Psychological Bulletin
2016, Vol. 142, No. 5, 472–497

0033-2909/16/$12.00 http://dx.doi.org/10.1037/bul0000030

From Primed Concepts to Action: A Meta-Analysis of the Behavioral Effects of Incidentally Presented Words

Evan Weingarten, Qijia Chen, Maxwell McAdams, and Jessica Yi
University of Pennsylvania

Justin Hepler
Facebook

Dolores Albarracín
University of Illinois at Urbana-Champaign

A meta-analysis assessed the behavioral impact of and psychological processes associated with presenting words connected to an action or a goal representation. The average and distribution of 352 effect sizes (analyzed using fixed-effects and random-effects models) was obtained from 133 studies (84 reports) in which word primes were incidentally presented to participants, with a nonopposite control group, before measuring a behavioral dependent variable. Findings revealed a small behavioral priming effect (d_{FE} = 0.332, d_{RE} = 0.352), which was robust across methodological procedures and only minimally biased by the publication of positive (vs. negative) results. Theory testing analyses indicated that more valued behavior or goal concepts (e.g., associated with important outcomes or values) were associated with stronger priming effects than were less valued behaviors. Furthermore, there was some evidence of persistence of goal effects over time. These results support the notion that goal activation contributes over and above perception-behavior in explaining priming effects. In summary, theorizing about the role of value and satisfaction in goal activation pointed to stronger effects of a behavior or goal concept on overt action. There was no evidence that expectancy (ease of achieving the goal) moderated priming effects.

Keywords: priming, automaticity, goal, motivation, meta-analysis

Supplemental materials: http://dx.doi.org/10.1037/bul0000030.supp

> The worst is yet to come for priming . . . over the next two or three years you're going to see an avalanche of failed replications published.
>
> —(Bartlett, 2013, referencing David Shanks)

In 1996, Bargh, Chen, and Burrows asked 34 undergraduates of New York University (NYU) to complete a brief research task and then request a second task from the researcher in a nearby room. The first task comprised scrambled sentences presented to participants as one of the following three lists of words: *they her respect see usually*, *they her bother see usually*, or *they her exercising see usually*. After unscrambling the sentences, participants went to look for the experimenter who was busy chatting with a friend.

It was not surprising that the NYU students were able to correctly form 30 sentences like *they usually respect her*, *they usually bother her*, or *they usually see her*. The surprise was that the content of some of the unscrambled sentences (containing politeness, rudeness, or neutral themes varying among participants) influenced the time students took to interrupt and request the experimenter to proceed with the second study. College students who had unscrambled sentences about rudeness were more likely to interrupt the conversation than were those who had unscrambled sentences about politeness or neutral topics (Bargh et al., 1996). In light of this evidence, Bargh and colleagues (1996) argued that priming, the activation of various mental constructs unbeknownst to individuals via perception of external stimuli (Bargh & Chartrand, 2000; Bargh et al., 2010), was not limited in its effects to social perception (Bargh, 1994; Higgins, Rholes, & Jones, 1977) but instead reached the more substantial domain of action. Since that time and for nearly two decades, social psychologists and scholars in many other fields have attempted to understand the perceptual and motivational principles responsible for the intriguing observations in Bargh et al.'s (1996) seminal study. For example, Bargh and his colleagues (2001) tasked students with solving a series of word search puzzles that either contained synonyms of achievement (e.g., *win*, *achieve*) or control words (e.g., *building*, *staple*). Students who initially found achievement

Editor's Note. Stephen P. Hinshaw served as the action editor for this article. —DA

This article was published Online First December 21, 2015.

Evan Weingarten, Marketing Department, University of Pennsylvania; Qijia Chen, Maxwell McAdams, Jessica Yi, Annenberg School for Communication, University of Pennsylvania; Justin Hepler, Facebook; Dolores Albarracín, Psychology Department, University of Illinois at Urbana-Champaign.

We thank John Bargh, Wes Hutchinson, Blair Johnson, and Uri Simonsohn for their helpful comments.

Correspondence concerning this article should be addressed to Evan Weingarten, Marketing Department, University of Pennsylvania, Philadelphia, PA 19103. E-mail: ewein@wharton.upenn.edu

words found more words on subsequent word search puzzles than did those who initially found neutral words. Students had been reminded of achievement goals, leading to improved intellectual performance.

Despite the excitement surrounding effects of primes on performance, the Zeitgeist changed as a result of failures to directly replicate the phenomenon (Doyen et al., 2012; Harris et al., 2013; Klein et al., 2014; Shanks et al., 2013). Recent years have seen a dramatic shift toward the more somber intellectual climate apparent in the quotes below, the former of which is taken from a leaked e-mail.

> As all of you know, of course, questions have been raised about the robustness of priming results. The storm of doubts is fed by several sources, including the recent exposure of fraudulent researchers, general concerns with replicability that affect many disciplines, multiple reported failures to replicate salient results in the priming literature, and the growing belief in the existence of a pervasive file drawer problem that undermines two methodological pillars of your field: the preference for conceptual over literal replication and the use of meta-analysis. . . . For all these reasons, right or wrong, your field is now the poster child for doubts about the integrity of psychological research. (Kahneman, 2012)

> Now, goal-priming experiments are coming under scrutiny—and in the process, revealing a problem at the heart of psychological research itself. (Satel, 2013, p. SR8)

Naturally, the researchers spearheading priming research raised concerns about both the nature and tone of these public statements. Some of the responses made reference to prior successful replications of an effect (Cesario, Plaks, & Higgins, 2006; Hull, Slone, Meteyer, & Matthews, 2002) in the face of other failures to replicate (Doyen et al., 2012), and called for meta-analytic approaches rather than singular replications (Bargh, 2012). Other responses questioned the practices leading to the publication of some replications with minimal or no peer review (e.g., Schnall, 2014a), and yet others pointed to inaccuracies in the methodology and inferences present in the replication failures (e.g., Schnall, 2014b; Schwarz & Strack, 2014). Representative quotes include:

> In science the way to answer questions about replicability of effects is through statistical techniques such as meta-analysis, as well as qualitative reviews of the literature. (Bargh, 2012)

> For the replication special issue all replication authors were deprived of this mechanism of quality control: There was no peer-review of the manuscript, not by authors of the original work, nor by anybody else . . . To make any meaningful scientific contribution the quality standards for replications need to be at least as high as for the original findings. (Schnall, 2014a)

> In the interest of a "direct" replication, the authors chose to go with the historical German values, resulting in a replication that can be described as "technically direct" while missing the goal of realizing psychological conditions that are comparable to the original study. . . . In general, meaningful replications need to realize the psychological conditions of the original study. (Schwarz & Strack, 2014, pp. 305–306)

Can a well-executed meta-analysis of the behavioral effects of incidentally presented concepts transform this controversy and inform the many disciplines concerned with this phenomenon? We think so, particularly through the use of sophisticated methods to detect and mitigate systematic elimination of null and negative findings (a form of publication bias often referred to as the *file drawer* problem; see Cooper, 2010; Cooper & Hedges, 1994). With the objective of gathering the most comprehensive evidence to date, we obtained published and unpublished research on the performance effects of priming concepts compared with a control condition. We calculated Cohen's *g* by subtracting the mean of the control group from the mean of the priming group and dividing that by the pooled *SD*, or used analogous methods for categorical dependent measures.

Our meta-analysis synthesized 352 published and unpublished effect sizes, obtained from research conducted in the United States and internationally. Priming methods included various forms of supraliminal and subliminal presentation of words clearly linked to a behavior or goal (desired end-state) concept (e.g., *go*, *affiliate*). The most commonly primed concepts were presented supraliminally (e.g., via scrambled sentences and word puzzles) and pertained to achievement, although such social behaviors as helping were also highly prevalent. Among many others, performance measures included a score for test performance (number of solved problems), time spent on a task, and observer ratings of overt behavior (e.g., rated anxious behavior in Geers et al., 2005). Nonperformance measures such as concept accessibility, as measures of intentions, attitudes, beliefs, and knowledge, were deemed ineligible in an attempt to model effects on *actual* cognitive and motor performance.

Another important objective of the present meta-analysis was to test the theoretical principles of goal-related priming. We sought to examine how value-related manipulations and satisfaction opportunities after priming affected effect sizes. Specifically, as scholars have theorized that goal pursuit increases with value (i.e., an evaluation based on the outcomes or abstract entities associated with a behavior or end-state; Forster, Liberman, & Friedman, 2007), we tested moderation by conditions coded as high versus low value. Moreover, as the strength of goals presumably remains or increases over time until fulfillment (Atkinson & Birch, 1970; Bargh et al., 2001; Fishbach & Ferguson, 2007; Förster et al., 2005, 2007; Zeigarnik, 1927), we examined moderation by relevant versus irrelevant filler tasks that, respectively, can and cannot satisfy the goal. Whereas nongoal concept priming should decay, goal mediated effects should be sustained or even increase as time goes by when the task following priming is irrelevant and thus unlikely to satisfy the evoked goal (DeCoster & Claypool, 2004; Higgins, Bargh, & Lombardi, 1985; Srull & Wyer, 1980). More important, although past primary research considered value and satisfaction separately, we were able to estimate these effects in combination as a way of precisely delineating the boundary conditions for strong behavioral priming effects.

Our meta-analysis is distinct from several other meta-analyses of priming in the field of psychology (Cameron, Brown-Iannuzzi, & Payne, 2012; DeCoster & Claypool, 2004; Van den Bussche, Van den Noortgate, & Reynvoet, 2009). Cameron, Brown-Iannuzzi, and Payne (2012) concentrated on precise but contrived computerized sequential priming tasks (e.g., AMP, Evaluative Priming, Lexical Decision Tasks, Eriksen flanker, and shooter tasks) and averaged behavior and intention effects. Furthermore, distinct from our synthesis, DeCoster and Claypool (2004) studied the influence of stereotype and trait primes as evaluative primes without behavioral endpoints. Finally, Van den Bussche, Van den Noortgate, and Reynvoet (2009) focused only on the effects of masked semantic priming (i.e., subliminal, visual semantic primes from semantic categorization, lexical decision tasks, or naming tasks) on response times during categorization or naming tasks of interest as measures of semantic interpretation rather than behavior per se. Therefore, our meta-analysis is not only timely but also novel.

The Origins of Priming Research in Social Psychology

The conception of priming, that environmental stimuli may affect subsequent responses by activating mental constructs without conscious realization, concerned cognitive psychological questions about semantic knowledge before its application to social psychology (Bargh & Chartrand, 2000). The initial presentation of a set of words, researchers proposed and found, facilitated responses to semantically related words (Bargh & Chartrand, 2000; Fazio, 2001; Forbach, Stanners, & Hochhaus, 1974; Meyer & Schvaneveldt, 1971; Neely, 1977) and increased the likelihood of generating those words later in a study (Bargh & Chartrand, 2000; Segal & Cofer, 1960; Storms, 1958). Cognitive psychology studies on carryover effects similar to Segal and Cofer (1960) and Storms (1958) further influenced multiple research streams about the influence of experimentally presented stimuli on social judgments (Bargh & Chartrand, 2000; Bargh et al., 2010; Higgins, Rholes, & Jones, 1977; Srull & Wyer, 1979, 1980) and related attitudes toward a stimulus (Collins & Loftus, 1975; Fazio, 2001; Fazio et al., 1986; Krosnick, Judd, & Wittinbrink, 2005), supposedly outside of awareness, intentionality, and/or control (Bargh, 1990; Bargh et al., 1996; Bargh & Gollwitzer, 1994; Logan, 1989).

Early research on the social effects of semantic priming investigated whether priming a trait category could influence subsequent impressions of a person (Bargh & Pietromonaco, 1982; DeCoster & Claypool, 2004; Fazio, 2001; Higgins et al., 1977; Srull & Wyer, 1979, 1980). In the classic demonstration inspired from Huttenlocher and Higgins (1971, 1972); Higgins, Rholes, and Jones (1977) asked participants to complete two ostensibly unrelated studies, the first of which involved memorizing a handful of words denoting positive or negative traits while identifying colors on a screen. After this task, participants read about and formed an impression of a hypothetical man named *Donald*, who was described as actively seeking excitement. Demonstrating how incidental word presentation could affect later judgments, exposure to positive words yielded more positive evaluations of Donald than exposure to negative words (Higgins et al., 1977). Several other scholars would later extend these findings to show automatic influences on social judgments after increasing the accessibility (e.g., the celerity with which a concept can be retrieved from memory; Higgins et al., 1977) of a number of concepts linked to the mental representations of a category (Bargh, Lombardi, & Higgins, 1988; Bargh & Pietromonaco, 1982; Higgins, 1989; Higgins, Bargh, & Lombardi, 1985; Kunda, 1990; Srull & Wyer, 1979, 1980). These early theories of the operation of priming on social perception established the principles of incidental influences on social judgments (Bargh & Pietromonaco, 1982; Higgins, 1989; Higgins et al., 1985; Higgins & King, 1981; Higgins et al., 1977; Srull & Wyer, 1979; Srull & Wyer, 1980;).

More critical to our analysis, in the late 1980s and early 1990s, the burgeoning application of priming to social judgments culminated in extending the ideomotor principle into the postulation of priming effects based on the *perception-behavior* link. Bargh and his colleagues (Bargh, 1990; Bargh et al., 1996; Bargh & Gollwitzer, 1994) argued that stimuli available in the environment could be relevant to and therefore activate chronic behavior concepts and ultimately behavioral outcomes linked to those concepts. Associations between an environmental stimulus and these mental representations supposedly strengthen over time and increase the effects of merely perceiving these stimuli on subsequent actions (Bargh et al., 1996; Bargh & Gollwitzer, 1994; Bargh & Morsella, 2009; Chartrand & Bargh, 1999; for related issues, see also Fazio et al., 1986; Higgins et al., 1977; Wegner, 1994).

Evidence supporting the perception-behavior link followed the ideomotor model defined by James (1890; also see Carpenter, 1852), albeit without the conscious (ideo) component from James (1890). First, early research accumulated to show how *exposure* to aggressive behaviors could trigger overt aggression (e.g., the media; Berkowitz, 1984; Carver, Ganellen, Froming, & Chambers, 1983) and how activating the schema of helping could promote altruism (Fong, 1984). More recent studies also revealed that mimicry, walking speed, and problem solving could follow changes in category accessibility (Bargh et al., 1996; Chartrand & Bargh, 1999; Higgins & Chaires, 1980; Shah & Kruglanski, 2003). In addition, scholars identified important preconditions for the effects, such as the existence of a prior association between the behavior or goal concept and the behavior (Bargh, 2002; Bargh, Gollwitzer, & Oettingen, 2010; for the general principle, see Lewin, 1951). Priming the concept of elderly only decreases memory for participants with frequent exposure to elderly individuals (Dijksterhuis, Aarts, Bargh, & van Knippenberg, 2000), and priming specific drinks or brands of drinks only influences drinking among thirsty consumers (Strahan et al., 2002; Veltkamp, Custers, & Aarts, 2011). Recent theorizing has also suggested that only primes that can be confused with the actor's thoughts have the potential to exert congruent effects on behavioral responses (Loersch, 2009; Loersch & Payne, 2011), and that invoked goals are an important explanatory device for the behavioral effects of priming (Bargh & Gollwitzer, 1994; Bargh et al., 2001).

Goal Mediated Effects of Primes on Behavior

In addition to the perception-behavior link, research on priming has emphasized that longstanding goals and motivations can be activated by stimuli in the environment without conscious awareness, intentionality, or control (Bargh et al., 2001; Bargh & Morsella, 2009; Bargh et al., 2012; Chartrand et al., 2008; Dijksterhuis & Aarts, 2010; Hassin, 2013). Stimuli associated with valued actions that have become routinized may elicit behavior following general motivational principles that researchers consider evidence of goal mediation (Aarts, Custers, & Veltkamp, 2008; Marien et al., 2012; Papies & Aarts, 2013; see also Masicampo & Baumeister, 2011).[1]

By definition, stimuli linked to valued actions or states have been expected to elicit greater goal activation than stimuli linked to actions or states of lesser value (Förster et al., 2005, 2007). Stronger effects have been shown in studies that introduce incentives (e.g., Förster et al., 2005; Marien et al., 2012), distinguish levels of the chronic value of a particular action (e.g., Fitzsimons, Chartrand, & Fitzsimons, 2008; Marien et al., 2012; Papies & Hamstra, 2010), associate primes with positive affect (e.g., Aarts et al., 2007; Hart & Gable, 2013), and induce high commitment to an action (e.g., Maltarich, 2009). For example, Levesque and Pelletier (2003) found that individuals who were high in autonomous motivation performed more tasks involving intrinsic motivation (e.g., working on puzzles during a "free-choice period") than individuals who were low in this trait. Clearly, valuing a concept can lead to more associations because people think more about that concept, but there is also the expectation that a valued concept is linked to goal representations that are ideally positioned to initiate action.

The expectancy or perceived probability of achieving a goal is another important consideration, although the direction of its association with goal pursuit has been controversial. Some scholars have proposed that increasing the expectancy or perceived probability of achieving a goal bolsters goal strength (Förster et al., 2005, 2007), whereas others have argued that difficult goals strengthen goal pursuit effects (Heath, Larrick, & Wu, 1999; Locke & Latham, 1990; Stajkovic, Locke, & Blair, 2006). According to Förster et al. (2005, 2007), expectancy should interact with goal value to intensify pursuit such that high-value concepts may only motivate action when people perceive a high probability of success (Forster et al., 2007). According to Locke and Latham (1990), however, higher task difficulty can intensify effort and therefore improve performance, which predicts a direct effect of expectancy on behavior priming (Locke & Latham, 2002). The discrepancy between these theories is curious: one suggests that attainable end-states hinder motivation and the other that attainable end-states enhance it. Although opposite predictions without much clarity on their boundary conditions can ultimately lead to a null effect, we still set to examine the effect of expectancy alone and in interaction with value.

As the behavior resulting from primed goals is supposed to involve persistence over obstacles (Bargh et al., 2001, 2010; Custers et al., 2008; Gollwitzer & Wicklund, 1985), a goal account includes the prediction that relative to perception-behavior effects, goal-mediated priming shows relatively less decay or even strengthens over time (e.g., Bargh et al., 2001; Crusius & Mussweiler, 2012; Custers et al., 2008). Thus, a major difference between goal-mediated priming and direct perception-behavior effects concerns the role of a temporal delay between the priming task and the dependent measure of behavior (Chartrand et al., 2008; Fishbach & Ferguson, 2007; Forster et al., 2007). The length of temporal delays used in past research has varied, including three (Chartrand & Bargh, 1996), five (e.g., Albarracin & Hart, 2011), and eight (Fitzsimons et al., 2008) minutes between the prime and the behavior measure. Whereas social perception priming effects should decay with a temporal delay (DeCoster & Claypool, 2004; Higgins, 1989; Higgins et al., 1985), primed goals can increase or persist until satiation (Atkinson & Birch, 1970; Bargh et al., 2001, 2010; Chartrand et al., 2008; Marsh, Hicks, & Bink, 1998). For example, in a series of consumer studies, Chartrand et al. (2008) used a manipulation of either 3 or 8 min between thrift versus prestige priming and a choice task, uncovering stronger effects after more time (8 min) had passed (e.g., Chartrand et al., 2008; Fitzsimons, Chartrand, & Fitzsimons, 2008; for use of this notion to rule out goal mediation, see Chartrand & Bargh, 1996). Furthermore, the effect of delay is contingent on having goal satisfaction opportunities during the time interval. For example, irrelevant filler tasks introduced during the post-priming delay (e.g., writing street names after impression-formation goals; Chartrand & Bargh, 1996) should preclude goal fulfillment, whereas relevant filler tasks (e.g., action vs. inaction task after priming an action or inaction goal; Albarracin et al., 2008) should cause goal fulfillment and weaken priming effects. Relevant tasks that provide satiation opportunities may lead to inhibition (i.e., based on that the activity should reduce the action tendency galvanized by the prime; Atkinson & Birch, 1970; Bargh et al., 2010), showing either elimination of the effect or a rebound, Zeigarnik-type effect (Heckhausen, 1991; Liberman & Forster, 2000; Osviankina, 1928; Zeigarnik, 1927). More important, however, these effects of satisfaction opportunity should be more apparent when behaviors are highly valued or have the needed level of expectancy to initiate a motivational process.

Of course, direct perception-behavior effects may co-occur with goal activation, or one process may exist to the exclusion of the other. First, if behavioral priming is because of mere perception-behavior effects, the effects of the primes should not be contingent on value, expectancy, or delay/satisfaction opportunities. Second, if behavioral priming *requires* goal-mediated processes, no priming effects should emerge when conditions are not conducive to goal activation. For example, conditions associated with lower value or expectancy with respect to a behavior or state may show no behavioral priming, particularly when there is insufficient time for motivational tension to escalate (Chartrand et al., 2008). Third, if both processes occur, then behavioral priming effects should be stronger in conditions leading to goal mediation but would still be apparent in conditions associated with lower value or expectancy with respect to a behavior or state, as well as in the absence of intervening time between the prime and the behavioral measure. For example, decay in priming effects in the absence of a delay would be indicative of perception-behavior effects rather than goal mediation, suggesting that perception-behavior exists in the absence of goal mediation.

The Present Meta-Analysis

The current meta-analysis synthesized research on behavioral priming effects resulting from the incidental presentation of words to estimate an overall effect and determine the plausibility of a goal-mediation theoretical account. We restricted word primes to those that directly related to a concept (e.g., rudeness-related words and rudeness; Bargh et al., 1996, Study 1) instead of those with metaphorical meaning (e.g., elderly stereotype, money; Bargh et al., 1996, Studies 2a–2b; Vohs, Mead, & Goode, 2006). We calculated and pooled standardized mean differences, Hedges' *d*s

[1] A minority of the studies that argue goal mediation directly test the motivational properties of their effects. Accruing over studies, however, allows meta-analysis to provide these tests.

(Hedges & Olkin, 1985) to compare the effects of words related to a concept with a nonopposite control set of words. Our survey of the literature and authors within that literature generated 352 calculable *d*s stemming from 133 studies and 84 papers. Our test of theory involved analyzing the effects of value, expectancy, and delay with satisfaction opportunity after priming, as well as interactions between value and expectancy, and of value or expectancy with satisfaction opportunity after priming.

We also aimed to explore the variation in methodologies used by various studies within the priming literature. Priming experiments have varied in prime liminality, dosage, and prime abstractness or generality (Albarracin et al., 2008; Bargh et al., 2001; Bargh et al., 2010; Fishbach & Ferguson, 2007; Kruglanski et al., 2002; Uysal, 2010; Vallecher & Wegner, 1987, 1989). Research has also been quite diverse with respect to the use of awareness checks (e.g., funneled debriefing or other awareness checks; Bargh & Chartrand, 2000) and types of behavioral measures. Common supraliminal priming methods comprise word searches (e.g., Bargh et al., 2001), scrambled sentence tasks (Bargh et al., 1996; Srull & Wyer, 1979), and word completions (Hart & Gable, 2013), whereas common subliminal methods include lexical decisions tasks (e.g., Strahan et al., 2002), parafoveal priming (i.e., primes appear in the parafoveal visual field at 45, 135, 225, and 315 degree angles; e.g., Chartrand & Bargh, 1996), and foveal priming (i.e., primes appear in the foveal visual field; presented in the center of screen; e.g., Aarts et al., 2007). Further, experiments also vary in the proportion of primes versus neutral-word or nonword controls, which may have a bearing on the ultimate strength of the priming (Bargh & Pietromonaco, 1982; Srull & Wyer, 1979, 1980). Some priming paradigms mixed primes with distractor words (e.g., 15 out of 30 words in Bargh et al., 1996; 7 out of 13 words in Bargh et al., 2001), whereas others presented only prime words (e.g., Chartrand & Bargh, 1996; McCulloch et al., 2008). Finally, studies have widely differed on their selection of behavioral measures, including performance measures (e.g., anagrams, word searches; Bargh et al., 2001; Crusius & Mussweiler, 2012), categorical choices of behavior or products (e.g., Albarracin, Wang, & Leeper, 2009; Sela & Shiv, 2009; Strahan et al., 2002), rates of helping or donation behavior (e.g., Macrae & Johnston, 1998; Smeesters, Wheeler, & Kay, 2009), and motor behaviors (e.g., time spent, number of pegs dropped during a manual dexterity task, the 9-hole peg test; Ginsberg, Rohmer, & Louvet, 2012; Mathiowetz et al., 1985; Wryobeck & Chen, 2003), among others. These factors were considered in an exploratory fashion, as a way of providing information on the future design of behavior priming studies.

Method

Literature Search

We searched PsycINFO, ProQuest Dissertations and Theses, the Reproducibility Project Open Science Framework, 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). We searched for empirical studies cited in PsycINFO using the following search string: *(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 a version of this search string without the last two restrictions while searching ProQuest Dissertations and the Theses database, and a version without the last four restrictions while searching ZPID, ERIC, Foreign Doctoral Dissertations, and PubMed. In addition, we requested 320 published authors for additional data and the listhosts of the Society for Personality and Social Psychology, the Society for Consumer Psychology, and the Society for Experimental Social Psychology. Anonymous data submission was allowed as a way of decreasing barriers to obtaining these data. Our search was conducted up through June 2014, and contained all results from as early back in time as the databases had records (i.e., 1855 for PsycINFO and PubMed, 1897 for Proquest Dissertations and Theses). In the beginning of our screening, we examined all titles and abstracts for relevant studies. In a second phase, all articles that passed the first phase of screening were examined for our inclusion criteria, specified below.

Inclusion Criteria

Reports were not selected based on language. Rather, reports written in languages other than English were translated by coders with good knowledge of the language. Research reports were included when at least one condition in a study met the following eligibility criteria:

1. *Experimental*: Studies must have an experimental manipulation of priming in which participants are randomly assigned into conditions.

2. *Presence of word prime*: Studies must have a prime manipulation (whether within- or between-subjects) that is a word directly associated with an action. For example, the impression-formation words (e.g., opinion, personality, and evaluation, impression) from Chartrand and Bargh (1996) and the achievement words (e.g., win, compete, succeed, strive, attain, achieve, and master) from Bargh et al. (2001) were included. In contrast, we excluded any studies in which a social inference (e.g., goal contagion; Aarts et al., 2004), a biography of another individual (e.g., *Mother Theresa* in Gollwitzer et al., 2011), a social target or group (e.g., one's mother, nurses; Custers et al., 2008; Fitzsimons & Bargh, 2003), a picture (e.g., a brand logo or picture of a woman winning a race; Fitzsimons, Chartrand, & Fitzsimons, 2008; Latham & Piccolo, 2012), physical movements (e.g., Natanzon & Ferguson, 2012), or sounds (e.g., Friedman, 2007) were used with the objective of priming a goal. These strict criteria allowed us to isolate the cases in which the primed words have a clear behavioral implication (i.e., achievement words promoting more achievement) without additional inferences about the associations participants have about the primed content. For example, if instead of priming creativity researchers primed the Apple logo (e.g., Fitzsimons et al., 2008), the

research report was not deemed eligible because of the more complex inferential chain assumed in this priming. Priming money with words relevant to money (e.g., Vohs et al., 2006) before measuring social behavior was not judged eligible because these words do not directly relate to prosocial action without more complex assumptions about intervening psychological processes (i.e., money leads to self-sufficiency that in turn leads to less social behavior). Similarly, the future-related words influencing consumer decisions in Mogilner et al. (2012) were not eligible because the psychological link is more complex than either perception-behavior or goal-mediated processes. In this particular case, future words supposedly induce the desire to be happy, which in turn influences consumption behavior, but future words have minimal correspondence with product decisions without these additional assumptions.

3. *Priming rather than direct goal induction*: Reports must include a goal primed incidentally, without calling attention to the connection between the priming task and the outcome task or trying to induce intentional behavior. For example, some research included incidentally primed goals with orthogonally manipulated explicitly assigned goals in the form of an instructional set (e.g., Stajkovic et al., 2006). Studies that only used direct instructional sets were excluded (e.g., Brunyé & Taylor, 2009).

4. *Presence of adequate control condition*: Eligible experiments compared a relevant goal-prime with a neutral or nonopposite goal control (e.g., impression vs. memory in Chartrand & Bargh, 1996; achievement vs. control in Crusius & Mussweiler, 2012). We excluded studies and contrasts that primed opposite goals (e.g., thrift vs. prestige in Chartrand et al., 2008) to ensure that we could isolate the effect of the prime relative to the nonopposite baseline.

5. *Presence of a behavior measure*: Studies were required to measure a behavioral outcome in the form of (a) performance on a scorable task (e.g., anagrams completed, WI Card Sort Task performance, GRE score; see Bargh et al., 2001; Hart & Gable, 2013; Hassin, Bargh, & Zimerman, 2009), (b) a motor behavior (e.g., holding a weight in Sambolec, Kerr, & Messe, 2007; guiding a ring along a wire, Legal, Meyer, & Delouvee, 2007), (c) a choice participants expected to enact (e.g., selecting a watch type that could be won in a lottery, Chartrand et al., 2008), (d) actual helping or donation behavior (e.g., actual dictator game giving in Harrell, 2012), (e) time spent working on a task as a reflection of either speed or persistence (e.g., Shah & Kruglanski, 2003), or (f) actual consumption (e.g., of food or drink in Strahan, Spencer, & Zanna, 2002). We excluded studies that only used hypothetical monetary decisions (e.g., behavior in a hypothetical trust game in DeMarree et al., 2012) or hypothetical scenarios (e.g., what the participant would do in a business scenario in Quinn & Schlenker, 2002), attitude or confidence scales (e.g., confidence in Erb, Bioy, & Hilton, 2002), neurophysiological or physiological measures (e.g., heart rate, ERP; see Williams, Bargh, Nocera, & Gray, 2009; Hepler & Albarracin, 2013), self-predictions or intentions (e.g., of choices in Kawada, Oettingen, Gollwitzer, & Bargh, 2004), and evaluations of other people (e.g., attractiveness in Huang & Bargh, 2008). If studies contained multiple behaviors (e.g., persistence and performance in Shah & Kruglanski, 2003) or codings of the same task (e.g., Bargh et al., 1996; Chartrand & Bargh, 1996), we calculated effect sizes for each of those measures and incorporated them into a repeated-measures analysis.

6. *Statistics*: Studies needed to have sufficient information (means, *SD*s, *F*-ratios, *t* tests, etc.) to calculate effect sizes. We contacted authors to complete reports and used this information when provided, which led to an additional 26 effect sizes. This criterion led to excluding 11 studies for which we could not calculate an effect size (Jia, 2012, Study 3; McCulloch, 2004, Study 2; Perugini, Conner, & O'Gorman, 2011, Studies 2–3; Shah & Kruglanski, 2002, Studies 1–4; Strahan et al., 2002, Study 1 and follow-up 1; Todd, 2010, Study 1).

Meta-Analytic Strategy

We coded effect sizes in terms of Cohen's g and later transformed them to Hedges d by correcting g for sample size bias (Hedges, 1981; Hedges & Olkin, 1985). Effect sizes were calculated from a variety of reported statistics including means, mean confidence intervals, log-odds and log-odds ratios, *SD*s, *t* tests, and *F*-ratios (DeCoster, 2009; Hedges & Olkin, 1985; Johnson & Eagly, 2000). We coded effect sizes at all theoretically relevant levels of experimentally and quasi-manipulated (e.g., split on a personality variable in Levesque, 1999) conditions and collapsed nonmanipulated factors that were not theoretically important (e.g., gender in Bargh et al., 2001, Study 1), by combining means and *SD*s or by applying a correction factor to supplied test statistics (Johnson & Eagly, 2000, 2014; Morris & DeShon, 1997). Effect sizes were coded as positive if the behavior was in line with the primed concept, such as better academic performance when achievement was primed. In contrast, effect sizes were coded as negative if the observed behavior was in contrast with the primed concept, such as in the case of low-achievement motivation individuals who responded to primed achievement goals by pursuing opposite behavior (Hart & Albarracin, 2009). We weighted effect sizes by the inverse of the associated fixed-effects variances (Borenstein et al., 2009; DeCoster, 2009; Johnson & Eagly, 2000; Lipsey & Wilson, 2001) and the random-effects variance τ^2 using the Metafor package (Borenstein et al., 2009; Viechtbauer, 2010).

We used the homogeneity statistic (Q), which has a χ^2 distribution with degrees of freedom equal to the total number of effect sizes minus one ($e - 1$), to test for the presence of significant variability in the effect sizes (Borenstein et al., 2009; Hedges & Olkin, 1985; Hunter & Schmidt, 2004; Lipsey & Wilson, 2001). We also used the I^2 statistic as a second measure of heterogeneity that is more useful to compare across meta-analyses and less dependent on the number of synthesized effects (Borenstein et al., 2009; Higgins & Thompson, 2002; Huedo-Medina et al., 2006).

Some studies contributed more than one effect size because multiple experimental contrast units (k) were compared with a single control prime and/or because of the use of multiple dependent behavioral measures. Given this violation of statistical independence, we used methods to account for potentially correlated error terms among observations (Gleser & Olkin, 1994). We modeled the results hierarchically with effect sizes as level one observations and studies as level two observations (Littell, Milliken, Stroup, Wolfinger, & Schabenberger, 2006; Raudenbush & Bryk, 2002). All dependent measures were analyzed simultaneously with each measure having its own unique weighting based on aforementioned variances. We also conducted analyses selecting a single effect, which led to the same conclusions but are not presented because of space constraints.

We addressed publication bias in four ways. First, we computed the fail-safe number on individual effect sizes (N_{FS}), which corresponds to the number of unpublished negative findings (i.e., disconfirming findings that are either opposite to predictions or null) required to reduce a statistically significant result to nonsignificance (DeCoster, 2009; Rosenthal, 1979; Rosenthal & Rosnow, 2008) using Rosenberg's (2005) calculator. If the fail-safe number exceeds a cutoff value ($5k + 10$) specified elsewhere in the literature, publication bias is not deemed to be the driver of the synthesized effect (DeCoster, 2009; Rosenthal & Rosnow, 2008). Second, we used funnel plots, which depict effect sizes against precision (plotted here as the effect-size *SE*s), and the trim-and-fill procedure to de-bias effects (Duval & Tweedie, 2000a, 2000b; Light & Pillemar, 1984; Sutton, 2009). In funnel plots, if no publication bias is present, the distribution of effect sizes should be symmetric and should, as sample size increases, tighten to form a funnel shape. If there is publication bias, an asymmetry should emerge in the lower-left-hand corner of the funnel where small studies with nonsignificant effects would not have been published (Egger et al., 1997; Sutton, 2009). Trim-and-fill excises points in the asymmetry of the funnel plot, recalculates the new effect size estimate, and fills in the original asymmetric studies and their mirror images to ensure accurate confidence intervals for the new effect size estimate (Borenstein et al., 2009; Duval & Tweedie, 2000a, 2000b). We used trim-and-fill procedures on individual effect sizes to guide our decision of outliers to cut. Third, we examined the Hedges' d values of individual effect sizes in a normal-quantile plot, which graphs observed effect sizes against expected effect sizes based on draws from a normal distribution. This graph includes a diagonal of $X = Y$ near which the effect sizes should rest if the effect sizes are from a normal distribution. If the effect sizes deviate from a normal distribution or are sparsely present near zero, there is evidence of publication bias (Borman & Grigg, 2009; Wang & Bushman, 1999). Fourth, we used a p-curve analysis to determine whether or not selective reporting can entirely explain a literature. The p-curve examines the spread of p values of researchers' focal hypotheses to test whether p values are clustered around $p = .05$ (suggestive of p-hacking), versus $p = .01$ or $p = .02$ (evidence that selective reporting cannot explain the effects). The p-curve also considers whether the studies are underpowered to test the effect of interest (possible selective reporting effects; Simonsohn et al., 2014).

When testing moderators, we fit both fixed- and random-effects models using both single-variable metaregressions testing the effect of a lone moderator and multiple metaregressions inclusive of all descriptive and methodological controls, still modeling the effects hierarchically at the level of the study. No substitution of missing data points was done; missing effect sizes were not estimated, and no moderator values were missing. We standardized all variables before analysis (Cohen, Cohen, West, & Aiken, 2003).

Moderator Coding

Moderators of the effect sizes were coded independently by four of the authors, all of whom were intensely involved in research in psychology, had taken graduate courses in the area, and were familiar with the literature, after an initial calibration session. Coding also had a validation stage in which two of the authors reviewed the work of other coders and resolved disagreements. Agreement for all variables was sufficient ($\kappa > .7$, $\alpha > .8$) unless noted below.

Theoretical Moderators

We coded for value, expectancy, and delay with satisfaction opportunity as a way of testing critical predictions about the possibility of goal mediation under certain conditions.

Value. We first recorded if a study distinguished higher, lower, or neither higher nor lower value conditions using a manipulation or separated conditions based on a subject variable signaling differences in concept importance, affect, commitment manipulations, and/or incentives (e.g., increased value of achieving a goal in Förster et al., 2005). As an example of concept importance, in Hart and Albarracin (2009), participants who a priori indicated high (vs. low) chronic-achievement motivation were listed as having higher (vs. lower) value for an achievement goal. As an example of affect, value was coded as higher when positive affect was coactivated with a positive concept or goal (e.g., socializing; Aarts et al., 2007) and its nonopposite control. As an example of value associated with actual incentives, Marien et al. (2012, Study 2) manipulated the higher or lower monetary value of a gift voucher to be received on the basis of task performance. As an example of a commitment manipulation, Maltarich's (2009) participants responded to statements encouraging (vs. discouraging) commitment to the goal, which increased (decreased) commitment.

Subjective coding was also used to measure value by rating outcome relevance and value importance through subjective ratings of the extent to which completing the task would (or would not) fulfill the primed concept or goal and was relevant (or irrelevant) to participants' values (e.g., extrinsic motivation and an intrinsic motivation task; Levesque, 1999). Specifically, we coded conditions as low when measures concerned behaviors that were not stated as being part of the study (e.g., touching one's face, walking down a hallway, consuming candy unknowingly while watching something; see Boland et al., 2013; Lakin & Chartrand, 2003; Spears et al., 2004; Wryobeck & Chen, 2003) or outcomes that were deemed unmotivating to a group of participants (e.g., individuals with low achievement motivation receiving an achievement task, low commitment toward obtaining an outcome; see Hart & Albarracin, 2009; Maltarich, 2009). We also recorded low value when behaviors were the stated focus of the study but were not associated with either important outcomes or important values from the point of view of the participants (e.g., anagrams, creative uses tasks; see Bargh et al., 2001; Stajkovic et al., 2006). In contrast, we recorded high value when they involved potential monetary rewards or future acquisition beyond the lab (e.g., winning a product, making money; see Chartrand et al., 2008; Hassin et al., 2009), actual academic outcomes (e.g., Lowery et al., 2007) or altruistic behaviors (e.g., Macrae & Johnston, 1998), or high value-relevant motivation (e.g., expressing held attitudes; see Albarracin & Handley, 2011). This value was coded by one author then confirmed by independent coding with 87% agreement (κ = .56; κ between .41–.60 is moderate agreement, and between .61 and .80 is substantial agreement; Landis & Koch, 1977).

The objective and subjective codes for value were ultimately used to create an **overall index of value**. Specifically, we standardized manipulated value and coded value, then averaged the two indexes (indexes correlated r = .20). Behaviors with high value should be particularly likely to follow goal-driven principles, whereas direct perception-behavior effects may be present with greater independence of value.

Expectancy. As with value, we coded if a study distinguished high and low expectancy conditions using a manipulation or separating conditions based on a subject variable (i.e., higher, lower, or neither manipulated nor preselected; e.g., increased value of achieving a goal in Förster et al., 2005). Expectancy was coded as lower if the requirements of the task were difficult (e.g., the difficult benchmark to generate many creative uses for an object from Stajkovic et al., 2006) and as lower if the requirements of the task were easy (e.g., the lower benchmark from Stajkovic et al., 2006 to generate a few creative uses for an object).

Filler tasks and satisfaction opportunity. To address persistence or strengthening versus decay of the priming effect, we recorded whether there were filler tasks between the priming task and the focal dependent measure in terms of (a) length of the filler task (in minutes), (b) identity of the task (e.g., crossing out specific letters in a body of text in Albarracin & Hart, 2011), and (c) filler task relevance (low or high) to the goal. The presence and length of the filler task should increase the effect size according to the temporal escalation criterion (goal activation increases over time; Birch et al., 1975; Chartrand et al., 2008; Förster et al., 2005, 2007) depending on filler task relevance. Relevance was coded based on whether there was a filler task, and if so, whether it could be construed as fulfilling the activated goal. Relevant filler tasks that provide satiation opportunities for the goal may prematurely fulfill the goal and lead to decay effects (Förster et al., 2005, 2007); irrelevant filler tasks (no satiation opportunity) should lead to escalation effects (e.g., Chartrand et al., 2008). For example, in Chartrand et al. (2008) the filler task (e.g., making a real decision between a thrift and prestige option before another subsequent choice) was highly relevant as it could fulfill the activated goal (thrift or prestige). In contrast, the filler task from Chartrand and Bargh (1996), in which participants listed street names after an impression-formation prime, was judged to have low relevance to the goal. We planned to examine whether this filler task code interacted with our value index to gauge whether decay and escalation principles are more prevalent in the more goal-inducing or motivating high-value behaviors (Forster et al., 2007; Higgins et al., 1985).

Exploratory Moderators

We recorded a number of characteristics of the studies, participants, and procedures that were used in exploratory moderator analyses and controlled for in the main theory-based analyses when necessary.

Descriptives. To describe the source and participants of the studies, we coded general information about the studies including (a) year of publication, (b) laboratory (e.g., ACME, Goallab, MaSC), (c), percentage of female participants, (d) publication type (journal article, dissertation, unpublished data, replication), and (e) country of sample.

Methodological characteristics. We also coded studies according to other methodological characteristics of (a) the priming task, (b) the goal primed, (c) tasks before priming, (d) the behavioral outcome measure, (e) the debriefing procedure, and (f) other descriptive task characteristics.

We coded the **priming task** on several dimensions including (a) whether the task was supraliminal (e.g., Scrambled-Sentence Task in Chartrand et al., 2008) or subliminal (e.g., parafoveal priming in Chartrand & Bargh, 1996), (b) the method of goal priming, including scrambled sentence tasks (Chartrand et al., 2008), parafoveal priming (Chartrand & Bargh, 1996), lexical decision tasks (e.g., Shah & Kruglanski, 2003), finding words in a word search (e.g., Bargh et al., 2001), crossword puzzles (e.g., Marquardt, 2011), word completion tasks (e.g., Albarracin & Hart, 2011), and parafoveal and foveal priming (e.g., Aarts et al., 2007). We also recorded (c) the numbers of priming trials and total number of trials, (d) the interstimulus interval and mask length, (e) whether multiple goals were primed, and (f) the nature of the control condition, such as a nonopposite control goal (e.g., memory goals as the nonopposite control for impression-formation goals in Chartrand & Bargh, 1996), no task (e.g., Keatley, Clarke, Ferguson, & Hagger, 2014), neutral words (e.g., Bargh et al., 2001), or nonsense words (e.g., Jia, 2012).

With respect to the **primed goal**, we coded the nature or content of the primed goal(s) (e.g., achievement, socializing, health, etc.). As for the **dependent measure**, we coded the (a) identity of the outcome task (i.e., anagrams, reaction time [RT], time spent on task, consumption choice, helping rate, distance seated from another, number of spacebar presses or thoughts listed, selective exposure bias, amount of face touching, speed, whether task was continued, memory performance, number of creative uses generated, volunteering for future studies, feedback provided, classification of items, cleanliness behavior, monetary donation or spending, and choice of tasks). We also coded (b) task flexibility based on how many means could be used to solve the task and whether the task allowed for multiple answers. For example, many RT-based tasks, product selection (Sela & Shiv, 2009), and simple motor behaviors like walking down the hallway (Wryobeck & Chen, 2003) were judged to be inflexible, whereas anagram tasks and creative-uses tasks (Shah & Kruglanski, 2003; Stajkovic et al., 2006) were deemed flexible. The last coded dimension with respect to the dependent measure was (c) social desirability based on whether the behavior in question was likely to be the target of impression management, and either desirable or undesirable, or was likely unrelated to social desirability. For example, the task in Macrae and Johnston (1998) in which the participant can help the experimenter was judged to be socially desirable, whereas most motor (e.g., Ginsberg et al., 2012) or achievement (e.g., Bargh et al., 2001) behaviors were judged unrelated to social desirability. Finally, we recorded whether the study had any explicit funneled debriefing or explicit awareness check questions.

Results

Description of Conditions and Studies

Overall, we obtained 352 total effect sizes (*e*) from 133 different studies. These 133 studies produced 283 contrasts units (*k*) before unpacking into multiple effect sizes between a primed concept and a nonopposite control. A description of the distribution of methods, primes, and study composition for the 133 studies (*K*) and 283 contrast units (*k*) can be found in Table 1.

As shown in Table 1, of the 133 studies (*K*), a majority (80%) were from published sources, were published during the median year of 2009, and were conducted in the United States (58%) as well as other countries (42%). Additionally, about 9% (7%) of units (*k*) involved increasing (decreasing) goal value manipulations, whereas 23% (77%) of units had high (low) value codings. Only 20% of units had filler tasks. A majority of the studies (72%) used supraliminal priming methods (e.g., scrambled sentence tasks) over subliminal priming methods, and 90% of studies used a control consisting of neutral words as opposed to nonword strings (e.g., Bargh et al., 2001). On average, a little over three-fourths (M = .76, SD = .23) of the words or trials of the priming task were primes as opposed to filler words. Our sample of studies generally included inflexible (66%) rather than flexible tasks as dependent measures, and a majority of studies (76%) implemented an explicit awareness check of some form (e.g., Bargh & Chartrand, 2000).

Average Effect Size

We first examined the weighted mean effect size from the 352 effect sizes (*e*) modeled at the level of 133 studies (*K*). This

Table 1
Study Description (K = 133 Studies and k = 283 Units)

Variable	Type of statistic	Study (*K*) summary	Unit (*k*) summary
Descriptives			
Year	*M* (*SD*)	2007.53 (4.79)	2007.71 (4.65)
	Md	2009	2009
Publication status			
	Published count (%)	106 (80)	212 (75)
	Unpublished count (%)	27 (20)	71 (25)
Country			
	U.S. count (%)	77 (58)	177 (63)
	Non-U.S. count (%)	56 (42)	106 (37)
Theoretical features			
Goal value manipulations			
	Increased count (%)		26 (9)
	Nonmanipulated count (%)		236 (84)
	Decreased count (%)		21 (7)
Value coding			
	High count (%)		66 (23)
	Low count (%)		217 (77)
Goal expectancy manipulated			
	Increased count (%)		13 (5)
	Nonmanipulated count (%)		256 (90)
	Decreased count (%)		14 (5)
Filler task			
	Present count (%)		58 (20)
	Absent count (%)		225 (80)
Methodological features			
Liminality			
	Supraliminal count (%)	96 (72)	211 (75)
	Subliminal count (%)	37 (28)	72 (25)
Proportion of primes			
	M (*SD*)	.76 (.23)	.74 (.23)
	Md	.80	.67
Control			
	Neutral count (%)	120 (90)	248 (88)
	Other count (%)	13 (10)	35 (12)
Flexibility			
	Inflexible count (%)	88 (66)	183 (65)
	Flexible count (%)	45 (34)	100 (35)
Debriefing			
	Yes count (%)	101 (76)	218 (77)
	No count (%)	32 (24)	65 (23)

Note. Theoretical features specified at contrast unit (*k*) level only due to variation within on those factors potentially being manipulated within study.

yielded a weighted-average fixed effect $d = 0.332$ (95% CI [confidence interval] [0.277, 0.387]; $t(132) = 11.78$, $p < .001$), and a weighted-average random effect $d = 0.352$ (95% CI [0.294, 0.409]; $t(132) = 11.89$, $p < .001$. For both of these models Cochran's $Q(351) = 934.77$, $p < .001$ rejected the null hypothesis of homogeneity, and I^2 indicated considerable nonrandom variability (Higgins & Thompson, 2002; Huedo-Medina et al., 2006) of 62.45% (95% CI [57.89, 66.51]). Specifically, an d of 62.45% implies moderate to large nonrandom heterogeneity (Borenstein et al., 2009; Higgins & Thompson, 2002; Huedo-Medina et al., 2006), justifying moderator analyses. The distribution of these effect sizes can be found in the histogram in Figure 1.

Publication Bias

In terms of publication bias, both Rosenthal's (1979) and Rosenberg's (2005) methods suggested that the number of mean-weighted filedrawer studies necessary to bring nonsignificance exceeds a reasonable (i.e., $5e + 10$) threshold. Specifically, we examined this average effect size via our aforementioned plan to examine publication bias, inclusive of checking the Failsafe number (Rosenberg, 2005, 1979; Rosenthal & Rosnow, 2008). Considering only the independent effect sizes, findings suggested the need of 57,426 (36,440) nonsignificant mean-weighted studies to reduce the overall effect to nonsignificance via the Rosenthal (Rosenberg) method. These numbers far exceed the 1,770 ($5^{*}352 + 10 = 1770$) study threshold previously set regarding at what point filedrawer studies become a concern (Rosenthal, 1991; Rosenthal & Rosnow, 2008).

A trim-and-fill analysis and Egger OLS regression confirmed potential concerns about publication bias, but revealed a confidence interval containing the true effect size that does not include zero (Duval & Tweedie, 2000a, 2000b; Egger et al., 1997; Stanley, 2005). A trim-and-fill analysis of the independently modeled effect sizes via the R0[2] estimator added nine studies that yielded a new, estimated fixed-effects $d = 0.295$ (95% CI [0.264, 0.325]; $z = 19.08$, $p < .001$) and random-effects $d = 0.312$ (95% CI [0.257, 0.366], $z = 11.15$, $p < .001$), suggesting a significant effect after accounting for publication bias. Similarly, an Egger OLS modeled at the study level indicated a small study effect (fixed effects: $t(218) = 4.25$, $p < .001$; random effects: $t(218) = 5.19$, $p < .001$). We additionally used these analyses to identify outliers to be removed from further analyses, which led to removing nine effect sizes from eight studies in subsequent analyses (Jefferis & Fazio, 2008, Study 1; Keatley et al., 2014, Study 1; Legal et al., 2007, Study 1; Levesque, 1999, Study 2; Macrae & Johnston, 1998, Study 2; Oettingen et al., 2006, Study 4; Roehrich, 1992, Study 1; Sela & Shiv, 2009, Study 3). The funnel plot of the RE analyses appears in Figure 2 (Light & Pillemar, 1984).

We also addressed a common concern within meta-analytic work about whether trim-and-fill estimates are biased in the presence of heterogeneity among studies (Johnson & Eagly, 2014; Terrin et al., 2003). According to this concern, the requirement for trim-and-fill effect sizes to be from one population deems the method inaccurate when there is high effect-size heterogeneity (Johnson & Eagly, 2014). Recognizing this criticism, we conducted separate trim-and-fill analyses for separate levels of moderators. We did not find any immediate indication that heterogeneity related to coded moderators skewed our trim-and-fill analyses (see Table 2).

After removing effect sizes from the trim-and-fill analysis and modeling the studies at the study level, we obtained an average effect size of $d = 0.315$ (95% CI [0.263, 0.368]; $t(132) = 11.75$, $p < .001$) from fixed-effects models and $d = 0.323$ (95% CI [0.270, 0.376]; $t(132) = 11.95$, $p < .001$) from random-effects models. Both of these analyses again rejected the null homogeneity hypothesis ($Q(342) = 806.43$, $p < .001$) and had a similar I^2 value of 57.59% (95% CI [52.19, 62.38]), again demonstrating between moderate and large heterogeneity. This new effect size has a Rosenthal (Rosenberg) failsafe number of 46,930 (31,623), which again exceeds the $5k + 10$ threshold and suggests that publication bias is unlikely to fully explain our findings (Rosenthal, 1991; Rosenthal & Rosnow, 2008).

We also ruled out that the effect sizes emerge from two different distributions using a normal-quantile plot (see Figure 3) of the 343 individual effect sizes, which also checks for nonnormality of the data (Wang & Bushman, 1998, 1999). The normal-quantile plot examines potential publication bias by reviewing whether the shape of the curve has any discontinuities around 0 (indicative of publication bias) or has an S-shaped structure that may signal two underlying populations (Wang & Bushman, 1998). A Shapiro-Wilk normality test on the 343 data points yielded a marginally significant p value ($W = 0.992$, $p = .073$), suggesting nonnormality of the data that might indicate publication bias, which we characterized as insufficient to explain our effect. The shape of the distribution did not immediately suggest that the studies come from two populations (curve not S-shaped; Wang & Bushman, 1998).

Finally, p-curve analyses suggested that selective reporting could not explain the results of the set of studies from which we drew our effect sizes (Simonsohn et al., 2014). We present two sets of p-curve analyses (based on continuous tests) of the studies in this meta-analysis: (a) a p-curve on all studies conducted using p values based on the researchers' focal hypotheses (Simonsohn et al., 2014), and (b) p-curves based on studies with the largest error

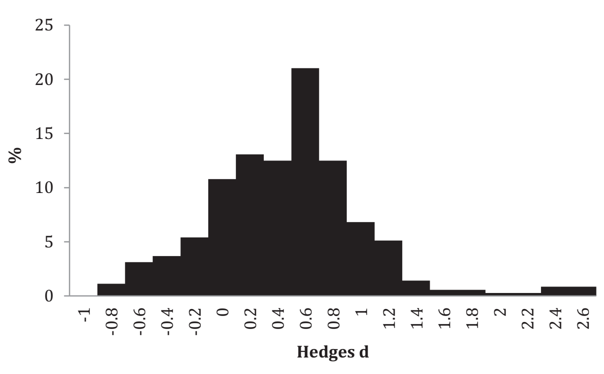


Figure 1. Histogram of effect sizes from the meta-analysis.

[2] We chose the R0 estimator per recommendation from Duval and Tweedie (2000b) to use the L0 estimator if the number of missing studies estimated exceeds 25% of the total studies. Because that was not the case, we opted to use the R0 estimator, but we include the L0 estimates in Table 2. These estimates again confirm that adjusting for publication bias cannot bring the effect to nonsignificance.

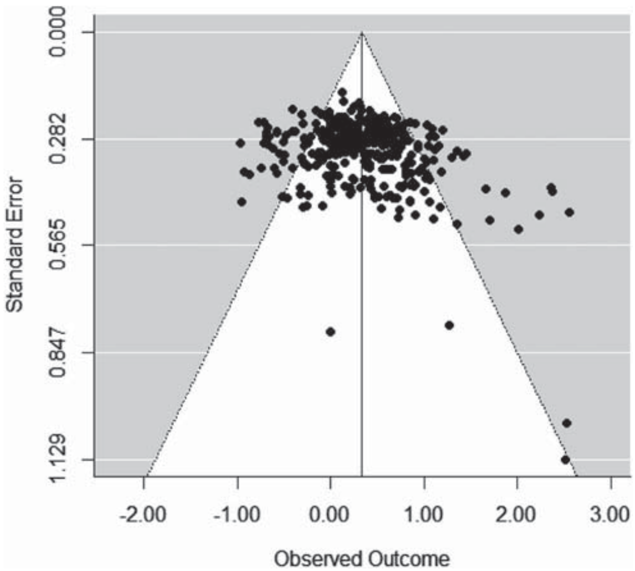


Figure 2. A funnel plot of the (random-effects weighted) effect sizes in the meta-analysis, with effect sizes on the X-axis and *SE* (precision) on the Y-Axis.

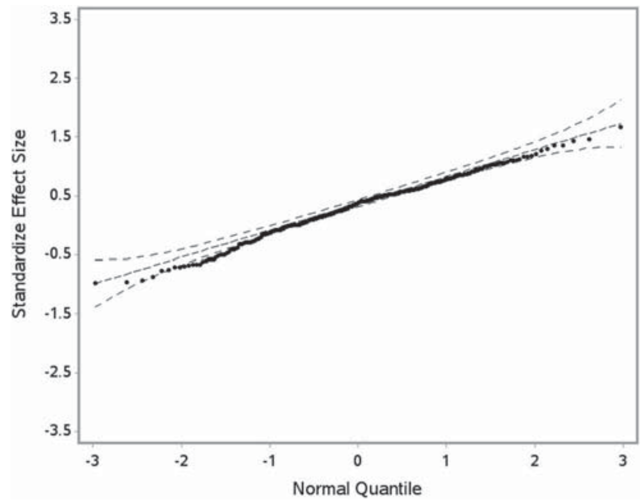


Figure 3. Normal-quantile plot of the effect sizes, excluding nine outliers, in the meta-analysis.

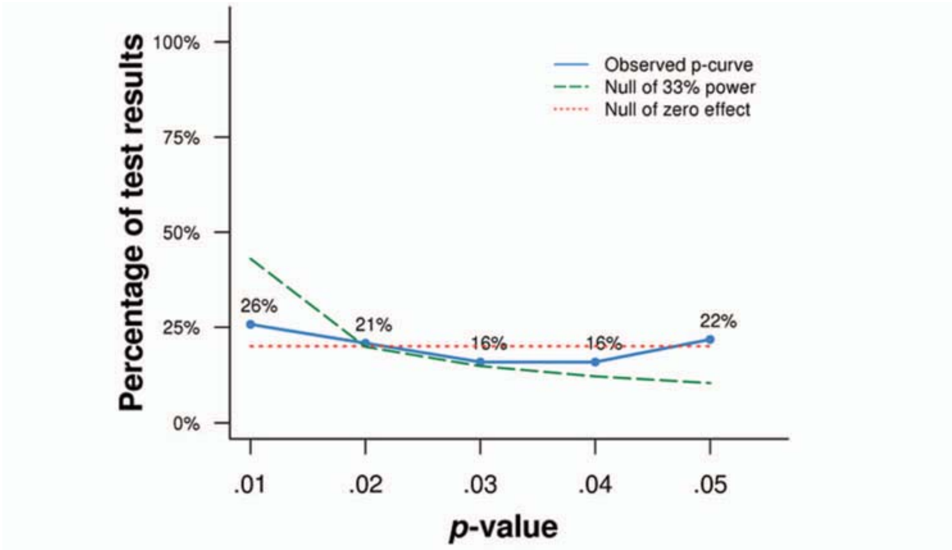
degrees of freedom. The former curve can be found in Figure 4 and concerns results based on the focal hypotheses of authors (often including interaction effects rather than mere differences between prime and control conditions). When we included all studies (published or unpublished) with clear hypotheses for behavioral measures (as outlined in our *p*-curve disclosure table), we found no evidence of *p*-hacking (no left-skew), but dual evidence of a right-skew and flatter than 33% power. The *p*-curve being flatter than 33% indicated that on average the studies considered in this meta-analysis are greatly underpowered to discover the effect of interest in the study, though selective reporting alone cannot explain the entirety of the evidence. We again found this pattern when we restricted the *p*-curve to studies in the top half of error degrees of freedom (*df*; see Figure 5). However, when we restricted the *p*-curve to studies in the top third (see Figure 6) or top quartile (see Figure 7) of *df*, we found a clear right skew, which also indicated that selective reporting alone cannot explain the study results. Disclosure tables for these *p*-curves can be found in the supplementary materials.

Moderator Analyses

Tables 3 and 4 present the results from the moderator analyses. These analyses predicted *d* in a particular case as a function of the level of a moderator(s), using both simple and multiple regressions. In the simple regressions, each moderator in the tables was introduced alone; in the multiple regressions, all moderators in Table 4 were added to the regression as covariates. *QB* comprises the Sums of Squares Regression or Sums of Squares Between in an analysis of variance (ANOVA) context. This statistic is distributed as a χ^2 and indicates if the effect of a moderator or an interaction

Table 2
Heterogeneity Analysis for Trim-and-Fill Analyses

Effect size calculation	R0 estimator		L0 estimator	
Studies added new ES (95% confidence interval)	Fixed effects	Random effects	Fixed effects	Random effects
All	9	9	57	58
	.295 [.264, .325]	.312 [.257, .366]	.212 [.183, .241]	.207 [.149, .264]
Subliminal	3	3	14	15
	.359 [.299, .418]	.371 [.272, .470]	.289 [.231, .346]	.279 [.174, .383]
Supraliminal	5	5	40	42
	.275 [.240, .310]	.295 [.230, .359]	.191 [.158, .225]	.184 [.115, .252]
Published	6	6	46	47
	.375 [.340, .411]	.395 [.331, .458]	.284 [.250, .317]	.277 [.210, .344]
Unpublished	0	0	17	17
	.103 [.044, .161]	.116 [.035, .197]	−.008 [−.062, .047]	−.010 [−.105, .085]
High coded value	5	5	45	46
	.268 [.234, .301]	.288 [.226, .349]	.177 [.145, .209]	.172 [.106, .238]
Low coded value	3	3	6	8
	.418 [.349, .487]	.416 [.306, .526]	.396 [.328, .464]	.368 [.251, .484]



Statistical Inference	Results: Binomial Test (Share of significant results p<.025)	Results: Continuous Test (Aggregate pp-values via Stouffer Method)
1) Studies contain evidential value. (Right skew)	p =.2754	Z = -1.87, p=.031
2) Studies' evidential value, if any, is inadequate. (Flatter than 33% power)	p =.0002	Z = -3.83, p=.0001
3) Studies exhibit evidence of intense p-hacking. (Left skew)	p =.7869	Z = 1.87, p=.969

Figure 4. p-Curve on dataset of authors' predictions (interactions and main effects). See the online article for the color version of this figure.

is statistically significant. Both fixed- and random-effects models are reported in the tables.

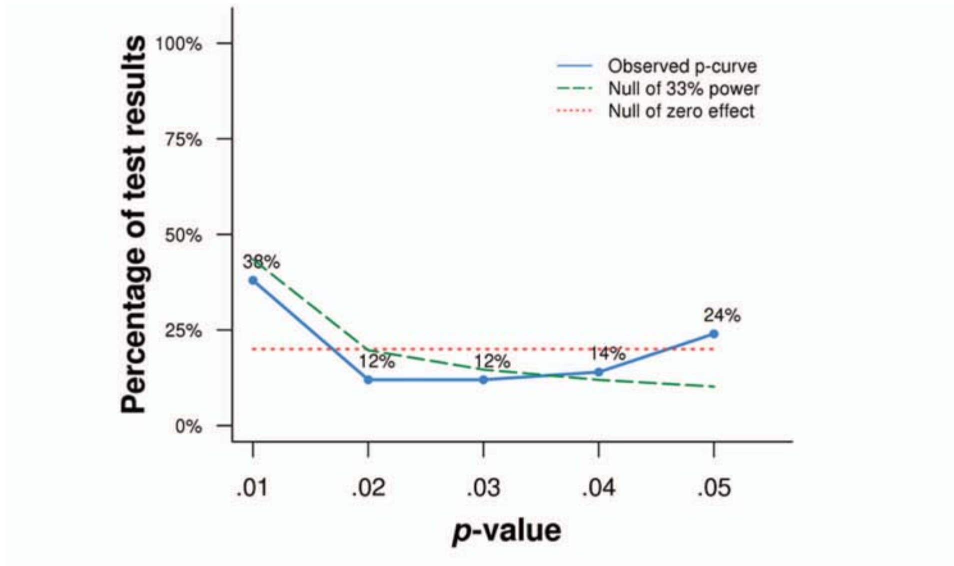
Theoretical moderators. Fixed effects and random effects analyses corresponding to our theoretical predictions appear in Table 3 and include analyses of value, expectancy, delay or satisfaction opportunity, and predicted interactions between value and expectancy and between value and delay or satisfaction opportunity. Both simple and multiple regressions were conducted and yielded similar results and exhibited no collinearity problems.

Value. The first three rows in Table 3 present the effects of value, for which we used the coded and overall value indexes. Consistent with prior literature suggesting that increases in value should yield larger goal mediated effects, we found a significant association between the overall value index and the behavioral priming effect for high (d_{FE} = 0.450; d_{RE} = 0.443) and low (d_{FE} = 0.195; d_{RE} = 0.210; see A.3 in Table 3) value (Fishbach & Ferguson, 2007; Förster et al., 2005, 2007).

Expectancy. Contrary to theorizing from both Forster et al. (2007) and Locke and Latham (1990), there were no differences in the strength of priming across high- (d_{FE} = 0.252; d_{RE} = 0.232) and low-expectancy (d_{FE} = 0.164; d_{RE} = 0.166) levels (see B in Table 3).

Delay with satisfaction opportunities. Responses to delay and satisfaction opportunities are useful to diagnose if perception-behavior, goal mediation, or both mechanisms underlie the obtained priming effect. For example, uniform decay across conditions would point to perception-behavior as the sole mechanism explaining our effects. Likewise, uniform persistence or escalation would point to goal mediation as the primary process implicated in our average priming effect. If high-value concepts are more likely to involve motivational principles, then the interaction between value and delay/satisfaction should enable us to test the plausibility of the proposed effect of delay for goals in the literature. An interaction between value and delay/satisfaction opportunity, however, would likely reveal that both mechanisms occur in different conditions.

There was little evidence suggesting that only perception-behavior or only goal mediation explains behavior priming effects. First, as shown in D in Table 3, we see little decay between those effect sizes that have no delay between priming (d_{FE} = 0.321; d_{RE} = 0.329) and the outcome measure and those with a delay without satisfaction (d_{FE} = 0.332; d_{RE} = 0.342; FE QB_2 = 0.02, *ns*; RE QB_2 = 0.03, *ns*), which is consistent with maintenance of goal activation. Second, the analysis (E) in Table 3 revealed a significant interaction between value and the three-level variable indicating no delay, delay without satisfaction opportunity, or delay with satisfaction opportunity. The decomposition of this interaction provided important information. Comparing across higher- and lower-value situations suggested that goal activation occurred over and above direct perception-behavior effects and, thus, strengthened the priming effect. Specifically, in the absence of a delay, higher-value (see E.1) conditions showed stronger priming than the corresponding lower-value (see E.2) conditions (FE QB_2 = 30.01, $p < .001$; RE QB_2 = 23.91, $p < .001$). Furthermore, higher-value conditions with a delay without satisfaction opportunities showed stronger priming than the corresponding lower-value conditions (FE QB_2 = 30.23, $p < .001$; RE QB_2 = 23.45, $p < .001$). These results support the notion that goal



Statistical Inference	Results: Binomial Test (Share of significant results p<.025)	Results: Continuous Test (Aggregate pp-values via Stouffer Method)
1) Studies contain evidential value. (Right skew)	p =.4439	Z = -2.04, p=.0209
2) Studies' evidential value, if any, is inadequate. (Flatter than 33% power)	p =.0036	Z = -2.13, p=.0167
3) Studies exhibit evidence of intense p-hacking. (Left skew)	p =.6641	Z = 2.04, p=.9791

Figure 5. p-Curve on the studies with the top half of error degrees of freedom. See the online article for the color version of this figure.

activation contributes over and above perception-behavior in explaining priming effects.

Descriptive and Methodological Moderators

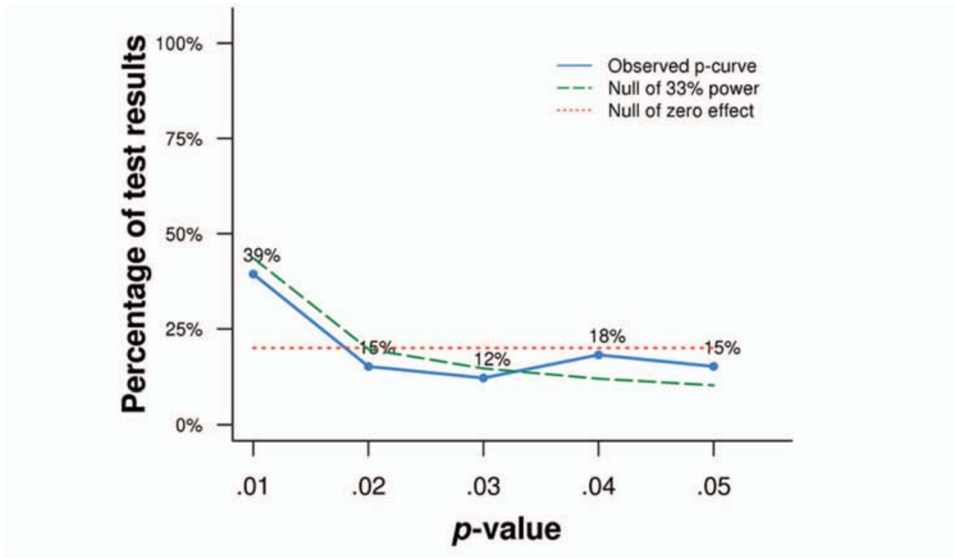
We also conducted exploratory analyses of descriptive study characteristics (year, publication type, and country) and methodological factors (goal type, liminality, proportion of primes, etc.) using simple and multiple metaregressions. These analyses were carried out for fixed-effects and random-effects models, and are reported in Table 4. Variance inflation factors for the multiple regression analyses were under 10, suggesting no immediate collinearity problems (Cohen et al., 2003).

For our descriptive moderators, although we observed no decline effect as a function of study year (Schooler, 2011), we found that published studies yielded larger priming effects than unpublished studies. We also observed larger effect sizes for non-U.S. studies but this moderation did not hold in the multiple regression models. Furthermore, our methodological moderators did not yield significant differences in either single or multiple regressions.

General Discussion

Since the early look at whether primes could guide behavior, a burgeoning literature has searched for the theoretical explanations and boundaries of the behavioral effects of priming. The initial findings gave credence to the perception-behavior model, one in which passive perception of an internalized behavior concept influences behavior (Bargh et al., 1996; Bargh & Morsella, 2009; Chartrand & Bargh, 1999). Researchers simultaneously broadened the theoretical horizon to encompass the key motivational concepts of goals, value, expectancy, and potential for goal satiation (Atkinson, 1974; Bargh et al., 2001; Chartrand et al., 2008; Fishbach & Ferguson, 2007; Forster, Liberman, & Friedman, 2007).

Beyond initial demonstrations, the behavior priming literature expanded the scope of priming to such diverse applications as health behaviors (Wryobeck & Chen, 2003), social mimicry (Lakin & Chartrand, 2003), and consumer choice (Chartrand et al., 2008; Sela & Shiv, 2009), suggesting that the effect is widely relevant. However, the study of priming has received immense censure because of several high-profile replication failures (Doyen et al., 2012; Harris, Coburn, Rohrer, & Pashler, 2013; Klein et al., 2014; Pashler, Harris, & Coburn, 2008; Shanks et al., 2013). In this context, we sought to synthesize past research on this effect in a precise way that might help to find answers to the controversy. We excluded studies without a nonopposite comparison (e.g., achievement vs. neutral, or impression-formation vs. memory, but not action vs. inaction) to verify that the effect of the prime is directionally consistent with predictions tested in a conservative fashion. We aggregated published and unpublished reports of the behavioral effects of priming and estimated publication bias with various contemporary techniques (Duval & Tweedie, 2000a, 2000b; Rosenberg, 2005; Rosenthal, 1979). We aimed to establish the magnitude and robustness of these effects, and test the theory-relevant moderators of value, expectancy, and goal satiation opportunities (e.g., by fulfilling the goal; Chartrand et al., 2008),



Statistical Inference	Results: Binomial Test (Share of significant results p<.025)	Results: Continuous Test (Aggregate pp-values via Stouffer Method)
1) Studies contain evidential value. (Right skew)	p =.2434	Z = -1.87, p=.0305
2) Studies' evidential value, if any, is inadequate. (Flatter than 33% power)	p =.0695	Z = -1.58, p=.0571
3) Studies exhibit evidence of intense p-hacking. (Left skew)	p =.8519	Z = 1.87, p=.9695

Figure 6. p-Curve on the studies with the top third of error degrees of freedom. See the online article for the color version of this figure.

while controlling for methodological differences (liminality, priming task, or use of funneled debriefing awareness checks) in the synthesized effects.

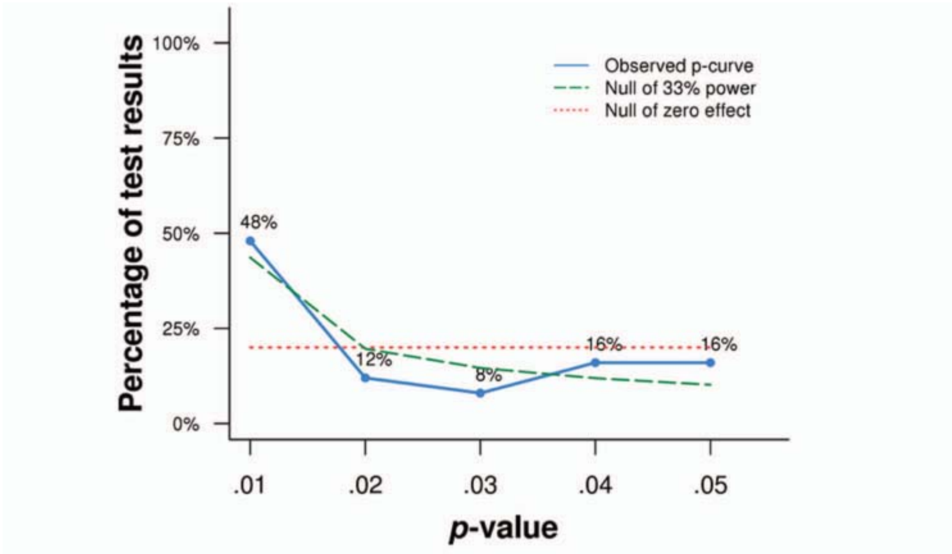
Findings and Importance of This Meta-Analysis

This meta-analysis integrated several hundred effect sizes concerning behavioral priming contemporaneously with clamor over the replicability of effects within priming (e.g., Klein et al., 2014). Whereas other work has concentrated on individual studies (e.g., Doyen et al., 2012; Harris et al., 2013), and other meta-analyses concerning priming have only concentrated on nonbehavioral or intention-based measures, we incorporated a variety of priming methods, primes, and outcome measures, offering a broader perspective on effect size variability. By observing multiple studies, we are able to circumvent the meaning of one or two individual failures to replicate in favor of observing the *population* of studies, so we could overcome the limitations of selective narrative reports either supporting or attacking the literature (Borenstein et al., 2009; Cooper & Hedges, 2009a, 2009b).

Further, this meta-analysis allowed us to shift the discussion of priming effects from individual failures or successes to the theoretical principles explaining behavior priming (i.e., Bargh & Chartrand, 2000; Bargh et al., 2010; Fishbach & Ferguson, 2007; Forster et al., 2007). We found support for two important tenets of how goals are conceptualized. First, as predicted, the effects of priming were stronger when participants valued the outcome of the measured behavior, either because value was manipulated to be high, such as in the use of incentives in exchange for accurate responses, or because the participants inherently valued the outcome (e.g., students attempting a graded intelligence test). These findings indicate that our results are more than just concept priming because these effects depend on valuation (less for low value, stronger effects for high value; Fishbach & Ferguson, 2007; Forster et al., 2007).

Contrary to both accounts of the effect of outcome expectancy on the strength of behavioral priming effects (Forster et al., 2007; Locke & Latham, 1990), we failed to observe significant moderating effects of expectancy manipulations. Our research synthesis showed no evidence that increasing expectancy boosts priming effects through increased motivation (Förster et al., 2005) or that individuals worked harder to achieve the more difficult goals (Locke & Latham, 1990). Of the two accounts, however, research in other domains indicates that lower-expectancy goals result in greater effort than higher-expectancy ones (Budden, 2007; Locke & Latham, 1990; Heath et al., 1999; Maltarich, 2009; Stajkovic et al., 2006).

Further, we found some support for goal tenets based on the impact of satiation opportunities provided by tasks introduced between priming and behavior measurement (Chartrand et al., 2008; DeCoster & Claypool, 2004; Fishbach & Ferguson, 2007; Förster et al., 2005, 2007; Higgins et al., 1985). Highly valued behaviors or states were directionally associated with more decay when there were satisfaction opportunities, as compared to when there were not. When behaviors or states were not valued, however, the delays were directionally associated with decay, suggesting that the smaller effect of priming was because of direct



Statistical Inference	Results: Binomial Test (Share of significant results p<.025)	Results: Continuous Test (Aggregate pp-values via Stouffer Method)
1) Studies contain evidential value. (Right skew)	p =.1148	Z = -2.33, p=.0098
2) Studies' evidential value, if any, is inadequate. (Flatter than 33% power)	p =.2832	Z = -0.78, p=.2186
3) Studies exhibit evidence of intense p-hacking. (Left skew)	p =.9461	Z = 2.33, p=.9902

Figure 7. p-Curve on the studies with the top quartile of error degrees of freedom. See the online article for the color version of this figure.

behavior activation. This effect is important and was not obtained in a prior meta-analysis of evaluative priming effects (DeCoster & Claypool, 2004).

In addition, our meta-analysis addresses whether behavioral priming effects and goal-mediation coexist or operate in a mutually exclusive way. As we observed significant priming effects in conditions not conducive to goal activation (e.g., lower value; Forster et al., 2007), perception-behavior seems to take place in a relatively default way although this effect is small. Behavioral priming effects can stem from mere concept accessibility similar to previously established effects in the area of interpretation of information (see Higgins et al., 1977). Correspondingly, we found stronger priming effects under conditions that are conducive to goal activation (e.g., higher value) and meaningful interaction between value and delay/satiation. The interaction in particular signaled that both perception-behavior and goal mediation can contribute to behavioral priming effects. When value is higher, there is a reduction of priming when the delay offers satiation opportunities relative to when it does not. When value is lower, there is a reduction of priming between an immediate follow up (no delay) and a delayed follow up without satisfaction opportunities. Therefore, our meta-analysis is compatible with the conclusion that both processes can contribute to behavioral priming effects (Bargh, 1994).

Methodological Recommendations for Future Behavior Priming Studies

Investigators must make a daunting number of methodological decisions when conducting a priming study such as the number of prime trials to use, the proportion of primes to control words, and the liminal threshold of the primes. To help with these decisions, many knowledgeable researchers have made recommendations about priming study parameters based on their own perceived successes and failures (e.g., Bargh & Chartrand, 2000). In contrast to prior recommendations based on anecdote or small empirical samples, we were able to quantitatively establish moderating effects of methodological procedures using the largest known sample to date ($e = 352$). Despite the large sample and thus relatively high statistical power, we found no effect of any methodological variable investigated, including whether the prime was supraliminal or subliminal, the proportion of priming task trials was higher or lower, the use of neutral primes or other control conditions, the selected type of dependent measure (performance, social, consumption, and other), or the flexibility of the chosen dependent measure. This large amount of evidence converges in the important conclusion that researchers have a high degree of flexibility when selecting priming study parameters without expecting major differences in effectiveness of the prime and strength of the effect.

Even though characteristics of the priming method or the dependent measure make little difference, our findings point to the importance of ensuring high value for the primed behavior or goal in a particular study, either because of introduced incentives or personal values. One potential concern for studies attempting to replicate the effects of achievement or social priming is that the participants of the replication sample may not value the primed goals in the same way as the original sample. This possibility is consistent with speculation that replicators may be stepping on a moderator that derails any priming effects (Cesario, 2014).

Table 3
Moderator Analyses Corresponding to Theoretical Predictions

Moderator	Level	k	Fixed-effects models: d. FE	95% CI	Simple QB	Multiple QB^a	Random-effects models: d.	95% CI	Simple QB	Multiple QB^a
A. Value										
1. Value manipulation					32.57***	35.73***			29.28***	33.19***
	Higher (1)	27	.637	[.455, .819]			.632	[.453, .810]		
	Neither selected nor manipulated for level (0)	293	.320	[.265, .375]			.326	[.270, .382]		
	Low (−1)	23	−.085	[−.276, .106]			−.056	[−.248, .136]		
2. Coded value					4.65*	2.31			3.21^	1.62
	Higher (1)	77	.427	[.313, .541]			.414	[.301, .526]		
	Lower (−1)	266	.287	[.229, .345]			.298	[.239, .357]		
3. Overall value index					25.86***	25.12***			21.72***	22.06***
	Higher (1)		.450	[.377, .522]			.443	[.371, .515]		
	Lower (−1)		.195	[.125, .265]			.210	[.138, .281]		
B. Expectancy					.29	.20			.15	.05
	Higher (1)	18	.252	[.027, .478]			.232	[.007, .458]		
	Neither selected nor manipulated for level (0)	306	.325	[.270, .380]			.335	[.280, .390]		
	Lower (−1)	17	.164	[−.065, .393]			.166	[−.063, .396]		
C. Value Index × Expectancy					0	0			0	0
Higher value										
	Higher expectancy		.460	[.350, .571]			.450	[.342, .557]		
	Lower expectancy		.438	[.330, .546]			.435	[.329, .541]		
Lower value										
	Higher expectancy		.209	[.119, .299]			.220	[.128, .311]		
	Lower expectancy		.180	[.091, .269]			.199	[.109, .289]		
D. Delay/satisfaction opportunity[b]					1.08	1.09			1.36	1.58
	No delay	266	.321	[.262, .380]			.329	[.269, .389]		
	Delay without satisfaction	68	.332	[.201, .463]			.342	[.214, .470]		
	Delay with satisfaction	9	.068	[−.272, .409]			.047	[−.292, .385]		
E. Overall Value Index × Delay/Satisfaction opportunity[b]					3.61*	3.33*			3.27*	2.97^
1. Higher value										
	No delay		.427	[.343, .511]			.430	[.347, .513]		
	Delay without satisfaction		.541	[.387, .695]			.532	[.375, .690]		
	Delay with satisfaction		.216	[−.130, .562]			.243	[−.116, .602]		
2. Lower value										
	No delay		.234	[.154, .313]			.241	[.159, .323]		
	Delay without satisfaction		.126	[−.032, .285]			.156	[−.006, .317]		
	Delay with satisfaction		−.910	[−1.820, .000]			−.915	[−1.743, −.087]		

Note. k = number of effect sizes; d = Hedges' d; QB = index of effect size homogeneity such that when it is significant, reject null hypothesis of homogeneity; CI = confidence interval.
[a] Multiple regression analyses were done by adding all moderators from Table 4 as covariates into the regression. [b] F-ratio for omnibus effect.
^ $p < .1$. * $p < .05$. ** $p < .01$. *** $p < .001$.

Table 4
Descriptive, Methodological, and Exploratory Moderator Analyses

			Fixed-effects models				Random-effects models			
Moderator	Level	*k*	*d.*	95% CI	Simple *QB*E	Multiple QB^{a}	*d.*	95% CI	Simple *QB*	Multiple QB^{a}
Descriptive moderators										
Year					.01	.21			.01	.22
Publication status					25.82^{***}	18.92^{***}			26.69^{***}	18.40^{***}
	Published	255	.377	[.322, .432]			.386	[.331, .441]		
	Unpublished	88	.095	[.001, .189]			.103	[.010, .195]		
Country					6.70^{*}	$2.84^{\wedge}$			5.48^{*}	1.78
	United States	215	.260	[.196, .324]			.272	[.207, .338]		
	Non-U.S.	128	.399	[.316, .482]			.399	[.315, .483]		
Methodological moderators										
Liminality					$3.78^{\wedge}$	1.25			2.71	.48
	Supraliminal	255	.286	[.225, .347]			.298	[.237, .360]		
	Subliminal	88	.408	[.301, .514]			.402	[.295, .509]		
Proportion of primes (dosage)					1.57	.44			1.14	.27
	High		.345	[.274, .417]			.349	[.277, .422]		
	Low		.280	[.205, .354]			.293	[.218, .368]		
Neutral control					1.15	.25			2.01	0
	Yes	307	.306	[.250, .361]			.310	[.254, .365]		
	No	38	.402	[.235, .569]			.435	[.270, .601]		
Type of prime achievement oriented prime					$3.36^{\wedge}$	2.40			$2.86^{\wedge}$	1.53
	Yes	143	.262	[.184, .340]			.272	[.191, .352]		
	No	200	.360	[.289, .431]			.364	[.293, .434]		
Social prime					1.62	.95			1.57	1.01
	Yes	37	.415	[.252, .579]			.420	[.259, .581]		
	No	306	.304	[.249, .359]			.311	[.256, .367]		
Consumption or health prime					.43	.26			.56	.22
	Yes	15	.412	[.118, .707]			.429	[.144, .715]		
	No	328	.312	[.259, .365]			.319	[.265, .373]		
Type of dependent measure Performance measure					.96	1.55			.81	1.44
	Performance	280	.303	[.246, .361]			.312	[.253, .370]		
	Nonperformance	63	.373	[.246, .500]			.374	[.250, .499]		
Consumption measure					.05	1.50			.02	1.37
	Consumption	46	.331	[.184, .477]			.334	[.189, .479]		
	Nonconsumption	297	.313	[.257, .370]			.322	[.264, .379]		
Flexibility					.12	$3.56^{\wedge}$			.17	2.50
	Flexible	124	.304	[.220, .388]			.309	[.223, .395]		
	Inflexible	219	.323	[.255, .392]			.332	[.265, .400]		
Social desirability					.34	$3.46^{\wedge}$			.79	3.92^{*}
	High	19	.253	[.036, .470]			.225	[.002, .448]		
	Low	324	.320	[.265, .374]			.329	[.274, .384]		
Debriefing					.12	.04			.13	0
	Yes	274	.321	[.261, .381]			.329	[.268, .389]		
	No	69	.298	[.186, .410]			.305	[.191, .419]		

Note. k = number of effect sizes; d = Hedges' d; QB = index of effect size homogeneity such that when it is significant, reject null hypothesis of homogeneity; CI = confidence interval.
[a] Multiple regression analyses were done by adding all moderators from Table 4 as covariates into the regression.
$^{\wedge} p < .1$. $^{*} p < .05$. $^{**} p < .01$. $^{***} p < .001$.

Despite this overall conclusion about methodological variability, three considerations are worth noting. First, the present results are necessarily bound by the range of values observed in the meta-analysis. For example, although the proportion of prime trials does not appear to influence effect size within the observed range of [0.25, 1], values outside this range may lead to differences in effectiveness. Second, these parameters did not moderate the results when collapsing across other parameters, but the various parameters may influence behavior priming in interaction with features of the task or sample. Third, moderation of effect size is not the only concern researchers have when creating studies and researchers may still have valid reasons for making certain parameter choices. For instance, even though the proportion of priming to neutral trials did not moderate our effect sizes in our analyses, it may influence the likelihood that participants spontaneously identify the researchers' hypotheses. Therefore, the lack of moderation found in our synthesis does not imply that these parameters are unimportant and should be ignored or chosen carelessly. Rather, the lack of moderation simply indicates that there is no evidence that researchers should prefer one level of these parameters to the other when considering effect size.

Finally, we encourage all researchers that argue that they are studying goal effects to include more tests of the temporal escalation and satiation. In our dataset, less than a third of the effect sizes tested introduced a delay between the priming and the dependent measure (e.g., Chartrand & Bargh, 1996). We urge more researchers to test (and journal editors to require tests of) these goal criteria to better categorize the phenomenon that future work may gain a better consensus over the underlying mechanisms of behavior priming.

Sample Sizes and a Potential Resolution to the Replicability Crisis Within Priming Research

In our meta-analysis, the majority of the effect sizes were calculated for between-subjects comparisons between priming and control conditions based on an average cell size $n = 24.29$ ($SD = 10.84$). Given that the fixed-effects average effect size with outliers was $d = 0.332$, an n of that the typical behavior priming study was severely underpowered to detect effects. Specifically, as shown in Table 5, with $d = 0.332$, $n = 24.29$, and $\alpha = .05$, a two-sample t test has a power of merely .20 to detect a significant effect if it exists. In contrast, obtaining a power of .80 requires a minimum sample size of $n = 143$ participants per condition, or $n = 159$ after excluding the nine outliers from our sensitivity analyses. Even if researchers of these effects had expected larger-than-average effects, the current average sample size of $n < 25$ per condition is inadequate and only appropriate for the unlikely effect size of $d \geq 0.81$.

Our estimates and considerations depicted in Table 5 have major implications for the current replicability crisis in the behavior priming arena. With the estimated average power of .20 obtained from our synthesized literature, replication attempts should produce nonsignificant results at a rate of 4:1, making a large number of failed replications uninformative about the existence of behavior priming effects. For example, Doyen et al. (2012) presented two failures (one nonsignificant reversal and one almost-null effect) to replicate Bargh et al.'s (1996) Experiment 2a. However, Doyen et al.'s sample sizes of 120 (Study 1) and 50 (Study 2) had power of .44 and .21, respectively. Thus, both of these replication attempts had a higher probability of failure than success, and the failures are not indicative of an earlier Type I error. The current replicability crisis may obey widespread misinterpretations of null results from low powered studies rather than appropriate acceptance of the null hypothesis in high powered studies. In this context, our meta-analysis synthesized effect size estimates from a very large number of studies to produce a high-power estimate of behavior priming effects, leading to the most credible assessment up to this point.

Our sample size recommendations of $n = 143$ (or $n = 159$) per condition are in line with current recommendations that sample sizes for two-group studies should use about $n = 100$ per condition (e.g., Fraley & Vazire, 2014). By using larger samples, researchers will increase the probability of detecting true effects, replicating previously identified effects, and improving the credibility of our scientific findings (Button et al., 2013; Ioannidis, 2005; Prinz et al., 2011; Sullivan, 2007).[3] Overall though, we hope that our approach to addressing replicability within behavioral priming research can serve as a model for other areas of science that are also experiencing drops in confidence (Button et al., 2013; Sullivan, 2007).

Future Directions

Despite the strengths of the current meta-analysis, there are limitations that remain to be addressed in future research. First, the current study only examined word primes that were directly related to the behavioral outcome assessed in the study. This decision may limit the generalizability of our results because other types of primes (e.g., pictures, physical body poses) and primes that have a less clear relation with the dependent measure may produce different effects. For example, stimuli such as stereotypes (e.g., elderly; e.g., Bargh et al., 1996, Studies 2a and 2b) and money (e.g., Vohs, Mead, & Goode, 2006), which have received attention in some replication attempts (e.g., Doyen et al., 2012; Klein et al., 2014; Pashler et al., 2008; Tate, 2009), may yield weaker effects because their relation to behavior is more nuanced. Even more recently, the literature has focused on nonword primes (e.g., faces; e.g., Hill & Durante, 2011) that may be intentionally introduced in the field (e.g., a picture of a woman winning a race; e.g., Shantz & Latham, 2009) or occur naturally (e.g., litter; e.g., Keizer, Lindenberg, & Steg, 2008). Future research should address these questions by examining whether prime type and prime specificity moderate behavioral priming effects in a potentially broader meta-analysis. Similarly, our results specifically addressed behavioral effects of primes, and the presence (or lack) of moderation by various parameters should not be expected to generalize to other outcomes, such as cognitive or affective dependent measures. The field of priming is sufficiently rich to allow for broad integration and understanding of these different findings.

Several other considerations deserve discussion. First, meta-analysis is a correlational research method. Before strong causal inferences are made concerning the relations we have identified, these claims must be experimentally addressed by manipulating goal value or whatever moderator researchers seek to understand. Another criticism may be that our meta-analysis does not account for priming effects with opposite stimuli (e.g., action vs. inaction, thrift vs. prestige, competition vs. collaboration). Priming opposites is clearly ambiguous for determining which prime has an effect, or whether both primes differ from a baseline in an unexpected way. For example, fast and slow primes may lead to the finding that fast primes lead to faster completion times than slow primes. However, the absence of a control prevents ascertaining whether the fast primes are expediting performance or slow primes are halting performance, or both. Of course, estimated effects may be much larger when comparing opposites but we chose to be conservative and precise.

Readers may also wonder whether our meta-analysis might have had an adequate number of unpublished experiments. Trying to

[3] We recognize that running studies with such large sample sizes can potentially be prohibitively expensive or a suboptimal allocation of resources. Requiring having almost 300 subjects to run a two-cell design may be difficult without relying entirely on Amazon Mechanical Turk (Goodman et al., 2013). An alternative is for behavioral priming to pioneer stronger within-subject designs that can enable more observations from fewer subjects (Rosenthal & Rosnow, 2008). Indeed, some other researchers are starting to explore these designs with fruitful results (Payne, Brown-Iannuzzi, & Loersch, in preparation).

Table 5
Analysis of Statistical Power for Replication Studies

Effect size calculation	FE effect size ($d_.$)	Average N	Power of average study from our meta-analysis	Cell size for power = .80	Power: Doyen et al., Study 1 (N = 120)	Power: Harris et al. (2013), Study 1 (N = 106)
Including outliers (e = 352)	.332	48.57 (n = 24.29)	.20	143	.44	.39
Excluding outliers (e = 343)	.315	49.17 (n = 24.59)	.19	159	.40	.36

Note. e is number of effect sizes, N is number of participants, n is number of participants per cell.

sample unpublished effect sizes, we requested such data from hundreds of authors and obtained a substantial number of unpublished observations (over 20%), comparable to that in van den Bussche et al. (2009) and higher than that in DeCoster and Claypool (2004). Furthermore, we used multiple meta-analytic methods to address concerns about publication bias (e.g., Duval & Tweedie, 2000a, 2000b; Rosenthal, 1979; Simonsohn et al., 2014; Wang & Bushman, 1998, 1999); thus, moving our understanding of the phenomenon in an important way.

Ethical Considerations of Priming

A small but robust behavioral priming effect suggests potential real world implications of priming in such diverse areas as student motivation (e.g., Bargh et al., 2001), health behavior (e.g., Connell & Mayor, 2013; Wryobeck & Chen, 2003), and altruism (e.g., Macrae & Johnston, 1998). Priming could have the potential to accompany recent decision making findings on defaults and nudges that improve economic and social outcomes such as savings and organ donation (Johnson & Goldstein, 2003; Thaler & Benartzi, 2004). Although the effect size of behavioral priming from this meta-analysis may appear small, its effect on the scale of thousands or hundreds of thousands of individuals could yield a sizable difference (Hunter & Schmidt, 2004). For example, according to our meta-analysis, the odds that people primed with eating will eat can be readily ascertained by transforming our average d (from fixed effects analyses when excluding outliers) into an odds and yield 1.78. An odds = 1.77 implies a 77% increase in the behavior as a consequence of the prime. Moreover, for high-value scenarios (1 *SD* above on the goal index, d = 0.450), the fixed effects d would yield an odds of 2.26, which would suggest a 126% boost in the effects of priming for scenarios in which individuals who valued a goal are primed.

However, despite the potent benefits from priming, there may be both backlash from individuals if they recognize they are being influenced (Brehm, 1966; Clee & Wicklund, 1980; Friestad & Wright, 1994) and concern over recipients' consent. Individuals who become cognizant of subtle attempts to shift their behavior may react in ways opposite of what the priming intended; thus, severely undermining the positive influence of priming (Clee & Wicklund, 1980). Further, in contrast to the imperative of informed consent to health and psychological treatment, the priming in this literature occurs without explicit awareness of the prime's presence or its effect. How to obtain consent without eliminating efficacy remains to be resolved before priming can be used with clinical objectives.

A potentially controversial side of priming is also well known if used in consumer advertising or political propaganda. The fable of James Vicary's success at increasing sales in movie theatres is always a reminder of the unethical implications of priming (Strahan et al., 2002). Although Vicary's claim and the mere existence of any of the research he reported is now known to be false, any effect of priming brings similar questions (Gibson & Zielaskowski, 2013; Strahan et al., 2002). Even though priming may be implemented with good intentions, there are also scenarios in which priming may become or be perceived to be coercion. However, it is important to note that priming cannot instill and trigger goals people do not possess (Bargh et al., 2010) and individuals may even react against goals they devalue (e.g., Hart & Albarracin, 2009).

Conclusion

The debate over whether incidentally presented stimuli can affect behavior has intensified in recent years after an explosive growth in research and several open science initiatives (Cesario, 2014; Dijksterhuis, 2014; Harris et al., 2013; Shanks et al., 2013).

The results of this meta-analysis showed small but robust significant effects of word primes on behavior and indicated that these effects obey the principles of value and goal satiation. Future work should continue to unify the theory and boundary conditions of behavioral priming effects as part of research attention to replicability. We hope that our meta-analysis will contribute to advance this mission.

References

*References marked with an asterisk indicate studies included in the meta-analysis.

*Aarons, G. A. (1996). *Does activation of alcohol expectancies mediate drinking*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. AAT 9637018).

*Aarts, H., Custers, R., & Holland, R. W. (2007). The nonconscious cessation of goal pursuit: When goals and negative affect are coactivated. *Journal of Personality and Social Psychology, 92,* 165–178. http://dx.doi.org/10.1037/0022-3514.92.2.165

Aarts, H., Custers, R., & Veltkamp, M. (2008). Goal priming and the affective-motivational route to nonconscious goal pursuit. *Social Cognition, 26,* 555–577. http://dx.doi.org/10.1521/soco.2008.26.5.555

Aarts, H., Gollwitzer, P. M., & Hassin, R. R. (2004). Goal contagion: Perceiving is for pursuing. *Journal of Personality and Social Psychology, 87,* 23–37.

*Albarracín, D., & Handley, I. M. (2011). The time for doing is not the time for change: Effects of general action and inaction goals on attitude retrieval and attitude change. *Journal of Personality and Social Psychology, 100,* 983–998. http://dx.doi.org/10.1037/a0023245

*Albarracín, D., Handley, I. M., Noguchi, K., McCulloch, K. C., Li, H., Leeper, J., . . . Hart, W. P. (2008). Increasing and decreasing motor and cognitive output: A model of general action and inaction goals. *Journal of Personality and Social Psychology, 95,* 510–523. http://dx.doi.org/10.1037/a0012833

*Albarracin, D., & Hart, W. (2011). Positive mood + action = negative mood + inaction: Effects of general action and inaction concepts on decisions and performance as a function of affect. *Emotion, 11,* 951–957. http://dx.doi.org/10.1037/a0024130

*Albarracin, D., Wang, W., & Leeper, J. (2009). Immediate increase in food intake following exercise messages. *Obesity, 17,* 1451–1452.

*Al-Ubaydli, O., Houser, D., Nye, J., Paganelli, M. P., & Pan, X. S. (2013). The causal effect of market priming on trust: An experimental investigation using randomized control. *PLoS ONE, 8,* e55968. http://dx.doi.org/10.1371/journal.pone.0055968

Atkinson, J. W. (1974). Strength of motivation and efficiency of performance. In J. W. Atkinson & J. O. Raynor (Eds.), *Motivation and achievement* (pp. 193–218). Washington, DC: V. H. Winston & Sons.

Atkinson, J. W., & Birch, D. (1970). *A dynamic theory of action*. New York, NY: Wiley.

*Bar-Anan, Y., Wilson, T. D., & Hassin, R. R. (2010). Inaccurate self-knowledge formation as a result of automatic behavior. *Journal of Experimental Social Psychology, 46,* 884–894. http://dx.doi.org/10.1016/j.jesp.2010.07.007

Bargh, J. A. (1990). Auto-motives: Preconscious determinants of social interaction. In E. T. Higgins & R. M. Sorrentino (Eds.), *Handbook of motivation and cognition* (Vol. 2, pp. 93–130). New York, NY: Guilford.

Bargh, J. A. (1994). The four horsemen of automaticity: Awareness, efficiency, intention, and control in social cognition. In R. S. Wyer, Jr., & T. K. Srull (Eds.), *Handbook of social cognition* (2nd ed., pp. 1–40). Hillsdale, NJ: Erlbaum.

Bargh, J. A. (2002). Losing consciousness: Automatic influences on consumer judgment, behavior, and motivation. *The Journal of Consumer Research, 29,* 280–285. http://dx.doi.org/10.1086/341577

Bargh, J. A. (2012). Priming effects replicate just fine, thanks. *Psychology Today*. Retrieved from https://www.psychologytoday.com/blog/the-natural-unconscious/201205/priming-effects-replicate-just-fine-thanks

Bargh, J. A., & Chartrand, T. L. (2000). Studying the mind in the middle: A practical guide to priming and automaticity research. In H. Reis & C. Judd (Eds.), *Handbook of research methods in social psychology* (pp. 253–285). New York, NY: Cambridge University Press.

*Bargh, J. A., Chen, M., & Burrows, L. (1996). Automaticity of social behavior: Direct effects of trait construct and stereotype-activation on action. *Journal of Personality and Social Psychology, 71,* 230–244. http://dx.doi.org/10.1037/0022-3514.71.2.230

Bargh, J. A., & Gollwitzer, P. M. (1994). Environmental control of goal-directed action: Automatic and strategic contingencies between situations and behavior. *Nebraska Symposium on Motivation, 41,* 71–124.

*Bargh, J. A., Gollwitzer, P. M., Lee-Chai, A., Barndollar, K., & Trötschel, R. (2001). The automated will: Nonconscious activation and pursuit of behavioral goals. *Journal of Personality and Social Psychology, 81,* 1014–1027. http://dx.doi.org/10.1037/0022-3514.81.6.1014

Bargh, J. A., Gollwitzer, P. M., & Oettingen, G. (2010). Motivation. In S. Fiske, D. T. Gilbert, & G. Lindzay (Eds.), *Handbook of social psychology* (pp. 268–316). New York, NY: Wiley. http://dx.doi.org/10.1002/9780470561119.socpsy001008

Bargh, J. A., Lombardi, W. J., & Higgins, E. T. (1988). Automaticity of chronically accessible constructs in person x situation effects on person perception: It's just a matter of time. *Journal of Personality and Social Psychology, 55,* 599–605. http://dx.doi.org/10.1037/0022-3514.55.4.599

Bargh, J. A., & Morsella, E. (2009). Unconscious behavioral guidance systems. In C. Agnew, D. Carlston, W. Graziano, & J. Kelly (Eds.), *Then a miracle occurs: Focusing on behavior in social psychological theory and research*. New York, NY: Oxford University Press. Retrieved from http://www.oxfordscholarship.com/view/10.1093/acprof:oso/9780195377798.001.0001/acprof-9780195377798-chapter-6. http://dx.doi.org/10.1093/acprof:oso/9780195377798.003.0006

Bargh, J. A., & Pietromonaco, P. (1982). Automatic information processing and social perception: The influence of trait information presented outside of conscious awareness on impression formation. *Journal of Personality and Social Psychology, 43,* 437–449. http://dx.doi.org/10.1037/0022-3514.43.3.437

Bargh, J. A., Schwader, K. L., Hailey, S. E., Dyer, R. L., & Boothby, E. J. (2012). Automaticity in social-cognitive processes. *Trends in Cognitive Sciences, 16,* 593–605. http://dx.doi.org/10.1016/j.tics.2012.10.002

Bartlett, T. (2013, January 30). The power of suggestion. *The Chronicle of Higher Education*. Retrieved from http://chronicle.com/article/Power-of-Suggestion/136907/

Berkowitz, L. (1984). Some effects of thoughts on anti- and prosocial influences of media events: A cognitive-neoassociation analysis. *Psychological Bulletin, 95,* 410–427. http://dx.doi.org/10.1037/0033-2909.95.3.410

Birch, D., Atkinson, J. W., & Bongort, K. (1975). Cognitive control of action. In B. Weiner (Ed.), *Cognitive views of human motivation* (pp. 71–84). New York, NY: Academic Press.

*Boland, W. A., Connell, P. M., & Vallen, B. (2013). Time of day effects on the regulation of food consumption after activation of health goals. *Appetite, 70,* 47–52. http://dx.doi.org/10.1016/j.appet.2013.06.085

Borenstein, M., Hedges, L. V., Higgins, J. P. T., & Rothstein, H. R. (2009). *Introduction to meta-analysis (statistics in practice)*. Chinchester: Wiley. http://dx.doi.org/10.1002/9780470743386

Borman, G. D., & Grigg, J. A. (2009). Visual and Narrative Interpretation. In J. C. Valentine, H. Cooper, & L. V. Hedges (Eds.), *The handbook of research synthesis and meta-analysis* (pp. 497–519). New York NY: Russell Sage.

Brehm, J. W. (1966). *A theory of psychological reactance*. Oxford: Academic Press.

Brunyé, T. T., & Taylor, H. A. (2009). When goals constrain: Eye movements and memory for goal-oriented map study. *Applied Cognitive Psychology, 23,* 772–787. http://dx.doi.org/10.1002/acp.1508

*Budden, J. S. (2007). *Achievement motivation: The effects of conscious, chronic, and nonconscious goals on task performance*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. AAT 3272144).

Button, K. S., Ioannidis, J. P., Mokrysz, C., Nosek, B. A., Flint, J., Robinson, E. S., & Munafò, M. R. (2013). Power failure: Why small sample size undermines the reliability of neuroscience. *Nature Reviews Neuroscience, 14,* 365–376. http://dx.doi.org/10.1038/nrn3475

Cameron, C. D., Brown-Iannuzzi, J. L., & Payne, B. K. (2012). Sequential priming measures of implicit social cognition: A meta-analysis of associations with behavior and explicit attitudes. *Personality and Social Psychology Review, 16,* 330–350. http://dx.doi.org/10.1177/1088868312440047

*Capa, R. L., Cleeremans, A., Bustin, G. M., & Hansenne, M. (2011). Long-lasting effect of subliminal processes on cardiovascular responses and performance. *International Journal of Psychophysiology, 81,* 22–30. http://dx.doi.org/10.1016/j.ijpsycho.2011.04.001

Carpenter, W. B. (1852). On the influence of suggestion in modifying and directing muscular movement, independent of volition. *Notices of the Proceedings at the Meetings of the Members of the Royal Institution, 1,* 147–153.

Carver, C. S., Ganellen, R. J., Froming, W. J., & Chambers, W. (1983). Modeling: An analysis in terms of category accessibility. *Journal of Experimental Social Psychology, 19,* 403–421. http://dx.doi.org/10.1016/0022-1031(83)90019-7

Cesario, J. (2014). Priming, replication, and the hardest science. *Perspectives on Psychological Science, 9,* 40–48. http://dx.doi.org/10.1177/1745691613513470

Cesario, J., Plaks, J. E., & Higgins, E. T. (2006). Automatic social behavior as motivated preparation to interact. *Journal of Personality and Social Psychology, 90,* 893–910. http://dx.doi.org/10.1037/0022-3514.90.6.893

*Chartrand, T. L. (1999). *Consequences of success and failure at automatic goal pursuit for mood, self-efficacy, and subsequent performance*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. AAT 9945261).

*Chartrand, T. L., & Bargh, J. A. (1996). Automatic activation of impression formation and memorization goals: Nonconscious goal priming reproduces effects of explicit task instructions. *Journal of Personality and Social Psychology, 71,* 464–478. http://dx.doi.org/10.1037/0022-3514.71.3.464

Chartrand, T. L., & Bargh, J. A. (1999). The chameleon effect: The perception-behavior link and social interaction. *Journal of Personality and Social Psychology, 76,* 893–910. http://dx.doi.org/10.1037/0022-3514.76.6.893

*Chartrand, T. L., Huber, J., Shiv, B., & Tanner, R. J. (2008). Nonconscious goals and consumer choice. *Journal of Consumer Research, 35,* 189–201. http://dx.doi.org/10.1086/588685

Clee, M. A., & Wicklund, R. A. (1980). Consumer behavior and psychological reactance. *Journal of Consumer Research, 6,* 389–405. http://dx.doi.org/10.1086/208782

Cohen, J., Cohen, P., West, S. G., & Aiken, L. S. (2003). *Applied multiple regression/correlation analysis for the behavioral sciences* (3rd ed.). New York, NY: Taylor and Francis Group.

Collins, A. M., & Loftus, E. F. (1975). A spreading-activation theory of semantic processing. *Psychological Review, 82,* 407–428. http://dx.doi.org/10.1037/0033-295X.82.6.407

*Connell, P. M., & Mayor, L. F. (2013). Activating health goals reduces (increases) hedonic evaluation of food brands for people who harbor highly positive (negative) affect toward them. *Appetite, 65,* 159–164. http://dx.doi.org/10.1016/j.appet.2013.02.014

Cooper, H. (2010). *Research synthesis and meta-analysis*. Thousand Oaks, CA: Sage.

Cooper, H., & Hedges, L. V. (1994). *The handbook of research synthesis*. New York, NY: The Russell Sage Foundation.

Cooper, H., & Hedges, L. V. (2009a). Research synthesis as a scientific process. In H. Cooper, L. V. Hedges, & J. C. Valentine (Eds.), *The handbook of research synthesis and meta-analysis* (2nd ed., pp. 3–16). New York, NY: Russell Sage Foundation.

Cooper, H., & Hedges, L. V. (2009b). Potentials and limitations. In H. Cooper, L. V. Hedges, & J. C. Valentine (Eds.), *The handbook of research synthesis and meta-analysis* (2nd ed., pp. 561–572). New York, NY: Russell Sage Foundation.

*Cougle, J. R., & Hawkins, K. A. (2013). Priming of courageous behavior: Contrast effects in spider fearful women. *Journal of Clinical Psychology, 69,* 896–902. http://dx.doi.org/10.1002/jclp.21961

*Crone, T. (2010). The effects of multiple nonconscious goal primes on goal-related behavior. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. AAT 3407344).

*Crusius, J., & Mussweiler, T. (2012). To achieve or not to achieve? Comparative mindsets elicit assimilation and contrast in goal priming. *European Journal of Social Psychology, 42,* 780–788. http://dx.doi.org/10.1002/ejsp.873

Custers, R., Maas, M., Wildenbeest, M., & Aarts, H. (2008). Nonconscious goal pursuit and the surmounting of physical and social obstacles. *European Journal of Social Psychology, 38,* 1013–1022. http://dx.doi.org/10.1002/ejsp.484

DeCoster, J. (2009). *Meta-analysis notes*. Retrieved from http://www.stat-help.com/Meta%20analysis%202009-07-31.pdf

DeCoster, J., & Claypool, H. M. (2004). A meta-analysis of priming effects on impression formation supporting a general model of informational biases. *Personality and Social Psychology Review, 8,* 2–27. http://dx.doi.org/10.1207/S15327957PSPR0801_1

DeMarree, K. G., Loersch, C., Briñol, P., Petty, R. E., Payne, B. K., & Rucker, D. D. (2012). From primed construct to motivated behavior: Validation processes in goal pursuit. *Personality and Social Psychology Bulletin, 38,* 1659–1670. http://dx.doi.org/10.1177/0146167212458328

Dijksterhuis, A., & Aarts, H. (2010). Goals, attention, and (un)consciousness. *Annual Review of Psychology, 61,* 467–490. http://dx.doi.org/10.1146/annurev.psych.093008.100445

Dijksterhuis, A., Aarts, H., Bargh, J. A., & Van Knippenberg, A. (2000). On the relation between associative strength and automatic behavior. *Journal of Experimental Social Psychology, 36,* 531–544. http://dx.doi.org/10.1006/jesp.2000.1427

Dijksterhuis, A. (2014). Welcome back theory! *Perspectives on Psychological Science, 9,* 72–75. http://dx.doi.org/10.1177/1745691613513472

Doyen, S., Klein, O., Pichon, C. L., & Cleeremans, A. (2012). Behavioral priming: It's all in the mind, but whose mind? *PLoS ONE, 7,* e29081. http://dx.doi.org/10.1371/journal.pone.0029081

Duval, S., & Tweedie, R. (2000a). A nonparametric 'trim and fill' method of accounting for publication bias in meta-analysis. *Journal of the American Statistical Association, 95,* 89–99.

Duval, S., & Tweedie, R. (2000b). Trim and fill: A simple funnel-plot-based method of testing and adjusting for publication bias in meta-analysis. *Biometrics, 56,* 455–463. http://dx.doi.org/10.1111/j.0006-341X.2000.00455.x

Egger, M., Smith, G. D., Schneider, M., & Minder, C. (1997). Bias in meta-analysis detected by a simple, graphical test. *British Medical Journal, 315,* 629–634. http://dx.doi.org/10.1136/bmj.315.7109.629

*Eitam, B., Hassin, R. R., & Schul, Y. (2008). Nonconscious goal pursuit in novel environments: The case of implicit learning. *Psychological Science, 19,* 261–267. http://dx.doi.org/10.1111/j.1467-9280.2008.02078.x

*Eitam, B., & Higgins, T. E. (2008). *The why of nonconscious motivation: Primes ad purpose when interpreting our actions*. [Unpublished].

*Engeser, S. (2009). Nonconscious activation of achievement goals moderated by word class and the explicit achievement motive? *Swiss Journal of Psychology, 68,* 193–200. http://dx.doi.org/10.1024/1421-0185.68.4.193

*Engeser, S., Wendland, M., & Rheinberg, F. (2006). Nonconscious activation of behavioral goals, a methodologically refined replication. *Psychological Reports, 99,* 963–970.

Erb, H., Bioy, A., & Hilton, D. J. (2002). Choice preferences without inferences: Subconscious priming of risk attitudes. *Journal of Behavioral Decision Making, 15,* 251–262. http://dx.doi.org/10.1002/bdm.416

Fazio, R. H. (2001). On the automatic activation of associated evaluations: An overview. *Cognition and Emotion, 15,* 115–141. http://dx.doi.org/10.1080/02699930125908

Fazio, R. H., Sanbonmatsu, D. M., Powell, M. C., & Kardes, F. R. (1986). On the automatic activation of attitudes. *Journal of Personality and Social Psychology, 50,* 229–238. http://dx.doi.org/10.1037/0022-3514.50.2.229

*Fishbach, A., Dhar, R., & Zhang, Y. (2006). Subgoals as substitutes or complements: The role of goal accessibility. *Journal of Personality and Social Psychology, 91,* 232–242.

Fishbach, A., & Ferguson, M. J. (2007). The goal construct in social psychology. In A. W. Kruglanski & T. E. Higgins (Eds.), *Social psychology: Handbook of basic principles* (pp. 490–515). New York, NY: Guilford.

*Fishtein, J. (1999). *Priming attachment goals: Effects on disclosure*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. AAT 9932311).

Fitzsimons, G. M., & Bargh, J. A. (2003). Thinking of you: Nonconscious pursuit of interpersonal goals associated with relationship partners. *Journal of Personality and Social Psychology, 84,* 148–164. http://dx.doi.org/10.1037/0022-3514.84.1.148

Fitzsimons, G. M., Chartrand, T. L., & Fitzsimons, G. J. (2008). Automatic effects of brand exposure on motivated behavior: How apple makes you "think different." *Journal of Consumer Research, 35,* 21–35. http://dx.doi.org/10.1086/527269

Fong, G. T. (1984). *The behavioral consequences of schema activation*. (Dissertation). Retrieved from ProQuest Dissertations and Theses.

Forbach, G. B., Stanners, R. F., & Hochhaus, L. (1974). Repetition and practice effects in a lexical decision task. *Memory & Cognition, 2,* 337–339. http://dx.doi.org/10.3758/BF03209005

Förster, J., Liberman, N., & Friedman, R. S. (2007). Seven principles of goal activation: A systematic approach to distinguishing goal priming from priming of non-goal constructs. *Personality and Social Psychology Review, 11,* 211–233. http://dx.doi.org/10.1177/1088868307303029

Förster, J., Liberman, N., & Higgins, E. T. (2005). Accessibility from active and fulfilled goals. *Journal of Experimental Social Psychology, 41,* 220–239. http://dx.doi.org/10.1016/j.jesp.2004.06.009

Fraley, R. C., & Vazire, S. (2014). The N-pact factor: Evaluating the quality of empirical journals with respect to sample size and statistical power. *PLoS ONE, 9,* e109019. http://dx.doi.org/10.1371/journal.pone.0109019

Friedman, R. (2007). *The sound of motivation: Utilizing the doppler effect to activate avoidance motivation*. (Dissertation). Retrieved from ProQuest Dissertations and Theses.

*Friedman, R. S., McCarthy, D. M., Pedersen, S. L., & Hicks, J. A. (2009). Alcohol expectancy priming and drinking behavior: The role of compatibility between prime and expectancy content. *Psychology of Addictive Behaviors, 23,* 329–333. http://dx.doi.org/10.1037/a0015704

Friestad, M., & Wright, P. (1994). The persuasion knowledge model: How people cope with persuasion attempts. *Journal of Consumer Research, 21,* 1–31. http://dx.doi.org/10.1086/209380

*Gantman, A. P., Adriaanse, M., Keller, L., Gollwitzer, P. M., & Oettingen, G. (2014). Holding a mirror up to priming: Self-awareness moderates prime-to-behavior effects in accordance with self-standards. [Retrieved from author].

*Geers, A. L., Weiland, P. E., Kosbab, K., Landry, S. J., & Helfer, S. G. (2005). Goal activation, expectations, and the placebo effect. *Journal of Personality and Social Psychology, 89,* 143–159.

*Gendolla, G. H. E., & Silvestrini, N. (2010). The implicit "go": Masked action cues directly mobilize mental effort. *Psychological Science, 21,* 1389–1393. http://dx.doi.org/10.1177/0956797610384149

Gibson, B., & Zielaskowski, K. (2013). Subliminal priming of winning images prompts increased betting in slot machine play. *Journal of Applied Social Psychology, 43,* 106–115. http://dx.doi.org/10.1111/j.1559-1816.2012.00985.x

*Ginsberg, F., Rohmer, O., & Louvet, E. (2012). Priming of disability and elderly stereotype in motor performance: Similar or specific effects? *Perceptual and Motor Skills, 114,* 397–406. http://dx.doi.org/10.2466/07.17.PMS.114.2.397-406

Gleser, L. J., & Olkin, I. (1994). Stochastically dependent effect sizes. In H. M. Cooper & L. V. Hedges (Eds.), *Handbook of research synthesis* (pp. 339–355). New York, NY: Russell Sage Foundation.

*Gollwitzer, P. M., Sheeran, P., Trötschel, R., & Webb, T. L. (2011). Self-regulation of priming effects on behavior. *Psychological Science, 22,* 901–907. http://dx.doi.org/10.1177/0956797611411586

Gollwitzer, P. M., & Wicklund, R. A. (1985). The pursuit of self-defining goals. In J. Kuhl & J. Beckmann (Eds.), *Action control: From cognition to behavior* (pp. 61–85). Heidelberg: Springer-Verlag. http://dx.doi.org/10.1007/978-3-642-69746-3_4

Goodman, J. K., Cryder, C. E., & Cheema, A. (2013). Data collection in a flat world: The strengths and weaknesses of mechanical Turk samples. *Journal of Behavioral Decision Making, 26,* 213–224. http://dx.doi.org/10.1002/bdm.1753

*Guinote, A., Weick, M., & Cai, A. (2012). Does power magnify the expression of dispositions? *Psychological Science, 23,* 475–482. http://dx.doi.org/10.1177/0956797611428472

*Hall, C. E. (2007). *The effect of perceptual fluency on goal pursuit*. (Dissertation). Retrieved from ProQuest Dissertations and Theses.

*Harrell, A. (2012). Do religious cognitions promote prosociality? *Rationality and Society, 24,* 463–482. http://dx.doi.org/10.1177/1043463112463930

*Harris, C. R., Coburn, N., Rohrer, D., & Pashler, H. (2013). Two failures to replicate high-performance-goal priming effects. *PLoS ONE, 8,* e72467. http://dx.doi.org/10.1371/journal.pone.0072467

*Hart, W., & Albarracín, D. (2009). The effects of chronic achievement motivation and achievement primes on the activation of achievement and fun goals. *Journal of Personality and Social Psychology, 97,* 1129–1141. http://dx.doi.org/10.1037/a0017146

*Hart, W., & Albarracin, D. (2012). Craving activity and losing objectivity: Effects of general action concepts on approach to decision-consistent information. *Social Psychological & Personality Science, 3,* 55–62. http://dx.doi.org/10.1177/1948550611408620

*Hart, W., & Gable, P. A. (2013). Motivating goal pursuit: The role of affect motivational intensity and activated goals. *Journal of Experimental Social Psychology, 49,* 922–926. http://dx.doi.org/10.1016/j.jesp.2013.05.002

Hassin, R. R. (2013). Yes it can: On the functional abilities of the human unconscious. *Perspectives on Psychological Science, 8,* 195–207. http://dx.doi.org/10.1177/1745691612460684

*Hassin, R. R., Bargh, J. A., & Zimerman, S. (2009). Automatic and flexible: The case of non-conscious goal pursuit. *Social Cognition, 27,* 20–36. http://dx.doi.org/10.1521/soco.2009.27.1.20

Heath, C., Larrick, R. P., & Wu, G. (1999). Goals as reference points. *Cognitive Psychology, 38,* 79–109. http://dx.doi.org/10.1006/cogp.1998.0708

Heckhausen, H. (1991). *Motivation and action*. Berlin: Springer-Verlag. http://dx.doi.org/10.1007/978-3-642-75961-1

Hedges, L. V. (1981). Distribution theory for Glass's estimator of effect size and related estimators. *Journal of Educational Statistics, 6,* 107–128. http://dx.doi.org/10.2307/1164588

Hedges, L. V., & Olkin, I. (1985). *Statistical methods for meta-analysis*. Orlando, FL: Academic Press.

*Hepler, J., & Albarracin, D. (2009). Change blindness. [Unpublished].

*Hepler, J., & Albarracin, D. (2009). Visual search. [Unpublished].

Hepler, J., & Albarracin, D. (2013). Complete unconscious control: Using (in)action primes to demonstrate completely unconscious activation of inhibitory control mechanisms. *Cognition, 128,* 271–279. http://dx.doi.org/10.1016/j.cognition.2013.04.012

Higgins, E. T. (1989). Knowledge accessibility and activation: Subjectivity and suffering from unconscious sources. In J. S. Uleman & J. A. Bargh (Eds.), *Unintended thought: The limits of awareness, intention and control* (pp. 75–123). New York, NY: Guilford Press.

Higgins, E. T., Bargh, J. A., & Lombardi, W. J. (1985). Nature of priming effects on categorization. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 11,* 59–69. http://dx.doi.org/10.1037/0278-7393.11.1.59

Higgins, E. T., & Chaires, W. M. (1980). Accessibility of interrelational constructs: Implications for stimulus encoding and creativity. *Journal of Experimental Social Psychology, 16,* 348–361. http://dx.doi.org/10.1016/0022-1031(80)90027-X

Higgins, E. T., & King, G. (1981). Accessibility of social constructs: Information processing consequences of individual and contextual variability. In N. Cantor & J. Kihlstrom (Eds.), *Personality, cognition, and social interaction* (pp. 69–121). Hillsdale, NJ: Erlbaum.

Higgins, E. T., Rholes, W. S., & Jones, C. R. (1977). Category accessibility and impression formation. *Journal of Experimental Social Psychology, 13,* 141–154. http://dx.doi.org/10.1016/S0022-1031(77)80007-3

Higgins, J. P. T., & Thompson, S. G. (2002). Quantifying heterogeneity in a meta-analysis. *Statistics in Medicine, 21,* 1539–1558. http://dx.doi.org/10.1002/sim.1186

Hill, S. E., & Durante, K. M. (2011). Courtship, competition, and the pursuit of attractiveness: Mating goals facilitate health-related risk taking and strategic risk suppression in women. *Personality and Social Psychology Bulletin, 37,* 383–394. http://dx.doi.org/10.1177/0146167210395603

*Hodgins, K. S., Yacko, H. A., & Gottlieb, E. (2006). Autonomy and nondefensiveness. *Motivation and Emotion, 30,* 283–293. http://dx.doi.org/10.1007/s11031-006-9036-7

Huang, J. Y., & Bargh, J. A. (2008). Peak of desire: Activating the mating goal changes life-stage preferences across living kinds. *Psychological Science, 19,* 573–578. http://dx.doi.org/10.1111/j.1467-9280.2008.02125.x

Huedo-Medina, T. B., Sánchez-Meca, J., Marín-Martínez, F., & Botella, J. (2006). Assessing heterogeneity in meta-analysis: Q statistic or I2 index? *Psychological Methods, 11,* 193–206. http://dx.doi.org/10.1037/1082-989X.11.2.193

Hull, J. G., Slone, L. B., Meteyer, K. B., & Matthews, A. R. (2002). The nonconsciousness of self-consciousness. *Journal of Personality and Social Psychology, 83,* 406–424.

Hunter, J. E., & Schmidt, F. (2004). *Methods of meta-analysis* (2nd ed.). Thousand Oaks, CA: Sage.

Huttenlocher, J., Higgins, E. T., & Clark, H. H. (1971). Adjectives, comparatives, and syllogisms. *Psychological Review, 78,* 487–504. http://dx.doi.org/10.1037/h0031858

Huttenlocher, J., Higgins, E. T., & Clark, H. H. (1972). On reasoning, congruence, and other matters. *Psychological Review, 79,* 420–427. http://dx.doi.org/10.1037/h0033127

Ioannidis, J. P. (2005). Why most published research findings are false. *PLoS Medicine, 2,* e124.

*Jacobson. (1999). *The effects of chronic accuracy goals on social judgment: The case of causal uncertainty*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. AAT 9941348).

James, W. (1890). *Principles of psychology*. New York, NY: Holt. http://dx.doi.org/10.1037/11059-000

*Jefferis, V. E., & Fazio, R. H. (2008). Accessibility as input: The use of construct accessibility as information to guide behavior. *Journal of Experimental Social Psychology, 44,* 1144–1150. http://dx.doi.org/10.1016/j.jesp.2008.02.002

Jia, L. (2012). *Depletion-activated counteractive control: The informational value of ego depletion in goal pursuit*. (Doctoral Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. AAT 3527754).

Johnson, B. T., & Eagly, A. H. (2000). Quantitative synthesis of social psychological research. In H. T. Reis & C. M. Judd (Eds.), *Handbook of research methods in social and personality psychology* (pp. 496–528). London: Cambridge University Press.

Johnson, B. T., & Eagly, A. H. (2014). Meta-analysis of social personality psychological research. In H. T. Reis & C. M. Judd (Eds.), *Handbook of research methods in social and personality psychology* (pp. 675–707). London: Cambridge University Press.

Johnson, E. J., & Goldstein, D. (2003). Medicine. Do defaults save lives? *Science, 302,* 1338–1339. http://dx.doi.org/10.1126/science.1091721

Kahneman, D. (2012, September 26). *A proposal to deal with questions about priming effects*. Retrieved from http://www.nature.com/polopoly_fs/7.6716.1349271308!/suppinfoFile/Kahneman%20Letter.pdf

Kawada, C. L. K., Oettingen, G., Gollwitzer, P. M., & Bargh, J. A. (2004). The projection of implicit and explicit goals. *Journal of Personality and Social Psychology, 86,* 545–559. http://dx.doi.org/10.1037/0022-3514.86.4.545

*Keatley, D. A., Clarke, D. D., Ferguson, E., & Hagger, M. S. (2014). Effects of pretesting implicit self-determined motivation on behavioral engagement: Evidence for the mere measurement effect at the implicit level. [Advance online publication]. *Frontiers in Psychology, 5,* 125. http://dx.doi.org/10.3389/fpsyg.2014.00125

Keizer, K., Lindenberg, S., & Steg, L. (2008). The spreading of disorder. *Science, 322,* 1681–1685. http://dx.doi.org/10.1126/science.1161405

*Kleiman, T., & Hassin, R. R. (2013). When conflicts are good: Nonconscious goal conflicts reduce confirmatory thinking. *Journal of Personality and Social Psychology, 105,* 374–387. http://dx.doi.org/10.1037/a0033608

Klein, R. A., Ratliff, K. A., Vianello, M., Adams, R. B., Jr., Bahnik, S., Bernstein, M. J., . . . Nosek, B. (2014). Investigating variation in replicability: A "many labs" replication project. *Social Psychology, 45,* 142–152. http://dx.doi.org/10.1027/1864-9335/a000178

Krosnick, J. Judd, & Wittinbrink, B. (2005). Attitude measurement. In D. Albarracin, B. T. Johnson, & M. P. Zanna (Eds.), *Handbook of attitudes* (pp. 21–78). Mahwah, NJ: Erlbaum.

Kruglanski, A. W., Shah, J. Y., Fishbach, A., Friedman, R., Chun, W. Y., & Sleeth-Keppler, D. (2002). A theory of goal systems. In M. P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 34, pp. 331–378). San Diego, CA: Academic Press.

Kunda, Z. (1990). The case for motivated reasoning. *Psychological Bulletin, 108,* 480–498. http://dx.doi.org/10.1037/0033-2909.108.3.480

*Lakin, J. L., & Chartrand, T. L. (2003). Using nonconscious behavioral mimicry to create affiliation and rapport. *Psychological Science, 14,* 334–339. http://dx.doi.org/10.1111/1467-9280.14481

Landis, J. R., & Koch, G. G. (1977). The measurement of observer agreement for categorical data. *Biometrics, 33,* 159–174. http://dx.doi.org/10.2307/2529310

Latham, G. P., & Piccolo, R. F. (2012). The effect of context-specific versus nonspecific subconscious goal on employee performance. *Human Resource Management, 51,* 511–523. http://dx.doi.org/10.1002/hrm.21486

*Legal, J., Meyer, T., & Delouvee, S. (2007). Effect of compatibility between conscious goal and nonconscious priming on performance. *Current Research in Social Psychology, 12,* 80–90.

*Levesque, C. S. (1999). *Automatic activation of intrinsic and extrinsic motivation*. (Dissertation). Retrieved from ProQuest Dissertations and Theses.

Levesque, C., & Pelletier, L. G. (2003). On the investigation of primed and chronic autonomous and heteronomous motivational orientations. *Personality and Social Psychology Bulletin, 29,* 1570–1584. http://dx.doi.org/10.1177/0146167203256877

Lewin, K. (1951). *Field theory in social science*. New York, NY: Harper.

Liberman, N., & Förster, J. (2000). Expression after suppression: A motivational explanation of postsuppressional rebound. *Journal of Personality and Social Psychology, 79,* 190–203. http://dx.doi.org/10.1037/0022-3514.79.2.190

Light, R. J., & Pillemar, D. B. (1984). *Summing up: The science of reviewing research*. Cambridge, MA: Harvard University Press.

Lipsey, M. W., & Wilson, D. (2001). *Practical meta-analysis (applied social research methods)*. Thousand Oaks, CA: Sage.

Littell, R. C., Milliken, G. A., Stroup, W. W., Wolfinger, R. D., & Schabenberger, O. (2006). *SAS for mixed models*. Cary, NC: SAS Institute Inc.

Locke, E. A., & Latham, G. P. (1990). *A theory of goal-setting and task performance*. Englewood Cliffs, NJ: Prentice Hall.

Locke, E. A., & Latham, G. P. (2002). Building a practically useful theory of goal setting and task motivation. A 35-year odyssey. *American Psychologist, 57,* 705–717. http://dx.doi.org/10.1037/0003-066X.57.9.705

Loersch, C. (2009). *The attributional model of priming: A single mechanism account of construal, behavior, and goal priming*. (Dissertation).

Retrieved from ProQuest Dissertations and Theses. (Accession Order No. 338719).

Loersch, C., & Payne, B. K. (2011). The situated inference model: An integrative account of the effects of primes on perception, behavior, and motivation. *Perspectives on Psychological Science, 6,* 234–252. http://dx.doi.org/10.1177/1745691611406921

Logan, G. D. (1989). Automaticity and cognitive control. In J. S. Uleman & J. A. Bargh (Eds.), *Unintended thought: Limits of awareness, intention, and control* (pp. 52–74). New York, NY: Guilford Press.

*Lowery, B. S., Eisenberger, N. I., Hardin, C. D., & Sinclair, S. (2007). Long-term effects of subliminal priming on academic performance. *Basic and Applied Social Psychology, 29,* 151–157. http://dx.doi.org/10.1080/01973530701331718

*Macrae, C. N., & Johnston, L. (1998). Help, I need somebody: Automatic action and inaction. *Social Cognition, 16,* 400–417. http://dx.doi.org/10.1521/soco.1998.16.4.400

*Maio, G. R., Pakizeh, A., Cheung, W. Y., & Rees, K. J. (2009). Changing, priming, and acting on values: Effects via motivational relations in a circular model. *Journal of Personality and Social Psychology, 97,* 699–715. http://dx.doi.org/10.1037/a0016420

*Maltarich, M. A. (2009). *A first examination of the effects of conscious and subconscious goal commitment and conscious and subconscious achievement goals on task performance*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. 3384137).

*Maner, J. K., Gailliot, M. T., Rouby, D. A., & Miller, S. L. (2007). Can't take my eyes off you: Attentional adhesion to mates and rivals. *Journal of Personality and Social Psychology, 93,* 389–401. http://dx.doi.org/10.1037/0022-3514.93.3.389

*Marien, H., Custers, R., Hassin, R. R., & Aarts, H. (2012). Unconscious goal activation and the hijacking of the executive function. *Journal of Personality and Social Psychology, 103,* 399–415. http://dx.doi.org/10.1037/a0028955

Markus, H. (1978). The effect of mere presence on social facilitation. *Journal of Experimental Social Psychology, 14,* 389–397. http://dx.doi.org/10.1016/0022-1031(78)90034-3

*Marquardt, M. J. (2011). *When the mind forges its own plans: The phenomenon of the implicit emergence of implementation intentions*. (Dissertation). Retrieved from ProQuest Dissertations and Theses.

Marsh, R. L., Hicks, J. L., & Bink, M. L. (1998). Activation of completed, uncompleted, and partially completed intentions. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 24,* 350–361. http://dx.doi.org/10.1037/0278-7393.24.2.350

Masicampo, E. J., & Baumeister, R. F. (2011). Unfulfilled goals interfere with tasks that require executive functions. *Journal of Experimental Social Psychology, 47,* 300–311. http://dx.doi.org/10.1016/j.jesp.2010.10.011

Mathiowetz, V., Weber, K., Kashman, N., & Volland, G. (1985). Adult norms for the Nine Hole Peg Test of finger dexterity. *Occupational Therapy Journal of Research, 5,* 24–38.

McCulloch, K. C. (2004). *Mechanics of motivation: A structural analysis of nonconscious goal pursuit*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. 3146682).

*McCulloch, K. C., Ferguson, M. J., Kawada, C. C. K., & Bargh, J. A. (2008). Taking a closer look: On the operation of nonconscious impression formation. *Journal of Experimental Social Psychology, 44,* 614–623. http://dx.doi.org/10.1016/j.jesp.2007.02.001

Meyer, D. E., & Schvaneveldt, R. W. (1971). Facilitation in recognizing pairs of words: Evidence of a dependence between retrieval operations. *Journal of Experimental Psychology, 90,* 227–234. http://dx.doi.org/10.1037/h0031564

*Milyavsky, M., Hassin, R. R., & Schul, Y. (2012). Guess what? Implicit motivation boosts the influence of subliminal information on choice. *Consciousness and Cognition, 21,* 1232–1241. http://dx.doi.org/10.1016/j.concog.2012.06.001

Mogilner, C., Aaker, J., & Kamvar, S. D. (2012). How happiness affects choice. *Journal of Consumer Research, 39,* 429–443. http://dx.doi.org/10.1086/663774

Morris, S. B., & DeShon, R. P. (1997). Correcting effect sizes computed from factorial analysis of variance for use in meta-analysis. *Psychological Methods, 2,* 192–199. http://dx.doi.org/10.1037/1082-989X.2.2.192

Natanzon, M., & Ferguson, M. J. (2012). Goal pursuit is grounded: The link between forward movement and achievement. *Journal of Experimental Social Psychology, 48,* 379–382. http://dx.doi.org/10.1016/j.jesp.2011.06.021

Neely, J. H. (1977). Semantic priming and retrieval from lexical memory: Roles of inhibitionless spreading activation and limited-capacity attention. *Journal of Experimental Psychology: General, 106,* 226–254. http://dx.doi.org/10.1037/0096-3445.106.3.226

*Oettingen, G., Grant, H., Smith, P. K., Skinner, M., & Gollwitzer, P. M. (2006). Nonconscious goal pursuit: Acting in an explanatory vacuum. *Journal of Experimental Social Psychology, 42,* 668–675. http://dx.doi.org/10.1016/j.jesp.2005.10.003

*Oikawa, M. (2004). Moderation of automatic achievement goals by conscious monitoring. *Psychological Reports, 95,* 975–980.

*Oikawa, M., & Oikawa, H. (2010). Consciousness and the unconscious in self-regulation: The effects of conscious compilation on goal priming. *Journal of Personality and Social Psychology, 81,* 485–491. http://dx.doi.org/10.4992/jjpsy.81.485

Osviankina, M. (1928). Die wiederaufnahme unterbrochener handlungen. *Psychologische Forschung, 11,* 302–379.

Papies, E. J., & Aarts, H. (2013). Nonconscious self-regulation, or the automatic pilot of human behavior. In K. D. Vohs & R. F. Baumeister (Eds.), *Handbook of self-regulation* (2nd ed., pp. 125–142). New York, NY: Guilford Press.

Papies, E. K., & Hamstra, P. (2010). Goal priming and eating behavior: Enhancing self-regulation by environmental cues. *Health Psychology, 29,* 384–388. http://dx.doi.org/10.1037/a0019877

*Park, J., Kim, K., Kwak, J., & Wyer, R. S., Jr. (2014). Priming thoughts about extravagance: Implications for consumer decisions about luxury products. *Journal of Experimental Psychology: Applied, 20,* 40–54. http://dx.doi.org/10.1037/xap0000006

Pashler, H., Harris, C., & Coburn, N. (2008). *Replication of Study 2a/2b of Bargh et al.* (1996). http://www.PsychFileDrawer.org/replication.php?attempt=MTU%3D

Payne, B. K., Brown-Iannuzzi, J., & Loersch, C. (in preparation). *Replicable effects of primes on human behavior*.

Perugini, M., Conner, M., & O'Gorman, R. (2011). Automatic activation of individual differences: A test of the gatekeeper model in the domain of spontaneous helping. *European Journal of Personality, 25,* 465–476. http://dx.doi.org/10.1002/per.826

Prinz, F., Schlange, T., & Asadullah, K. (2011). Believe it or not: How much can we rely on published data on potential drug targets? *Nature Reviews Drug Discovery, 10,* 712–712. http://dx.doi.org/10.1038/nrd3439-c1

Quinn, A., & Schlenker, B. R. (2002). Can accountability produce independence? Goals as determinants of the impact of accountability on conformity. *Personality and Social Psychology Bulletin, 28,* 472–483. http://dx.doi.org/10.1177/0146167202287005

*Radel, R., Sarrazin, P., Legrain, P., & Gobance, L. (2009). Subliminal priming of motivational orientation in educational settings: Effect on academic performance moderated by mindfulness. *Journal of Research in Personality, 43,* 695–698. http://dx.doi.org/10.1016/j.jrp.2009.02.011

*Radel, R., Sarrazin, P., & Pelletier, L. (2009). Evidence of subliminally primed motivational orientations: The effects of unconscious motivational processes on the performance of a new motor task. *Journal of Sport & Exercise Psychology, 31,* 657–674.

Raudenbush, S. W., & Bryk, A. S. (2002). *Hierarchical linear models* (2nd ed.). Thousand Oaks, CA: Sage.

*Roehrich, L. (1992). *Priming the pump: Activation of the alcohol expectancy construct increases drinking behavior*. (Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. 9235084).

Rosenberg, M. S. (2005). The file-drawer problem revisited: A general weighted method for calculating fail-safe numbers in meta-analysis. *Evolution: International Journal of Organic Evolution, 59,* 464–468. http://dx.doi.org/10.1111/j.0014-3820.2005.tb01004.x

Rosenthal, R. (1979). The "file drawer problem" and tolerance for null results. *Psychological Bulletin, 86,* 638–641. http://dx.doi.org/10.1037/0033-2909.86.3.638

Rosenthal, R. (1991). *Meta-analytic procedures for social research* (rev. ed.). Beverly Hills, CA: Sage. http://dx.doi.org/10.4135/9781412984997

Rosenthal, R., & Rosnow, R. (2008). *Essentials of behavioral research: Methods and data analysis* (3rd ed.). New York, NY: McGraw-Hill.

*Sambolec, E. J. (2006). *The effectiveness of overt and covert motivators on intrinsic interest and attributions*. (Doctoral Dissertation). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. 3236417).

*Sambolec, E. J., Kerr, N. L., & Messe, L. A. (2007). The role of competitiveness at social tasks: Can indirect cues enhance performance? *Journal of Applied Sport Psychology, 19,* 160–172. http://dx.doi.org/10.1080/10413200601185164

Satel, S. L. (2013, February 23). Primed for controversy. *New York Times,* Retrieved from http://www.nytimes.com/2013/02/24/opinion/sunday/psychology-research-control.html

Schnall, S. (2014a). *An experience with a registered replication project*. Retrieved from http://www.psychol.cam.ac.uk/cece/blog

Schnall, S. (2014b). *Moral intuitions, replication, and the scientific study of human nature*. Retrieved from http://edge.org/conversation/simone-schnall-moral-intuitions-replication-and-the-scientific-study-of-human-nature

Schooler, J. W. (2011). Unpublished results hide the decline effect. *Nature, 470*. http://dx.doi.org/10.1038/470437a

Schwarz, N., & Strack, F. (2014). Does merely going through the same moves make for a "direct" replication? Concepts, contexts, and operationalizations. *Social Psychology, 45,* 299–311.

Segal, S. J., & Cofer, C. N. (1960). The effect of recency and recall on word association. *American Psychologist, 15,* 451.

*Sela, A., & Shiv, B. (2009). Unraveling priming: When does the same prime activate a goal versus a trait? [Advance online publication]. *The Journal of Consumer Research, 36,* 418–433. http://dx.doi.org/10.1086/598612

Shah, J. Y., & Kruglanski, A. (2002). Priming against your will: How accessible alternatives affect goal pursuit. *Journal of Experimental Social Psychology, 38,* 368–383. http://dx.doi.org/10.1016/S0022-1031(02)00005-7

*Shah, J. Y., & Kruglanski, A. W. (2003). When opportunity knocks: Bottom-up priming of goals by means and its effects on self-regulation. *Journal of Personality and Social Psychology, 84,* 1109–1122. http://dx.doi.org/10.1037/0022-3514.84.6.1109

Shanks, D. R., Newell, B. R., Lee, E. H., Balakrishnan, D., Ekelund, L., Cenac, Z., . . . Moore, C. (2013). Priming intelligent behavior: An elusive phenomenon. [Advance online publication]. *PLoS ONE, 8,* e56515. http://dx.doi.org/10.1371/journal.pone.0056515

Shantz, A., & Latham, G. P. (2009). An exploratory field experiment of the effect of subconscious and conscious goals on employee performance. *Organizational Behavior and Human Decision Processes, 109,* 9–17. http://dx.doi.org/10.1016/j.obhdp.2009.01.001

Simonsohn, U., Nelson, L. D., & Simmons, J. P. (2014). P-curve: A key to the file-drawer. [Advance online publication]. *Journal of Experimental Psychology: General, 143,* 534–547. http://dx.doi.org/10.1037/a0033242

*Smeesters, D., Wheeler, S. C., & Kay, A. C. (2009). The role of interpersonal perceptions in the prime-to-behavior pathway. *Journal of Personality and Social Psychology, 96,* 395–414. http://dx.doi.org/10.1037/a0012959

*Spears, R., Gordijn, E., Dijksterhuis, A., & Stapel, D. A. (2004). Reaction in action: Intergroup contrast in automatic behavior. *Personality and Social Psychology Bulletin, 30,* 605–616. http://dx.doi.org/10.1177/0146167203262087

Srull, T. K., & Wyer, R. S. (1979). The role of category accessibility in the interpretation of information about persons: Some determinants and implications. *Journal of Personality and Social Psychology, 37,* 1660–1672. http://dx.doi.org/10.1037/0022-3514.37.10.1660

Srull, T. K., & Wyer, R. S. (1980). Category accessibility and social perception: Some implications for the study of person memory and interpersonal judgments. *Journal of Personality and Social Psychology, 38,* 841–856. http://dx.doi.org/10.1037/0022-3514.38.6.841

*Stajkovic, A. D., Locke, E. A., & Blair, E. S. (2006). A first examination of the relationships between primed subconscious goals, assigned conscious goals, and task performance. *Journal of Applied Psychology, 91,* 1172–1180. http://dx.doi.org/10.1037/0021-9010.91.5.1172

Stanley, T. D. (2005). Beyond publication bias. *Journal of Economic Surveys, 19,* 309–345. http://dx.doi.org/10.1111/j.0950-0804.2005.00250.x

*Stein, K. D., Goldman, M. S., & Del Boca, F. K. (2000). The influence of alcohol expectancy priming and mood manipulation on subsequent alcohol consumption. *Journal of Abnormal Psychology, 109,* 106–115. http://dx.doi.org/10.1037/0021-843X.109.1.106

Storms, L. H. (1958). Apparent backward association: A situational effect. *Journal of Experimental Psychology, 55,* 390–395. http://dx.doi.org/10.1037/h0044258

*Strahan, E. J., Spencer, S. J., & Zanna, M. P. (2002). Subliminal priming and persuasion: Striking while the iron is hot. *Journal of Experimental Social Psychology, 38,* 556–568. http://dx.doi.org/10.1016/S0022-1031(02)00502-4

Sullivan, P. F. (2007). Spurious genetic associations. *Biological Psychiatry, 61,* 1121–1126. http://dx.doi.org/10.1016/j.biopsych.2006.11.010

Sutton, A. J. (2009). Publication bias. In J. C. Valentine, H. Cooper, & L. V. Hedges (Eds.), *The handbook of research synthesis and meta-analysis* (pp. 435–452). New York, NY: Russell Sage Foundation.

*Takarangi, M. T., Polaschek, D. L. L., Hignett, A., & Garry, M. (2008). Chronic and temporary aggression causes hostile false memories for ambiguous information. *Applied Cognitive Psychology, 22,* 39–49. http://dx.doi.org/10.1002/acp.1327

Tate, J. A. (2009). *Individual differences in the social psychology of money*. (Thesis). Retrieved from ProQuest Dissertations and Theses.

Terrin, N., Schmid, C. H., Lau, J., & Olkin, I. (2003). Adjusting for publication bias in the presence of heterogeneity. *Statistics in Medicine, 22,* 2113–2126.

Thaler, R. H., & Benartzi, S. (2004). Save more tomorrow: Using behavioral economics to increase employee saving. *Journal of Political Economy, 112,* S164–S187. http://dx.doi.org/10.1086/380085

Todd, A. (2010). *Just say om: Does priming a meditation-like schema reduce the attentional blink?* (Unpublished Thesis). Retrieved from ProQuest Dissertations and Theses. (Accession Order No. 1479296).

*Trudel, R., & Murray, K. B. (2011). Why didn't I think of that? Self-regulation through selective information processing. *Journal of Marketing Research, 48,* 701–712. http://dx.doi.org/10.1509/jmkr.48.4.701

*Uysal, A. (2010). *Autonomy and pain: A self-determination theory approach to psychological aspects of physical pain*. (Dissertation).

Vallacher, R. R., & Wegner, D. M. (1987). What do people think they're doing? Action identification and human behavior. *Psychological Review, 94,* 3–15. http://dx.doi.org/10.1037/0033-295X.94.1.3

Vallacher, R. R., & Wegner, D. M. (1989). Levels of personal agency: Individual variation in action identification. *Journal of Personality and*

Social Psychology, 57, 660–671. http://dx.doi.org/10.1037/0022-3514.57.4.660

Van den Bussche, E., Van den Noortgate, W., & Reynvoet, B. (2009). Mechanisms of masked priming: A meta-analysis. *Psychological Bulletin, 135,* 452–477. http://dx.doi.org/10.1037/a0015329

*Veltkamp, M., Custers, R., & Aarts, H. (2011). Motivating consumer behavior by subliminal conditioning in the absence of basic needs: Striking even while the iron is cold. *Journal of Consumer Psychology, 21,* 49–56. http://dx.doi.org/10.1016/j.jcps.2010.09.011

*Verbruggen, F., & Logan, G. D. (2009). Automaticity of cognitive control: Goal priming in response-inhibition paradigms. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 35,* 1381–1388. http://dx.doi.org/10.1037/a0016645

*Verplanken, B., & Holland, R. W. (2002). Motivated decision making: Effects of activation and self-centrality of values on choices and behavior. *Journal of Personality and Social Psychology, 82,* 434–447. http://dx.doi.org/10.1037/0022-3514.82.3.434

Viechtbauer, W. (2010). Conducting meta-analyses in R with the metaphor package. *Journal of Statistical Software, 36,* 1–48. http://dx.doi.org/10.18637/jss.v036.i03

Vohs, K. D., Mead, N. L., & Goode, M. R. (2006). The psychological consequences of money. *Science, 314,* 1154–1156. http://dx.doi.org/10.1126/science.1132491

Wang, M. C., & Bushman, B. J. (1998). Using the normal quantile plot to explore meta-analytic data sets. *Psychological Methods, 3,* 46–54. http://dx.doi.org/10.1037/1082-989X.3.1.46

Wang, M., & Bushman, B. (1999). *Integrating results through meta-analytic review using SAS software*. Cary NC: SAS Publishing.

Wegner, D. M. (1994). Ironic processes of mental control. *Psychological Review, 101,* 34–52. http://dx.doi.org/10.1037/0033-295X.101.1.34

*Welsh, D. T., & Ordonez, L. D. (2014). Conscience without cognition: The effects of subconscious priming on ethical behavior. *Academy of Management Journal, 57,* 723–742. http://dx.doi.org/10.5465/amj.2011.1009

Williams, L. E., Bargh, J. A., Nocera, C. C., & Gray, J. R. (2009). The unconscious regulation of emotion: Nonconscious reappraisal goals modulate emotional reactivity. *Emotion, 9,* 847–854. http://dx.doi.org/10.1037/a0017745

*Wryobeck, J., & Chen, Y. (2003). Using priming techniques to facilitate health behaviours. *Clinical Psychologist, 7,* 105–108. http://dx.doi.org/10.1080/13284200410001707553

*Zabelina, D. L., Felps, D., & Blanton, H. (2013). The motivational influence of self-guides on creative pursuits. *Psychology of Aesthetics, Creativity, and the Arts, 7,* 112–118. http://dx.doi.org/10.1037/a0030464

Zeigarnik, B. (1927). On finished and unfinished tasks. *Psychologische Forschung, 9,* 1–15.

*Zogmaister, C., Arcuri, L., Castelli, L., & Smith, E. R. (2008). The impact of loyalty and equality on implicit ingroup favoritism. *Group Processes & Intergroup Relations, 11,* 493–512. http://dx.doi.org/10.1177/1368430208095402

Received March 23, 2015
Revision received August 8, 2015
Accepted August 17, 2015 ■