. If a dependent measure was measured on a binary scale we first computed the log odds ratio and then converted the log odds ratio first into a standardized mean difference and subsequently into Hedges' *g* (including the appropriate standard errors) using the R-package *compute.es* (Del Re, 2015).

7. We excluded any dependent measure that also served as a money priming manipulation (e.g., word-completion tasks used in Kouchaki, Smith-Crowe, Brief, & Sousa, 2013, Study 2).

8. We coded the effect sizes either positive (+) or negative (−) according to whether the effects were as predicted. When we could not infer the direction of an effect from the article, we asked the authors about their predictions.[8]

Meta-Analysis

We performed all our meta-analyses with the Metafor package (Version 2.0–0; Viechtbauer, 2010) in the open source software R (https://www.r-project.org/). We did not expect all included studies to tap the same underlying effect, because money priming studies vary in the type of money prime (e.g., descrambling task or visual prime), the type of dependent measure (e.g., charity or political values), and the type of study setting (lab, online or field). In light of these differences between study designs, we considered a random effects model to be the most appropriate model for our meta-analysis.

Publication Bias

We used three techniques to check for publication bias in the money priming literature meta-analysis: (a) We created funnel plots and tested them for asymmetry by regressing study outcomes on the standard error of the effect size (i.e., Egger's test; Sterne & Egger, 2006). The standard error of a study is a measure of its precision and the lower the standard error the higher the precision of the effect size estimate. Publication bias might be present if more precise studies show smaller effect sizes than less precise studies. (b) We also used the *p*-uniform method (van Assen, van Aert, & Wicherts, 2015) to test for the presence of publication bias. *p*-uniform corrects for publication bias based on significance by only including studies with significant effects. *p*-uniform yields a fixed effect estimate that is corrected for publication bias. *p* value methods like −*p*-uniform and the related *p*-curve method (Simonsohn, Nelson, & Simmons, 2014) that aim to correct for publication bias might provide biased results when the distribution of effect sizes shows substantial heterogeneity (van Aert et al., 2016; van Assen et al., 2015). Because this method performs best when effect sizes are fixed across studies or when they show little heterogeneity (i.e., such that $I^2 < 50\%$), we tested for heterogeneity within subsets of studies based on different combinations of study settings and prime types. These subsets are more homogeneous than the total sample of studies and therefore more compatible with *p*-uniform and the Egger's test. To investigate publication bias, we used *p*-uniform because *p*-curve does not offer a formal publication bias test and because simulation studies show that *p*-uniform is a serious alternative to *p*-curve in providing an estimate corrected for publication bias (van Aert, Wicherts, & van Assen, 2016; van Assen et al., 2015). However, other simulation studies have shown that both *p*-curve and *p*-uniform are outperformed by selection methods, especially when the effects show a substantial amount of heterogeneity (Carter, Schönbrodt, Hilgard, & Gervais, 2018; McShane, Böckenholt, & Hansen, 2016).

Selection methods explicitly model the publication bias process using both a data model (describing how the effect sizes are generated in absence of publication bias) and a selection model (describing the factor[s] determining whether a study will get published or not). Both *p*-curve and *p*-uniform can be considered special instances of the original selection model by Hedges (1984), both assuming that effects sizes are homogeneous and normally distributed and that *only* significant results were published. One could argue that both assumptions are often unrealistic in psychological research. In our meta-analyses, we expected substantial amounts of heterogeneity and also published articles showing nonsignificant results (e.g., Klein et al., 2014). Therefore, we used the R-package *weightr* (Coburn, 2017) to also investigate publication bias using the *three-parameter selection model* (3PSM), which relaxes the two likely stringent assumptions made by *p*-uniform and *p*-curve. We used a simple selection model with one

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[8] The lack of clear predictions in many money priming studies raises the possibility that many studies were exploratory rather than confirmatory (e.g., Wagenmakers et al., 2012). We nonetheless consider it relevant to meta-analyze these effects, while noting that the exploratory nature of studies might enhance the risk of biases in how researchers analyze data and report results (Wicherts et al., 2016).

cutpoint located at $p < .05$ and with no additional moderator variables.

Moderator Analyses

Because the money priming manipulation contains a wide array of dependent measures, study settings and prime types, we expected to find considerable heterogeneity in our meta-analysis. We therefore conducted metaregression analyses in an attempt to determine whether this heterogeneity can be explained by the type of prime and the study setting. We also investigated the moderating influence of other study characteristics, such as publication status, whether the study was preregistered, whether the study used several dependent measures, and whether the study involved an interaction design.

Money priming studies vary widely according to several factors: (a) study setting, (b) type of money prime, and (c) type of dependent measure. We aimed to reduce this variety by recoding each factor to a limited number of categories. However, it was difficult to reduce the large variety of dependent measures to a limited number of categories. Therefore, besides coding for study setting (lab, online, field) and prime type (visual, descrambling, handling, thinking, combination), we decided to only code the dependent measure as behavioral versus nonbehavioral. Two authors independently coded these factors for each included study. Disagreement between the coders was discussed and whenever necessary resolved by consulting a third expert.

Using the p-uniform technique requires a relatively homogeneous effect size distribution. Because of the expected heterogeneity we chose to create subsets of studies that share similar designs and to subsequently conduct a separate meta-analysis within each subset. These subset analyses can illustrate what type of study design tends to show the most reliable effect after controlling for publication bias. We created subsets according to two procedures: (a) based on all different combinations of the three coded factors study setting, prime type, and behavioral versus nonbehavioral dependent measure; (b) based on the most frequently used dependent measures. We expected that some of the subsets would contain a small number of studies. Sterne and colleagues (2011) recommend a minimum of 10 studies when using funnel plots to investigate publication bias in meta-analyses. We decided to be somewhat less strict and included all subsets that contained five or more studies.

Within each subset, we conducted a separate meta-analysis. We investigated homogeneity using the I^2 statistic, which expresses the percentage of heterogeneity caused by between study (compared to within study) variability in effect sizes. I^2 ranges from 0–100% and because lower percentages imply more homogeneity we expect our subsets to show lower I^2 values than our main meta-analysis. Within each subset, alongside Egger's test for funnel plot asymmetry, we also conducted tests for publication bias and provided estimates corrected for publication bias (p-uniform & 3PSM).

Results

The search protocol yielded 608 potential papers that were screened for eligibility. Of these 608, we excluded 567 because they did not meet our inclusion criteria.[9] A total of 41 published

articles met the inclusion criteria of our meta-analysis, yielding 146 suitable experiments. Personal communications with researchers in the field resulted in the inclusion of an additional 100 unpublished experiments. In total, we included 246 experiments in our meta-analysis. Table 1 lists for of all experiments included in our meta-analysis the author(s), year, publication status, study setting, prime type, and dependent measure. This table does not contain information on effect sizes, because for studies involving interaction effects we derived multiple effect sizes (based on convention 2 in the Method section). Nevertheless, this project's OSF page (Lodder et al., 2019) contains the Excel database (including the complete list of effect size estimates) and the R script used for our analyses. Of all included studies, 42 experiments investigated a between-subjects interaction. As noted in our method section, we included only the simple effects of these interactions that were a priori expected to show the largest money priming effect. Appendix A shows the meta-analytic results when taking into account all levels of these interaction effects and all analyses consistently show smaller effect size estimates. In line with recommendations by van Aert et al. (2016) we checked the money priming literature on reporting errors in p values. Appendix B shows the results of the *statcheck* analysis, and it turns out that of the published articles included in our meta-analysis, 53.7% contained at least one reporting *error* and 9.8% contained a *decision error*. However, these results are similar to those of other fields within psychology (Nuijten, Hartgerink, van Assen, Epskamp, & Wicherts, 2016).

Main Meta-Analysis

Figure 1 shows a funnel plot for all experiments included in our meta-analysis, as well as separate funnel plots for published studies, unpublished studies, preregistered studies, main effects, and simple effects drawn from studies focusing on interactions. Each black dot is a single experiment with its own Hedges' g effect size estimate (x axis) and standard error (y axis). The dotted lines show the overall Hedges' g effect size estimates and the dashed lines mark its 95% confidence interval. The funnel plots of the published and unpublished studies are strikingly different, and both differ dramatically from the funnel plot of preregistered studies.

The random effects model shows a significant effect for the entire sample of studies ($g = 0.31$, $p < .001$, 95% CI [0.26, 0.36]), and also for the 146 published ($g = 0.42$, $p < .001$, 95% CI [0.35, 0.49]) and 101 unpublished studies separately ($g = 0.15$, $p < .001$, 95% CI [0.09, 0.21]). However, the 47 preregistered experiments did not show a significant overall effect ($g = 0.01$, $p = .692$, 95% CI [-0.03, 0.05]). It is important to note, however, that of those 47 preregistered experiments, 38 focused on the dependent measure *system justification*. The effect size gap between preregistered and non-pre-registered studies should therefore be interpreted with caution as this difference may be confounded by the type of dependent measure used in these preregistered studies. Forty-two

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[9] A large number of excluded studies were published in financial journals and focused on monetary issues. Furthermore, because the word prime can have different meanings (e.g. prime minister), we also encountered many studies published in political journals. Note that we have included all of these excluded studies in our supplemental Excel database, including a reason for exclusion.

Table 1

Details on All Experiments Included in Our Meta-Analysis[a]

Study	Journal	Setting	Prime type	Dependent measure
Aarts et al. (2005), Study 2	*Social Cognition*	Lab	Descrambling	Time taken on a mouse-click task (lesser time is indicative of greater goals directed behavior).
Balcerowicz, Gotowala, Kaczkowska, and Zalewska (2014), Study R2	Unpublished	Field	Handling	Amount of money spent on a birthday gift for best friend.
Boucher and Kofos (2012), Study 1	*Journal of Experimental Social Psychology*	Lab	Descrambling	Difference in response time between congruent and incongruent trials on Stroop Task
Boucher and Kofos (2012), Study 2	*Journal of Experimental Social Psychology*	Lab	Descrambling	Number of correct responses in a timed anagram task
Capaldi and Zelenski (2016), Study American Sample	*The Journal of Social Psychology*	Online	Descrambling	DV1: Sustainable Willingness. DV2: Social Value Orientation. DV3: Social Connectedness
Capaldi and Zelenski (2016), Study Canandian Sample	*The Journal of Social Psychology*	Lab	Descrambling	DV1: Sustainable Willingness. DV2: Social Value Orientation. DV3: Social Connectedness
Caruso and Shapira (2015), Study CLAMS	Unpublished	Field	Handling	DV1: Cassidy and Lynn (1989) Achievement Motivation Scale (CLAMS). DV2: Cognitive reflection test.
Caruso and Shapira (2015), Study Competitiveness	Unpublished	Field	Handling	Competitiveness Subscale of Cassidy and Lynn's (1989) Achievement Motivation Scale
Caruso and Shapira (2015), Study CRT	Unpublished	Lab	Handling	Cognitive reflection test
Caruso and Shapira (2015), Study Entitlement	Unpublished	Online	Descrambling	Psychological entitlement scale
Caruso and Shapira (2015), Study Equity Sens.	Unpublished	Field	Handling	Equity sensitivity. DV1: Entitlement subscale. DV2: Benevolence subscale.
Caruso and Shapira (2015), Study Grades	Unpublished	Field	Descrambling	Satisfaction with grade on a science class exam
Caruso and Shapira (2015), Study GRE	Unpublished	Lab	Visual	DV1: GRE Verbal. DV2: GRE Math.
Caruso and Shapira (2015), Study Input-Outcome	Unpublished	Online	Descrambling	Deservingness of an evaluation on a group project at work
Caruso and Shapira (2015), Study Parenting 1	Unpublished	Field	Visual	Meaning in parenting
Caruso and Shapira (2015), Study Parenting 2	Unpublished	Field	Visual	Meaning in parenting
Caruso and Shapira (2015), Study REI	Unpublished	Online	Descrambling	DV1: Rational Engagement Scale. DV2: Experiential Ability Scale
Caruso and Shapira (2015), Study Subjective Wealth	Unpublished	Online	Handling	DV1: Subjective Wealth. DV2: Relative Subjective Wealth. DV3: Agency. DV4: Communion
Caruso and Shapira (2015), Study SVI	Unpublished	Online	Handling	Achievement valuation
Caruso and Shapira (2015), Study SVO	Unpublished	Lab	Descrambling	Social value orientation (higher -> more prosocial)
Caruso et al. (2017), Study 1	Published	Online	Combination	Various
Caruso et al. (2017), Study 2	Published	Online	Combination	Various
Caruso et al. (2017), Study 3	Published	Lab	Combination	Various
Caruso and Shapira (2015), Study Acceptability of purchasing nonarket goods	Unpublished	Online	Descrambling	Acceptability of purchasing nonmarket goods
Caruso and Shapira (2015), Study Belief in just world	Unpublished	Field	Descrambling	Belief in a just world
Caruso and Shapira (2015), Study Dollars vs. Rupees - Indian Sample	Unpublished	Online	Visual	Fair market ideology
Caruso and Shapira (2015), Study Dollars vs. Rupees - US Sample	Unpublished	Online	Visual	Fair market ideology
Caruso and Shapira (2015), Study Economic System Justification	Unpublished	Online	Visual	Economic system justification
Caruso and Shapira (2015), Study FMI	Unpublished	Online	Visual	Fair market ideology
Caruso and Shapira (2015), Study FMI	Unpublished	Online	Visual	Fair market ideology
Caruso and Shapira (2015), Study Ideology order effect	Unpublished	Online	Visual	Fair market ideology

(*table continues*)

Table 1 (*continued*)

Study	Journal	Setting	Prime type	Dependent measure
Caruso and Shapira (2015), Study Social Dominance Orientation 1	Unpublished	Field	Descrambling	Social dominance orientation
Caruso and Shapira (2015), Study Social Dominance Orientation 2	Unpublished	Field	Descrambling	Social dominance orientation
Caruso and Shapira (2015), Study System Justification	Unpublished	Lab	Visual	System justification
Caruso et al. (2013), Study 1	*Journal of Experimental Psychology: General*	Lab	Visual	System justification
Caruso et al. (2013), Study 2	*Journal of Experimental Psychology: General*	Lab	Descrambling	Belief in a just world
Caruso et al. (2013), Study 3	*Journal of Experimental Psychology: General*	Field	Descrambling	Social dominance orientation
Caruso et al. (2013), Study 4	*Journal of Experimental Psychology: General*	Online	Visual	Fair market ideology
Caruso et al. (2013), Study 5	*Journal of Experimental Psychology: General*	Field	Visual	Fair market ideology
Chen, Kemp, and Gaffikin (2014), Study 1	Unpublished	Lab	Thinking	Importance government goods
Chen et al. (2014), Study 2	Unpublished	Online	Thinking	Importance government goods
Chen et al. (2014), Study 3	Unpublished	Lab	Thinking	Vaccination intention
Crawford et al. (2016), Study 1	Unpublished	Online	Descrambling	System Justification
Gasiorowska (2013), Study 1	*Psychologia Spoleczna*	Field	Handling	Amount of money spent on a birthday gift for best friend.
Gasiorowska (2013), Study 2	*Psychologia Spoleczna*	Online	Handling	DV1: How many people participant plan to buy Christmas gift for. DV2: How much money participant plan to spend on these gifts altogether. DV3: Average value of a gift.
Gasiorowska (2013), Study 3	*Psychologia Spoleczna*	Online	Handling	DV1: How much money participant plan to spend on birthday gift for best friend. DV2: How much money best friend should at least spend on a birthday gift for them. DV3: Ratio of DV2:DV1.
Gąsiorowska and Hełka (2012), Study 1	*Polish Psychological Bulletin*	Lab	Visual	DV1: Money transferred. DV2: Level of negative emotion. DV3: Level of satisfaction.:
Gasiorowska et al. (2016), Study 1	*Psychological Science*	Lab	Handling	DV1: Task persistence. DV2: Task performance.
Gasiorowska et al. (2016), Study 2	*Psychological Science*	Lab	Handling	Task persistence
Gasiorowska et al. (2016), Study 3a	*Psychological Science*	Lab	Handling	Helpfulness
Gasiorowska et al. (2016), Study 3b	*Psychological Science*	Lab	Handling	Helpfulness
Gasiorowska et al. (2016), Study 4	*Psychological Science*	Lab	Handling	DV1: Donation (Stickers). DV2: Desire for rewards.
Gąsiorowska, Zaleśkiewicz, and Kesebir (2018), Study 1	*Journal of Experimental Social Psychology*	Lab	Visual	Death related thoughts
Gąsiorowska et al. (2018), Study 2	*Journal of Experimental Social Psychology*	Lab	Combination	Death related thoughts
Gąsiorowska et al. (2018), Study 3	*Journal of Experimental Social Psychology*	Online	Descrambling	Death related thoughts
Gąsiorowska et al. (2018), Study 4	*Journal of Experimental Social Psychology*	Online	Descrambling	Death related thoughts
Gąsiorowska et al. (2018), Study 5	*Journal of Experimental Social Psychology*	Online	Visual	Death related thoughts
Gąsiorowska et al. (2018), Study 6	*Journal of Experimental Social Psychology*	Online	Visual	Death related thoughts
Gasiorowska et al. (2012), Study 1	*Journal of Economic Psychology*	Lab	Visual	DV1: Non-selfish choice in "Prosocial game". DV2: Non-selfish behavior in "sharing game".
Gasiorowska et al. (2012), Study 2	*Journal of Economic Psychology*	Lab	Handling	Number of crayons brought to experimenter (indicative of willingness to help)
Gino and Mogilner (2014), Study 1	*Psychological Science*	Lab	Descrambling	Extent of cheating.
Gleibs et al. (2013), Study 1	*British Journal of Social Psychology*	#N/A	Descrambling	Correlation between income and well-being

Table 1 (*continued*)

Study	Journal	Setting	Prime type	Dependent measure
Goltermann and Dorrough (2010), Study 1	Unpublished	Lab	Descrambling	Trust
Hansen et al. (2013), Study 1	*Journal of Consumer Research*	Field	Descrambling	Level of abstractness on behavioral identification task
Hansen et al. (2013), Study 2	*Journal of Consumer Research*	Field	Handling	Number of categories in the objects classification task (indicative of abstractness)
Hansen et al. (2013), Study 3	*Journal of Consumer Research*	Lab	Visual	Letter task that measures abstractness (repeated measure)
Hansen et al. (2013), Study 4	*Journal of Consumer Research*	#N/A	Visual	Product evaluation
Hansen et al. (2013), Study 5	*Journal of Consumer Research*	#N/A	Visual	Product evaluation
Harding and Jannine (2014), Study 1	Unpublished	Online	Handling	Number of caffeinated jelly beans they intent to take in a hypothetical scenario. (after removing 7 participants who never consume caffeinated product).
Hüttl-Maack and Gatter (2017), Study 1	*International Journal of Advertising*	Field	Visual	Product evaluation
Hüttl-Maack and Gatter (2017), Study 2	*International Journal of Advertising*	Field	Visual	Product evaluation
Jiang et al. (2014), Study 1	*Journal of Experimental Social Psychology*	Lab	Visual	Emotional expressivity
Jiang et al. (2014), Study 2	*Journal of Experimental Social Psychology*	Online	Visual	Expression of negative emotions
Jiang et al. (2014), Study 3	*Journal of Experimental Social Psychology*	Lab	Visual	Expression of positive emotions
Jiang et al. (2014), Study 4	*Journal of Experimental Social Psychology*	Lab	Descrambling	Judge intensity of others' emotions
Jiang et al. (2014), Study 5	*Journal of Experimental Social Psychology*	Online	Visual	Judge intensity of others' emotions
Jiang et al. (2014), Study 6	*Journal of Experimental Social Psychology*	Lab	Descrambling	Willingness to interact with individuals with different emotions (happiness, anger, neutral).
Jin et al. (2015), Study China sample	*Psychological Reports*	Lab	Visual	Response time of correct responses on a classification task.
Jin et al. (2015), Study Japan sample	*Psychological Reports*	Lab	Visual	Response time of correct responses on a classification task.
Kim (2017), Study 1	*Psychological Reports*	Lab	Visual	Product evaluation
Kim (2017), Study 2	*Psychological Reports*	Lab	Visual	Product evaluation
Kim (2017), Study 3	*Psychological Reports*	Lab	Visual	Product evaluation
Klein et al. (2014), Study abington	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study brasilia	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study charles	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study conncoll	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study csun	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study help	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study ithaca	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study jmu	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study ku	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study laurier	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study lse	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study luc	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study mcdaniel	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study msvu	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study mturk	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study osu	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study oxy	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study pi	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study psu	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study qccuny	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study qccuny2	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study sdsu	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study swps	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study swpson	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study tamu	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study tamuc	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study tamuon	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study tilburg	*Social Psychology*	Lab	Visual	System justification

(*table continues*)

Table 1 (continued)

Study	Journal	Setting	Prime type	Dependent measure
Klein et al. (2014), Study ufl	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study unipd	*Social Psychology*	Online	Visual	System justification
Klein et al. (2014), Study uva	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study vcu	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study wisc	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study wku	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study wl	*Social Psychology*	Lab	Visual	System justification
Klein et al. (2014), Study wpi	*Social Psychology*	Lab	Visual	System justification
Kouchaki et al. (2013), Study 1	*Organizational Behavior and Human Decision Process*	Lab	Descrambling	Intentions to engage in unethical behavior
Kouchaki et al. (2013), Study 3	*Organizational Behavior and Human Decision Process*	Lab	Descrambling	Deceive another participant in a game to potentially earn more money.
Kouchaki et al. (2013), Study 4	*Organizational Behavior and Human Decision Process*	Lab	Descrambling	DV1: Intention to engage in unethical behavior in an business setting vignette. DV2: Unethical behavior (cheating when reporting task performance)
Kushlev et al. (2012), Study 2	*Journal of Experimental Social Psychology*	Field	Visual	Sense of meaning and purpose
Kuzminska (2015), Study 1	Unpublished	Lab	Visual	Trust
Kuzminska and Wieczorkowska-Wierzbinska (2016), Study 1	Unpublished	Online	Visual	DV1: Desire to engage in leisure activities. DV2: Tolerance for social exclusion. DV3: Cooperativeness DV4: Helpfulness
Kuzminska and Wieczorkowska-Wierzbinska (2016), Study 2 (replication)	Unpublished	#N/A	Visual	DV1: Desire to engage in leisure activities. DV2: Tolerance for social exclusion. DV3: Cooperativeness DV4: Helpfulness
Kuzminska et al. (2016), Study 1	Unpublished	Lab	Visual	Trust
Kuzminska et al. (2016), Study 2	Unpublished	Lab	Handling	DV1: Trust towards a confederate. DV2: Physical distance between chairs (indicator of social closeness).
Kuzminska et al. (2016), Study 3	Unpublished	Online	Visual	DV1: Trusting behavior in a trust game. DV2: Trustworthy behavior in a trust game.
Kuzminska et al. (2016), Study 4	Unpublished	Lab	Thinking	DV1: Trusting behavior in a trust game. DV2: Trustworthy behavior in a trust game.
Kuzminska et al. (2016), Study 5	Unpublished	Lab	Visual	Trust
Lantian and Muller (2013), Study 1	Unpublished	Lab	Visual	Libertarianism (a measure of belief in free will.)
Lantian and Muller (2013), Study 2	Unpublished	Lab	Handling	Libertarianism (a measure of belief in free will.)
Lantian and Muller (2013), Study 3	Unpublished	Lab	Handling	Feeling of freedom
Lantian and Muller (2013), Study 4	Unpublished	Lab	Descrambling	Feeling of freedom
Lantian and Muller (2013), Study 5	Unpublished	Lab	Descrambling	Aggression
Ma et al. (2017), Study 1	*Social Behavior and Personality*	Lab	Thinking	Need for uniqueness
Ma et al. (2017), Study 2	*Social Behavior and Personality*	Lab	Visual	Need for uniqueness
Ma et al. (2017), Study 3	*Social Behavior and Personality*	Lab	Visual	Need for uniqueness
Mackowiak, Zmaczynska, and Zedzian (2016), Study R3	Unpublished	Field	Handling	Amount of money spent on a birthday gift for best friend.
Mans and Bault (2012), Study 1	Unpublished	Online	Descrambling	Trust
Mead (2016a), Study 1	Unpublished	Lab	Descrambling	Social appeal
Mead (2016a), Study 2	Unpublished	Online	Descrambling	Prosociality
Mead (2016), Study 3	Unpublished	Lab	Visual	Egocentricity
Mead (2016a), Study 4	Unpublished	Lab	Descrambling	Likability of participants (video recorded and rated by judges)
Mead (2015), Study 1	Unpublished	Lab	Visual	DV: choice between pen of same color (vs. different color) as peer (indicative of exchange vs. communal relationship desirability).
Mead (2015), Study 2	Unpublished	Lab	Descrambling	Trackability (Desire to keep one's input to a task separate from partner's input.)
Mead (2013), Study 3	Unpublished	Lab	Descrambling	Social appeal
Mead, Jiang, and Quoidbach (2017), Study 1	Unpublished	Online	Combination	Attitude towards casual sex

Table 1 (*continued*)

Study	Journal	Setting	Prime type	Dependent measure
Mogilner (2010), Study 1a	*Psychological Science*	Online	Descrambling	Intentions to engage in the following 4 DVs. DV1: Intimate relations. DV2: Socializing. DV3: Working. DV4: Commuting
Mogilner (2010), Study 1b	*Psychological Science*	Online	Descrambling	DV1: Time to spend on socializing. DV2: Time to spend in on working.
Mogilner (2010), Study 2	*Psychological Science*	Field	Descrambling	DV1: Time spent socializing. DV2: Time spent working.
Mok and De Cremer (2016), Study 1	*European Journal of Work and Organizational Psychology*	Online	Thinking	Prosociality
Mok and De Cremer (2016), Study 2	*European Journal of Work and Organizational Psychology*	Online	Thinking	Prosociality
Mok and De Cremer (2016), Study 3	*European Journal of Work and Organizational Psychology*	Online	Combination	DV1: Prosocial intentions. DV2: Organizational Identification
Mok and De Cremer (2017), Study 1	*Journal of Business Psychology*	Online	Thinking	Self-centeredness
Mok and De Cremer (2017), Study 2	*Journal of Business Psychology*	Online	Thinking	Helpfulness
Mok and De Cremer (2017), Study 3	*Journal of Business Psychology*	Lab	Thinking	Self-centeredness
Molinsky et al. (2012), Study 1	*Organizational Behavior and Human Decision Process*	Lab	Descrambling	DV1: Compassion on Sarah Scenario. DV2: Compassion on John scenario. DV3: Empathy on Sarah Scenario. DV4L Empathy on John Scenario. DV5: Unprofessionalism on Sarah scenario. DV6: Unprofessionalism on John scenario.
Molinsky et al. (2012), Study 2	*Organizational Behavior and Human Decision Process*	Lab	Thinking	DV1: Compassion. DV2: Empahty. DV3: Unprofessionalism.
Molinsky et al. (2012), Study 3	*Organizational Behavior and Human Decision Process*	Lab	Descrambling	DV1: Compassion. DV2: Empathy. DV3: Unprofessionalism.
Mukherjee et al. (2013), Study 1	*Frontiers in Psychology*	Lab	Visual	Willingness to disclose personal information.
Mukherjee et al. (2014), Study 1	*Psychological Studies*	Field	Visual	DV1: Self-reported life satisfaction. DV2: Predicted life satisfaction of classmates. DV3: Difference in life satisfaction between new and old IIT.
Park, Gasiorowska, and Vohs (2014), Study 2	Unpublished	#N/A	Handling	Preference for solo leisure activities
Pashler and Harris (2016), Study 1	Unpublished	Lab	Combination	Time before requesting help
Pfeffer and DeVoe (2009), Study 2	*Journal of Economic Psychology*	Lab	Descrambling	Helpfulness
Piechowska, Szatkowska, Tajchman, and Trembacz (2016), Study R1	Unpublished	Field	Handling	Amount of money spent on a birthday gift for best friend.
Poulin (2017), Study 1	Unpublished	Online	Descrambling	Self-perception as economic evaluator
Poulin (2017), Study 2	Unpublished	Online	Descrambling	Self-perception as economic evaluator
Reutner (2016), Study 1	Unpublished	Lab	Visual	Perceived agency vs. communion
Reutner et al. (2015), Study 1	*Social Psychological and Personality Science*	Lab	Handling	Estimated ambient temperature
Reutner et al. (2015), Study 2	*Social Psychological and Personality Science*	Lab	Handling	Estimated water temperature
Rohrer et al. (2015), Study 1	*Journal of Experimental Psychology: General*	Lab	Visual	System justification
Rohrer et al. (2015), Study 2	*Journal of Experimental Psychology: General*	Online	Descrambling	Belief in a just world
Rohrer et al. (2015), Study 3	*Journal of Experimental Psychology: General*	Lab	Descrambling	Social dominance orientation
Rohrer et al. (2015), Study 4a - US	*Journal of Experimental Psychology: General*	Online	Visual	Fair market ideology
Rohrer et al. (2015), Study 4b - Non US	*Journal of Experimental Psychology: General*	Online	Visual	Fair market ideology
Savani, King, Ma, and Vohs (2012), Study 1	Unpublished	#N/A	Visual	Number of recalled choices in the past day.
Savani et al. (2016), Study 1	*Self and Identity*	Lab	Visual	Helpfulness
Savani et al. (2016), Study 2	*Self and Identity*	Field	Descrambling	Helpfulness
Savani et al. (2016), Study 3	*Self and Identity*	Online	Descrambling	DV1: Willingness to help romantic partner. DV2: Upset by request for help.

(*table continues*)

Table 1 (*continued*)

Study	Journal	Setting	Prime type	Dependent measure
Savani et al. (2016), Study 4	*Self and Identity*	Lab	Descrambling	DV1: Willingness to help romantic partner. DV2: Upset by request for help.
Schuler (2016), Study 1	Unpublished	Online	Visual	Helpfulness
Schuler (2016), Study 12	Unpublished	Online	Descrambling	Fair market ideology
Schuler (2016), Study 13	Unpublished	Online	Visual	Categorization task (Construal level)
Schuler (2016), Study 2	Unpublished	Online	Thinking	aesthetic appeal of a product favored by the majority
Schuler (2016), Study 3	Unpublished	Online	Descrambling	DV1: Importance of helping goals (Aspiration). This was predicted by authors to have largest effect.
Schuler (2016), Study 4	Unpublished	Online	Descrambling	Belief in a just world
Schuler (2016), Study 6	Unpublished	Field	Handling	Hiring scenario/unethical decision making (see Kouchaki et al., 2013)
Schuler (2016), Study 8	Unpublished	Lab	Descrambling	DV1: Helping intention predicted to have the largest effect
Schuler and Wanke (2016), Study 1	*Social Psychological and Personality Science*	Online	Descrambling	System justification
Schuler and Wanke (2016), Study 2	*Social Psychological and Personality Science*	Online	Descrambling	Belief in a just world
Schuler and Wanke (2016), Study 3	*Social Psychological and Personality Science*	Online	Descrambling	Belief in a just world
Shi et al. (2013), Study 1	*Social Behavior and Personality*	Lab	Combination	DV1: Reactance rate in Asch task. DV2: Conformity rate in Asch task.
Su and Gao (2014), Study 2b-Resort	*Journal of Consumer Research*	Lab	Thinking	Choice of vacation location (indicative of holistic vs analytical mindset).
Su and Gao (2014), Study 2b-Vacation	*Journal of Consumer Research*	Lab	Thinking	Choice of vacation location (indicative of holistic vs analytical mindset).
Teng et al. (2016), Study 1	*Organizational Behavior and Human Decision Processes*	Lab	Visual	Approach intentions
Teng et al. (2016), Study 2	*Organizational Behavior and Human Decision Processes*	Lab	Descrambling	Approach intentions
Teng et al. (2016), Study 3	*Organizational Behavior and Human Decision Processes*	Lab	Descrambling	Approach intentions
Teng et al. (2016), Study 4	*Organizational Behavior and Human Decision Processes*	Lab	Visual	Approach intentions
Tong et al. (2013), Study 1	*Marketing Letters*	Lab	Descrambling	Relative preference for utilitarian (vs hedonic) objects
Tong et al. (2013), Study 2	*Marketing Letters*	Lab	Visual	Relative preference for utilitarian (vs hedonic) objects
Trzcinska and Kubicka (2017), Study	Unpublished	Lab	Visual	Self-worth
Trzcińska and Sekścińska (2016), Study 1	*Frontiers in Psychology*	Lab	Handling	Task perseverance
Trzcińska and Sekścińska (2016), Study 2	*Frontiers in Psychology*	Lab	Handling	Delay gratification
Vohs (2015), Study 1	Unpublished	Lab	Descrambling	Task persistence
Vohs et al. (2006), Study 1	*Science*	Lab	Combination	Task persistence
Vohs et al. (2006), Study 3	*Science*	Lab	Descrambling	Helpfulness
Vohs et al. (2006), Study 4	*Science*	Lab	Descrambling	Helpfulness
Vohs et al. (2006), Study 5	*Science*	Lab	Handling	Number of pencils participants help to pick up
Vohs et al. (2006), Study 6	*Science*	Lab	Descrambling	Donation
Vohs et al. (2006), Study 7	*Science*	Lab	Visual	Donation
Vohs et al. (2006), Study 8	*Science*	Lab	Visual	Choice of individually focused leisure activity (vs. those for 2 or more person)
Vohs et al. (2006), Study 9	*Science*	Lab	Visual	Decision to work on a task with a peer (vs. alone)
Weng, Huang, and Lin (2014), Study 1	Unpublished	Lab	Handling	Social perception (competence)
Weng et al. (2014), Study 2	Unpublished	Lab	Visual	Social perception (competence)
Weng et al. (2014), Study 3	Unpublished	Lab	Visual	Social perception (competence)
Wierzbicki and Zawadzka (2016), Study 1	*Current Psychology*	Lab	Handling	Willingness to donate to a student association.
Wierzbicki and Zawadzka (2016), Study 2	*Current Psychology*	Lab	Descrambling	Amount of time willing to devote to help others
Wierzbicki and Zawadzka (2016), Study 1	*Current Psychology*	Lab	Handling	Donation
Wierzbicki and Zawadzka (2016), Study 2	*Current Psychology*	Lab	Descrambling	Helpfulness

Table 1 (*continued*)

Study	Journal	Setting	Prime type	Dependent measure
Zaleskiewicz et al. (2013), Study 1	*PLoS ONE*	Lab	Thinking	DV1: Fear of death. (DV2: Fear of dentist. But they expected no effect for this DV)
Zaleskiewicz et al. (2013), Study 4	*Journal of Economic Psychology*	Lab	Handling	Fear of death questionnaire
Zhou et al. (2009), Study 3	*Psychological Science*	Lab	Handling	Distress rating
Zhou et al. (2009), Study 4	*Psychological Science*	Lab	Handling	Reported pain
Zhou et al. (2009), Study 5	*Psychological Science*	Lab	Thinking	Distress rating
Zhou et al. (2009), Study 6	*Psychological Science*	Lab	Thinking	Reported pain

[a] Since completing our data extraction some of the included unpublished experiments might have already been published. The full sharing of the dataset used in our meta-analysis enables future updates based on newly published studies.

included studies investigated whether a money priming effect would be more pronounced at a specific value of a moderating variable. These effects tended to be larger ($g = 0.52$, $p < .001$, 95% CI [0.39, 0.64]) than those of studies investigating main effects ($g = 0.27$, $p < .001$, 95% CI [0.22, 0.32]).

However, these summary results should be interpreted with caution. Visual inspection of Figure 1 shows that the study effect sizes are not symmetrically distributed within the white funnel. This asymmetry is confirmed by the results from Egger's test, indicating that the standard error significantly predicts the size of the money priming effects. Indeed, less precise studies with lower sample sizes show larger effects than more precise studies with higher sample sizes, which is a clear small study effect hinting at publication bias and related biases caused by researchers' pursuit of significance (e.g., their exploitation of researcher's degrees of freedom in the analysis). Remarkably, the only funnel plot in Figure 1 with a symmetrical effect size distribution and a nonsignificant Egger's test is the sample of preregistered studies. This suggests that the small study effect among nonregistered studies might indeed be caused by selection for significance that did not similarly operate among registered studies.

Metaregressions

Although the standard error significantly explains variation in effects across the entire sample of studies, a substantial amount of heterogeneity remains unexplained. The Q-test for heterogeneity of effect sizes is significant, $Q(245) = 1048.65$, $p < 0.001$, $I^2 = 81.3\%$, $\tau^2 = 0.117$ ($SE = 0.014$), indicating that the included studies are not evaluating a similar effect. To explain this large amount of heterogeneity, we performed metaregression analyses to predict variation in effect size across studies using several moderator variables, such as prime type and study setting.[10]

Table 2 shows the results of the metaregression analyses. We found the money priming effect to significantly vary both across prime types, $Q(4) = 21.05$, $p < .001$, study settings, $Q(2) = 14.83$, $p < .001$, and depending on whether a behavioral or nonbehavioral dependent measure was used, $Q(1) = 34.00$, $p < .001$. Lab studies showed significantly larger effects than online studies (reference group), while the estimated effect of field studies lay somewhere in between. Studies wherein people were asked to handle money averaged significantly larger effects than studies that used combinations of prime types (reference group). Lastly, studies using behavioral dependent measures showed significantly larger effects than studies using nonbehavioral dependent measures.

The six funnel plots in Figure 2 show the distribution of effects in our meta-analysis for behavioral and nonbehavioral experiments separately (first row). For each of those dependent measure types separate funnel plots are shown for published (second row) and unpublished studies (third row). Visual inspection of these plots shows that studies using a behavioral dependent measure showed significantly larger effect sizes ($g = 0.67$, $p < .001$, 95% CI [0.50, 0.85]) than studies using nonbehavioral dependent measures ($g = 0.24$, $p < .001$, 95% CI [0.19, 0.28]). Although published experiments showed larger effects than unpublished experiments, this difference was especially pronounced for studies using a behavioral outcome measure. Published behavioral experiments showed no asymmetric funnel plot according to Egger's test and a very large overall effect size estimate ($g = 0.85$, $p < .001$, 95% CI [0.67, 1.02]). Although one published behavioral experiment with an effect size of $g = 2.95$ (Gasiorowska, Zaleskiewicz, & Wygrab, 2012; Experiment 2) could be considered an outlier, exclusion of this study still resulted in a very large overall effect size estimate for published behavioral experiments ($g = 0.67$, $p < .001$, 95% CI [0.40, 0.95]). In general, behavioral dependent measures were much less often used ($n = 42$) than nonbehavioral dependent measures ($n = 200$). Furthermore, although 46 of the 200 experiments with nonbehavioral outcomes were preregistered, this was true for only one experiment using a behavioral outcome measure, a significant difference, $\chi^2(1) = 9.697$, $p = .002$.

The bottom part of Table 2 displays metaregressions predicting the money priming effect using other study characteristics than the three design types reported above. It turned out that effect sizes were significantly predicted by the study's standard error (Egger's test; $\beta = 2.59$, 95% CI [1.96, 3.22], $p < .001$), by whether a study was published or not ($\beta = 0.26$, 95% CI [0.17, 0.34], $p < .001$), and by whether a study was preregistered or not ($\beta = -0.34$, 95% CI [-0.44 -0.24], $p < .001$). Overall, smaller effects were found for preregistered studies, for unpublished studies, and for more precise studies with larger sample sizes. All of these results align with the notion of substantial biases in the literature on money priming.

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[10] For all studies included in our meta-analysis, the first two authors independently coded each study's setting and prime type. The coders agreed on 95.7% of the study settings, on 96.2% of the prime types, and on 94.8% of the behavioral versus nonbehavioral dependent measures. After discussing these initial differences, the two coders reached a 100% agreement on all categories.
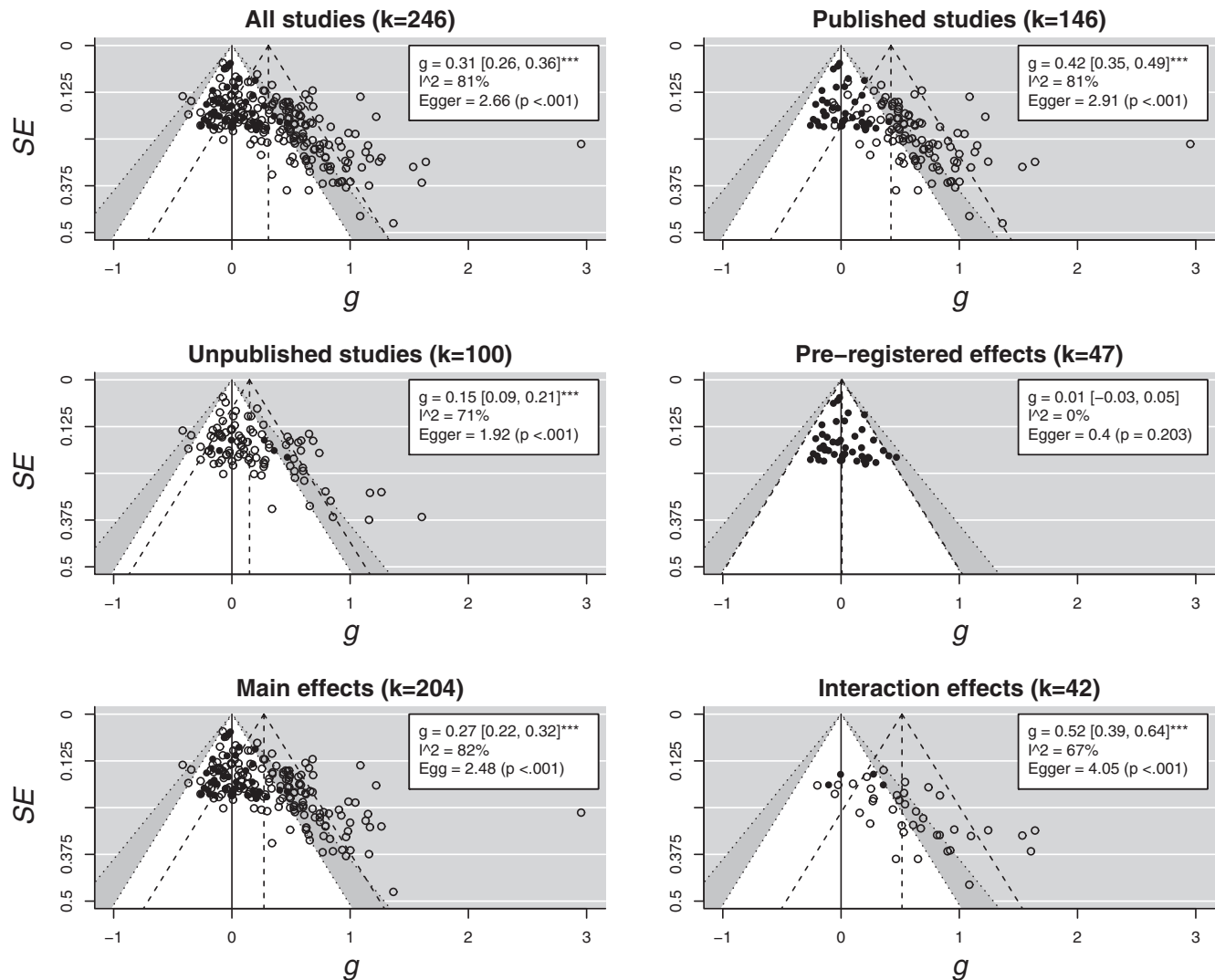
Figure 1. Funnel plots of all studies, published studies, unpublished studies, preregistered studies, main effects, and interaction effects. k = number of included effects. g = Hedges' g random effects model estimate (center of dotted funnel), including 95% confidence interval. I^2 = heterogeneity measure; Egger = Egger's test regression coefficient and p value; The white- and gray funnel represent a 95% and 99% confidence level, respectively. Black dots represent preregistered studies. *** $p < .001$.

Subset Analyses for Prime Types and Study Settings

In the metaregression analyses reported above, the Q tests for residual heterogeneity indicated that substantial amounts of unexplained differences across effect size remained after taking into account the effect of the study characteristics. To further reduce the heterogeneity in the entire sample of studies on money priming, we considered more specific subsets of the data by splitting the dataset based on all different combinations of study settings, prime types, and behavioral versus nonbehavioral dependent measures. For subsets with five or more experiments, Table 3 shows the estimates of the random effects meta-analysis, as well as regression coefficients of Egger's test and the p-uniform and 3PSM effect size estimates adjusted for publication bias. Subsets shown in italic subsets yielded asymmetric funnel plots according to Egger's test, whereas subsets

printed in bold showed significant evidence of publication bias according to the three-parameter selection model.

Figure 3 shows the funnel plots for each subset listed in Table 3. To aid interpretation of potential biases related to significance, we centered the white funnels at a Hedges' g of zero and let its boundaries denote the 95% confidence interval, so that dots outside the white funnel mark a significant result. The boundaries of the gray funnel surrounding the white funnel mark the 99% confidence region, and any dots within this region represent p values between .01 and .05. The dotted funnel is centered around the subsets' mean effect size estimate resulting from the random effects model. Besides the I^2 heterogeneity statistic, each funnel plot displays a p value for Egger's test and the p values from the publication bias tests based on p-uniform and 3PSM. When the I^2 heterogeneity statistic exceeds

Table 2
Moderating Influence of Prime Type, Study Setting, and Other Characteristics

Moderator	Metaregression		Subgroup meta-analysis		
	Beta coefficient	95% CI	k	Hedges' g	95% CI
Prime type					
Intercept (Combination)	.16	[−.11, .42]	9	.29	[−.02, .59]
Visual	.08	[−.19, .35]	106	.22***	[.16, .28]
Descrambling	.12	[−.16, .39]	79	.26***	[.19, .33]
Handling	.41**	[.12, .71]	36	.6***	[.40, .80]
Thinking	.28	[−.04, .61]	17	.49***	[.22, .75]
Moderator test: $Q(4) = 22.48$***					
Residual heterogeneity test: $Q(241) = 1007.48$*** ($I^2 = 79.2\%$)					
Study setting					
Intercept (Online)	.14***	[.06, .22]	81	.13***	[.07, .18]
Lab	.27***	[.16, .37]	134	.43***	[.35, .51]
Field	.12	[−.06, .31]	25	.33***	[.19, .47]
Moderator test: $Q(2) = 16.50$***					
Residual heterogeneity test: $Q(243) = 976.62$*** ($I^2 = 79.8\%$)					
Dependent measure type					
Intercept (Non-behavioral)	.25***	[.20, .30]	200	.24***	[.19, .28]
Behavioral	.40***	[.26, .53]	42	.67***	[.50, .85]
Moderator test: $Q(1) = 34.00$***					
Residual heterogeneity test: $Q(240) = 897.72$*** ($I^2 = 77.8\%$)					
Other study characteristics					
Intercept	−.30***	[−.42, −.17]			
Standard error (Egger's test)	2.60***	[1.96, 3.22]			
Published	.26***	[.17, .34]			
Preregistered	−.34***	[−.44, −.24]			
Multiple dependent measures	.05	[−.04, .15]			
Interaction effect	.03	[−.08, .14]			
Moderator test: $Q(5) = 218.92$***					
Residual heterogeneity test: $Q(240) = 555.73$*** ($I^2 = 60.9\%$)					

** $p < .01$.   *** $p < .001$.

50%, the *p*-uniform method typically overestimates the effect size estimate (van Aert et al., 2016). Hence, for such subsets we suggest to interpret only the random effects model mean effect size estimate, or on the 3PSM estimate adjusted for publication bias. When the I^2 statistics exceeds 50% (in all but two subsets), we will base our publication bias test upon the 3PSM only.

Almost all subsets show random effects models with statistically significant mean effect size estimates. For instance, lab studies using visual, descrambling, handling, or thinking primes all showed significant overall effects. Online studies showed significant mean effect size estimates when visual, descrambling, or thinking primes were used. When inspecting the funnel plots in Figure 3 it becomes clear that for each prime type the plots involving lab studies (first column) showed larger effects than those involving online studies (second column). Furthermore, independent of study setting, priming studies wherein people were asked to handle money (third row) tended to show larger effects than other prime types.

However, when interpreting these results, we should take into account the possibility of bias. After adjusting for the influence of publication bias, the 3PSM showed only two subsets with a significant mean money priming effect. Subset 2, containing 12 lab studies with a descrambling prime type and a behavioral outcome measure, showed a medium 3PSM adjusted effect size estimate ($g = 0.42$, 95% CI [0.11, 0.74], $p = .008$). However, these results should be interpreted with caution because Egger's test indicated an asymmetric funnel plot and the 3PSM found support for the presence of publication bias. Because this subset showed an I^2 smaller than 50%, we are

allowed to interpret the p-uniform estimate adjusted for publication bias ($g = 0.41$, 95% CI [−0.54, 0.78], $p = .122$), which is similar to the estimate provided by the 3PSM, although the much wider confidence interval renders the *p*-uniform estimate nonsignificant (thereby highlighting its uncertainty). The second subset with a significant 3PSM effect size estimate was subset 3, containing 13 lab studies with a money-handling prime type and a behavioral outcome measure. This subset showed a large 3PSM adjusted effect size estimate ($g = 0.77$, 95% CI [0.08, 1.46], $p = .029$), an even larger random effects model estimate ($g = 0.92$, 95% CI [0.52, 1.31], $p < .001$) and no sign of publication bias based on Egger's test, *p*-uniform, and 3PSM. Subset 3 is not the only subset without evidence of publication bias, as indicated by the absence of both bold and italic print in Table 3. Subsets 9 and 13 concern experiments using a nonbehavioral outcome measure and a descrambling prime. They show no evidence of publication bias and a small yet significant random effects model estimates in online ($g = 0.12$, 95% CI [0.04, 0.21], $p = .003$) and field settings ($g = 0.20$, 95% CI [0.01, 0.38], $p = .035$).

Subset Analyses for Dependent Measures

A second way to reduce the heterogeneity in our meta-analysis is to create subsets based on frequently occurring dependent measures. This allows for less crude inferences on how the money priming effect varies across dependent measures than that provided by the distinction between behavioral and nonbehavioral outcomes. For dependent measures that have been used in five or
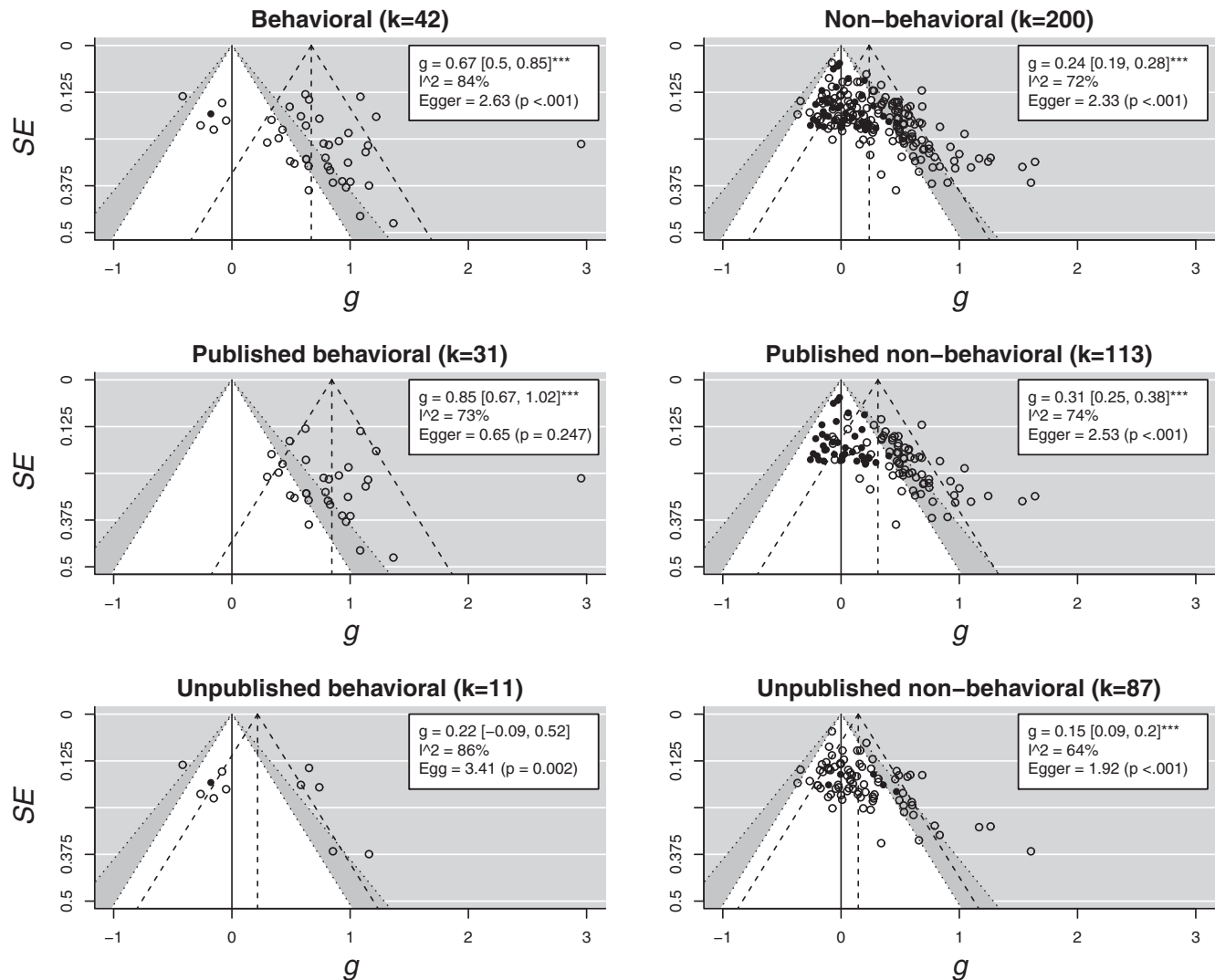
Figure 2. Funnel plots of all behavioral, published behavioral, unpublished behavioral, nonbehavioral, published nonbehavioral, and unpublished nonbehavioral studies. k = number of included effects. g = Hedges' g random effects model estimate (center of dotted funnel), including 95% confidence interval. I^2 = heterogeneity measure; Egger = Egger's test regression coefficient and p value; The white- and gray funnel represent a 95% and 99% confidence level, respectively. Black dots represent preregistered studies. *** $p < .001$.

more studies, Table 4 shows the estimates of the random effects meta-analysis, as well as regression coefficient of Egger's test and the p-uniform and 3PSM effect size estimates adjusted for publication bias. Figure 4 shows a funnel plot for each subset listed in Table 4.

Four of the frequently used dependent measures did not show statistically significant mean effect estimates in the random effects models: belief in a just world, fair market ideology, and system justification. On the other hand, significant mean effect size estimates were found for the dependent measures trust, product evaluation, helpfulness, death related thoughts, and experiments where we had to aggregate the effect across multiple dependent measures. However, most of these subsets show either an asymmetric funnel plot according to Egger's test, or signs of publication bias according to 3PSM or p-uniform. The only subset without signs of bias

concerns studies using the belief in a just world questionnaire as outcome measure, yet this subset's random effects model estimate fails to reach statistical significance ($g = 0.11$, 95% CI [−0.08, 0.30], $p = .239$). After adjusting for publication bias, only one dependent measure showed a significant 3PSM effect size estimate, namely product evaluation. The 3PSM estimate adjusted for publication bias ($g = 0.34$, 95% CI [0.16, 0.52], $p < .001$) was comparable with the p-uniform estimate ($g = 0.36$, 95% CI [0.02, 0.64], $p = .022$), except that the 3PSM estimate had a narrower confidence interval. This particular subset showed almost no heterogeneity ($I^2 = 0\%$) and only one of the tests for publication bias showed a significant results (3PSM). Although nonsignificant results on these tests might imply the absence of publication bias in a subsets, we have to keep in mind the possibility of making a Type II error in testing for publication bias with five to seven

Table 3
Subsets of Money Priming Studies Based on Combinations of Settings and Prime Types

Nr.	Prime	Setting	k	I^2	Random effect model estimate Hedges' g [95% CI]	p-uniform adjusted estimate [95% CI]	3PSM adjusted estimate [95% CI]	Egger's test regression coefficient [95% CI]
					Behavioral dependent measures			
1	*Visual*	*Lab*	*10*	*66.6%*	*.71 [.43, .99]***	*.77 [.52, 1.03]***	*.42 [−.05, .90]*	*3.87 [2.10, 5.65]***
2	*Descrambling*	*Lab*	*12*	*42.5%*	*.58 [.38, .78]***	*.41 [−.54, .78]*	*.42 [.11, .74]***	*2.51 [.21, 4.80]**
3	Handling	Lab	13	90.3%	.92 [.52, 1.31]***	.95 [.71, 1.15]***	.77 [.08, 1.46]*	.78 [−1.03, 2.59]
					Nonbehavioral dependent measures			
4	*Visual*	*Lab*	*51*	*56.6%*	*.20 [.12, .28]***	*.29 [.10, .46]***	*−.03 [−.05, .11]*	*1.79 [.86, 2.72]***
5	*Descrambling*	*Lab*	*23*	*60.5%*	*.33 [.21, .45]***	*.40 [.20, .56]***	*.14 [−.02, .30]*	*3.09 [1.72, 4.46]***
6	*Handling*	*Lab*	8	*75.4%*	*.51 [.16, .86]***	.69 [.05, 1.15]*	.25 [−.26, .76]	*7.49 [4.07, 10.92]***
7	*Thinking*	*Lab*	10	*86.0%*	*.66 [.31, 1.01]***	.77 [.44, 1.22]***	.52 [−.03, 1.08]	*6.14 [4.35, 7.93]***
8	*Visual*	*Online*	31	*65.2%*	*.11 [.03, .20]***	.37 [.06, .56]*	.10 [−.03, .24]	*1.48 [.60, 2.37]***
9	Descrambling	Online	32	54.7%	.12 [.04, .21]***	.27 [.04, .44]*	.08 [−.03, .18]	.14 [−1.08, 1.36]
10	*Handling*	*Online*	5	*81.2%*	*.17 [−.09, .44]*	.29 [−.63, .70]	.10 [−.27, .47]	*2.82 [1.26, 4.38]***
11	*Thinking*	*Online*	6	*76.9%*	*.36 [.01, .72]**	.61 [−.13, .93]*	.22 [−.27, .72]	*3.07 [.70, 5.44]***
12	**Visual**	**Field**	**7**	**26.9%**	**.33 [.16, .50]***	**−.02 [−.88, .37]**	**.02 [−.13, .18]**	**2.02 [−.78, 4.81]**
13	Descrambling	Field	8	59.9%	.20 [.01, .38]*	−.09 [−16.20, .66]	.06 [−.08, .21]	1.81 [−.49, 4.11]
14	*Handling*	*Field*	*9*	*87.4%*	*.44 [.09, .80]**	1.02 [.59, 1.32]***	.56 [−.01, 1.13]	*4.37 [2.84, 5.90]***

Note. Bold subsets show evidence of publication bias according to either *p*-uniform or the three-parameter selection model (3PSM). Italic subsets show an asymmetric funnel plot according to Egger's test.
* $p < .05$. ** $p < .01$. *** $p < .001$.

studies per subset. Thus, we wish to emphasize that with respect to publication bias in small subsets, absence of evidence does not imply evidence of absence.

Discussion

In this study, we performed a comprehensive meta-analysis on both published and unpublished experiments that used a money priming manipulation. Our results yielded a significant overall effect size estimate. This not only applies to the complete dataset, but also to the subsets of published and unpublished studies. However, large preregistered studies that control for commonly identified biases in the analysis of data and reporting of results failed to show a robust mean effect of the money primes. Our moderator analyses indicated that the money priming effects varied across study design. Overall, the largest money priming effects were found in lab-studies investigating a behavioral dependent measure using a money priming technique where people actively handled money (e.g., counting bank notes).

Our metaregression indicated that studies with a small sample size showed larger effects than studies with larger sample size, suggesting the presence of publication bias in the money priming field. This finding was corroborated by another moderator analysis, showing that the money priming effect tended to be larger for published studies than for unpublished studies. Although there was an apparent difference in money priming effects between published and unpublished studies, the contrast between preregistered and non-pre-registered studies was most noticeable. Preregistered studies were often highly powered and hence precisely estimated the money priming effect to be absent, with almost no heterogeneity across studies' effect sizes. However, this result may not be generalizable to the entire money priming field because most of the preregistered studies show very specific design with visual money primes and the nonbehavioral dependent measure system justification.

Our subset analyses showed that, even without adjustment for publication bias, three frequently used dependent measures did not show a statistically significant effect: belief in a just world, fair market ideology, and system justification. Interestingly, these dependent measures all focused on social or political constructs. It is safe to conclude that our meta-analysis fails to support an effect of money priming for these kinds of measures. Our subset analyses also identified four frequently used dependent measures with statistically significant and positive money priming effects: helpfulness, trust, death-related thoughts, and product evaluation. However, after adjusting these effects for the influence of publication bias only the effect of the product evaluation dependent measures remained significant.

Our subset analyses based on different combinations of prime types, study settings, and behavioral versus nonbehavioral outcome measures identified three subsets without any evidence for publication bias. Two of those subsets showed small effect size estimates and involved experiments using nonbehavioral outcome measures and descrambling primes in either online or field settings. The third subset concerned lab studies investigating a behavioral outcome measure with money handling primes. Although this subset showed large effects without any sign of publication bias, it still displays substantial heterogeneity. To investigate the source of this heterogeneity, future research should use sufficiently powerful preregistered replications either using one of these exact subset designs, or contrasting multiple design types in a factorial design (along the lines of Caruso et al., 2017). Although experiments with behavioral outcome measures showed much larger effects than experiments with nonbehavioral outcomes, we found only one preregistered study using a behavioral outcome. This makes it especially important that future research focuses on replicating these behavioral outcome studies in a sufficiently powered preregistered replication.
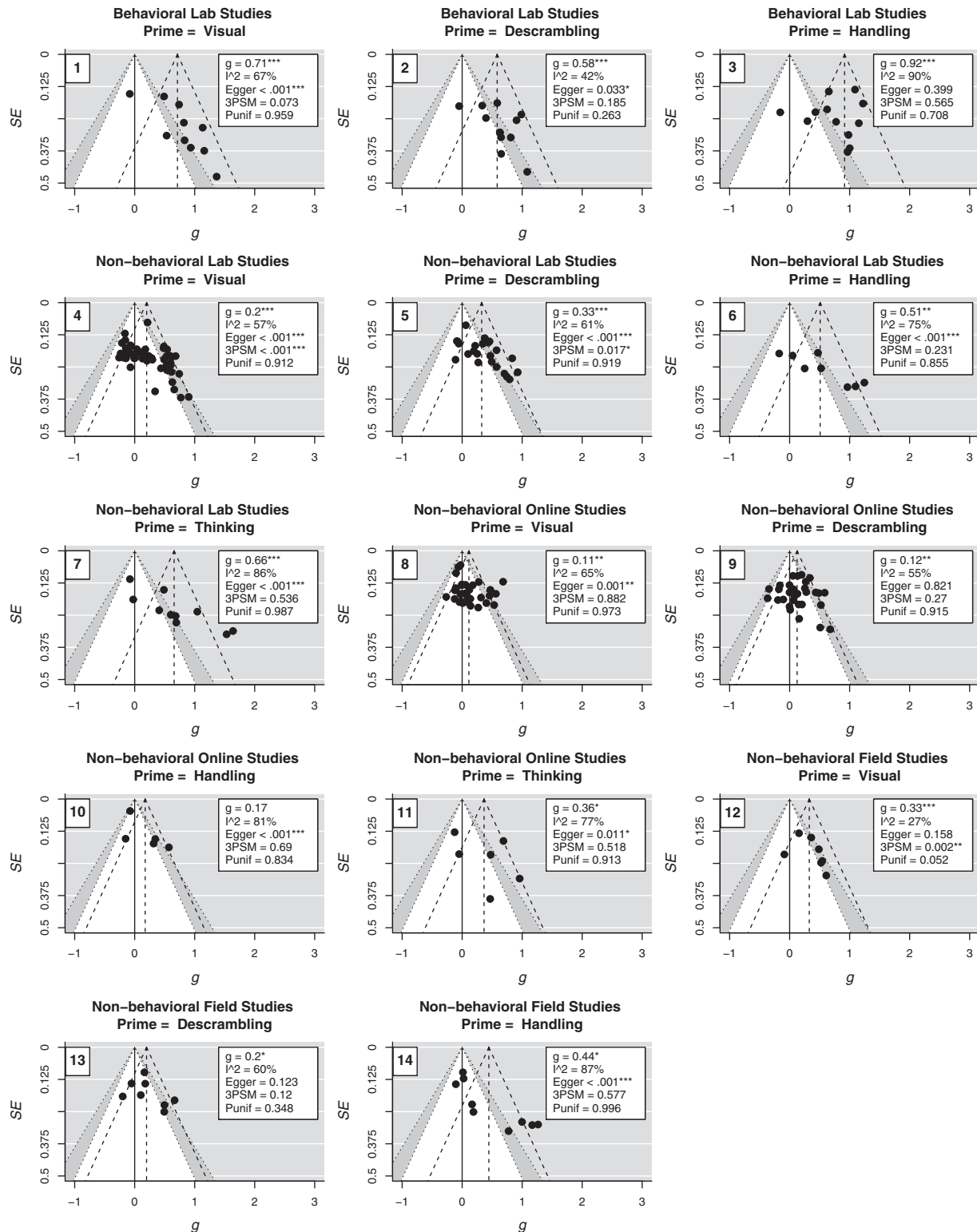
Figure 3. Funnel plots for subsets (number in top left) based on different study designs. g = Hedges' g random effects model estimate (center of dotted funnel); I^2 = heterogeneity measure; Egger = Egger's test p value; 3PSM = Three-parameter selection model p value for publication bias test; Punif = p-uniform p value of publication bias test. The white- and gray funnel represent a 95% and 99% confidence level, respectively. * $p <$ .05. ** $p <$ .01. *** $p <$ .001.

Table 4
Subsets of Money Priming Studies Based on Frequently Used Dependent Measures

Nr.	Dependent measure	k	I^2	Random effect model estimate Hedges' g [95% CI]	p-uniform adjusted estimate [95% CI]	3PSM adjusted estimate [95% CI]	Egger's test regression coefficient [95% CI]
1	*Aggregated*	34	87.9%	.27 [.14, .4]***	.47 [.33, .60]***	.18 [−.01, .38]	2.85 [2.13, 3.58]***
2	Belief in a just world	7	62.1%	.11 [−.08, .3]	.36 [−.29, .73]	.20 [−.21, .61]	.32 [−1.70, 2.33]
3	*Death related thoughts*	12	73.1%	.35 [.16, .55]***	.51 [.35, .67]***	.23 [−.05, .51]	6.15 [3.20, 9.11]***
4	*Fair market ideology*	15	16.8%	.07 [−.02, .15]	−.20 [−3.96, .37]	.02 [−.08, .12]	1.74 [.06, 3.42]*
5	*Helpfulness*	10	76.3%	.67 [.37, .97]***	.69 [.05, 1.02]*	.34 [−.30, .97]	3.28 [.76, 5.80]*
6	**Product evaluation**	7	0%	.53 [.36, .7]***	.36 [.02, .64]*	.34 [.16, .52]***	2.32 [−.56, 5.19]
7	*System justification*	41	0%	0 [−.05, .05]	−.24 [−5.42, .59]	−.02 [−.07, .02]	.80 [.07, 1.53]*
8	**Trust**	5	58.4%	.50 [.17, .84]**	.35 [−.55, .76]	.02 [−.58, .62]	2.81 [−1.41, 7.04]

Note. Bold subsets show evidence of publication bias according to either *p*-uniform or the three-parameter selection model (3PSM). Italic subsets show an asymmetric funnel plot according to Egger's test.
* $p < .05$.   ** $p < .01$.   *** $p < .001$.

Such preregistered replications are important to the money priming field (and psychological science in general), because they provide a good tool to determine whether a particular effect is present or not (arguably better than meta-analysis; van Elk et al., 2015). Meta-analyses are as good as the studies they contain and do therefore not provide a final answer to a particular research question. Similar to empirical research, conclusions in metaresearch are conditional on the quality of the data and the methods used. It is often difficult to detect within study biases, such as experimenter bias, demand characteristics, and the opportunistic use of the many degrees of freedom in the analyses of data and reporting of results in their pursuit for significant effects. Although meta-analyses themselves can certainly suffer from selection and publication bias, they still provide researchers with valuable design information. In the present study, for instance, we have provided researches in the money priming field with a clear direction for future research, so they can test specific hypotheses on potential moderators.

In recent years, there have been increasing concerns that scientific research may be vulnerable to various biases that threaten the veracity of scientific findings. Examples of these biases include publication bias (Ioannidis, 2005), flexibility in collecting and analyzing data, and selective reporting of findings (Bakker, van Dijk, & Wicherts, 2012; Simmons, Nelson, & Simonsohn, 2011; Wicherts et al., 2016). Ioannidis (2005) also highlighted several risk factors for publication biases (e.g., small sample size, small effect sizes, high number of dependent variables, high flexibility in designing and analyzing data, high popularity of the field), many of which may be applicable to several popular lines of psychological research. For instance, published findings in psychology appear to exhibit excessive significance in relation to their power (e.g., Schimmack, 2012) and underreporting of experimental conditions and outcome variables (Franco, Malhotra, & Simonvits, 2016). Further, a large-scale replication attempt in social psychology found a successful replication rate between 39% to 47% (depending on the criterion used; Open Science Collaboration, 2015). Taken together, these findings suggest that psychological research may indeed be plagued by the aforementioned biases.

Currently, based on our statcheck analysis, we have no direct evidence for a particularly high prevalence of reporting errors in the money priming field. However, errors in statistical reporting are only one (and possibly not the most prevalent) type of researcher degree of freedom. For instance, Wicherts et al. (2016) identified more than 30

researcher's degrees of freedom in setting up, running, analyzing, and reporting of psychological experiments that could all be used opportunistically in the pursuit of significance. Emerging fields typically allow for more maneuverability in the analysis of data than more established fields, leading to more potential for bias (Ioannidis, 2005). Several tests have been developed to detect such biases. The asymmetric funnel plots and the results from Egger's test, *p*-uniform, and the three-parameter selection model render publication bias and related biases in studies on money priming likely. Combining publication bias with the opportunistic use of researcher degrees of freedom increases the chance on false positive findings (i.e., Type I errors) and inflated effect sizes (Bakker et al., 2012). Yet even by itself, publication bias might still result in an overabundance of false positive findings, especially when a field contains many underpowered studies (Button et al., 2013; Ioannidis, 2005). Earlier research suggests that the money priming field contains a substantial number of underpowered studies (Vadillo et al., 2016). Based on four different methods, the authors argued for the presence of publication bias and p-hacking. In our study, we used different methods (e.g., *p*-uniform and 3PSM) on a more extensive selection of studies and arrived at similar conclusions, suggesting that various biases render false positive findings likely in the money priming field.

To minimize future risk of publication bias and the opportunistic use of researcher degrees of freedom, experiments should be preregistered including a detailed analysis plan for each tested hypothesis and a clear distinction between the exploratory and confirmatory hypotheses that will be tested (Chambers, 2013; Wagenmakers, Wetzels, Borsboom, van der Maas, & Kievit, 2012). After collecting the data, the results can be summarized at the preregistration website, even when the study does not get published. To make science more transparent, researchers could also to consider to publish the raw data alongside a research article (Wicherts, 2013; Wicherts & Bakker, 2012).

We urge researchers designing a replication study to invest in collecting a large sample size. Recent evidence suggests that mere replication is not always beneficial (Nuijten, van Assen, Veldkamp, & Wicherts, 2015). When the statistical power of a replication study is smaller than the statistical power of the original study, then the effect size estimate can become biased if publication bias works on the replications. This finding highlights the importance of aiming for high-powered (replication) studies. High-powered studies will lead to more precise
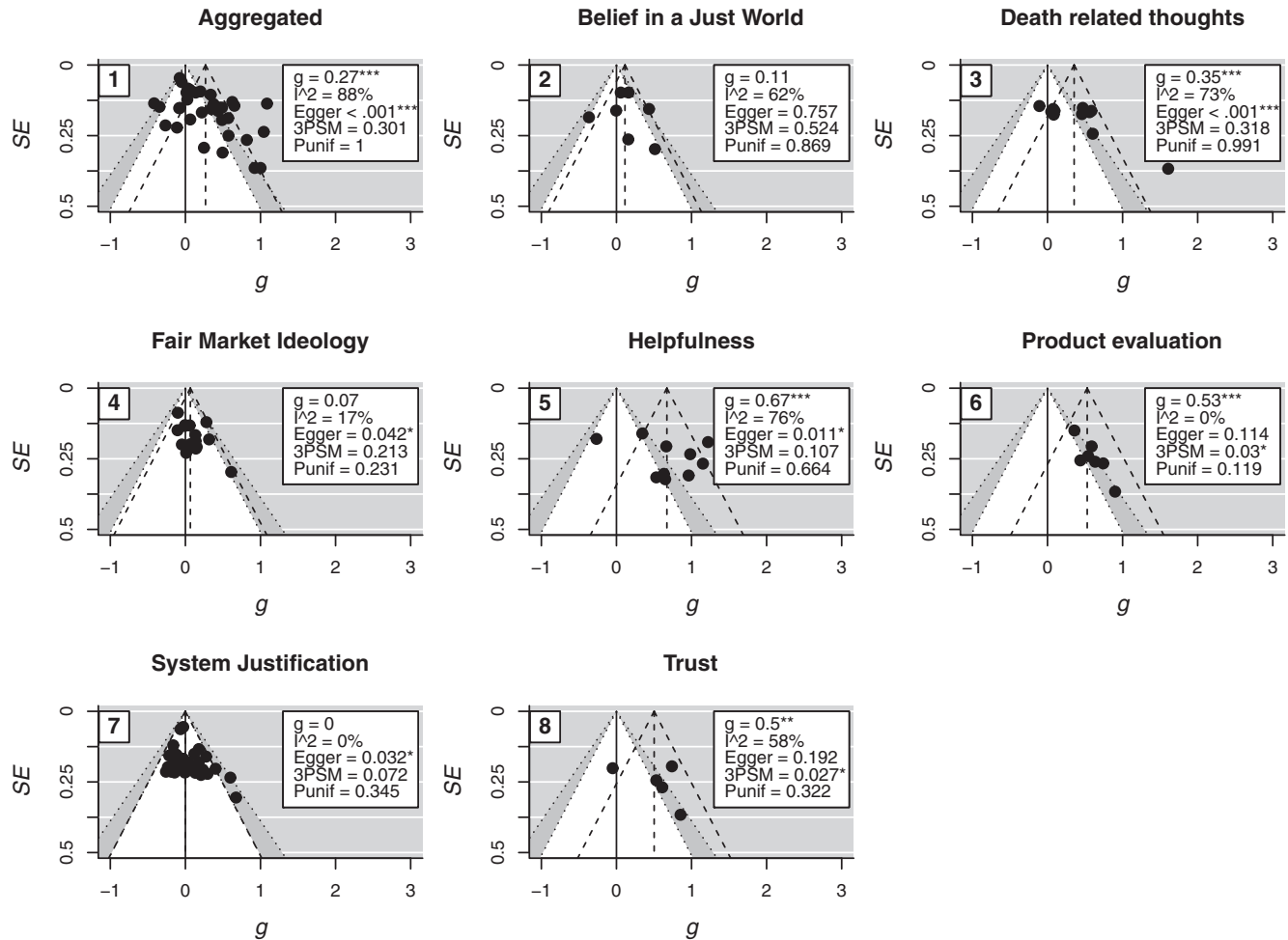
Figure 4. Funnel plots for subsets (number in top left) based on frequently used dependent measures. g = Hedges' g random effects model estimate (center of dotted funnel); I^2 = heterogeneity measure; Egger = Egger's test p value; 3PSM = Three-parameter selection model p value of the publication bias test; Punif = p-uniform p value of the publication bias test. The white- and gray funnel represent a 95% and 99% confidence level, respectively. * $p < .05$. ** $p < .01$. *** $p < .001$.

effect size estimates and have a higher chance to be published, which makes publication bias and opportunistic use of researcher degrees of freedom less of an issue. A proposed solution to improve the replicability of psychological science is to use a lower significance threshold before concluding a finding to be significant, especially with regard to novel claims and in fields where less than half of all studies are expected to reflect a real effect.[11] However, experts still disagree about whether the significance level of 0.05 is the leading cause of the nonreplicability and whether a lower (but still fixed) threshold will solve the problem without undesired negative consequences (Benjamin et al., 2018; Lakens et al., 2018).

This first comprehensive meta-analysis on money priming shows that it is difficult to draw general conclusions about *the* money priming effect. The field is quite heterogeneous, with many different prime types and dependent measures. We reduced this wide variety of study designs to a small number of more homogeneous subsets. Most of these either showed no

effect, or signs of publication bias suggesting that those effects should not be trusted without further evidence. However, several subsets passed our bias tests and showed small to large effect size estimates. Two subsets of online and field experiments both using a descrambling prime type were unbiased and showed small but significant overall effects. The largest unbiased effect involved a subset of lab studies investigating a behavioral dependent measure with a money handling prime type. We consider this subset to potentially show a valid money priming effect, yet sufficiently powerful preregistered (replication) studies are needed to confirm these findings.

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[11] Appendix C illustrates how the false discovery rate depends on the percentage of real effects in a field.

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(Appendices follow)

Appendix A

Main Results When Including All Levels of Interaction Effect

Figure A1 shows a funnel plot for all experiments included in our meta-analysis, as well as separate funnel plots for the published, unpublished, and preregistered experiments. The x axis shows the Hedges' g effect size estimate and the y axis its standard error. The dotted funnels are centered around the random effects model Hedges' g estimate. The random effects model shows a significant effect for all studies ($g = 0.26$, $p < .001$, 95% CI [0.21, 0.30]), and also for all published ($g = 0.35$, $p < .001$, 95% CI [0.28, 0.41]) and all unpublished studies separately ($g = 0.13$, $p < .001$, 95% CI [.08, .18]). However, the preregistered experiments did not show a signifi-

cant overall effect ($g = 0.02$, $p = .425$, 95% CI [-0.02, 0.06]).

The Q test for heterogeneity of effect sizes is significant, $Q(288) = 1167.21$, $p < .001$, $I^2 = 80.6\%$, $\tau^2 = 0.12$ ($SE = 0.014$). indicating that the included studies are not evaluating a similar effect. Table A1 shows the results of the metaregression analyses where we regress the effect size estimates on prime type, study setting, and other characteristics. Although the results are quite similar to those presented in Table 3, most estimates are smaller because we now took into account all levels of the interaction effects rather than only the levels predicted to show the largest effects.
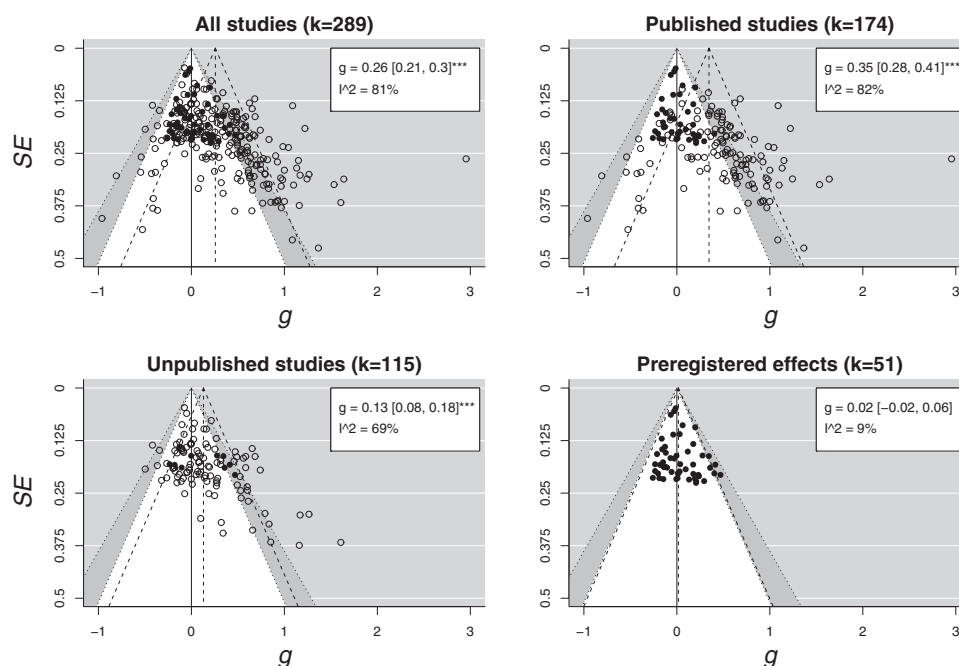

Figure A1. Funnel plots of all studies, published studies, unpublished studies and preregistered studies. $g =$ Hedges' g random effects model estimate (center of dotted funnel), including 95% confidence interval. $I^2 =$ heterogeneity measure; The white- and gray funnel represent a 95% and 99% confidence level, respectively. Black dots represent preregistered studies. *** $p < .001$.

(*Appendices continue*)

Table A1

Moderating Influence of Prime Type, Study Setting, and Other Characteristics

Moderator	Metaregression		Subgroup meta-analysis		
	Beta coefficient	95% CI	k	Hedges' g	95% CI
Prime type					
Intercept (Combination)	.01	[−.22, .25]	13	.13	[−.14, .41]
Visual	.19	[−.05, .43]	117	.19***	[.13, .24]
Descrambling	.22	[−.02, .47]	93	.22***	[.16, .29]
Handling	.54***	[.28, .81]	38	.58***	[.39, .78]
Thinking	.25	[−.03, .52]	28	.27*	[.05, .48]
Moderator test: $Q(4) = 26.86$***					
Residual heterogeneity test: $Q(284) = 1121.80$*** ($I^2 = 78.3\%$)					
Study setting					
Intercept (Online)	.11**	[.03, .19]	96	.10***	[.04, .15]
Lab	.24***	[.14, .34]	154	.37***	[.30, .45]
Field	.10	[−.08, .27]	30	.26***	[.14, .39]
Moderator test: $Q(2) = 16.70$***					
Residual heterogeneity test: $Q(286) = 1104.58$*** ($I^2 = 79.4\%$)					
Dependent measure type					
Intercept (Nonbehavioral)	.21***	[.16, .25]	239	.20***	[.16, .24]
Behavioral	.37***	[.24, .50]	46	.59***	[.42, .77]
Moderator test: $Q(1) = 30.84$***					
Residual heterogeneity test: $Q(283) = 1034.65$*** ($I^2 = 77.7\%$)					
Other study characteristics					
Intercept	−.21**	[−.35, −.07]			
Standard error (Egger's test)	2.26***	[1.58, 2.93]			
Published	.17***	[.09, .26]			
Preregistered	−.25***	[−.36, −.14]			
Multiple dependent measures	.05	[−.05, .14]			
Interaction effect	−.21	[−.30, −.11]			
Moderator test: $Q(5) = 118.71$***					
Residual heterogeneity test: $Q(283) = 807.36$*** ($I^2 = 70.3\%$)					

* $p < .05$.　** $p < .01$.　*** $p < .001$.

Appendix B

Methods and Results on Reporting Error in the Money Priming Field

Prior studies have indicated that a substantial proportion of published empirical psychology articles contain errors in the reporting of *p* values. For example, a study that examined more than 30,000 journal articles in psychology found that approximately half of them contained at least one *p* value that was inconsistent with its test statistics and degrees of freedom, and one in eight articles contained "grossly inconsistent" *p* values that affect the statistical conclusions drawn (e.g., misreporting $p = .054$ as $p < .05$; Nuijten et al., 2016). We seek to evaluate the prevalence of reporting errors within the studies included in our meta-analysis, as reporting errors may be indicative of other biases resulting from the researcher's opportunistic use of the many degrees of freedom in the analyses of data and reporting of their results (John, Loewenstein, & Prelec, 2012).

To do so, we used the R package (R Core Team, 2014) *statcheck* (version 1.3.0; Epskamp & Nuijten, 2016). This R package can automatically extract the reported test statistics (e.g., *t, r, F*) and their corresponding degrees of freedom from journal articles. Using the extracted information, the package then recalculates the *p* values and compare them with those reported in the article. If the two *p* values are inconsistent, the result is marked as an *error*. If the error results in a change in statistical conclusion, the result is also marked as a *decision error*.

The results of the statcheck procedure are shown in Table B1. *statcheck* was able to read 607 *p* values in 41 published studies. Overall, the results suggest that the prevalence of reporting errors among published studies included our meta-analysis is largely similar to previous estimates. Specifically, 53.7% of the published articles included in our meta-analysis contained at least one *error* and 9.8% contained a *decision error*.

(Appendices continue)

Table B1
Results of Statcheck Procedure and Prior Estimates in Nuijten et al. (2016)

Measure	Nuijten et al. (2016)	Current meta-analysis
Valid papers	16,695	41
Number of *p* values	258,105	607
Total number of errors	24,961	56
Total number of decision errors	3,581	7
Percentage of *p* values with Error	9.7%	9.2%
Percentage of *p* values with Decision Error	1.4%	1.2%
Percentage of papers with error	49.6%	53.7%
Percentage of papers with decision error	12.9%	9.8%

Appendix C

False Discovery Rate in Money Priming Field

If a field is lacking a clear theory from which to derive predictions, then a relatively small percentage of studies are expected to reflect real effects. Figure C1 shows for different percentages of real effects the percentages of all positive findings that are either false (left) or true (right), given a power of 0.7 (average of studies in Table 1 in Vohs, 2015). It turns out that when using a significance level of 0.05,

the actual false discovery rate can be kept at 5% if 60% or more of the studies reflect a true effect. However, if researchers use a significance level of 0.01, then only 20% of the studies have to reflect a true effect. Less true effects imply more false positive findings. Thus, we advise to use lower boundaries of significance if a field is lacking a clear theory and still exploratory in nature.
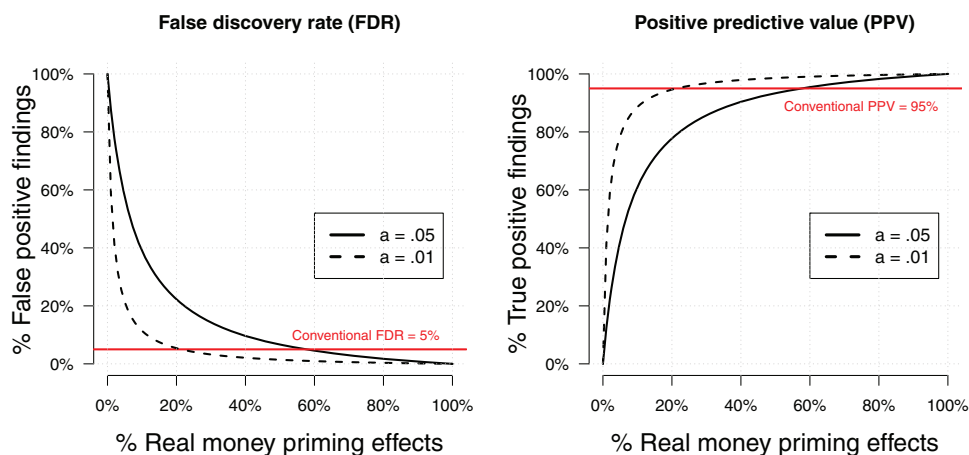

Figure C1. False discovery rate (left) and positive predictive value (right) for experiments in the money priming field, for a significance level of 0.05 or 0.01, a power of 0.7 (average of Table 1 in Vohs, 2015) and different percentages of true money priming effects. See the online article for the color version of this figure.