CAN IDEAS ACTIVATE BEHAVIOR?
A META-ANALYSIS OF THE EFFECTS OF A VARIETY OF CONCEPTUAL PRIMING OBJECTS AND STRATEGIES ON BEHAVIORAL OUTCOMES

BY

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THESIS

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Abstract

Behavioral priming traces its roots back to 1890, but recently has been criticized for failure to replicate. In response, researchers argued the need for more meta-analyses (Bargh, 2012). Accordingly, the following meta-analysis sought to estimate its mean effect size and to quantify publication bias as well as the effects of moderators like goal expectancy (ease), filler tasks, goal value, and the directness of the association between prime and outcome. We estimated a mean of $d = 0.4220$ across 682 eligible effect sizes. Despite evidence of some publication bias, the trim and fill procedure recalculated the mean effect to be $d = 0.2661$, indicating that behavioral priming is likely a true effect, if perhaps overestimated in the literature. Our findings regarding goal expectancy, filler tasks, and goal value are somewhat mixed, but we consistently found that indirect primes, which require more interpretation to produce the outcome, had larger effect sizes than more direct primes.

For Tom Purcell

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Chapter 1: Introduction

William James, one of psychology's earliest founding fathers, coined the term "ideomotor action" in 1890 to describe the phenomenon wherein merely observing or thinking about a behavior makes one more likely to engage in that behavior. This was one of the earliest conceptions of behaviors as mental constructs, subject to activation and accessibility similar to attitudes or cognitions. Despite this early recognition, researchers ignored the implications of behavior as a representation until fairly recently. When Lashley first used the term *priming* in 1951, and for several decades after, it was assumed that the effects of this surreptitious activation of concepts could only affect the activation of other internal-only representations – attitudes, cognitions, feelings, but not behavior. It wasn't until 1996 that Bargh, Chen, and Burrows, building on James' notion of behaviors as representations, hypothesized that behaviors too could be automatically activated through priming. The success of their experiments changed the field of priming – and arguably of social psychology as a whole – forever.

The Perception-Behavior Link

In 1996, Bargh and colleagues dared to hypothesize that behaviors themselves, not just the attitudes or cognitions that might lead one to consciously choose a behavior, could be induced via primes presented without conscious awareness of their content. They called it the *perception-behavior link*: that people will match their behavior to a prime without any required intent or conscious awareness; that is, merely perceiving a concept is enough to elicit corresponding behavior.

Bargh and colleagues tested their theory with three studies. In the first, participants were instructed to unscramble sets of 5 words to form 4-word sentences, then come find the experimenter to begin a second task. Embedded in these sets were words related to rudeness or politeness (or neither, in the control condition), and the dependent variable of interest was how long participants waited to interrupt the experimenter, who was engaged in conversation with a confederate, to get on with the second task. Consistent with the team's hypotheses, the rudeness-primed participants interrupted much sooner than the other groups, and the politeness-primed participants interrupted much later, supporting the hypothesis and the underlying perception-behavior theory. The second study also included a scrambled sentence task, but this time the concept being primed was *elderly* (or neutral words in the control condition). After completing this task, participants thought the study was over and left the experimental room. A confederate surreptitiously watched the participants exit and timed their walk from the door to a mark on the floor, a distance of 9.75 meters. The team had hypothesized that participants primed with the concept *elderly* would walk more slowly than the control participants, in line with elderly people's stereotypically slow movement. Once again, the hypothesis was supported, further bolstering the perception-behavior link. Finally, the third study subliminally exposed participants to white or black male faces, then staged a computer malfunction that would force participants to start the task over. Expecting that participants exposed to the black male faces would enact the hostility stereotypically attributed to this group, researchers measured participants' reactions to the inconvenience. As expected, the black-primed group expressed greater levels of hostility than the white-primed group. Taken together, these studies showed support for the perception-behavior link, and the field of social behavior priming was born.

The Goal Activation Account

While the perception-behavior link presents behavior as a mirror-like response to the primed category or trait, other theories present behavior as the result of goals activated by the stimulus. Researchers connected automatic behavior to goals in the auto-motive model, but in this model, deliberate, conscious behavior had to precede automatic behavior (Bargh, 1990; Bargh & Gollwitzer, 1994). Behaviors could be unconsciously enacted in service of a goal upon the mere perception of a stimulus, but only if those behaviors had been previously *consciously* enacted in service of that goal in the presence of the same stimulus a sufficient number of times to create an association. This model did not propose full automaticity of behavior from start to finish, but rather, “it would be appropriate to say that automatic behavior due to the operation of enduring goals or motives is unintentional at the time but intentional in the sense that the choice of the behavior was made in the past, not the present” (Bargh & Gollwitzer, 1994, p. 82). They noted, however, that implementation intentions seem to produce automatic behavior, even though these implementation intentions are, by definition, only a one time pairing of stimulus and response (i.e., the decision to associate “when the clock strikes 9” with “I will leave my apartment”). This exception hinted that repeated association may not always be necessary to produce goal-directed automatic behavior (Bargh & Gollwitzer, 1994).

This notion came to fruition following Bargh et al.’s (1996) finding on the perception-behavior link, when new theories were born to explain primed behavior as the result of goal activation (Bargh, Gollwitzer, Lee-Chai, Barndollar, & Trötschel, 2001; Cesario, Plaks, & Higgins, 2006). Even in Bargh et al.’s (1996) original piece, they conceded that automatic behavior would only occur if it was a reasonable response in the situation, allowing for the possibility that situational factors (like goals) could be relevant in producing the effect. Based on the assumption that goals are stored in the same way as any other concept, and that they therefore could be activated automatically, the new accounts posited (1) that automatic behavior may be mediated by goals, rather than a direct response to the stimulus (as in Bargh et al., 1996), and (2) that these goals may arise suddenly in the presence of relevant stimuli, rather than requiring previous conscious association (as in Bargh, 1990; Bargh & Gollwitzer, 1994). For instance, one may develop an affiliation goal in the presence of another person, even if that person is a stranger – meaning it would be impossible to have previously associated that goal with them. Experiments bore out this hypothesis. When primed with a cooperation goal in a novel task, participants indeed behaved more cooperatively despite no prior association between the goal and the situation (Bargh et al., 2001). In fact, participants showed near identical levels of cooperation when primed with cooperation, explicitly instructed to cooperate, or both, demonstrating that such priming can indeed function as a goal (Bargh et al., 2001). Furthermore, primed participants reported similar levels of intended cooperation as their unprimed, control condition counterparts, despite higher levels of enacted cooperation. This indicates that goal-directed behavior can be produced automatically, without any conscious awareness of the goal or intent to engage in the behavior, and does not require a concurrent conscious goal to exert its effect. Finally, the researchers showed that primed goals produce escalating effects over time until satisfaction (experiment 3) and persist across obstacles (experiment 4) and interruptions (experiment 5; Bargh et al., 2001). These results indicate that primed goals function in very much the same way as do conscious goals (Förster, Liberman, & Higgins, 2005).

Cesario et al. (2006) furthered the idea of primes acting as goals by framing these effects as motivated preparation to interact with the target stimulus, especially when that stimulus is a person or category of people (e.g., elderly people in Bargh et al., 1996). Cesario et al. took issue with the *direct expression* tenet of the perception-behavior link; that is, that perception automatically activates representations of behaviors *ascribed to* the target group, regardless of their relevance or appropriateness for the perceiver. They argued that the perceiver's behavior should be informed by their attitudes towards the target and what would be the most adaptive way to interact with the target – as they put it, “one does not respond to slow enemies and slow friends with the same behavior” (Cesario et al., 2006, p. 895). The question became: do people respond to primes with identical behavior (perception-behavior link, Bargh et al., 1996) or with interactional behavior (motivated preparation, Cesario et al., 2006)? Cesario and colleagues tested this question by priming participants with the category gay men[1], a group with distinct stereotypes and subject to widespread disapproval and prejudice. A direct expression (perception-behavior link) response to this category would be passive behavior, but a motivated preparation response would be hostility. As predicted, participants primed with gay men showed increased hostility, not passivity.

An important distinction between the goal activation account and the perception-behavior link by how activation rises or falls over time. The perception-behavior link would predict each encounter with the stimulus to increase activation of the concept (Bargh, 1990; Bargh et al., 1996), but goal activation models predict that an opportunity to achieve the goal would satiate it and lead to diminished, even inhibited, activation of the concept (Förster et al., 2005). In testing the motivated preparation account, Cesario et al. demonstrated this decrease in accessibility after a satiation opportunity, strengthening their claim that automatic behavior results from goals, not the direct expression of primed stereotypes.

It is important to note that the perception-behavior link and the goal activation account are not mutually exclusive. Researchers of both theories readily acknowledge the other account’s legitimacy and probable existence under the right circumstances. Bargh, an early scholar of the auto-motive model (1990) and perception-behavior link (et al., 1996), has also

[1] Despite drawing extensively from Bargh et al.’s 1996 study, Cesario et al. explained that the elderly/African-American categories were inappropriate tests of the theory because both the direct expression account and the preparation to interact account would predict slower/hostile behavior.

written about goal-directed automatic behavior (et al., 2001). In their premiere article on the motivated preparation account, Cesario et al. did not disparage earlier models as untrue, but rather suggested that a motivational model was more complete and adaptive when the target is a social object (i.e., people) and direct expression models could suffice for non-social objects. Similarly, we do not intend the present meta-analysis to test or disprove either theory, but rather to identify their relative domains and effect strengths.

Goal Theory

As mentioned earlier, goals and perceptions have unique patterns of activation over time. Perception-related accessibility fades rapidly but is reactivated by any re-exposure to the construct, while the accessibility of goal-related constructs behaves in an almost opposite manner (Bargh et al., 2001; Cesario et al., 2006; Förster et al., 2005). Goal-related accessibility escalates over time until an opportunity for satiation occurs. Upon satiation, goal-related accessibility plummets, and may even be *inhibited*, falling below levels of activation for no-goal control groups. By comparison, no-goal control groups experience an encounter with the "goal" stimuli as just another source of activation and that helps maintain accessibility over time (Bargh et al., 2001; Förster et al., 2005). Finally, for goals, the level of accessibility during goal pursuit and the level of inhibition post-satiation are proportional to motivation, but no such parallel has been documented (or is logical) for perception-only accounts (Förster et al., 2005). Given the assumption that automatic behaviors are stored and accessible in the same way as any other mental construct (Bargh et al., 1996), the same hallmarks can differentiate goal-directed from direct expression automatic behavior (Cesario et al., 2006; Förster et al., 2005).

Because perceptual and goal-related effects can be distinguished by their pattern of activation over time, many researchers include "filler" tasks in their procedures (for further discussion, see Bargh et al., 2001). These tasks occur after the priming task but before the dependent measure of interest, and serve to put some time between the activation of the concept and the assessment of its outcomes. Filler tasks take a variety of forms, including copying figures (Aarts, Gollwitzer, & Hassin, 2004), solving math problems (Kim, 2011), or drawing one's family tree (Bargh et al., 2001; Hart & Albarracín, 2009). Despite this variance, filler tasks can be defined by one characteristic: relevance to the primed concept or goal. In a perception-behavior paradigm, a relevant filler task could serve as additional activation, increasing both the accessibility of the concept and its eventual outcome on the dependent measure of interest (Bargh et al., 2001; Förster et al., 2005). In a goal-activation paradigm, any filler task that could be used to satisfy the goal (e.g., a word search task after an achievement goal) will cause goal satiation and goal-related construct inhibition during the dependent measure of interest (Bargh et al., 2001; Cesario et al., 2006). Therefore, filler tasks can be used to differentiate perceptual from goal-driven effects by comparing the rise and fall of these effects over time.

One component of motivation to achieve goals is the goal's expectancy (Förster et al., 2005; Locke & Latham, 2002). Expectancy is the perceived probability of achieving the goal, and is closely related to task difficulty such that more difficult tasks usually have lower expectancy. When experimentally manipulated, goal expectancy seems to increase participants' motivation to achieve the goal (Förster et al., 2005; Locke & Latham, 2002). In accordance with goal theory, this motivation translates into goal-related accessibility such that expectancy is positively associated with accessibility. In their experiments, Förster et al. found no difference in pre-satiation accessibility or post-satiation inhibition between participants in the no-goal control condition and those in the low expectancy goal condition; only those in the high expectancy goal showed distinct escalating accessibility and post-satiation inhibition (2005). This pattern suggests a positive linear association between expectancy and goal-related accessibility, including automatic behaviors. This assertion regarding goal expectancy is supported by Locke and Latham's (2002) theory paper; however, they distinguish between *goal* expectancy and *task* expectancy. While the goal expectancy is the perceived likelihood of achieving the goal, the task expectancy is the actual difficulty of the task, which has an inverse curvilinear association with performance (Locke & Latham, 2002).

Another critical component of goal motivation is the value of attaining the goal. Intuitively, participants are believed to be more motivated to achieve goals that hold great objective or personal value. While personal value is hard to manipulate, the objective value of experimental tasks is easily altered by differing the compensation awarded to participants based on their performance. Förster et al. (2005) conducted such an experiment, offering some participants $1 to find a target in a search task and others only $0.05. Goal-related accessibility escalated pre-satiation and was inhibited post-satiation for participants in the $1 condition, but not for those in the $0.05 condition or no-goal control groups. These results indicated that goal value, like goal expectancy, is an important component in determining motivation and the subsequent automatic goal-pursuit behaviors (Förster et al., 2005).

Goal expectancy and goal value show strong effects on their own, but evidence suggests that they actually interact to inform goal motivation. In another experiment, Förster et al. (2005) manipulated both components at once in a 2 (high and low goal value) X 2 (high and low expectancy) design, with a no-goal control. Intuitively, the high value/high expectancy condition should produce most goal-directed accessibility and the low value/low expectancy condition the least, but what about the mixed conditions? If one component is outweighs the other, there would be clear differences between these conditions, but if they combine into a composite indicator, the two conditions should produce similar results. Results confirmed that the high/high combination produced the clearest pattern of goal activation, with much greater pre-satiation escalation of accessibility and post-satiation inhibition. The low/low condition rarely differed from the no-goal control group. It seems a goal people are unlikely to attain, that would not give much reward even if they did, is hardly a goal at all. Finally, consistent with a combination theory, the two mixed conditions were in between the high/high and low/low extremes, and did not significantly differ from each other. This underscores the importance of the value X expectancy interaction; a deficiency in either leads to a decrease in motivation that the other component cannot singly overcome. This is a logical mechanism: a goal with no value is not worth attaining, even if it would be easy to do so, and an impossible goal is not worth pursuing, no matter how attractive the rewards would be. It would not be adaptive to expend energy and resources on worthless or pointless goal striving.

Replication Crisis

Behavioral priming has come under fire in recent years for failure to replicate, and in fact has become a favorite poster child for the so-called "replication crisis" (Cesario, 2014). Doyen, Klein, Pichon, and Cleeremans (2012) were only able to replicate Bargh et al.'s (1996) ground-breaking finding on walking speed when experimenters were led to believe their participants would walk more slowly; without this expectancy effect, there was no difference between participants primed with the concept *elderly* or not. Harris, Coburn, Rohrer, and Pashler (2013) attempted to replicate two of Bargh et al.'s (2001) successful experiments (Experiment 1: achievement prime/neutral prime, and Experiment 3: achievement prime/neutral prime X filler task/no filler task) on achievement primes increasing performance on a word search. Not only were they unable to replicate either results, but their group means were either virtually identical or in the opposite of the predicted direction ($p = ns$; Harris et al., 2013)!

These and other failures to replicate foundational studies in the field cast doubt on the very existence of priming, but some authors have advised caution in overestimating the meaning of a few failed attempts. Cesario (2014) wrote about the importance of individual differences, and how priming's infancy as a field makes it difficult to know which components of a study are necessary to produce effects. Backing his assertion is research showing that priming effects are malleable to factors like motivations and attitudes (Cesario et al., 2006), financial status (Aarts et al., 2004), time of day (Boland, Connell, & Vallen, 2013), type of self-construal (independent vs. interdependent; Bry, Follenfant, & Meyer, 2008), dispositional submissiveness (Van Capellen, Corneille, Cols, & Saroglou, 2011), prior contact with target category (Dijksterhuis, Aarts, Bargh, & Van Knippenberg, 2000), chronic self-consciousness (Hull, Slone, Meteyer, & Matthews, 2002), habits (Sheeran et al., 2005), and culture (Wheeler, Smeesters, & Kay, 2011). Furthermore, Bargh (2012) identified an internal focus of attention, or the chronic tendency to use internal states to guide behavior, as a key moderator in priming effects. Given this variety of influences, it is possible that true effects obtained in one situation may not replicate in another due to an uncontrolled or yet-unknown moderator.

Because of the great uncertainty that still surrounds priming, Cesario counseled patience. He argued that we should allow priming to mature as a field and identify more of these moderators before using a few ill-fated replication attempts to dismiss it all as Type I error (2014). Similarly, Bargh argued that trying to directly replicate a handful of studies, whose success or failure may ride on unseen factors, was less informative than meta-analyses overviewing the field as a whole (2012). The current meta-analysis is a contribution such an overview, and an attempt to identify moderators as Cersario suggested.

The Current Meta-Analysis

The current meta-analysis sought to estimate the mean effect size of behavioral priming, and to quantify publication bias and any potential decline effect, as well as the effects of moderators like goal expectancy (ease), filler tasks, goal value, and prime stimulus type (e.g., pictures versus words). Several of our moderators of interest pose conflicting hypotheses, which we will investigate in an exploratory manner. Increased expectancy (an easy dependent task) may increase effect sizes by making goals more attainable (Förster, Liberman, & Friedman, 2007), but decreased expectancy may increase effect sizes by increasing motivation (Locke & Latham, 2002). Because priming may function by the perception-behavior link or by activating goals, one might expect filler tasks to diminish effect sizes (as perceptions fade; Bargh et al., 2001) or to increase them (as goals escalate; Förster et al., 2007), and for goal value to be irrelevant (to perception; Bargh et al., 1996) or proportional to effect sizes (Förster et al., 2007). Since these two paths likely operate under different circumstances, it is worthwhile to explore any potential moderators that differentiate primes that function as goals from those which operation via the perception-behavior link. Finally, specific primes (for review, see Weingarten et al., 2015) may have larger effect sizes because their meaning is more clear, or abstract primes may have larger effect sizes because their ambiguity allows for multiple paths of activation (Förster et al., 2007). For instance, while a direct *politeness* prime only activates the concept of politeness, a *disciplinarian* prime may activate the concepts of politeness, quietness, and deference, activate the emotion of fear, and remind participants of the consequences for misdeeds, which could combine additively to reduce the likelihood of a participant interrupting an experimenter far below the effect of *politeness* alone. Given that multiple sources of activation seem to combine additively to increase activation above that of a single source (Bargh, Bond, Lombardi, & Tota, 1986), it is not unreasonable to speculate that indirect primes may similarly lead to increased effect sizes by activating multiple channels.

Chapter 2: Method

Literature Search

We searched major databases as far back as possible and contacted authors in the field for unpublished reports. Databases searched include PsycINFO, ProQuest Dissertations and Theses, the ReproducibilityProjectOpenScienceFramework,PsychFileDrawer.Org, Communication Abstracts, Advances in Consumer Research (Proceedings of the Association for Consumer Research), the Foreign Doctoral Dissertations Database of the Center for Research Libraries (http://www.crl.edu), PubMed, the Education Resources Information Center (ERIC), and ZPID on the Databases of the Institute of Psychology Information for the German-Speaking Countries (http://www.zpid.de). In PsycINFO, we used the search terms (prime OR priming OR primed OR automatic OR automatically OR nonconscious! OR incidental!) AND (behavior OR goal OR action OR motivation) NOT ("semantic prim!") NOT ("affect! prim!") AND me.exact("Empirical Study") AND pop.exact("Human"). We used the same string, minus the last two specifications, to search ProQuest Dissertations and the Theses database, and minus the last four for ERIC, Foreign Doctoral Dissertations, PubMed and ZPID. We made 320 requests to authors for unpublished data, and sent requests to the listhosts of the Society for Personality and Social Psychology, the Society for Consumer Psychology, and the Society for Experimental Social Psychology. To increase the likelihood of submission, we accepted anonymous data. We concluded our search in June 2014. In phase 1 of the search we checked all results for relevant experiments, and in phase 2, we screened the studies that made it through phase 1 for our inclusion criteria, which follow.

Inclusion Criteria

To be eligible, studies must include:

- an experimental manipulation in which participants are randomly assigned to priming conditions.
- a non-opposite control group. In order to assess the effect of the prime relative to a neutral baseline, studies must include a control or comparison group whose primed goal is not semantically opposed to the target group. For example, a study with only two groups, one of which received action primes and the other of which received inaction primes, would not be eligible. Eligible non-opposite control groups may involve neutral primes, nonsense primes, unrelated goal primes (e.g., action priming vs. God priming), or no task.
- a visually-presented word or image prime with a clear meaning. Primes could be directly or indirectly related to their intended goal, such as invoking the concept of honesty through words like *truth* (directly linked) or *divine* (God priming intended to evoke honest behavior; indirect). Primes in the form of sounds, smells, or movements were excluded, as were primes whose intended goal was ambiguous (e.g., priming the color red).
- incidental priming rather than overt directions. The activation of the concept must be due to the prime activating a schema, not due to direct instruction from the experimenter to behave a certain way. For example a study measuring length of time spent working on a puzzle after completing a scrambled sentence task priming achievement (or neutral) would be eligible, but a study measuring length of time spent working on a puzzle after being explicitly told (or not) to work until the puzzle was finished would not be eligible.
- a behavioral dependent variable. Outcome measures must assess enacted behavior, not intentions. Dependent variables also cannot merely be measures of the accessibility of

the primed concept (e.g., an IAT). When it was unclear whether a measure represented accessibility or true behavior, the research team discussed it together to reach a consensus.

- adequate statistics from which to calculate the effect size (e.g., means and standard deviations or standard errors, F statistics, t statistics). Unless otherwise reported, we assumed that all conditions had equal cell sizes and standard deviations. If there was not enough information with which to calculate the effect size, we contacted the original authors for a more complete report.

The research team discussed as a group and came to a consensus on any study whose eligibility was unclear.

Moderator Coding

In addition to the effect size, we coded several variables that could potentially moderate the priming effect, including prime type, goal expectancy, goal value and any manipulations thereof, filler task(s), type of control, type of dependent variable, and descriptive characteristics of the study. The primary coding group established high interrater reliability ($\kappa > .7$), with the exceptions of goal value (87% agreement; $\kappa = .56$).

Prime type. Priming stimuli were coded based on liminality, stimuli type, task type, dosage, goal content, and directness. Primes could be subliminal (e.g., parafoveal images) or supraliminal (e.g., word completion tasks) and could take the form of words or pictures. The task type refers to how the priming stimuli were delivered to the participants; common techniques involved scrambled sentence tasks, anagrams, and lexical decision tasks for words and foveal and parafoveal presentation for images. The priming task could also be a writing prompt designed to evoke a goal. The proportion of primes was also recorded as the ratio of prime-related stimuli to total stimuli during the priming task; for example, priming achievement with the words ***win***, *bread*, *chair*, ***goal***, and *window* would have a 0.4 proportion of primes because two of the five words are priming stimuli and the others are fillers. We coded the main goal suggested by the prime, as well as any secondary goals. Finally, primes were categorized as either directly related to the outcome if the priming stimuli had an close and unambiguous semantic link to the action to be performed (e.g., using the prime *slow* and the outcome of walking speed) or as indirectled related if the link between prime and behavior required inference (e.g., using the prime *elderly* and the outcome of walking speed, and expecting that participants associate slow with elderly, as in Bargh et al., 1996).

Goal expectancy. Manipulations of goal expectancy, or the ease of achieving the goal, were coded to represent no manipulation, an increase in expectancy (the goal is easier to attain in one condition), or a decrease in expectancy (the goal is more difficult to attain in one condition). Manipulating goal expectancy, as coded here, could represent changes to objective difficulty of the task (see Locke & Latham, 2002), or altering participants *perceptions* of their likelihood of attaining the task, without manipulating the actual likelihood (e.g., Förster et al., 2005).

Goal value and manipulations of goal value. Goal value was coded in two ways: the relevance of the dependent measure to the primed goal and the personal importance to the participants of achieving the goal. Dependent measures that would not necessarily provide satisfaction of the primed goal were coded as low value; whereas tasks that would completely fulfill the goal were coded as high value. Dependent measures could also be coded as high value if they provided tangible benefits to the participants (e.g., monetary rewards) or if they pertained to personal values (e.g., expressing an attitude). Tasks were coded as low value if they were unlikely to intrinsically motivate the participants (e.g., solving anagrams). We also coded if the goal value was manipulated across conditions (e.g., offering a greater monetary reward in one group than another).

Filler task(s). We coded for the presence of a filler task between the priming phase and the dependent measure(s). Filler tasks must have been intended as a filler task; ineligible dependent measure(s) that occurred between the priming and the eligible dependent measure(s) were not coded as filler tasks. If present, we coded filler tasks on their type, approximate length in minutes (when provided), and relevance to the primed goal.

Type of control. The control group was coded on its relationship to the experimental group(s). The control condition could not be the opposite of the experimental condition, but could take several other forms including neutral or nonsense stimuli, priming a different goal unrelated to the experimental goal, or having no task.

Type of dependent variable. Dependent measures had to be behavioral in nature and were categorized by type. Common types included task performance with scoreable answers (e.g., anagrams), persistence on a task, reaction time, consumption of food or drink, and enacted choices regarding products, spending, donations, volunteering, etc. We also coded dependent variables on their flexibility, that is, how many solutions are possible. Measures that could be solved with multiple answers or multiple strategies (e.g., puzzles) were coded as flexible, but measures with only one answer or one possible strategy were coded as inflexible (e.g., choosing a product).

Descriptive characteristics. In addition to the methodological factors described above, we also coded descriptive characteristics such as year, country, percent female per study, and source type (published article, dissertation, author manuscript, etc.). We also recorded any funneled or non-funneled debriefing reported.

Meta-Analytic Strategy

The research team coded eligible articles and recorded all effect sizes resulting from eligible group comparisons and eligible measures. Effect sizes were calculated as $(M_1 - M_2)/SD_{pooled}$. Regardless of the original author's hypotheses, we assumed all priming effects would be assimilative. From that assumption, we determined which group would have a higher mean if the proposed effect were true and assigned that to be M_1 while M_2 came from the other group. Usually this resulted in $M_{Treatment} - M_{Control}$, but in some cases (e.g., God priming expected to inhibit unethical behavior) it was reversed. We also assumed homoscedasticity across groups in a study and therefore calculated SD_{pooled} as a simple average of the sample standard deviations. If there was not enough information to calculate the effect size directly (e.g., sample standard deviations not reported), we attempted to derive them from *t* or *F* statistics or confidence intervals and contacted the original authors when necessary.

Effect sizes were weighted by their inverse variances via the Metafor package in R (Viechtbauer, 2010). We computed the weighted mean effect size using both fixed and random effects models. Random effects were calculated with a maximum-likelihood estimator. We tested for heterogeneity with Cochran's *Q* statistic, which weights each effect size's squared deviation from the mean effect by its inverse variance, and quantified any heterogeneity with I^2, which represents the proportion of the variance due to between-study variance as opposed to within-study variance (Higgins & Thompson, 2002; Huedo-Medina, Sánchez-Meca, Marín-Martínez, & Botella, 2006).

All eligible effect sizes were recorded from each study, meaning that we could not assume independence across all effect sizes. Effect sizes from the same article likely had shared contributors of variance (e.g., location of the lab, ambient temperature, disposition of the experimenter), and effect sizes from the same study may have even more similarities if multiple treatment groups were compared to the same control group. To account for this, studies were used as level two variables in all meta-regressions, allowing a random intercept for each study.

We looked for publication bias using funnel plots and trim and fill procedures, and a rank correlation and regression tests for funnel plot asymmetry (Begg & Mazumdar, 1994; Egger, Smith, Schneider, & Minder, 1997). Funnel plots show the distribution of effect sizes around their mean against their standard error. In theory, the observed effect sizes should be a representative sample of an underlying normal distribution of possible effect sizes. If true, this would indicate that the observed effect sizes should be closest to their mean when their standard error is low and should deviate from it symmetrically as standard error increases. However, if there is publication bias in the field, findings that deviate down from the mean (and are less likely to be statistically significant due to their smaller size) would be suppressed, leading to an asymmetrical gap in the lower left-hand corner of the plot. Trim and fill procedures fill this gap with a mirror image of the lower right-hand corner (effect sizes far above their mean) and re-estimates the mean to account for these effect sizes. Egger et al.'s (1997) regression test provides a quantitative assessment of this asymmetry by regressing each point's standard normal deviate (which they "defined as the odds ratio divided by its standard error," p. 3) on its precision. The *z* test on the intercept of this regression acts as a test of asymmetry; the further the intercept is from zero, the more asymmetrical the funnel plot is. Begg and Mazumdar's (1994) rank test derives from Kendall's tau and is based on the assumption that effect sizes will be correlated with their variances if publication bias is present. In the absence of bias, large variances would appear on both sides of the distribution and be associated with abnormally high and low effect sizes. However, if non-significant p-values are selected out of the sample, the points with large variances and small effect sizes will disappear, leaving only those with large variances and large effect sizes. Finally, we used Vevea and Hedge's (1995) procedure to compare the expected density plot of effect sizes if there were no publication bias against the observed density plot, and to compare a general linear model wherein effect sizes are weighted by their p-value (i.e., in which certain p-values are more likely than others) against an unweighted model. If there is no publication bias, the weighted model should fit the data no better than the unweighted model. However, in the presence of publication bias favoring significant effects, smaller p-values will have much larger weights than larger ones, and the model accounting for this bias will fit the data better than the unweighted model.

Moderator tests were conducted using meta-regression in Metafor. Moderators were tested as singly and in combination with other moderators to predict effect size. Studies were used as a level two variable in all meta-regressions to account for nonindependence.

Chapter 3: Results

We included 159 papers containing 264 studies (*K*) and 682 effect sizes (*k*). Papers ranged in date from 1984 to 2015 and were a mix of published and unpublished sources. Country of origin varied across papers with the United States contributing the most papers (55%), but other countries also contributed, especially the Netherlands (13.46%), Canada (5.77%), the United Kingdom (5.38%), and Germany (5%). See Table 1 for more descriptive information.

Results indicate a moderate effect size of random effects $d = 0.4220$ ($z = 21.1082$, $p < .0001$, 95% CI [confidence interval] [0.3828, 0.4612]) and fixed effects $d = 0.3870$ ($z = 34.6899$, $p < .0001$, 95% CI [0.3651, 0.4088]). Cochran's Q rejected the null hypothesis of homogeneity for both models, $Q(681) = 1964.1339$, $p < .0001$. Similarly, results indicated considerable heterogeneity in the random effects model, $I^2 = 66.77\%$.

Publication Bias

Funnel plots of the effect sizes appear fairly symmetrical to the naked eye (see Figures 1–4); however, the rank test indicates asymmetry ($\tau = 0.1622$, $p < .0001$), as does the regression test (random effects, $z = 6.4511$, $p < .0001$; fixed effects, $z = 9.1925$, $p < .0001$). The trim and fill procedure on the random effects model[2] indicated 132 missing studies in the lower left quadrant of the plot ($SE = 17.0719$) and recalculated the effect size to be $d = 0.2661$ ($z = 11.8360$, $p < .0001$). Accounting for these studies increases the estimated heterogeneity in the data, $I^2 = 76.92\%$, $Q(813) = 3147.7037$, $p < .0001$. While it is undeniably concerning to have so many missing studies, we are still confident that a true effect of priming does exist given that the recalculated confidence interval around *d* does not contain zero (95% CI [0.2221, 0.3102]) and

[2] Fixed effects model: 135 missing studies, $SE = 17.0731$; $d = 0.2622$, $z = 25.0545$, $p < .0001$, 95% CI [0.2417, 0.2827]; $Q(816) = 3175.6496$, $p < .0001$.

the new *d* is similar to prior estimates (e.g., Weingarten et al., 2015). The Vevea and Hedges (1995) procedure also indicated some publication bias, with the weighted model (log likelihood = -115.9) showing improvement over the unweighted model (-69.34; 2*difference = 93.03, df = 11, p < .0001). The density plot of these models showed fewer effect sizes than expected in the high p-value range and more than expected in the low p-value range (see Figure 5). Taken together, this evidence clearly points towards some publication bias, but not enough to seriously question the existence of the priming effect.

After examining publication bias in the sample as a whole, we split the data into directly and indirectly linked primes and tested separately for publication bias in each. The directly linked subset indicated a mean effect size of $d = 0.3368$ ($z = 11.6092$, $p < .0001$, random effects; $d = 0.3139$, $z = 18.7675$, $p < .0001$, fixed effects). These estimates are similar, albeit somewhat smaller, than the overall estimates. Figures 6–9 show the funnel plots for this subset, calculated with random and fixed effects, and showing a reference line either at the estimated mean or at zero. These funnel plots show subtle asymmetry on the lower left side, consistent publication bias driven by p-value. Accordingly, the trim and fill procedure estimated 50 missing studies ($SE = 11.2029$) in this area, and re-estimated the effect size to be $d = 0.1989$ ($z = 6.1232$, $p < .0001$, random effects[3]). The rank test also indicated asymmetry, $\tau = 0.1068$ ($p = .0063$), as did the regression test (random effects: $z = 4.1014$, $p < .0001$; fixed effects: $z = 5.2162$, $p < .0001$). The density plot produced by the Vevea and Hedges (1995) procedure was similar to the ones for the entire data set, with systematically fewer small effect sizes and more large effect sizes observed than would be expected in the absence of bias (see Figure 10). The model weighted by p-values had significantly better fit (log likelihood = -58.83) than the unweighted model (log

[3] Fixed effects: 46 missing studies, $SE = 11.1768$, $d = 0.2168$, $z = 13.6358$, $p < .0001$

likelihood = -43.28; 2*difference = 31.1, df = 11, $p < .0011$). Similar to the overall dataset, the analyses on the directly-linked primes converge to indicate some publication bias, but not enough to dismiss the entire effect as Type I error.

The indirectly linked primes had considerably larger effect size estimates of $d = 0.4896$ ($z = 18.1619$, $p < .0001$, random effects; $d = 0.4455$, $z = 29.7588$, $p < .0001$, fixed effects). Figures 11–14 show the funnel plots of the indirectly linked primes with random or fixed effects and a reference line at the estimated mean or at zero. The asymmetry in these plots is difficult to perceive with the naked eye, but the trim and fill procedure indicates 80 missing studies on the left side (*SE* = 12.8845, random effects[4]) and re-estimates the effect size to be a more modest $d = 0.3246$ ($z = 10.6575$, $p < .0001$). The rank and regression tests also indicated asymmetry consistent with publication bias driven by p-values ($\tau = 0.1750$, $p < .0001$; random effects: $z = 4.6536$, $p < .0001$; fixed effects: $z = 7.5045$, $p < .0001$). The results of the Vevea and Hedges (1995) procedure were similar for these data as they were in the other analyses. The density plot (Figure 15) showed fewer small observed effect sizes and more large observed effect sizes than chance would predict, and the weighted model (log likelihood = -67.61) fit the data better than the unweighted model (log likelihood = -33.42; 2*difference = 68.37, df = 11, p < .0001). Consistent with the other sets of analyses, this evidence points to undeniable publication bias among studies of indirectly linked primes, but the effect does not disappear when it is corrected.

Moderator Analyses

Moderators were first tested as single predictors in a simple meta-regression and then were tested in several multivariate meta-regressions. Studies were used as level two variables in all meta-regressions. Year and proportion of primes were scaled before analysis; all others were

[4] Fixed effects: 87 missing studies (*SE* = 12.8674), $d = 0.3041$, $z = 21.8222$, $p < .0001$

not. The two facets of goal value (personal importance of goal, relevance of task to goal) were combined into one composite variable of coded value for analyses. See Table 2a for a full report of single-predictor, random effects analyses. Table 2b presents the same analyses using fixed effects, but these analyses are not discussed at length in the text because the high heterogeneity in the data suggests that a random effects model is likely more appropriate. All moderator analysis statistics in the text are from random effects analyses unless otherwise stated.

Descriptive characteristics. There were no significant differences in effect sizes across year or country in the random effects analyses, but both moderators showed significant differences in the fixed effects analyses. There was a slight downward trend over time and the Netherlands, the UK, Canada, France, and the combination of countries represented in our "other" category had effects significantly higher than the intercept value of the United States' effect ($\beta_0 = 0.3246$, $z = 22.0639$, $p < .0001$). Unpublished sources tended to have smaller effects than published sources in both the random ($\Delta\beta = -0.1412$, $z = -2.8243$, $p = .0047$) and fixed ($\Delta\beta = -0.1882$, $z = -7.4141$, $p < .0001$) analyses.

Methodological features. Liminality, control type, task flexibility, and presence of funneled debriefing were not significant predictors of effect size in the single-predictor meta-regressions. Proportion of primes[5] was significant, indicating a slight increase in effect size as the proportion increases. Most methods of priming were not significant, but a few stood out: scrambled sentence tasks, word searches, and having participants write about their own experiences each diminished the effect size compared to the intercept.   In contrast, almost all of the prime types significantly differed from their intercept (corresponding to stimuli words not associated with people). Prime types[6] that increased effect sizes above the intercept (β_0 =

[5] Also significant in fixed effects analyses.
[6] Results were similar in fixed effects analyses.

0.3630) included non-human pictures ($\Delta\beta$ = 0.3256), human pictures ($\Delta\beta$ = 0.2875), and unique primes from our "other" category ($\Delta\beta$ = 0.1594). The only prime type that did not emerge as significant was words associated with people.

Most primed goals/concepts were not significant predictors beyond the intercept (achievement; β_0 = 0.3548, z = 8.1353, p < .0001), but a few were. Helping/cooperation goals almost exactly doubled the intercept ($\Delta\beta$ = 0.3541), closely followed by impression-formation ($\Delta\beta$ = 0.3215) and consumption ($\Delta\beta$ = 0.3196). Elderly primes nearly tripled effect sizes ($\Delta\beta$ = 0.7410), whereas courage primes decimated them ($\Delta\beta$ = -1.1214). The collection of unusual goals in our "other" category also increased effect size ($\Delta\beta$ = 0.1429), although this is hard to interpret. These same goals, as well as failure, intimacy, and defensiveness, were also significant in the fixed effects analyses.

Most dependent measures[7] had similar effects (intercept: achievement task with scoreable right or wrong answer, β_0 = 0.4020, z = 10.9139, p = .0009), but a few deviated. Consumption tasks were associated with increased effect sizes ($\Delta\beta$ = 0.2398), as was the "other" category ($\Delta\beta$ = 0.2042), which is again difficult to interpret. In contrast, approach tasks and certain puzzles were associated with dramatic decreases in effect size (respectively, $\Delta\beta$ = -1.1686; $\Delta\beta$ = -0.5174).

Theoretical Features. Compared to when primes were directly linked to their outcomes (β_0 = 0.3760, z = 10.5766, p < .0001), effect sizes were significantly larger (by close to 40%) when primes were indirectly linked[8] ($\Delta\beta$ = 0.1430, z = 3.0860, p = .0020). Coded value[9] significantly predicted effect size across experiments ($\Delta\beta$ = 0.0641). When manipulated across

[7] Results were similar in fixed effects analyses.
[8] Also significant in fixed effects analyses.
[9] Also significant in fixed effects analyses.

conditions within one experiment, decreasing goal value dramatically reduced effect sizes ($\Delta\beta$ = -0.5146), but increasing goal value did not significantly differ from the intercept. It is worth noting, however, that the two manipulation categories had small *n*s compared to the nonmanipulated category (nonmanipulated $n = 487$, value reduced $n = 29$, value increased $n = 59$). The mere presence of a filler task did not predict differences in effect size, but the relevance of the filler task did. Effect sizes on the outcome measure decreased as the filler task relevance increased ($\Delta\beta$ = -0.2886). Neither was significant in the fixed effects analyses. Effect sizes diminished when goal expectancy was manipulated, regardless of whether the task was made easier or harder (easier, $\Delta\beta$ = -0.1520; harder, $\Delta\beta$ = -0.2186).

Predictive Models

We collapsed across levels of certain moderators to simplify the model and eliminate overfitting. To see the effect of theoretical and descriptive/methodological moderators working in concert, we fit seven regressions containing one theoretical moderator as the variable of interest and all descriptive and methodological moderators as covariates. The results of these regressions on the variables of interest can be seen in Tables 3a (random effects) and 3b (fixed effects). All results discussed in text are from the random effects models unless otherwise stated.

Directly versus indirectly linked primes were not significantly different when tested with all covariates, but because the prime type “pictures” was defined as indirect, this model had considerable redundancy. To account for this, we retested the model without the covariate of prime type. This decreased the overall model fit (ΔAIC = -100.6397, ΔBIC = -90.9183), but restored the effect of direct versus indirect primes as significant such that indirect primes had larger effect sizes than direct primes did ($\Delta\beta$ = 0.2010, $z = 2.7136$, $p < .0067$). This was consistent with the results of the single predictor model, suggesting a robust effect.

As in the single-predictor analyses, manipulations of value were only significant when it was manipulated downward ($\Delta\beta$ = -0.4634). Coded value itself, when constant within an experiment, was not as straightforward. In the random effects model, the intercept (corresponding to low value) was not significantly different from zero, but the coefficient for the high value condition was. This was reversed in the fixed effects analyses, however, where the intercept was significant but the coefficient for the high value condition was not.

In contrast to the findings of Weingarten et al. (2015), but consistent with the single-predictor results, goal expectancy emerged as a significant predictor. However, this finding offers little help in settling the debate over goal expectancy because it shows a decrease in predicted effect size whether goal expectancy is increased *or* decreased (increased: $\Delta\beta$ = -0.3035; decreased: $\Delta\beta$ = -0.2496). These findings must be interpreted with caution, however, given that relatively few studies manipulated goal expectancy, leading to much smaller samples sizes (of contrast units k) in the manipulation conditions compared to the no-manipulation intercept (k = 617).

Motivation is believed to be a function of both goal value and goal expectancy (Förster et al., 2005), so we tested the interaction between value and manipulations of expectancy. Not only were the interaction terms nonsignificant (ps > .37), but the intercept was nonsignificant (albeit close, β_0 = 0.2299, z = 1.8506, p = .0642) and most of the main effects disappeared (ps > .09) as well. The only remaining significant coefficient was that of increasing expectancy ($\Delta\beta$ = -0.3406).

The presence of an irrelevant filler task did not significantly modify the effect size estimate beyond the intercept (no filler) value of β_0 = 0.3842, but the presence of a filler task relevant to the goal/concept significantly decreased effect sizes ($\Delta\beta$ = -0.3515). The interaction of filler task presence/relevance and coded value was only significant at the intercept level. This lack of significance may be consistent with the goal-activation theory, but only to the extent of goal persistence, not goal escalation.

In all of the moderator-covariate models, unpublished status predicted lower effect sizes and the use of consumption or health as the goal and/or pictures as the priming stimuli both predicted larger effect sizes. Most models had a few other covariates reach significance as well, but only those three were consistent across models. This is consistent with a model including only descriptive and methodological features (Table 4).

When all theoretical moderators[10] are included in one regression[11] ($\beta_0 = 0.4007$, $z = 8.1113$, $p < .0001$), only a few emerge as significant predictors. Using indirect primes increases the effect ($\Delta\beta = 0.1260$), whereas manipulating the goal value downward ($\Delta\beta = -0.5213$), manipulating goal expectancy upward ($\Delta\beta = -0.2624$), and the presence of a relevant filler task ($\Delta\beta = -0.4218$) all diminish the effect. In the fixed effects model, these significant coefficients are joined by the interaction term for high value goals and manipulating expectancy upward, which increased effects ($\Delta\beta = 0.2893$), and the presence of an irrelevant filler task ($\Delta\beta = -0.0929$), which decreased effects. Statistics for all moderators are available in Table 5a (random effects) and Table 5b (fixed effects).

When all descriptive and methodological variables[12] are added as covariates to the theoretical moderators in one regression[13] ($\beta_0 = 0.2905$, $z = 2.2397$, $p = .0251$), several variables

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[10] Directness of the prime, manipulations of value, composite value [high/low], manipulations of expectancy, interaction of composite value and manipulation of expectancy, filler task presence/absence and relevance to goal, interaction of composite value and filler task presence/absence and relevance to goal.
[11] Random effects presented in text; fixed effects available in Table 5b.
[12] Year, publication status, country, liminality, proportion of primes, control type, goal(s) primed, dependent measure category, presence of funneled debriefing, task flexibility, and modality of prime. Another model was tested with all these predictors and method of priming, but method of priming was not significant at any level and the model fit statistics improved when it was removed ($\Delta AIC = -4.1168$; $\Delta BIC = -19.6943$).
[13] Random effects presented in text; fixed effects available in Table 5.

exerted a significant effect. Indirect primes were again more effective than direct primes ($\Delta\beta$ = 0.2020), consistent with the results of other models. Also consistent were the effects of manipulating the goal value downward ($\Delta\beta$ = -0.4469), manipulating goal expectancy upward ($\Delta\beta$ = -0.3601), and the presence of a relevant filler task ($\Delta\beta$ = -0.6077), all of which again diminished effect sizes. Publication bias remains a concern, as unpublished status predicted a sizeable drop in effect size ($\Delta\beta$ = -0.1914). Of the methodological characteristics, using a goal related to opinions of the self or others (coded category containing autonomy, efficacy, and impression formation goals) predicted a spike in effect size ($\Delta\beta$ = 0.2790), as did using a flexible dependent measure ($\Delta\beta$ = 0.1699) or a consumption ($\Delta\beta$ = 0.4286) or product choice ($\Delta\beta$ = 0.7401) dependent task. Statistics for all moderators are available in Table 6a (random effects) and Table 6b (fixed effects).

Chapter 4: Discussion

The current meta-analysis surveyed every record we could find of behavioral dependent measures in response to a non-affective, incidentally primed goal or concept with a non-opposite control goal, resulting in 159 eligible papers containing 264 eligible studies (*K*) and 682 eligible effect sizes (*k*). These papers were published and unpublished, from the U.S. and other countries, and spanned from 1984 to 2015. They used a wide variety of goals (e.g., achievement, socialization, consumption, health, etc.), priming methods (e.g., scrambled sentence tasks, word completions, word searches, (para)foveal presentation, reading/writing tasks, etc.), priming stimuli (words and pictures with direct or indirect relationships to the goal), non-opposite control conditions (neutral primes, nonsense primes, non-opposite goal primes, no task, etc.), and dependent measures (achievement or persistence on tasks like anagrams or puzzles, motor behaviors like sitting or standing, enacted allocation of time or money, choice of product, etc.). Studies could contain supra- or subliminal stimuli; a relevant, irrelevant, or no filler task between priming and the dependent measure; flexible or inflexible dependent tasks, and goals with high or low, manipulated or stable value and/or expectancy. All eligible comparisons were coded on these and other moderators and all eligible effect sizes were included in analyses, using study as a level two variable to account for non-independence between effect sizes from the same study.

Weighted mean estimates indicate a moderate effect size of $d = 0.4220$ (random effects) and $d = 0.3870$ (fixed effects), but with moderate to large heterogeneity ($I^2 = 66.77\%$). There was also considerable evidence of publication bias (132 missing studies), but the trim and fill procedure's recalculated $d = 0.2661$ and 95% CI [0.2221, 0.3102] nonetheless indicate a real effect. The published effects of behavioral priming are at worst inflated, not spurious.

Theoretical descriptive, and methodological features of the studies were subjected to single and multiple regressions with study as a level two variable. Perhaps the most interesting result is that of direct versus indirect linkage between primes and outcomes. There were competing hypotheses regarding linkage; it could be that direct primes are more effective because they offer clearer prototypes of the desired outcome behavior, or it could be that indirect primes were more effective because they could capitalize on multiple channels of activation. Equally interesting is the null hypothesis, that somehow direct primes and indirect primes could accomplish the same effects. Despite the multitude of possibilities, our results are clear, strong, and consistent: indirect primes lead to larger effect sizes than direct primes. In the single-predictor regression and the multiple regressions of one moderator with all the covariates, all the moderators with no covariates, and the full model, indirect primes emerged with a significant and sizable positive coefficient each time. While an experimental test is of course necessary to establish causation, we feel the statistical evidence presented here is a strong foundation in favor of indirect primes.

Goal value is theorized (and intuitively assumed) to predict effect size via motivation to achieve, and was a highly effective predictor in past research (Weingarten et al., 2015), but it was not as robust in the present meta-analysis. Coded value was indeed positively associated with effect size in the single-predictor regression, but its predictive performance in other models was surprisingly lackluster. When it was tested as a moderator along with the descriptive and methodological covariates, the intercept of the model failed to reach significance, but the coefficient for value did. Although troubling that an intercept would be nonsignificant, this is may be a statistical artifact. Coded value (along with all the other covariates) is at its lowest point at the intercept, explaining the corresponding low effect size. That the intercept was not statistically significantly different from zero actually highlights the importance of goal value – when the goal is of extremely low value, people are unmotivated to pursue it and outcomes suffer. Supporting this theory is coded value's significant positive coefficient, indicating that effect sizes rise when value does. Goal value was not significant when combined with other covariates, but theory insists that it should interact with expectancy (Förster et al., 2005).

Despite coded value's poor predictive utility, measures of goal value manipulations were significant in all models. Specifically, when goal value was manipulated downward, effect sizes followed (manipulating value upward was never significant). It is unclear why manipulation of value within a study would emerge as predictive but variations of value across studies would not, especially because the current investigation only used comparisons of goal-prime versus control-prime conditions; if value was also manipulated in a study, this would result in multiple contrast units, not in mismatched goal value within contrast units. One possible explanation is that the delineation of high versus low value conditions required experimenters to create scenarios more extreme along the spectrum of value than those scenarios used in studies in which goal value was a constant. This would imply that it really is goal value driving effect size, but that this effect was ironically captured better by our coding of value manipulation than it was by our coding of goal value itself.

We set out to test expectancy in hopes of supporting one or more preexisting theory, but our results seem to muddy the issue even further. Locke and Latham (2002) and Förster et al. (2005) agree that goal expectancy should be positively associated with goal-related accessibility. Assuming that automatic behaviors are stored as any other construct and are susceptible to accessibility (Bargh et al., 1996), this should translate to a positive association between expectancy and effect size of behavioral priming. Locke and Latham also suggest that task expectancy (difficulty) should be inversely curvilinearly associated with performance (2002). Neither of these theories wholly explain what we found. Manipulating expectancy upward (e.g., making the task more difficult) was negatively related to effect size in every model. In the single-predictor regression and single theoretical moderator regression, manipulating expectancy downward was also negatively related to effect size. These results are puzzling, because they contradict established theory. It could be that expectancy as we coded it was closer to task expectancy than goal expectancy, and thus should form an inverse curvilinear function where performance suffers at the extremes and peaks in the middle (Locke & Latham, 2002). The current data provide no evidence of a central peak; however, the variable was coded as manipulation of expectancy, not expectancy itself. Given that deliberate manipulation of a variable tends to create more extreme forms of it than occur sans manipulation, it is possible that the effect sizes caught in these codes are those from the far ends of that inverse curvilinear distribution. Further research is needed to clarify this issue, and we encourage future researchers of priming effects to include expectancy manipulations and grow the pool of effect sizes to analyze. We also encourage future meta-analysts to code expectancy itself, not just manipulation thereof, despite having firsthand knowledge of how difficult this can be to judge.

The interaction of goal value and expectancy was not significant in the full model or a model of all theoretical moderators without any covariates. This defies established theory, which says goal value and expectancy combine to determine motivation and accessibility (Förster et al., 2005), which should also inform performance. When this interaction and the two main effects were tested with the covariates but no other theoretical moderators, the only significant effect that emerged was the main effect that manipulating expectancy upward diminishes effect sizes. Perhaps this model’s sole theoretical survivor is the clue to why this interaction does not work, despite its backing in theory. As discussed above, expectancy did not conform to its theoretical expectations, perhaps because it was coded only in terms of manipulation. If these manipulations resulted in particularly extreme levels of high or low expectancy, in the tails of the distribution where performance suffers, it is likely that it would not combine in the normal way with value, which was coded on its own merits rather than in terms of manipulation only. Given these lackluster findings, we reiterate our call for future researchers to include manipulations of expectancy and for future meta-analysts to code expectancy itself.

The presence and relevance of filler tasks also has important theoretical implications. In the single-predictor regression, the mere presence (vs. absence) a filler task was not significant, but filler task relevance was significantly and negatively associated with effect size. This pattern makes theoretical sense. Perception-behavior and goal-activation accounts have opposing predictions for filler tasks, and both mechanisms are likely at work in this data. This conflict may have overshadowed any possible main effect of filler task mere presence. However, once filler tasks are examined in terms of their relevance to a primed goal, a pattern emerged. The intercept of this model (where filler tasks are present but irrelevant to the primed goal) is much larger than the overall estimate, likely because the irrelevant filler is causing goal tension to build and focusing activation on the goal-related task to come (Bargh et al., 2001; Förster et al., 2005). As filler tasks become more relevant, effect size diminishes drastically. This is in line with goal theory's expectation that goal satiation will lead to diminished accessibility and even inhibition of goal-related constructs, impairing performance on a second goal-related task (Bargh et al., 2001; Förster et al., 2005). This pattern of results continued into the multiple regressions, where relevant filler tasks decreased effect sizes and irrelevant filler tasks were not significant across all multiple regression models. This offers strong support for post-satiation inhibition. It is strange that irrelevant filler tasks were nonsignificant rather than a boost to effect sizes, given that they should facilitate goal escalation; however, it is possible that the conflicting effects within the data of the perception-behavior link and goal-activation continued to obscure any such effect. Finally, we attempted to replicate Weingarten et al.'s (2015) previous findings regarding an interaction between filler task and goal value. They found that filler tasks interacted with value such that, when the goal had high value, priming effects persisted across irrelevant filler tasks and only diminished if the filler task included an opportunity to satiate the goal, but when goal value is low, priming effects diminished regardless of the relevance of the filler task. These findings indicate that goal value determines which mechanism applies; high value goals lead to goal-activation patterns whereas low value goals lead to perception-behavior patterns (Weingarten et al., 2015). We were unable to replicate this interaction with value in any model, but as discussed above, our goal value variable defied many theory-based expectations. Due to these apparent abnormalities, we feel that our findings represent ambiguity rather than nuance and leave it to future researchers to replicate or disprove Weingarten et al.'s assertion.

We analyzed descriptive and methodological in the same way as the theoretical moderators, with surprisingly few significant results. Country and year were significant in some fixed effects analyses, but given the heterogeneity of the data, their lack of significance in the random effects analyses (single and multiple) is likely more telling. Effect sizes from unpublished studies were consistently smaller than their published counterparts in all models, which is consistent with the statistical evidence of publication bias in this field. Using a greater proportion of primes was positively associated with effect size in the single regressions, but this effect disappeared when it was used as a covariate among other predictors. Liminality, type of control group used, and presence of funneled debriefing were not significant predictors in any models. Having participants write about their own experiences as the priming task looked promising in the single-predictors regressions, but was no longer significant in the larger model. Task flexibility showed the opposite pattern; it was a significant covariate in the full model, but was not significant in the single-predictor regression.

Because many priming objects were eligible for inclusion, we were able to draw comparisons between them. The effect size resulting from words with a metaphorical connection to the goal did not appear to alter the effect size compared to words with a more direct link, but using pictures rather than words had a large, consistent, and significant effect, often doubling or even tripling the intercept value (which, in single predictor tests, corresponds to directly linked word primes). Pictures increased effect sizes in the single-predictor regression and as a covariate in every test of theoretical moderators[14], usually by a great deal ($\beta \geq 0.3765$). In the interests of parsimony and power, most analyses merged the human and non-human pictures categories; however, in initial tests, both reached significance. Although an experimental comparison of words and pictures is necessary to establish causality, this finding offers strong correlational evidence that pictures may be more effective stimuli than words. Using an unusual prime that we coded as "other" also increased effect sizes in most models, but not in every one. This finding is difficult to interpret because of the wide variety of stimuli included in that category, but taken along with the robust effect of pictures, it may indicate a reward available to researchers who use novel and creative primes rather than the more traditional word primes (which comprised 64.90% of the current sample).

Many different dependent variables were coded, but very few deviated from the intercept (which, in the single-predictor regression, corresponded to achievement tasks). The catch-all

[14] This variable was dropped in the model containing all theoretical moderators and covariates because of redundancy with the direct vs indirect variable.

category of "other" rose to significance, increasing effect size in the single-predictor regression, but did not reemerge in the full model with all moderators and covariates. Conversely, product choice measures emerged to increase effect size in the full model, but not in the single-predictor regression. Consumption tasks, however, increased effect sizes in both the single-predictor and full regression models. They did not serve as a significant covariate in the single moderator tests, but their emergence in the other two models is enough to say they warrant further research. They may be particularly sensitive to priming effects and/or particularly well suited targets of automatic behavior. Aside from consumption, no other tasks stood out as particularly remarkable. This gives researchers a large amount of freedom in selecting the dependent measures they deem fit without fear of it negatively affecting the effect size they can expect.

In the single-predictor regression each type of primed goal/concept coded was examined separately, and a few yielded significant results. Helping/cooperation, impression formation, consumption, and elderly goals/concepts each increased the effect size, many of them dramatically. The "other" category was also significant, although this is difficult to interpret due to its variety. In later models, some levels of goal/concept were merged to increase parsimony and eliminate categories with very few members (e.g., biographical reading about another person, $k = 2$). After this merge, in the full model, only the category containing autonomy, efficacy, and impression formation goals significantly increased effect sizes.

After reviewing the effects of a wide variety of theoretical, descriptive, and methodological moderators, we have a few recommendations for future priming researchers. First, and perhaps most excitingly, is the finding that indirect primes predict larger effect sizes than direct primes. Therefore, we recommend using indirect primes whenever possible. We also recommend that researchers create experiments with direct and indirect priming conditions to address the question of causality in this effect. It is tempting to say that indirect primes *cause* larger effects (and we have certainly entertained the notion in this article), but such assertions are best reserved until there is empirical evidence to support them. We also recommend the use of novel stimuli, particularly images, whenever possible, as these mediums consistently produced larger effect sizes than more traditional, word-based primes. Researchers should also investigate the use of consumption tasks as dependent variables, as this feature also predicted larger effect sizes. Aside from these, however, many of the factors we examined did not have a strong or significant influence on effect size, leading us to conclude that fretting over details like liminality, type of control group, task flexibility, or exact proportion of primes is not warranted. Researchers should feel confident designing the experiment that best serves their hypothesis and not be weighed down with concerns over minute methodological details.

In conclusion, we assert that behavioral priming effects are not spurious Type I errors, despite several failed replications. These effects are subject to publication bias and are likely inflated, but statistically correcting for such inflation still leaves a respectable effect size of about $d = 0.27$. An examination of multiple descriptive and methodological moderators indicates wide leeway for researchers design experiments without fear of losing their effects, but we have also presented evidence that indirect primes outperform direct primes and that novel priming stimuli offer advantages over more traditional stimuli. Furthermore, some of our results indicate how behavioral priming effects adhere to goal theory, but these results are incomplete and inconclusive, requiring the continued attention of other researchers in the field to solidify how behavioral priming fits into preexisting theories of perception and goal pursuit.

Chapter 5: Tables and Figures

Table 1

Sample Characteristics

Variable	Type of Statistic	Study (K) Summary		Units (k) Summary	
Descriptive characteristics					
Year	*M (SD)*	2007.52	(5.12)	2007.38	(5.51)
	Md	2007		2009	
Country	US Count (%)	143	(54.17)	396	(57.31)
	Non-US Count (%)	91	(34.47)	284	(41.1)
Publication status	Published Count (%)	214	(81.06)	519	(75.11)
	Unpublished Count (%)	53	(20.08)	168	(24.31)
Theoretical moderators					
Directness of the prime	Direct Count (%)			302	(43.7)
	Indirect Count (%)			389	(56.3)
Manipulations of value	Nonmanipulated Count (%)			487	(70.48)
	Decreased Count (%)			29	(4.2)
	Increased Count (%)			59	(8.54)
Coded value	Low Count (%)			326	(47.18)
	High Count (%)			179	(25.9)
Manipulations of expectancy	Nonmanipulated Count (%)			617	(89.29)
	Decreased Count (%)			28	(4.05)
	Increased Count (%)			44	(6.37)
Filler task presence and relevance	Absent Count (%)			560	(81.04)
	Irrelevant Count (%)			96	(13.89)
	Relevant Count (%)			23	(3.33)
Methodological features					
Liminality	Subliminal Count (%)	57	(21.59)	135	(19.54)
	Supraliminal Count (%)	208	(78.79)	556	(80.46)
Proportion of Primes	*M (SD)*	0.83	(0.23)	0.82	(0.23)
	Md	1		1	
Control type	Neutral Count (%)	168	(63.64)	429	(62.08)
	Other Count (%)	78	(29.55)	203	(29.38)
Funneled Debriefing	Absent Count (%)	138	(52.27)	329	(47.61)
	Present Count (%)	117	(44.32)	328	(47.47)
Task Flexibility	Inflexible Count (%)	131	(49.62)	315	(45.59)
	Flexible Count (%)	136	(51.52)	365	(52.82)
Modality of Prime	Words Unrelated to People Count (%)	139	(52.65)	358	(51.81)
	Words Related to People Count (%)	12	(4.55)	34	(4.92)
	Pictures Count (%)	33	(12.5)	56	(8.1)
	Other Count (%)	54	(20.45)	124	(17.95)

Note. Theoretical moderators are not displayed at the study (K) level because they may have been manipulated within some of those studies.

Table 2

Results of Single-Predictor Meta-Regressions

	Random Effects					Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
β_0 Year Int.	0.4608	0.0241	19.1440	0.0000	[0.4136, 0.508]	0.3912	0.0113	34.7595	0.0000	[0.3692, 0.4133]
Δβ Year	-0.0454	0.0254	-1.7876	0.0738	[-0.0951, 0.0044]	-0.0328	0.0115	-2.8486	0.0044	[-0.0554, -0.0102]
Δβ Pub Int (Published)	0.4892	0.0260	18.8230	0.0000	[0.4383, 0.5402]	0.4356	0.0130	33.4058	0.0000	[0.41, 0.4612]
Δβ Unpublished	-0.1412	0.0500	-2.8243	0.0047	[-0.2391, -0.0432]	-0.1882	0.0254	-7.4141	0.0000	[-0.2379, -0.1384]
β_0 Country: US	0.4182	0.0323	12.9557	0.0000	[0.3549, 0.4814]	0.3246	0.0147	22.0639	0.0000	[0.2958, 0.3534]
Δβ Country: Canada	0.0234	0.1051	0.2228	0.8237	[-0.1826, 0.2294]	0.1043	0.0502	2.0789	0.0376	[0.006, 0.2026]
Δβ Country: Netherlands	0.1399	0.0758	1.8456	0.0650	[-0.0087, 0.2884]	0.2113	0.0388	5.4498	0.0000	[0.1353, 0.2873]
Δβ Country: Germany	0.0355	0.1172	0.3026	0.7622	[-0.1943, 0.2652]	-0.0046	0.0600	-0.0767	0.9388	[-0.1223, 0.1131]
Δβ Country: UK	0.1971	0.1163	1.6939	0.0903	[-0.0309, 0.4251]	0.2531	0.0662	3.8251	0.0001	[0.1234, 0.3828]
Δβ Country: France	0.0538	0.1164	0.4623	0.6439	[-0.1743, 0.2819]	0.1009	0.0486	2.0753	0.0380	[0.0056, 0.1961]
Δβ Country: Japan	-0.1581	0.2678	-0.5901	0.5551	[-0.683, 0.3669]	-0.0794	0.1286	-0.6171	0.5372	[-0.3315, 0.1728]
Δβ Country: Hong Kong	-0.2901	0.3575	-0.8114	0.4171	[-0.9909, 0.4107]	-0.1965	0.1600	-1.2281	0.2194	[-0.5102, 0.1171]
Δβ Country: Other	0.1130	0.0815	1.3856	0.1659	[-0.0468, 0.2728]	0.1890	0.0367	5.1560	0.0000	[0.1172, 0.2609]
β_0 No Value Manipulation	0.4573	0.0262	17.4769	0.0000	[0.406, 0.5086]	0.3795	0.0131	28.8974	0.0000	[0.3538, 0.4052]
Δβ Manipulating Value Downward	-0.5146	0.0866	-5.9452	0.0000	[-0.6843, -0.345]	-0.4861	0.0624	-7.7959	0.0000	[-0.6083, -0.3639]
Δβ Manipulating Value Upward	0.0495	0.0690	0.7167	0.4736	[-0.0858, 0.1848]	-0.0094	0.0400	-0.2355	0.8138	[-0.0878, 0.0689]
β_0 No Expectancy Manipulation	0.4718	0.0241	19.5488	0.0000	[0.4245, 0.5191]	0.4100	0.0117	34.9335	0.0000	[0.387, 0.4331]
Δβ Manipulating Expectancy Downward	-0.2186	0.0825	-2.6490	0.0081	[-0.3803, -0.0568]	-0.3485	0.0618	-5.6420	0.0000	[-0.4696, -0.2275]
Δβ Manipulating Expectancy Upward	-0.1520	0.0737	-2.0616	0.0392	[-0.2965, -0.0075]	-0.1855	0.0469	-3.9591	0.0001	[-0.2773, -0.0937]
β_0 Filler Task Absent	0.4647	0.0260	17.8402	0.0000	[0.4136, 0.5157]	0.3964	0.0125	31.7089	0.0000	[0.3719, 0.4209]
Δβ Filler Task Present	-0.0319	0.0547	-0.5825	0.5602	[-0.139, 0.0753]	-0.0488	0.0279	-1.7503	0.0801	[-0.1034, 0.0058]
β_0 Irrelevant Filler Task	0.5321	0.0719	7.4008	0.0000	[0.3912, 0.673]	0.3559	0.0285	12.4939	0.0000	[0.3001, 0.4118]
Δβ Relevant Filler Task	-0.2886	0.0915	-3.1538	0.0016	[-0.468, -0.1093]	-0.0566	0.0399	-1.4190	0.1559	[-0.1348, 0.0216]
β_0 Liminality: Subliminal	0.4612	0.0530	8.7031	0.0000	[0.3573, 0.565]	0.3793	0.0258	14.7136	0.0000	[0.3288, 0.4298]
Δβ Liminality: Supraliminal	-0.0025	0.0594	-0.0428	0.9658	[-0.1189, 0.1138]	0.0094	0.0286	0.3284	0.7426	[-0.0467, 0.0654]
β_0 Proportion of Primes Intercept	0.4421	0.0249	17.7445	0.0000	[0.3933, 0.4909]	0.3617	0.0117	30.8119	0.0000	[0.3387, 0.3847]
Δβ Proportion of Primes	0.0672	0.0246	2.7338	0.0063	[0.019, 0.1154]	0.0593	0.0120	4.9344	0.0000	[0.0358, 0.0829]

Table 2 Continued

	Random Effects					Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
β_0 Control: Neutral Words	0.4625	0.0311	14.8503	0.0000	[0.4015, 0.5235]	0.3945	0.0142	27.8004	0.0000	[0.3667, 0.4224]
Δβ Control: Nonsense Words	-0.0082	0.1412	-0.0578	0.9539	[-0.2849, 0.2685]	0.0487	0.0696	0.7000	0.4839	[-0.0877, 0.1852]
Δβ Control: No Task	0.0377	0.0750	0.5027	0.6152	[-0.1093, 0.1847]	0.0265	0.0366	0.7247	0.4687	[-0.0452, 0.0982]
Δβ Control: Neutral Reading/Writing	0.0084	0.1305	0.0646	0.9485	[-0.2473, 0.2642]	0.0437	0.0663	0.6599	0.5093	[-0.0862, 0.1737]
Δβ Control: Non-opposite Goal	-0.0965	0.1036	-0.9320	0.3514	[-0.2995, 0.1065]	-0.0343	0.0529	-0.6476	0.5172	[-0.1381, 0.0695]
Δβ Control: Other	0.0144	0.0841	0.1712	0.8641	[-0.1504, 0.1792]	-0.1068	0.0413	-2.5834	0.0098	[-0.1878, -0.0258]
β_0 Flexibility Intercept	0.4474	0.0516	8.6685	0.0000	[0.3463, 0.5486]	0.3991	0.0274	14.5777	0.0000	[0.3454, 0.4527]
Δβ Flexibility	0.0076	0.0269	0.2814	0.7784	[-0.0452, 0.0604]	-0.0041	0.0147	-0.2823	0.7777	[-0.0329, 0.0246]
β_0 Funneled Debriefing Absent	0.4553	0.0326	13.9713	0.0000	[0.3914, 0.5192]	0.3729	0.0159	23.4467	0.0000	[0.3417, 0.4041]
Δβ Funneled Debriefing Present	0.0037	0.0451	0.0821	0.9346	[-0.0848, 0.0922]	0.0145	0.0230	0.6311	0.5280	[-0.0305, 0.0595]
β_0 Method Category: Blank	0.5560	0.0629	8.8429	0.0000	[0.4328, 0.6792]	0.5380	0.0279	19.3046	0.0000	[0.4834, 0.5926]
Δβ Method Category: Scrambled Sentence Task	-0.2294	0.0799	-2.8708	0.0041	[-0.386, -0.0728]	-0.2497	0.0370	-6.7448	0.0000	[-0.3222, -0.1771]
Δβ Method Category: Word Completion	-0.1125	0.1377	-0.8169	0.4140	[-0.3825, 0.1574]	-0.1560	0.0646	-2.4164	0.0157	[-0.2826, -0.0295]
Δβ Method Category: Other	0.0661	0.0752	0.8797	0.3790	[-0.0812, 0.2134]	-0.0231	0.0366	-0.6329	0.5268	[-0.0948, 0.0485]
Δβ Method Category: Stroop Task	0.3408	0.2993	1.1386	0.2549	[-0.2458, 0.9274]	0.4396	0.2057	2.1367	0.0326	[0.0364, 0.8428]
Δβ Method Category: Free Association	-0.0982	0.3428	-0.2863	0.7746	[-0.7701, 0.5738]	-0.0802	0.1678	-0.4779	0.6327	[-0.4091, 0.2487]
Δβ Method Category: Social Goal Contagion Task	0.0072	0.2345	0.0306	0.9756	[-0.4524, 0.4667]	0.0067	0.1495	0.0450	0.9641	[-0.2863, 0.2997]
Δβ Method Category: Parafoveal Priming Task	-0.1697	0.1316	-1.2888	0.1975	[-0.4277, 0.0884]	-0.3069	0.0551	-5.5721	0.0000	[-0.4148, -0.1989]
Δβ Method Category: Lexical Decision Task	-0.1002	0.1585	-0.6321	0.5273	[-0.411, 0.2105]	-0.0982	0.0926	-1.0607	0.2888	[-0.2797, 0.0833]
Δβ Method Category: Self-story (write about own experience(s))	-0.3379	0.1509	-2.2397	0.0251	[-0.6336, -0.0422]	-0.2750	0.0716	-3.8400	0.0001	[-0.4154, -0.1346]
Δβ Method Category: Autobiographical Writing (write about someone else)	-0.1904	0.1952	-0.9756	0.3293	[-0.5729, 0.1921]	-0.1531	0.1077	-1.4212	0.1553	[-0.3643, 0.0581]
Δβ Method Category: Autobiographical Reading (read about someone else)	-0.2860	0.2654	-1.0777	0.2812	[-0.8062, 0.2342]	-0.3407	0.1460	-2.3335	0.0196	[-0.6269, -0.0545]
Δβ Method Category: Foveal Priming Task	-0.1240	0.0946	-1.3106	0.1900	[-0.3094, 0.0614]	-0.1279	0.0479	-2.6690	0.0076	[-0.2219, -0.034]

Table 2 Continued

	Random Effects					Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
Δβ Method Category: Word Search	-0.2390	0.0860	-2.7791	0.0055	[-0.4075, -0.0704]	-0.2778	0.0411	-6.7630	0.0000	[-0.3583, -0.1973]
β_0 Low Value	0.3404	0.0453	7.5084	0.0000	[0.2516, 0.4293]	0.3275	0.0216	15.1435	0.0000	[0.2851, 0.3699]
Δβ High Value	0.0641	0.0183	3.5023	0.0005	[0.0282, 0.1]	0.0268	0.0094	2.8419	0.0045	[0.0083, 0.0452]
β_0 Prime: Words Not Related to People	0.3630	0.0324	11.2185	0.0000	[0.2996, 0.4264]	0.3158	0.0155	20.3106	0.0000	[0.2853, 0.3462]
Δβ Prime: Words Related to People	0.1799	0.1170	1.5373	0.1242	[-0.0495, 0.4092]	0.0508	0.0513	0.9903	0.3220	[-0.0498, 0.1514]
Δβ Prime: Non-Human Pictures	0.3256	0.1295	2.5151	0.0119	[0.0719, 0.5794]	0.1521	0.0671	2.2660	0.0234	[0.0205, 0.2836]
Δβ Prime: Human Pictures	0.2875	0.0930	3.0923	0.0020	[0.1053, 0.4698]	0.2755	0.0494	5.5744	0.0000	[0.1786, 0.3724]
Δβ Prime: Other	0.1594	0.0617	2.5860	0.0097	[0.0386, 0.2803]	0.0873	0.0303	2.8786	0.0040	[0.0279, 0.1468]
β_0 Goal: Achievement	0.3548	0.0436	8.1353	0.0000	[0.2693, 0.4403]	0.3177	0.0214	14.8576	0.0000	[0.2758, 0.3596]
Δβ Goal: Helping/Cooperation	0.3541	0.1072	3.3029	0.0010	[0.144, 0.5643]	0.3669	0.0540	6.7929	0.0000	[0.2611, 0.4728]
Δβ Goal: Action	0.0362	0.1095	0.3309	0.7407	[-0.1784, 0.2509]	0.0289	0.0609	0.4736	0.6358	[-0.0906, 0.1483]
Δβ Goal: Inaction	0.0012	0.1244	0.0097	0.9922	[-0.2426, 0.245]	0.0102	0.0796	0.1279	0.8982	[-0.1458, 0.1661]
Δβ Goal: Aggression	-0.0491	0.3842	-0.1277	0.8984	[-0.8022, 0.704]	-0.0120	0.2436	-0.0492	0.9608	[-0.4894, 0.4655]
Δβ Goal: Socialization	0.0833	0.1357	0.6138	0.5394	[-0.1827, 0.3492]	0.1291	0.0878	1.4700	0.1416	[-0.043, 0.3013]
Δβ Goal: Autonomy	0.0905	0.1562	0.5792	0.5624	[-0.2157, 0.3967]	-0.0157	0.1037	-0.1511	0.8799	[-0.219, 0.1877]
Δβ Goal: Studying	0.0009	0.2528	0.0034	0.9973	[-0.4947, 0.4964]	0.0193	0.1338	0.1439	0.8856	[-0.2429, 0.2814]
Δβ Goal: Failure	0.7813	0.4049	1.9295	0.0537	[-0.0123, 1.575]	0.8184	0.2751	2.9751	0.0029	[0.2793, 1.3576]
Δβ Goal: Efficacy	0.4252	0.4431	0.9595	0.3373	[-0.4433, 1.2937]	0.4623	0.3287	1.4062	0.1597	[-0.182, 1.1066]
Δβ Goal: Consumption	0.3196	0.1553	2.0586	0.0395	[0.0153, 0.6239]	0.3262	0.0821	3.9723	0.0001	[0.1652, 0.4871]
Δβ Goal: Impression Formation	0.3215	0.1543	2.0833	0.0372	[0.019, 0.6239]	0.2430	0.0825	2.9444	0.0032	[0.0812, 0.4047]
Δβ Goal: Health/Dieting	-0.0138	0.1066	-0.1292	0.8972	[-0.2226, 0.1951]	0.0436	0.0582	0.7497	0.4535	[-0.0704, 0.1576]
Δβ Goal: Fairness	0.0833	0.2055	0.4053	0.6852	[-0.3194, 0.486]	0.0575	0.1013	0.5681	0.5700	[-0.1409, 0.256]
Δβ Goal: Seeking Casual Sex	0.1582	0.1651	0.9585	0.3378	[-0.1653, 0.4818]	0.0115	0.1245	0.0925	0.9263	[-0.2325, 0.2556]
Δβ Goal: Other	0.1429	0.0543	2.6328	0.0085	[0.0365, 0.2493]	0.0690	0.0278	2.4817	0.0131	[0.0145, 0.1234]
Δβ Goal: Elderly	0.7410	0.2618	2.8301	0.0047	[0.2278, 1.2542]	0.6249	0.2277	2.7440	0.0061	[0.1786, 1.0713]
Δβ Goal: Courage	-1.1214	0.4680	-2.3964	0.0166	[-2.0386, -0.2043]	-1.0843	0.3615	-2.9995	0.0027	[-1.7929, -0.3758]
Δβ Goal: Defensiveness/Intimacy	-0.5874	0.3499	-1.6787	0.0932	[-1.2731, 0.0984]	-0.5503	0.1847	-2.9787	0.0029	[-0.9123, -0.1882]
Δβ Goal: Entertain	0.4745	0.2701	1.7569	0.0789	[-0.0548, 1.0038]	0.2157	0.2096	1.0290	0.3035	[-0.1951, 0.6264]

Table 2 Continued

	Random Effects					Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
Δβ Goal: Self-Regulation	-0.1191	0.1379	-0.8636	0.3878	[-0.3895, 0.1512]	-0.1173	0.0768	-1.5282	0.1265	[-0.2678, 0.0331]
Δβ Goal: Speed	0.1795	0.3793	0.4732	0.6361	[-0.5639, 0.9228]	0.2166	0.2357	0.9188	0.3582	[-0.2454, 0.6785]
Δβ Goal: Slowness	0.0661	0.4410	0.1499	0.8809	[-0.7983, 0.9305]	0.1032	0.3259	0.3167	0.7515	[-0.5355, 0.7419]
Δβ Goal: Indulgence	0.1828	0.3395	0.5384	0.5903	[-0.4826, 0.8481]	-0.0870	0.2855	-0.3048	0.7605	[-0.6467, 0.4726]
Δβ Goal: Extrinsic/Intrinsic Motivation	-0.0440	0.3176	-0.1384	0.8899	[-0.6666, 0.5786]	-0.0069	0.1122	-0.0613	0.9511	[-0.2269, 0.2131]
Δβ Goal: Market/Trade	-0.2288	0.2581	-0.8865	0.3753	[-0.7347, 0.2771]	-0.1480	0.1170	-1.2644	0.2061	[-0.3774, 0.0814]
Δβ Goal: Unkindness	0.3603	0.2734	1.3179	0.1875	[-0.1755, 0.8962]	0.3976	0.1721	2.3106	0.0209	[0.0603, 0.7348]
β_0 DV: Achievement	0.4020	0.0368	10.9139	0.0000	[0.3298, 0.4742]	0.3714	0.0185	20.1211	0.0000	[0.3352, 0.4076]
Δβ DV: Reaction Time	0.0657	0.0919	0.7148	0.4747	[-0.1144, 0.2457]	-0.1065	0.0398	-2.6723	0.0075	[-0.1845, -0.0284]
Δβ DV: Time Spent on Task	-0.0265	0.0648	-0.4082	0.6831	[-0.1534, 0.1005]	-0.0207	0.0379	-0.5465	0.5847	[-0.095, 0.0535]
Δβ DV: Enacted Monetary Donation	-0.0489	0.1107	-0.4417	0.6587	[-0.266, 0.1681]	0.0924	0.0732	1.2620	0.2069	[-0.0511, 0.2358]
Δβ DV: Enacted Monetary Spending	-0.2230	0.1876	-1.1889	0.2345	[-0.5908, 0.1447]	-0.1785	0.0900	-1.9833	0.0473	[-0.3548, -0.0021]
Δβ DV: Enacted Volunteering Time	0.2428	0.1365	1.7787	0.0753	[-0.0247, 0.5104]	0.2900	0.0764	3.7966	0.0001	[0.1403, 0.4397]
Δβ DV: Consumption	0.2398	0.0889	2.6980	0.0070	[0.0656, 0.4139]	0.1321	0.0467	2.8258	0.0047	[0.0405, 0.2237]
Δβ DV: Enacted Choice of Product	0.1088	0.1515	0.7181	0.4727	[-0.1882, 0.4059]	-0.0445	0.0688	-0.6467	0.5178	[-0.1794, 0.0904]
Δβ DV: Other	0.2042	0.0612	3.3359	0.0009	[0.0842, 0.3242]	0.0811	0.0323	2.5144	0.0119	[0.0179, 0.1444]
Δβ DV: Helping Rates	0.4821	0.3224	1.4954	0.1348	[-0.1498, 1.1139]	0.3479	0.2226	1.5626	0.1182	[-0.0885, 0.7843]
Δβ DV: Spacebar Presses	-0.1808	0.3368	-0.5369	0.5913	[-0.841, 0.4793]	-0.1503	0.1316	-1.1421	0.2534	[-0.4082, 0.1076]
Δβ DV: Number of Thoughts Listed	-0.2064	0.3602	-0.5731	0.5666	[-0.9123, 0.4995]	-0.1758	0.1833	-0.9594	0.3374	[-0.5351, 0.1834]
Δβ DV: Approach Task	-1.1686	0.4761	-2.4543	0.0141	[-2.1018, -0.2354]	-1.1380	0.3613	-3.1494	0.0016	[-1.8462, -0.4298]
Δβ DV: Free Association/Selective Information Exposure	0.0825	0.2424	0.3405	0.7335	[-0.3926, 0.5577]	0.1206	0.0955	1.2626	0.2067	[-0.0666, 0.3077]
Δβ DV: Task Choice	-0.0229	0.2995	-0.0764	0.9391	[-0.6099, 0.5642]	0.0559	0.2005	0.2789	0.7803	[-0.3371, 0.449]
Δβ DV: Face Touching	0.1065	0.3323	0.3203	0.7487	[-0.5449, 0.7578]	0.1207	0.2478	0.4868	0.6264	[-0.3651, 0.6064]
Δβ DV: Choice of Stairs or Elevator	0.3216	0.3764	0.8545	0.3928	[-0.4161, 1.0592]	0.3522	0.2133	1.6507	0.0988	[-0.066, 0.7703]
Δβ DV: 20	-0.3637	0.3848	-0.9450	0.3447	[-1.1179, 0.3906]	-0.3331	0.2280	-1.4612	0.1440	[-0.7799, 0.1137]
Δβ DV: Amount of Information Selected	-0.3113	0.3547	-0.8776	0.3802	[-1.0066, 0.384]	-0.2807	0.1724	-1.6288	0.1034	[-0.6186, 0.0571]
Δβ DV: Adjustment to Change	0.2042	0.3376	0.6048	0.5453	[-0.4575, 0.8658]	0.2347	0.1335	1.7585	0.0787	[-0.0269, 0.4964]

Table 2 Continued

	Random Effects					Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
Δβ DV: Curiosity	0.1085	0.3849	0.2818	0.7781	[-0.646, 0.8629]	0.1390	0.2281	0.6095	0.5422	[-0.3081, 0.5861]
Δβ DV: Cleanliness	0.0499	0.3867	0.1289	0.8974	[-0.7082, 0.8079]	0.0804	0.2312	0.3479	0.7279	[-0.3727, 0.5335]
Δβ DV: Arm-Holding Task	-0.5300	0.2838	-1.8679	0.0618	[-1.0862, 0.0261]	-0.4784	0.1757	-2.7228	0.0065	[-0.8227, -0.134]
Δβ DV: Time Spent on Puzzles	-0.5174	0.2141	-2.4170	0.0156	[-0.937, -0.0978]	-0.3720	0.1697	-2.1916	0.0284	[-0.7047, -0.0393]
Δβ DV: Choice of Game	0.1142	0.3884	0.2941	0.7687	[-0.647, 0.8755]	0.1448	0.2339	0.6189	0.5360	[-0.3137, 0.6033]
Δβ DV: 35	0.7326	0.3924	1.8668	0.0619	[-0.0365, 1.5016]	0.6809	0.3437	1.9813	0.0476	[0.0073, 1.3545]

Table 3

Regression Results Single Moderators with All Covariates Included

	3a: Random Effects						3b: Fixed Effects					
	(Δ)β	SE	z	p	k	95% CI	(Δ)β	SE	z	p	k	95% CI
β_0 Direct Primes	0.3166	0.1032	3.0682	0.0022	503	[0.1143, 0.5188]	0.3594	0.051	7.0517	0	503	[0.2595, 0.4593]
Δβ Indirect Primes	0.201	0.0741	2.7136	0.0067	503	[0.0558, 0.3462]	0.1592	0.0392	4.0573	0	503	[0.0823, 0.2361]
β_0 Manipulating Value	0.3561	0.1072	3.3227	0.0009	445	[0.146, 0.5661]	0.3552	0.0543	6.5381	0	445	[0.2488, 0.4617]
Δβ Decreasing Value	-0.4634	0.091	-5.095	0	445	[-0.6417, -0.2852]	-0.4399	0.0656	-6.7079	0	445	[-0.5684, -0.3114]
Δβ Increasing Value	0.1377	0.0829	1.662	0.0965	445	[-0.0247, 0.3001]	-0.0047	0.0545	-0.0854	0.9320	445	[-0.1114, 0.1021]
β_0 Low Value	0.1952	0.1269	1.5378	0.1241	397	[-0.0536, 0.444]	0.2587	0.0622	4.1591	0	397	[0.1368, 0.3806]
Δβ High Value	0.1426	0.07	2.0362	0.0417	397	[0.0053, 0.2798]	0.0047	0.0396	0.1193	0.9050	397	[-0.073, 0.0824]
β_0 Manipulating Expectancy	0.3609	0.106	3.4045	0.0007	445	[0.1531, 0.5686]	0.3477	0.0543	6.4037	0	445	[0.2413, 0.4541]
Δβ Decreasing Expectancy	-0.2496	0.1034	-2.4144	0.0158	445	[-0.4522, -0.047]	-0.2465	0.0778	-3.1667	0.0015	445	[-0.399, -0.0939]
Δβ Increasing Expectancy	-0.3035	0.1071	-2.8325	0.0046	445	[-0.5135, -0.0935]	-0.2419	0.0601	-4.0234	0.0001	445	[-0.3597, -0.124]
β_0 Value X Manipulating Expectancy (Low Value, No Manipulation of Expectancy)	0.2299	0.1242	1.8506	0.0642	397	[-0.0136, 0.4733]	0.2963	0.0627	4.7251	0	397	[0.1734, 0.4193]
Δβ High Value	0.1158	0.0701	1.6518	0.0986	397	[-0.0216, 0.2531]	-0.0435	0.0418	-1.0402	0.2983	397	[-0.1254, 0.0384]
Δβ Decreasing Expectancy	-0.1392	0.1285	-1.0829	0.2789	397	[-0.3911, 0.1127]	-0.2354	0.0964	-2.4418	0.0146	397	[-0.4244, -0.0465]
Δβ Increasing Expectancy	-0.3406	0.134	-2.5425	0.0110	397	[-0.6032, -0.078]	-0.3706	0.0797	-4.652	0	397	[-0.5267, -0.2144]
Δβ High Value and Decreased Expectancy	-0.2362	0.2685	-0.8798	0.3790	397	[-0.7624, 0.29]	-0.0292	0.1831	-0.1596	0.8732	397	[-0.3882, 0.3297]
Δβ High Value and Increased Expectancy	0.0817	0.3367	0.2425	0.8084	397	[-0.5783, 0.7416]	0.2726	0.2182	1.2495	0.2115	397	[-0.155, 0.7002]
β_0 No Filler Task	0.3842	0.1161	3.3083	0.0009	439	[0.1566, 0.6118]	0.3489	0.0577	6.0441	0	439	[0.2358, 0.4621]

Table 3 Continued

	3a: Random Effects						3b: Fixed Effects					
	(Δ)β	SE	z	p	k	95% CI	(Δ)β	SE	z	p	k	95% CI
Δβ Irrelevant Filler Task	0.0838	0.0884	0.9483	0.3430	439	[-0.0894, 0.257]	-0.0327	0.0419	-0.7807	0.4350	439	[-0.1149, 0.0494]
Δβ Relevant Filler Task	-0.3515	0.1665	-2.1112	0.0348	439	[-0.6778, -0.0252]	-0.0922	0.0828	-1.1136	0.2654	439	[-0.2546, 0.0701]
β_0 Value X Filler Task (Low Value, No Filler Task)	0.2781	0.1362	2.0415	0.0412	393	[0.0111, 0.5451]	0.3277	0.0669	4.898	0	393	[0.1966, 0.4588]
Δβ High Value	0.0955	0.0806	1.1861	0.2356	393	[-0.0623, 0.2534]	-0.0517	0.0436	-1.1858	0.2357	393	[-0.1373, 0.0338]
Δβ Irrelevant Filler Task	0.0289	0.1107	0.2611	0.7940	393	[-0.1881, 0.2459]	-0.1226	0.052	-2.3561	0.0185	393	[-0.2245, -0.0206]
Δβ Relevant Filler Task	-0.5694	0.2364	-2.4088	0.0160	393	[-1.0327, -0.1061]	-0.2771	0.1219	-2.2736	0.0230	393	[-0.5159, -0.0382]
Δβ High Value and Irrelevant Filler Task	0.0973	0.1677	0.5803	0.5617	393	[-0.2314, 0.426]	0.2423	0.098	2.4719	0.0134	393	[0.0502, 0.4344]
Δβ High Value and Relevant Filler Task	0.2395	0.3232	0.741	0.4587	393	[-0.3939, 0.8729]	0.1709	0.173	0.9884	0.3230	393	[-0.168, 0.5099]

Table 4

Results of a Regression Containing All Descriptive and Methodological Variables

	4a: Random Effects					4b: Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
Intercept	0.3018	0.1905	1.5844	0.1131	[-0.0715, 0.6752]	0.3018	0.1905	1.5844	0.1131	[-0.0715, 0.6752]
Year (scaled)	-0.0165	0.035	-0.4724	0.6366	[-0.0852, 0.0521]	-0.0165	0.035	-0.4724	0.6366	[-0.0852, 0.0521]
Publication status: Unpublished	-0.1825	0.0762	-2.3944	0.0166	[-0.3319, -0.0331]	-0.1825	0.0762	-2.3944	0.0166	[-0.3319, -0.0331]
Country: Non-US	0.0367	0.0657	0.5584	0.5766	[-0.0921, 0.1654]	0.0367	0.0657	0.5584	0.5766	[-0.0921, 0.1654]
Liminality: Supraliminal	-0.0331	0.1588	-0.2082	0.8350	[-0.3444, 0.2782]	-0.0331	0.1588	-0.2082	0.8350	[-0.3444, 0.2782]
Proportion of Primes (scaled)	-0.0103	0.0371	-0.2785	0.7807	[-0.0831, 0.0624]	-0.0103	0.0371	-0.2785	0.7807	[-0.0831, 0.0624]
Control Type: Not Neutral Words	-0.2386	0.0827	-2.8841	0.0039	[-0.4008, -0.0765]	-0.2386	0.0827	-2.8841	0.0039	[-0.4008, -0.0765]
Goal/Concept: Social	0.1294	0.1267	1.0212	0.3072	[-0.119, 0.3778]	0.1294	0.1267	1.0212	0.3072	[-0.119, 0.3778]
Goal/Concept: Consumption	0.2001	0.1586	1.2614	0.2072	[-0.1108, 0.5109]	0.2001	0.1586	1.2614	0.2072	[-0.1108, 0.5109]
Goal/Concept: Self Concept	0.2977	0.1285	2.3169	0.0205	[0.0459, 0.5495]	0.2977	0.1285	2.3169	0.0205	[0.0459, 0.5495]
Goal/Concept: Other	0.1075	0.0778	1.3827	0.1668	[-0.0449, 0.26]	0.1075	0.0778	1.3827	0.1668	[-0.0449, 0.26]
DV: Reaction Time	-0.1	0.134	-0.7461	0.4556	[-0.3626, 0.1627]	-0.1	0.134	-0.7461	0.4556	[-0.3626, 0.1627]
DV: Usage of Time	-0.0604	0.0876	-0.6895	0.4905	[-0.2321, 0.1113]	-0.0604	0.0876	-0.6895	0.4905	[-0.2321, 0.1113]
DV: Usage of Money	-0.195	0.1588	-1.2279	0.2195	[-0.5061, 0.1162]	-0.195	0.1588	-1.2279	0.2195	[-0.5061, 0.1162]
DV: Consumption	0.1085	0.1525	0.7112	0.4770	[-0.1905, 0.4074]	0.1085	0.1525	0.7112	0.4770	[-0.1905, 0.4074]
DV: Product Choice	-0.1613	0.2198	-0.7339	0.4630	[-0.592, 0.2694]	-0.1613	0.2198	-0.7339	0.4630	[-0.592, 0.2694]
DV: Other	-0.1095	0.0764	-1.4335	0.1517	[-0.2593, 0.0402]	-0.1095	0.0764	-1.4335	0.1517	[-0.2593, 0.0402]
Presence of Funneled Debriefing	0.0416	0.0657	0.6325	0.5270	[-0.0872, 0.1703]	0.0416	0.0657	0.6325	0.5270	[-0.0872, 0.1703]
High Task Flexibility	0.0819	0.0654	1.2527	0.2103	[-0.0463, 0.2102]	0.0819	0.0654	1.2527	0.2103	[-0.0463, 0.2102]
Priming Method: Word Tasks	-0.0052	0.0898	-0.058	0.9537	[-0.1812, 0.1708]	-0.0052	0.0898	-0.058	0.9537	[-0.1812, 0.1708]
Priming Method: Pictures	0.0627	0.1715	0.3657	0.7146	[-0.2734, 0.3989]	0.0627	0.1715	0.3657	0.7146	[-0.2734, 0.3989]
Priming Method: Reading/Writing Tasks	-0.0789	0.217	-0.3634	0.7163	[-0.5043, 0.3465]	-0.0789	0.217	-0.3634	0.7163	[-0.5043, 0.3465]
Priming Method: Other	0.2579	0.1181	2.1828	0.0291	[0.0263, 0.4895]	0.2579	0.1181	2.1828	0.0291	[0.0263, 0.4895]
Priming Method: Writing About Self	-0.0497	0.2396	-0.2073	0.8358	[-0.5192, 0.4199]	-0.0497	0.2396	-0.2073	0.8358	[-0.5192, 0.4199]
Primes: Words Related to People	0.3034	0.1482	2.047	0.0407	[0.0129, 0.5939]	0.3034	0.1482	2.047	0.0407	[0.0129, 0.5939]
Primes: Pictures	0.2434	0.1271	1.9151	0.0555	[-0.0057, 0.4925]	0.2434	0.1271	1.9151	0.0555	[-0.0057, 0.4925]
Primes: Other	0.1115	0.1362	0.8183	0.4132	[-0.1555, 0.3784]	0.1115	0.1362	0.8183	0.4132	[-0.1555, 0.3784]

Table 5

Results of a Regression Containing All Moderators, No Covariates

	5a: Random Effects					5b: Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
β_0 Intercept	0.4007	0.0494	8.1113	0	[0.3039, 0.4975]	0.3849	0.0238	16.1672	0	[0.3383, 0.4316]
Δβ Indirect Primes	0.126	0.0562	2.2432	0.0249	[0.0159, 0.236]	0.0981	0.0276	3.5552	0.0004	[0.044, 0.1522]
Δβ Manipulating Value Downward	-0.5159	0.0948	-5.4403	0	[-0.7017, -0.33]	-0.4335	0.0707	-6.1355	0	[-0.572, -0.295]
Δβ Manipulating Value Upward	-0.0188	0.0808	-0.2327	0.8160	[-0.1772, 0.1396]	0.0655	0.0523	1.2523	0.2105	[-0.037, 0.1681]
Δβ High Value	0.0743	0.0609	1.2198	0.2225	[-0.0451, 0.1936]	0.006	0.0324	0.1847	0.8535	[-0.0576, 0.0695]
Δβ Manipulating Expectancy Downward	0.0588	0.1133	0.5189	0.6039	[-0.1632, 0.2807]	-0.0493	0.0891	-0.553	0.5802	[-0.2238, 0.1253]
Δβ Manipulating Expectancy Upward	-0.265	0.1312	-2.0193	0.0435	[-0.5222, -0.0078]	-0.327	0.0707	-4.6253	0	[-0.4656, -0.1884]
Δβ Irrelevant Filler Task	0.0673	0.0959	0.7012	0.4832	[-0.1207, 0.2553]	-0.0929	0.0425	-2.1878	0.0287	[-0.1761, -0.0097]
Δβ Relevant Filler Task	-0.3895	0.1789	-2.1767	0.0295	[-0.7401, -0.0388]	-0.167	0.0754	-2.2146	0.0268	[-0.3148, -0.0192]
Δβ High Value X Decreasing Expectancy	-0.3537	0.2392	-1.4782	0.1394	[-0.8226, 0.1153]	-0.3149	0.1746	-1.8035	0.0713	[-0.6571, 0.0273]
Δβ High Value X Increasing Expectancy	0.2353	0.2392	0.984	0.3251	[-0.2334, 0.7041]	0.2893	0.1376	2.1022	0.0355	[0.0196, 0.5591]
Δβ High Value X Irrelevant Filler	-0.0228	0.148	-0.1543	0.8774	[-0.3129, 0.2672]	0.1249	0.0823	1.5174	0.1292	[-0.0364, 0.2861]
Δβ High Value X Relevant Filler	0.0981	0.2784	0.3525	0.7244	[-0.4475, 0.6437]	0.0692	0.138	0.5014	0.6161	[-0.2013, 0.3397]

Table 6

Results of a Regression Containing All Variables

	Random Effects					Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
Intercept	0.2905	0.1297	2.2397	0.0251	[0.0363, 0.5448]	0.3884	0.0661	5.8724	0	[0.2587, 0.518]
Indirect Primes	0.202	0.0901	2.2419	0.0250	[0.0254, 0.3787]	0.2174	0.0515	4.2189	0	[0.1164, 0.3183]
Manipulating Value Downward	-0.4469	0.1035	-4.3175	0	[-0.6497, -0.244]	-0.3825	0.0781	-4.8956	0	[-0.5357, -0.2294]
Manipulating Value Upward	0.1246	0.1009	1.2347	0.2170	[-0.0732, 0.3224]	0.2451	0.075	3.266	0.0011	[0.098, 0.3922]
High Value	-0.0138	0.0795	-0.1738	0.8620	[-0.1697, 0.1421]	-0.1776	0.044	-4.0316	0.0001	[-0.2639, -0.0912]
Manipulating Expectancy Downward	-0.0499	0.1337	-0.373	0.7091	[-0.3118, 0.2121]	-0.1294	0.1055	-1.2269	0.2198	[-0.3361, 0.0773]
Manipulating Expectancy Upward	-0.3601	0.137	-2.6274	0.0086	[-0.6287, -0.0915]	-0.5236	0.0842	-6.2178	0	[-0.6886, -0.3585]
Irrelevant Filler Task	0.0359	0.1091	0.3289	0.7422	[-0.178, 0.2498]	-0.1265	0.0559	-2.2615	0.0237	[-0.2361, -0.0169]
Relevant Filler Task	-0.6077	0.2313	-2.6266	0.0086	[-1.0611, -0.1542]	-0.3033	0.1219	-2.4885	0.0128	[-0.5423, -0.0644]
Year (scaled)	-0.0385	0.04	-0.9619	0.3361	[-0.1169, 0.0399]	-0.0558	0.0213	-2.6243	0.0087	[-0.0974, -0.0141]
Publication status: Unpublished	-0.1914	0.0823	-2.3251	0.0201	[-0.3527, -0.0301]	-0.1866	0.0414	-4.5112	0	[-0.2677, -0.1055]
Country: Non-US	0.0888	0.0735	1.2087	0.2268	[-0.0552, 0.2329]	0.1182	0.0387	3.0581	0.0022	[0.0424, 0.194]
Liminality: Supraliminal	-0.0261	0.101	-0.2588	0.7958	[-0.2242, 0.1719]	-0.0448	0.0539	-0.8312	0.4059	[-0.1505, 0.0609]
Proportion of Primes (scaled)	0.0531	0.0396	1.3429	0.1793	[-0.0244, 0.1307]	0.056	0.0204	2.7488	0.0060	[0.0161, 0.0959]
Control Type: Not Neutral Words	-0.1489	0.0883	-1.6875	0.0915	[-0.3219, 0.0241]	-0.0872	0.049	-1.7786	0.0753	[-0.1833, 0.0089]
Goal/Concept: Social	0.1388	0.1399	0.9917	0.3214	[-0.1355, 0.4131]	0.0147	0.0865	0.1701	0.8650	[-0.1548, 0.1842]
Goal/Concept: Consumption	0.0004	0.1888	0.0023	0.9982	[-0.3696, 0.3705]	0.1404	0.1075	1.3054	0.1917	[-0.0704, 0.3511]
Goal/Concept: Self Concept	0.279	0.1358	2.0536	0.0400	[0.0127, 0.5452]	0.1757	0.0831	2.113	0.0346	[0.0127, 0.3387]
Goal/Concept: Other	0.048	0.0941	0.5099	0.6102	[-0.1365, 0.2325]	-0.1073	0.0519	-2.0652	0.0389	[-0.2091, -0.0055]
DV: Reaction Time	0.1236	0.1675	0.7378	0.4606	[-0.2047, 0.4519]	0.0392	0.0813	0.4824	0.6296	[-0.1201, 0.1985]
DV: Usage of Time	-0.0983	0.0906	-1.085	0.2779	[-0.2759, 0.0793]	-0.1205	0.0463	-2.5994	0.0093	[-0.2113, -0.0296]
DV: Usage of Money	-0.2012	0.1875	-1.0729	0.2833	[-0.5686, 0.1663]	-0.0502	0.1105	-0.4539	0.6499	[-0.2668, 0.1665]
DV: Consumption	0.4286	0.1802	2.3787	0.0174	[0.0754, 0.7817]	0.295	0.0996	2.961	0.0031	[0.0997, 0.4902]
DV: Product Choice	0.7401	0.3677	2.0126	0.0442	[0.0193, 1.4609]	0.4456	0.2076	2.1464	0.0318	[0.0387, 0.8525]
DV: Other	-0.0516	0.0869	-0.5934	0.5529	[-0.2219, 0.1187]	0.0348	0.0483	0.72	0.4715	[-0.0599, 0.1294]
Presence of Funneled Debriefing	0.0677	0.0715	0.9475	0.3434	[-0.0724, 0.2078]	0.0543	0.037	1.4666	0.1425	[-0.0183, 0.1269]
High Task Flexibility	0.1699	0.076	2.234	0.0255	[0.0208, 0.3189]	0.1715	0.0408	4.1995	0	[0.0915, 0.2516]

Table 6 Continued

	Random Effects					Fixed Effects				
	(Δ)β	SE	z	p	95% CI	(Δ)β	SE	z	p	95% CI
High Value X Decreasing Expectancy	-0.3179	0.2714	-1.1712	0.2415	[-0.8499, 0.2141]	-0.117	0.1865	-0.6273	0.5304	[-0.4826, 0.2486]
High Value X Increasing Expectancy	0.0995	0.3392	0.2933	0.7693	[-0.5653, 0.7642]	0.3831	0.2204	1.7381	0.0822	[-0.0489, 0.815]
High Value X Irrelevant Filler	0.076	0.1631	0.4661	0.6412	[-0.2437, 0.3958]	0.2116	0.0977	2.1656	0.0303	[0.0201, 0.403]
High Value X Relevant Filler	0.3414	0.3161	1.08	0.2802	[-0.2782, 0.961]	0.2153	0.1724	1.2492	0.2116	[-0.1225, 0.5532]

Figure 1

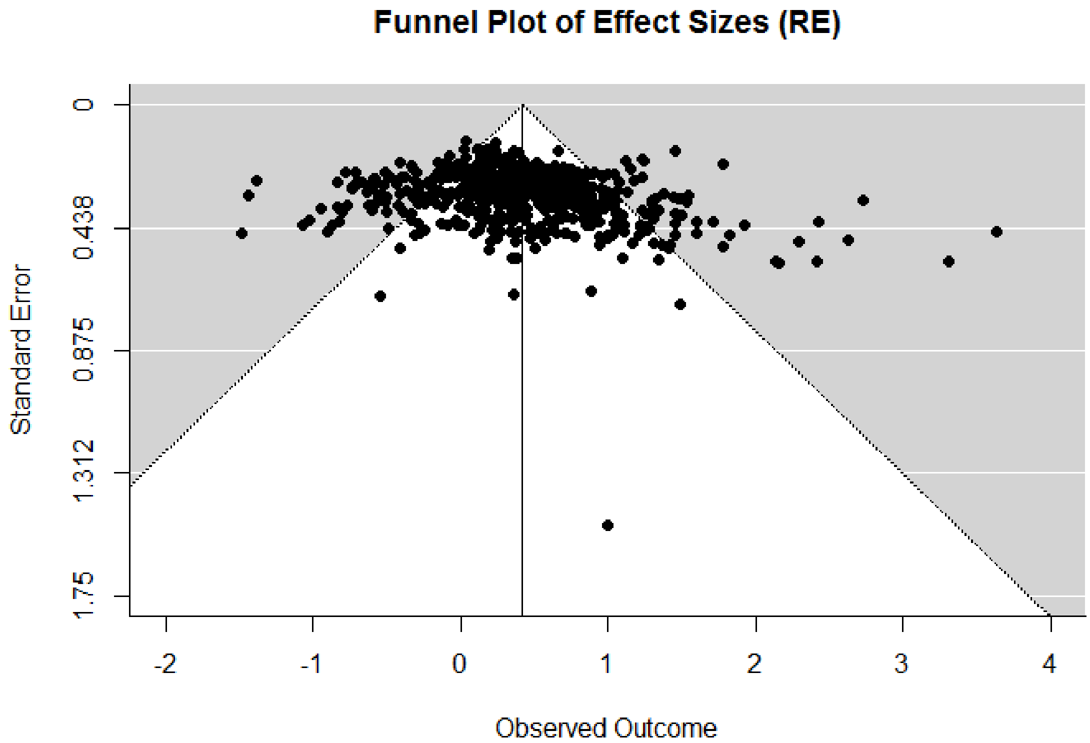


Figure 2

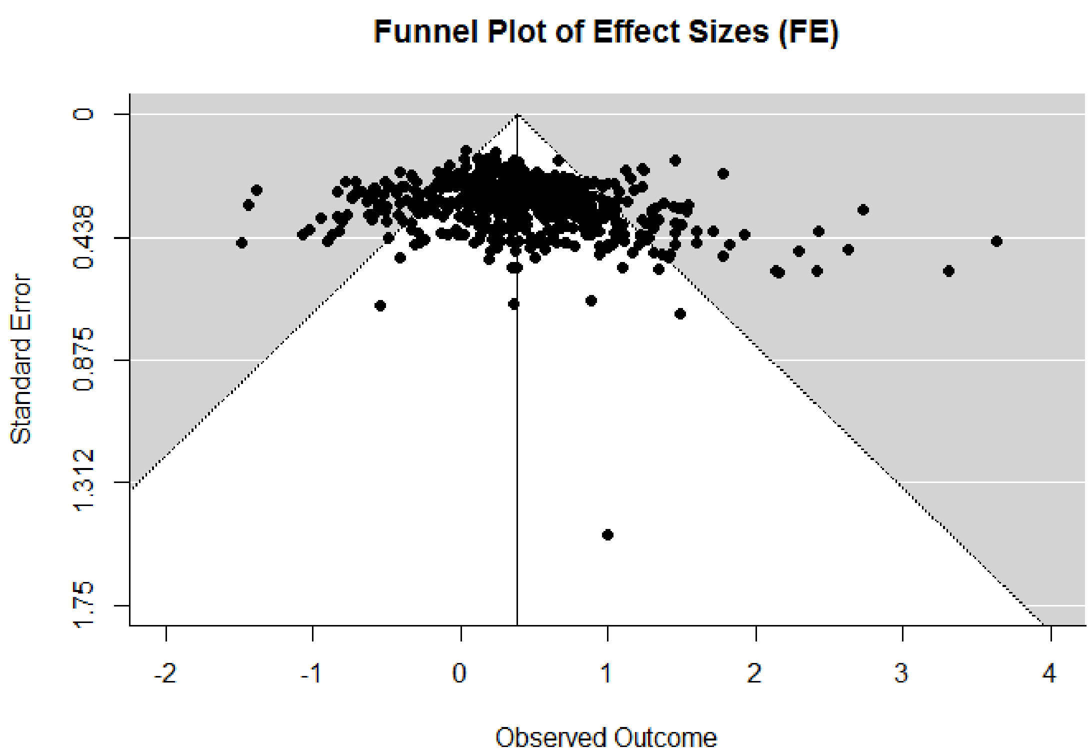


Figure 3

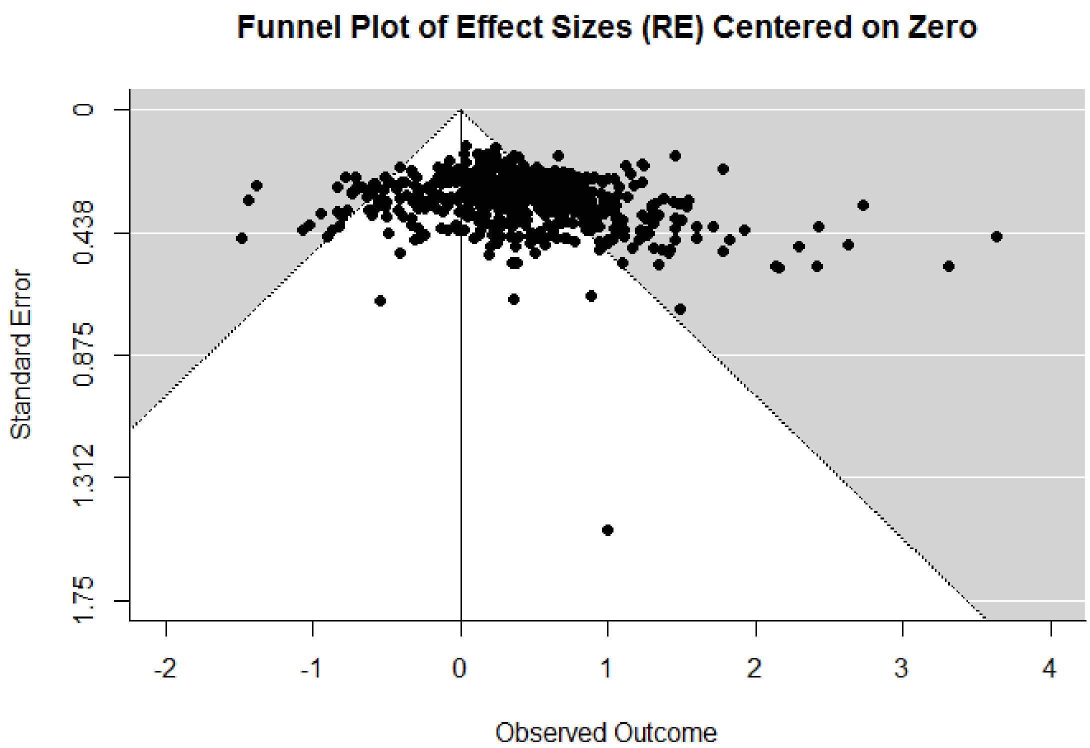


Figure 4

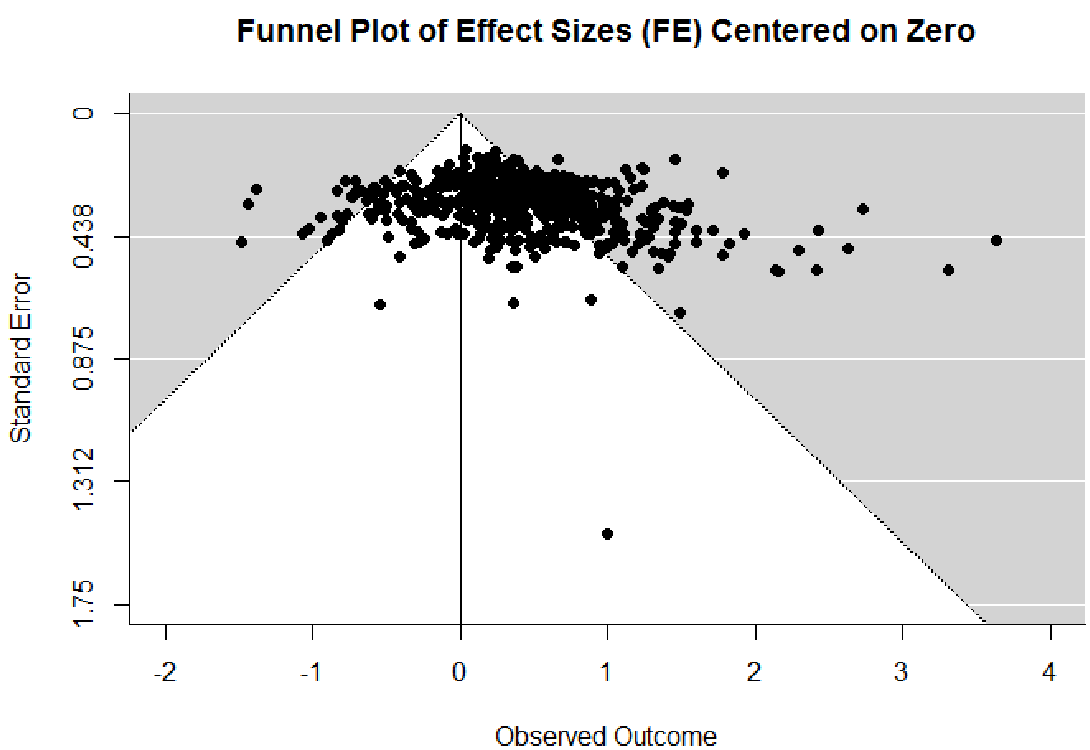


Figure 5

Vevea and Hedges (1995) Density Plot of All Data

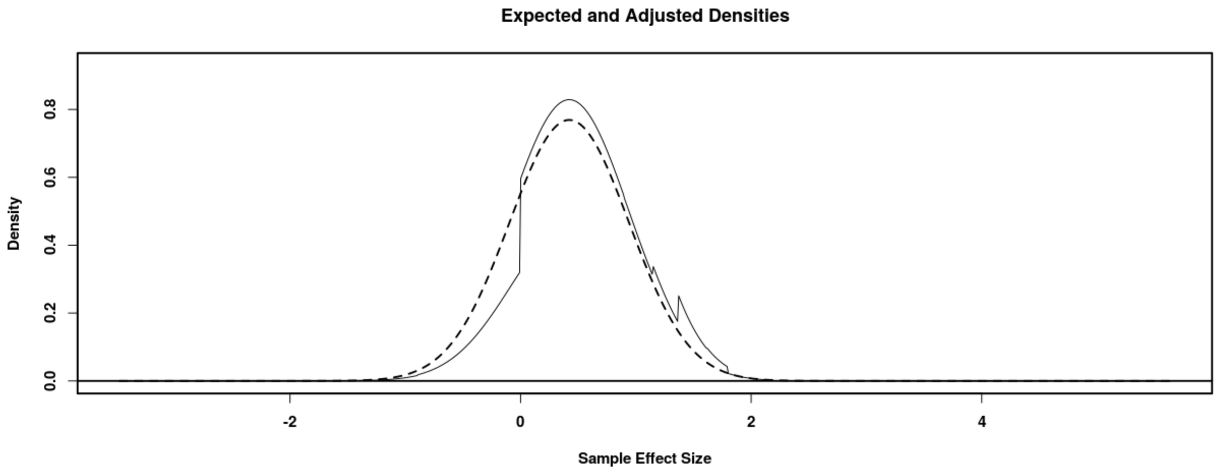


Figure 6

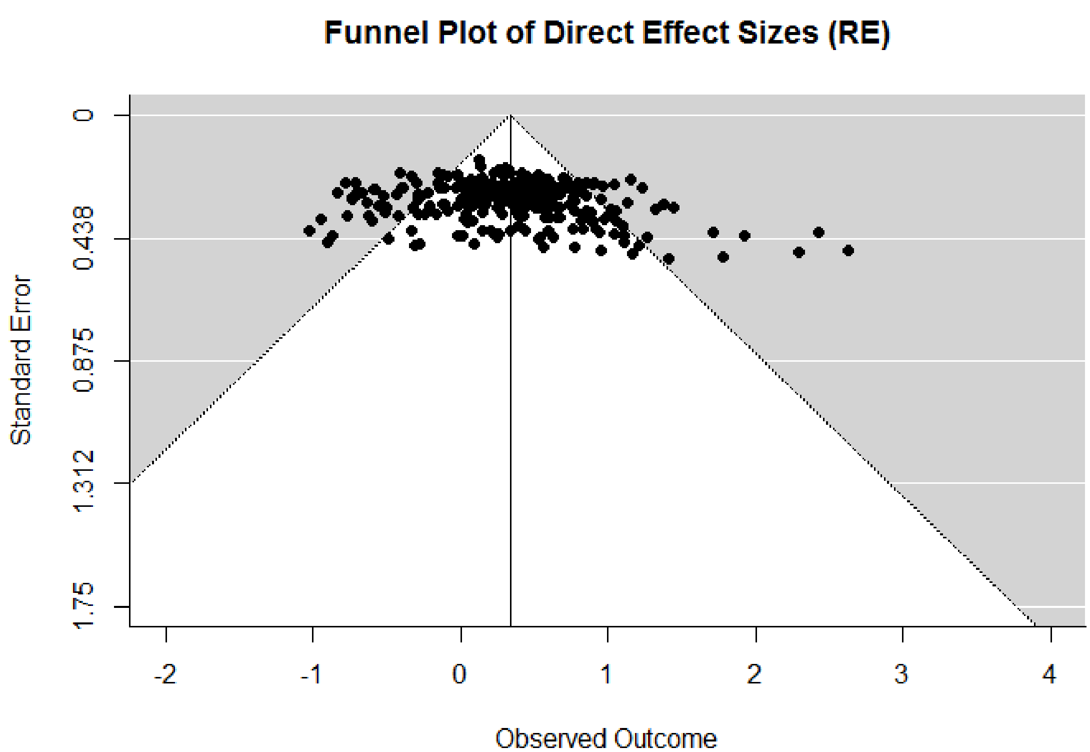


Figure 7

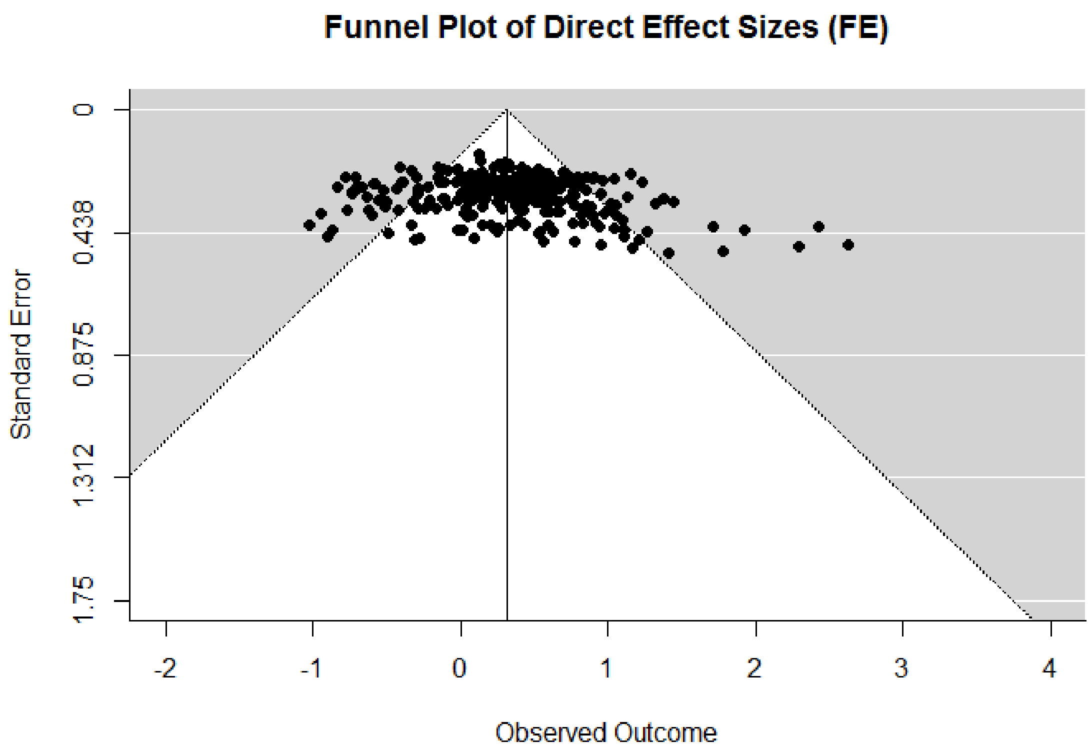


Figure 8

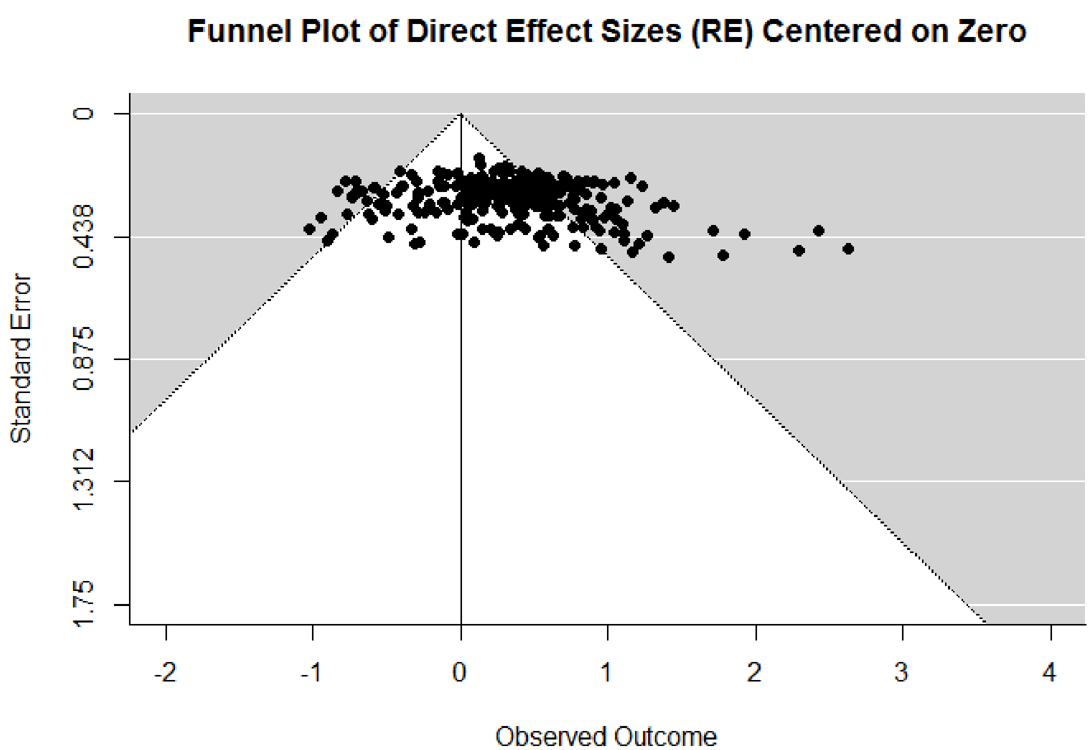


Figure 9

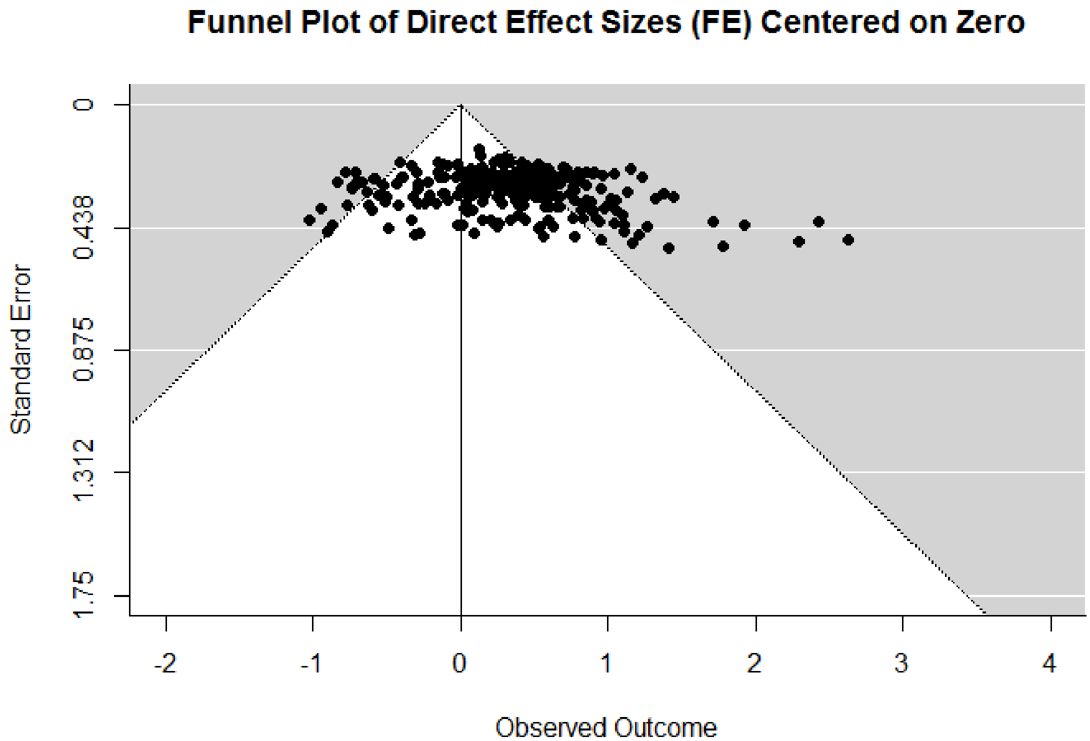


Figure 10

Vevea and Hedges (1995) Density Plot of Directly Linked Primes Data

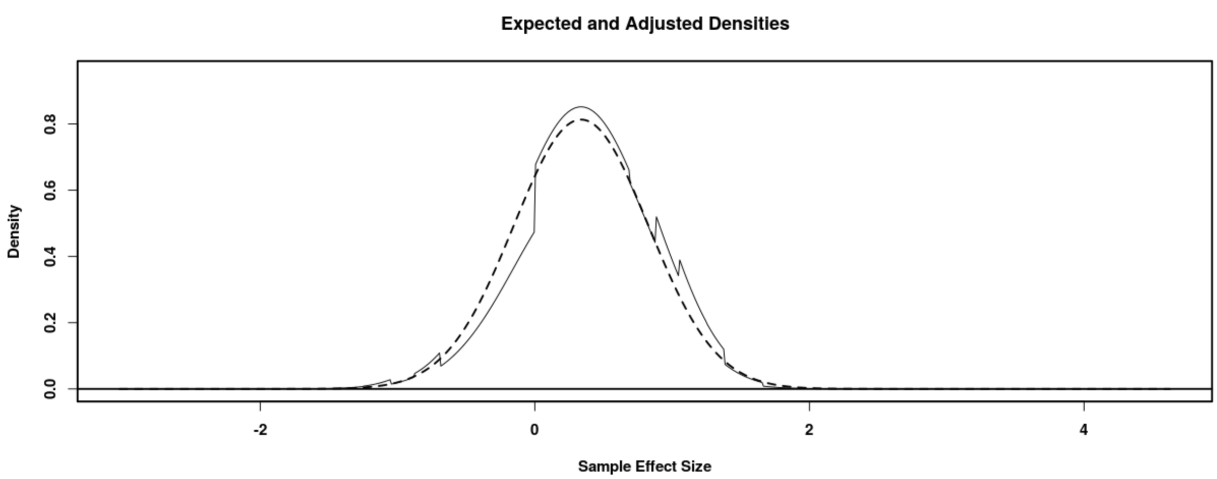


Figure 11

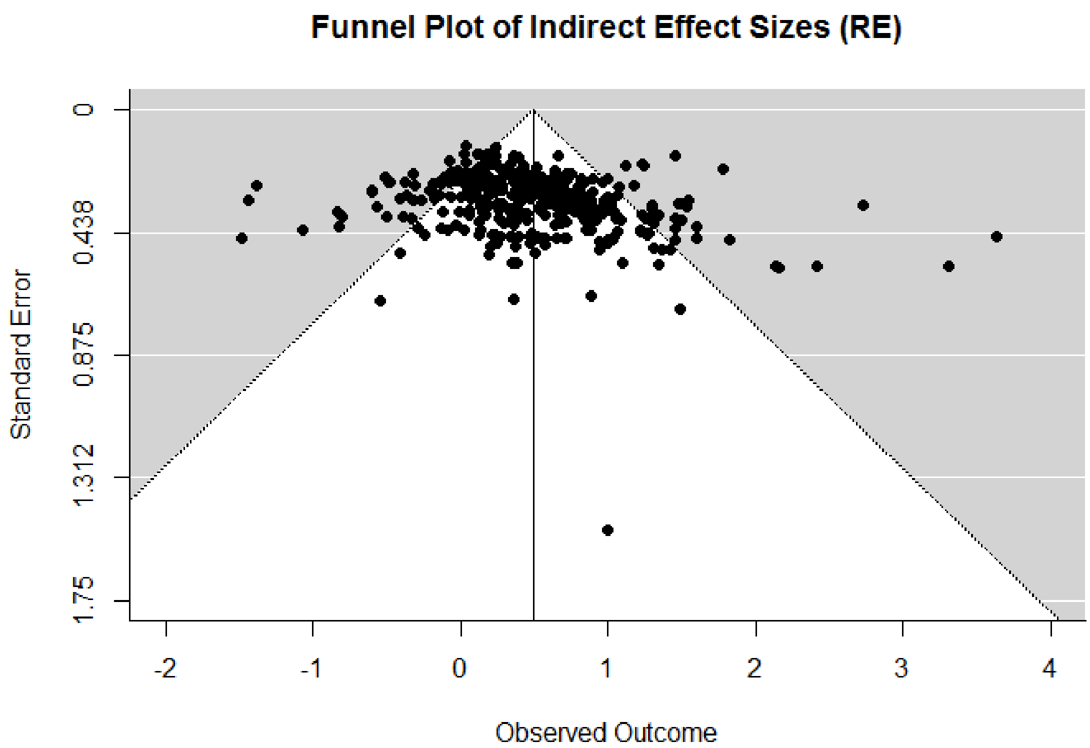


Figure 12

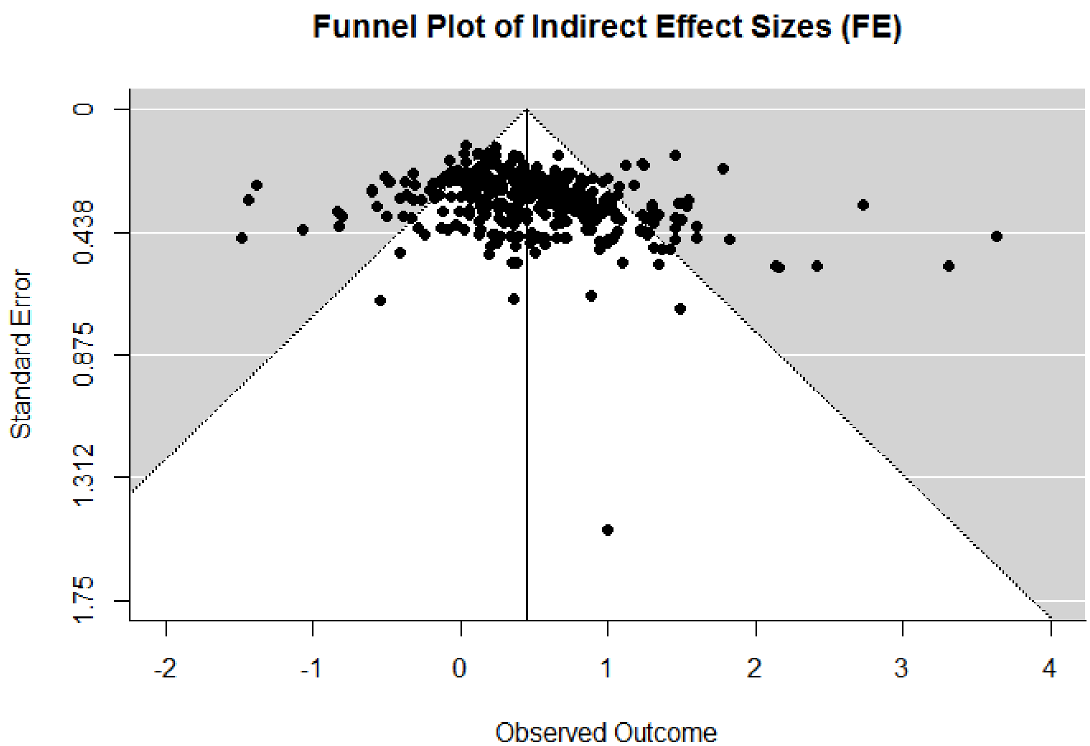


Figure 13

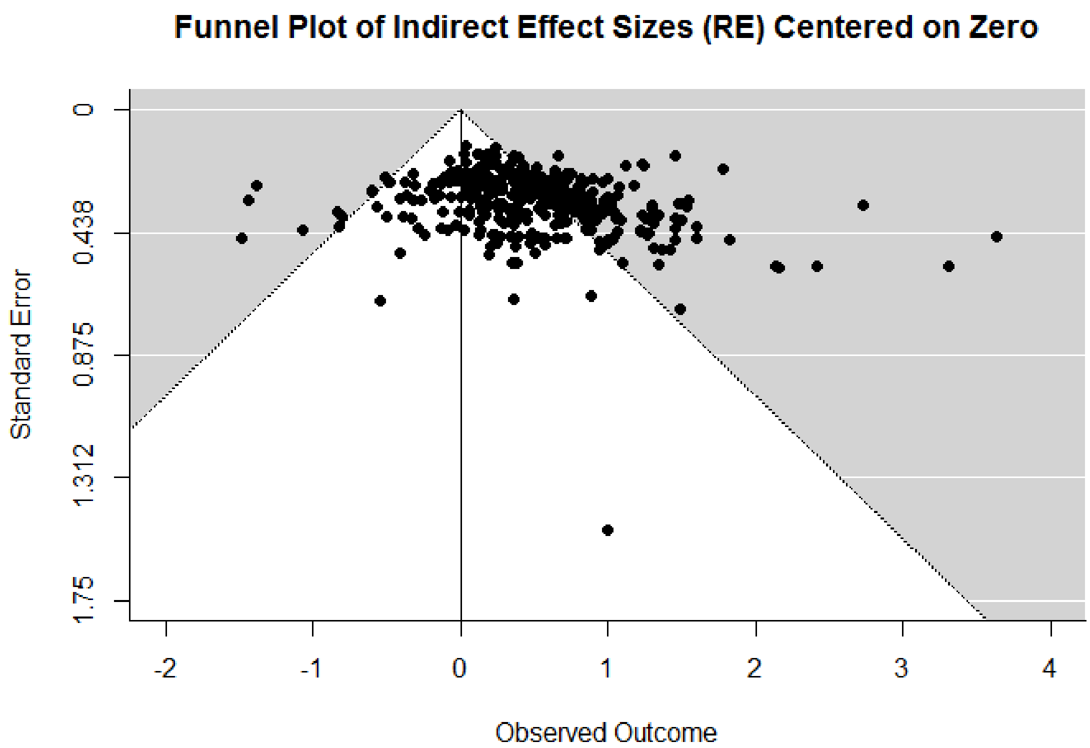


Figure 14

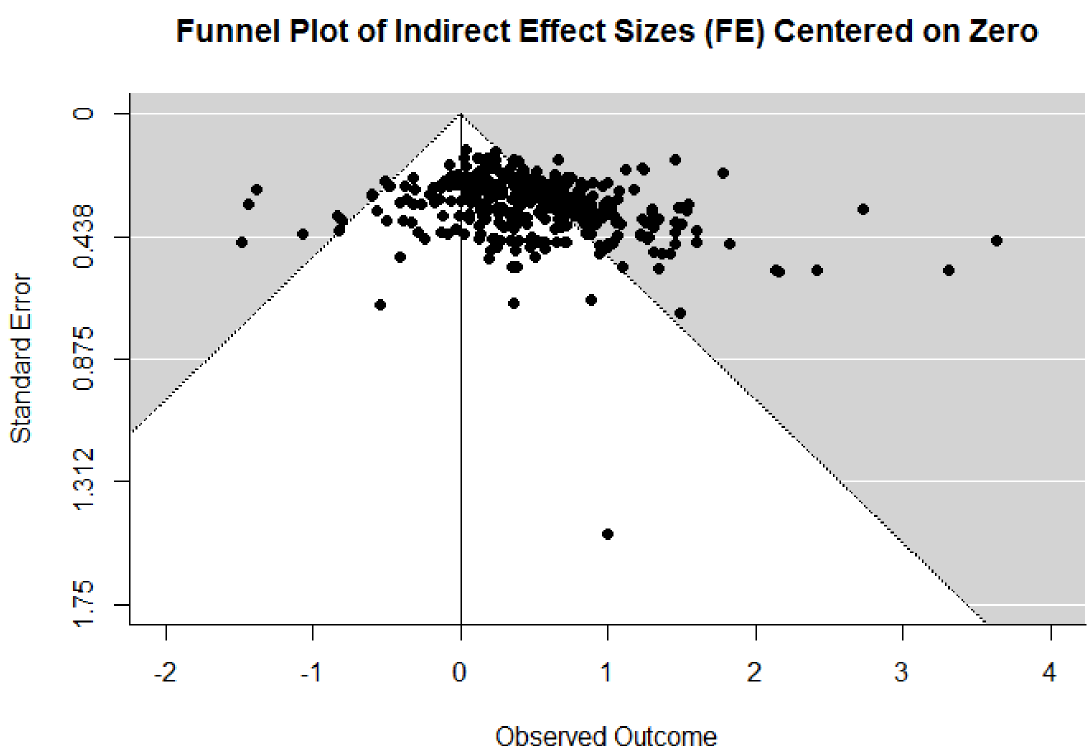
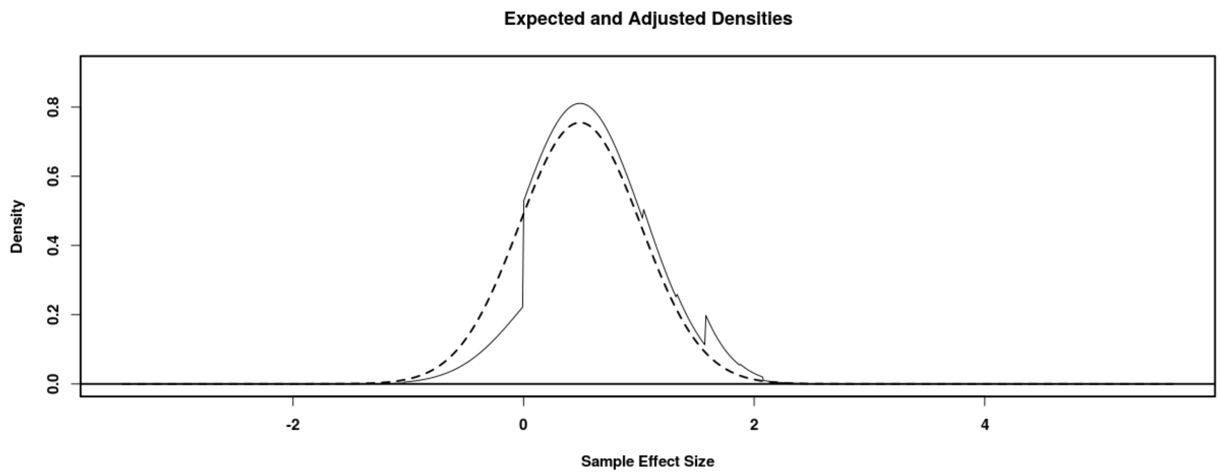


Figure 15

Vevea and Hedges (1995) Density Plot of Indirectly Linked Primes Data



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