RESEARCH ARTICLE

The money priming debate revisited: A review, meta-analysis, and extension to organizations

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Summary

The effects of primed money have received widespread attention in social psychology but not in the organizational literature. This research examines whether priming money causes people to think and behave in ways consistent with how they would act if they received real money. Money priming research has not made many inferences to organizations, but the potential implications of the findings for the workplace are thought-provoking. However, because some money priming studies failed to replicate and many findings are mixed, we review this literature and meta-analyze the effects of primed money on two organization-related outcomes. Meta-analytic results (12 259 participants, 90 effects adjusted for dependence, and 34 studies) showed that primed money increased performance ($d = .38$), but it also boosted selfishness ($d = .33$). These effects were moderated by perceptual or behavioral outcome measures, where primed money increased behavioral selfishness ($d = .72$) over twice as much as it did behavioral performance ($d = .33$). These meta-analytic results clarify extant literature, provide a new platform for future money priming research in organizations, and offer new alternatives for managers looking for viable options to improve employee functioning.

KEYWORDS
individual differences (personality, values, traits), rewards and recognition, work motivation

1 | INTRODUCTION

> Without a doubt ... the principle of reinforcement must be included among the most substantiated findings in experimental psychology and is at the same time among the most useful findings for applied psychology .... (Vroom, 1964, p. 13)

Research has examined the effectiveness of many reinforcers (Luthans & Kreitner, 1985; Pfeffer, 1998; Stajkovic & Luthans, 2001), but only a few have received as ample empirical support as money in the work motivation literature (Latham, 2012). Meta-analyses have also documented positive effects of money on work outcomes (Gupta & Shaw, 1998; Stajkovic & Luthans, 1997, 2003). Short of a few skeptical views (Deci, 1972), work motivation research has shown that money is a useful concept for advancing scientific and practical

understanding of how organizational behavior (OB) is reinforced by contingent consequences. Taken together, money is an effective tool in performance management in organizations (Aguinis, 2019; Latham, 2018).

This line of inquiry has focused solely on the effects of real money, leaving unaddressed what role exposure to money primes might have on organization-relevant outcomes. Vohs et al. (2006) reported intriguing findings of the effects of primed money on a host of outcomes. To prime money is to present a cue to trigger money mental representation outside of awareness, which then automatically activates behaviors associated with the representation. Research in social psychology has examined whether money priming causes people to think and behave in ways consistent with how they would if they received real money (Vohs, 2015a) In a typical money priming study (e.g., Zhou et al., 2009), participants in the experimental group are primed with money cues (e.g., asked to count money), and those

in the control group receive a value-neutral treatment (e.g., asked to count paper). The difference between the two conditions on an outcome measure represents a money priming effect.

Money priming is potentially relevant to organizations because real money is scarce, yet the cost of priming money is virtually zero. Latham (2007) argued that transfer of knowledge from the basic behavioral science to organizational research is a fruitful avenue to complement existing theories. In particular, studies have demonstrated desirable effects of primed money on performance, where primed money helps athletic routines (Beus & Whitman, 2017), counteracts ego depletion (Boucher & Kofos, 2012), and boosts energy (Mok & de Cremer, 2018). The positive effects of primed money on performance are contrasted with less desirable effects on selfishness, defined as being uncooperative, uncaring, and disinterested in social connectedness and prosociality (Capaldi & Zelenski, 2016). Primed money tapers cooperation attitudes (Pfeffer & DeVoe, 2009), attenuates inclination to volunteer even among children (Gasiorowska et al., 2012), reduces helping behavior (Guéguen & Jacob, 2013), and weakens ethical conduct (Kouchaki et al., 2013).

Extrapolating the effects of primed money on performance and selfishness to work has captivating implications. If simple exposure to money primes generates similar performance effects to those of real money, then the upward economic potential is substantial. Yet, at the same time, primed money might have more insidious effects than previously ascribed to real money by ardent critics (e.g., Kohn, 1999) if it affects selfishness at work without awareness. That is, subconscious monetization can erode the collective fiber at work, which already suffers from growing incivility (Cortina et al., 2013). Because conflicted behaviors along these lines might be inexplicable if the cause occurred outside of awareness, by studying primed money effects, greater understanding can be gained of inconsistencies in workplace behaviors caused by exposure to money primes.

Broadening psychological theory of money has natural consequences for organizations. However, generalizing the current findings to the workplace is premature because a number of money priming studies failed to replicate and some were retracted (Chatterjee et al., 2013). Briefly, Vohs et al. (2006) reported a host of money priming effects, as did Caruso et al. (2013). Shortly thereafter, a many-labs replication ($n = 6333$) failed to find an effect of primed money (Klein et al., 2014). Rohrer et al. (2015) conducted replications with emphasis on statistical power and were unable to detect a primed money effect. Reflecting on unsuccessful replications, Vadillo et al. (2016) surmised that early effects might have been due to *p*-hacking. Given this debate (see Vohs, 2015a), Caruso et al. (2017, p. 1148) conducted, what they called, a "systematic exploration of manipulations, moderators, and mechanisms of priming effects" in a preregistered replication and, again, observed virtually no effects of primed money.

Successful replications are the gold standard of science, but unsuccessful attempts fuel debate further. In such instances, conclusions are typically drawn that the initial findings were Type I errors (Simmons et al., 2011), that the effects were too fickle to be of interest (Cesario, 2014), or that the replications did not exhibit sufficient sophistication (Caruso et al., 2017), because "priming effects are

subtle and their design requires high-level skills" (Bargh, 2012, p. 1). In her response to critics, Vohs (2015a) suggested two possible reasons for the failed replications. First, primed money produces countervailing concurrent effects. People primed with money "exert effort on challenging tasks, demonstrate good performance, and feel efficacious" (Vohs, 2015a, p. e86). At the same time, they are "less interpersonally attuned. They are not prosocial, caring or warm" (Vohs, 2015a, p. e86). Because money priming is, ostensibly, still a method in search of a theory, replications were guided by methods, as exemplified by Caruso et al. (2017). Consequently, the two opposing effects might have nullified each other. Second, Vohs (2015a) counted significant and null findings and concluded the former outnumber the latter. A vote counting approach, however, does not account for sample sizes and effect sizes, effectively biasing the conclusions reached from this method (see Hedges & Olkin, 1985).

At this juncture in money priming research, a review of the literature coupled with meta-analysis can juxtapose replication failures and Vohs's (2015a) defense of the original effects by quantitatively synthesizing estimates based on available studies, as was recently called for (Vadillo et al., 2016). A theory-driven meta-analysis can clarify to scholars and practitioners what to expect from this presently debated, though possibly impactful research to organizations. With these ideas in mind, we review the literature and meta-analyze money priming effects on performance and selfishness and their behavioral and perceptual manifestations. We make the case that these two outcomes are organization relevant and justify why their different measures might explain more variance. In the specific language of meta-analysis, we examine if type of outcome (performance, selfishness) is a moderator of money priming effects by testing differential effects based on the countervailing findings in prior research. We also examine whether measurement (behavioral, perceptual) of performance and selfishness explains additional systematic variance in the second-level nested moderator meta-analysis. On the basis of meta-analytic results, we propose a new platform and agenda for OB research. We discuss what theories might explain money priming effects on performance and selfishness, offer research questions and study designs that would move the field forward, and, given the new evidence, explore the potential practical implications of money priming at work.

2 | LITERATURE REVIEW

To prime money is to trigger the concept of money without a person's awareness, which then automatically triggers behaviors in line with the representation to affect an outcome. We first review interventions used to prime money followed by the types of outcomes examined.

2.1 | Money priming interventions

Money primes could be presented in multiple forms. In a scrambled sentence task (Bargh et al., 2001), participants are asked to perform a "psycholinguistic tasks" for which they are presented with scrambled

words that need to be unscrambled into a meaningful sentence. In the experimental group, some sentences contain money-related words, and the rest of the words are neutral. In the control group, all sentences and words are neutral (Kouchaki et al., 2013). Photos have also been used to prime money (Vohs et al., 2006), such as inserting a photo of money (vs. a flower) in a desktop screensaver (El Hazzouri, 2014). In other studies, participants are asked to think about money, for example, "imagine having ample access to money and never having to worry about paying their bills," whereas those in the control group are asked to "imagine what they would do and how they would feel tomorrow" (Caruso et al., 2017, p. 3). Having participants count or sort coins (Gasiorowska et al., 2012), use an ATM (Guéguen & Jacob, 2013), or see a transparent donation jar (Ekici & Shiri, 2018) also represent money priming interventions.

These interventions all represent supraliminal priming, in which stimuli are hidden in plain sight, for example, presenting a prime within the threshold of visibility (e.g., words in sentences), but in such a manner that participants are unaware of the intended causal connection between the prime and the subsequent behavior or perception. Post-experimental surveys are used as manipulation checks of potential awareness by the participants (see Bargh & Chartrand, 2000).[1]

2.2 | Money priming outcomes

Priming money has been found to attenuate fear of death (Zaleskiewicz et al., 2013), lift mood about a just world and fair economic system (Caruso et al., 2013), and inhibit emotional expressions (Jiang et al., 2014). Money priming also triggers market-pricing mindsets (Mead & Stuppy, 2014), manifested by increased shopping price sensitivity (Kim, 2017; Ma et al., 2017) and consumer choice selectivity (Tong et al., 2013). Though primed money positively affects motivation and performance (Beus & Whitman, 2017; Moran, 2018), it also feeds need for uniqueness (Ma et al., 2017), surges self-evaluations (Trzcińska, 2020), tilts mating strategies in a way of monetarily-slanted preferences (Li et al., 2016), boosts self-sufficiency (Vohs et al., 2006), and fosters selfishness (Yang et al., 2013).

2.2.1 | Organization-relevant outcomes

In this meta-analysis, we focus on organization-relevant outcomes. Prior meta-analyses in OB have ascribed organization-relevance to behaviors that "could be assumed to be plausibly related to tasks performed in organizations ..." (Stajkovic & Luthans, 1998, p. 244) and/or "behaviors that were plausibly related to tasks performed in organizational settings ..." (Judge et al., 2007, p. 110). Said differently, the cognitive mechanisms behind the behaviors are at stake. For example,

though the Stroop task (Stroop, 1935) entails pushing buttons as lightbulbs are turned on and off, or disentangling words and colors (e.g., word green presented in color red), the driving force behind the speed of these choices is fluid intelligence (Kranzler & Jensen, 1989), which is organization relevant (Schmidt & Hunter, 1998). Applying these definitions to our meta-analysis narrows the focus to the effects on performance (including motivation) and selfishness (including self-sufficiency) (Table 1). Aside from the fact that splitting data into four moderator groups would reduce number of estimates in each compared to splitting data into two moderator groups, and that the second-level moderation (perceptual and behavioral measures) would have eight groups (4 × 2) versus four (2 × 2), we elaborate on theory reasons for combining performance and motivation and selfishness and self-sufficiency in the analysis.

Performance and motivation

We pooled these measures in one group for the following reasons. In work motivation research, the target of motivated behavior is fairly specific, "motivation to work" (Locke, 1997, p. 375). In particular, three theories often researched in OB are expectancy (Vroom, 1964), goal-setting (Locke & Latham, 1990), and self-efficacy (Bandura, 1997). These three theories all have the prediction of performance as key criterion, as demonstrated by related meta-analyses (Stajkovic & Luthans, 1998; Van Eerde & Thierry, 1996; Wright, 1990). For example, Judge and Ilies (2002) also combined performance and motivation in their meta-analysis of the links between personality and "performance motivation" (p. 797).

Relatedly, one "truism" in work motivation research (Bell & Kozlowski, 2002, p. 497) is that performance (P) is a function of motivation (M) and skill (S), where this relationship is multiplicative (P = W * S) (Kanfer & Ackerman, 1989; Lawler & Porter, 1967; Locke & Latham, 1990; Porter & Lawler, 1968; Stajkovic, 2006; Vroom, 1964). In this relationship, one factor has little meaning without the other, because at the extremes, zero motivation kills performance regardless of skill level as does zero skill regardless of motivation level. Also, one factor hardly substitutes for the other because motivation does not fly a plane, skill does. To make the skill operational, effort is needed. Said differently, in work motivation literature, the purpose of studying motivation that does not move (e.g., by effort) behavior is unclear, as is explaining voluntary (cf., oppression) behavior that occurs without motivation (Latham, 2012).

Pertaining to money priming, the only field study we found showed that professional athletes seem to be more motivated during their final contract year because the salience of money is increased during that time. Because contracts expiring that year primed motivation, professional athletes performed better than any other year of the contract; professional hockey players scored more goals per game and professional basketball players scored more points per game (Beus & Whitman, 2017). As noted by the authors, a limitation of this field study was that only performance data were available and money motivation had to be inferred. Experimental money priming studies, however, could manipulate both motivation and performance and

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[1]Priming interventions can also be subliminal, where a cue is presented on a screen below the field of focal vision so that people are unable to report awareness of it. Subliminal priming has been criticized for lack of ecological validity (Bargh & Morsella, 2008). All studies herein used supraliminal priming.

TABLE 1 Studies included in the primary meta-analysis

Authors and year	Pub	Study	IV category	Specific IV[a]	DV category	Specific DV	DV type	N	d
Aarts et al. (2005)	1	Exp. 2	Subconscious goal to make money	Scrambled sentence task	Motivation	Speed of task completion	Behavior	80	0.36
Balcerowicz et al. (2016)	0	Exp. 2	Thinking of money	Counting coins in a photo	Selfishness	Amount intention to spend on a birthday gift for best friend	Behavior	52	1.28
Beus and Whitman (2017)	1	Study 1	Thinking of money	Final year of athlete's employment contract	Performance	Points scored by player per game	Behavior	615	0.38
Beus and Whitman (2017)	1	Study 2	Thinking of money	Final year of athlete's employment contract	Performance	Goals scored for players in a year	Behavior	166	2.20
Boucher and Kofos (2012)	1	Exp. 1	Subconscious concept of money	Scrambled sentence task	Performance	Performance on Stroop task	Behavior	53	0.00
Boucher and Kofos (2012)	1	Exp. 2	Subconscious concept of money	Scrambled sentence task	Performance	Anagrams completed in 5 min	Behavior	41	0.00
Caruso and Shapira (2015)	0	GRE	Thinking of money	Screensaver photo of money	Performance	GRE score	Behavior	102	−0.10
Caruso and Shapira (2015)	0	REI	Thinking of money	Scrambled sentence task	Motivation	Aggregate rational engagement and experiential ability scales	Perception	146	0.14
Caruso and Shapira (2015)	0	CLAMS	Thinking of money	Choosing sizes for bills vs. carpets	Motivation	Achievement Motivation Scale	Perception	130	0.02
Caruso and Shapira (2015)	0	CRT	Thinking of money	Choosing sizes for bills vs. carpets	Performance	Cognitive reflection test	Behavior	79	−0.20
Caruso et al. (2017)	1	Exp. 1a	Subconscious concept of money	Image of $100 bills in background instructions	Self-sufficiency	Composite measure of 8 different items	Perception	454	0.01
Caruso et al. (2017)	1	Exp. 1b	Subconscious concept of money	Scrambled sentence task	Self-sufficiency	Composite measure of 8 different items	Perception	365	0.00
Caruso et al. (2017)	1	Exp. 1c	Thinking of money	Imagine abundant money and write about it	Self-sufficiency	Composite measure of 8 different items	Perception	327	−0.10
Caruso et al. (2017)	1	Exp. 1d	Thinking of money	Select sizes for new $10, $20, and $50 bills	Self-sufficiency	Composite measure of 8 different items	Perception	456	−0.10
Caruso et al. (2017)	1	Exp. 2a	Subconscious concept of money	Image of $100 bills in background instructions	Self-sufficiency	Composite measure of 8 different items	Perception	435	0.00

(Continues)

TABLE 1 (Continued)

Authors and year	Pub	Study	IV category	Specific IV[a]	DV category	Specific DV	DV type	N	d
Caruso et al. (2017)	1	Exp. 2b	Subconscious concept of money	Scrambled sentence task	Self-sufficiency	Composite measure of 8 different items	Perception	341	0.02
Caruso et al. (2017)	1	Exp. 2c	Thinking of money	Imagine abundant money and write about it	Self-sufficiency	Composite measure of 8 different items	Perception	342	−0.20
Caruso et al. (2017)	1	Exp. 2d	Thinking of money	Select sizes for new $10, $20, and $50 bills	Self-sufficiency	Composite measure of 8 different items	Perception	425	−0.10
Caruso et al. (2017)	1	Exp. 3a	Subconscious concept of money	Scrambled sentence task	Self-sufficiency	Composite measure of 9 different items	Perception	109	0.17
Caruso et al. (2017)	1	Exp. 3b	Thinking of money	Imagine abundant money and write about it	Self-sufficiency	Composite measure of 9 different items	Perception	110	−0.10
Caruso et al. (2017)	1	Exp. 3c	Thinking of money	Select sizes for new $10, $20, and $50 bills	Self-sufficiency	Composite measure of 9 different items	Perception	113	−0.10
Ekici and Shiri (2018)	1	Exp. 3a	Interacting with money	Seeing transparent money vs. non-transparent donation box	Selfishness	Percentage of people who donated to charity	Behavior	257	0.33
Ekici and Shiri (2018)	1	Exp. 1	Interacting with money	Seeing transparent money vs. non-transparent donation box	Selfishness	Rating 1–5 of willingness to donate	Perception	206	0.48
Ekici and Shiri (2018)	1	Exp. 2	Interacting with money	Seeing transparent money vs. non-transparent donation box	Selfishness	Rating 1–5 of willingness to donate	Perception	124	0.53
El Hazzouri (2014)	0	Exp. 5	Subconscious concept of money	Desktop background picture of money	Performance	Puzzles completed	Behavior	67	0.50
Gasiorowska and Hełka (2012)	1	Exp. 1	Exposure to money	Counting money	Selfishness	Amount of money given away in game	Behavior	67	0.53
Gasiorowska et al. (2016)	1	Exp. 1	Exposure to money	Sorting 25 coins	Performance	Number of completed puzzles in time limit	Behavior	68	0.62
Gasiorowska et al. (2016)	1	Exp. 2	Exposure to money	Sorting 30 coins	Self-sufficiency	Time spent trying solving puzzle before seeking help	Behavior	90	0.64
Gasiorowska et al. (2016)	1	Exp. 3a	Exposure to money	Sorting 30 coins	Selfishness	Crayons picked up	Behavior	129	1.24
Gasiorowska et al. (2016)	1	Exp. 3b	Exposure to money	Sorting 20 coins	Selfishness	Crayons picked up	Behavior	64	1.18

TABLE 1 (Continued)

Authors and year	Pub	Study	IV category	Specific IV[a]	DV category	Specific DV	DV type	N	d
Gasiorowska et al. (2016)	1	Exp. 4	Exposure to money	Sorting 30 coins	Selfishness	Number of stickers left for experimenter	Behavior	125	1.03
Gasiorowska et al. (2012)	1	Exp. 1	Thinking of money	Describe poster of money	Selfishness	Allocation of money to others in game	Behavior	126	−0.60
Gasiorowska et al. (2012)	1	Exp. 2	Exposure to money	Counting money	Selfishness	Crayons picked up	Behavior	120	3.02
Guéguen and Jacob (2013)	1	Exp. 1	Exposure to money	Using an ATM	Selfishness	Acceptance of confederate asking participants to answer a short survey	Behavior	100	0.58
Guéguen and Jacob (2013)	1	Exp. 2	Exposure to money	Using an ATM	Selfishness	Telling experimenter that s/he dropped item	Behavior	50	0.96
Jiang et al. (2014)	1	Exp. 6	Subconscious concept of money	Scrambled sentence task	Selfishness	Willingness to interact with emotional others	Perception	73	0.62
Kouchaki et al. (2013)	1	Exp. 3	Subconscious concept of money	Scrambled sentence task	Selfishness	Choosing to lie to earn more money	Behavior	91	0.50
Kouchaki et al. (2013)	1	Exp. 4	Subconscious concept of money	Scrambled sentence task	Selfishness	Likelihood to hire candidate with confidential, beneficial information	Behavior	65	0.63
Kuźmińska et al. (2015)	0	Exp. 1	Subconscious concept of money	Background images of money	Self-sufficiency	(lack of) Level of general social trust	Perception	57	0.62
Kuźmińska et al. (2015)	0	Exp. 2	Exposure to money	Finance professionals vs. administration professionals	Self-sufficiency	(lack of) Level of general social trust	Perception	922	0.55
Kuźmińska et al. (2015)	0	Exp. 3	Exposure to money	Counting money	Self-sufficiency	(lack of) Reported trust toward confederate	Behavior	74	0.75
Kuźmińska et al. (2015)	0	Exp. 4	Subconscious concept of money	Background images of money	Selfishness	Allocation of points in trust game	Behavior	54	−0.80
Mackowiak et al. (2016)	0	Exp. 3	Thinking of money	Counting coins in a photo	Selfishness	Amount intention to spend on a birthday gift for best friend	Behavior	62	0.19
Mok and De Cremer (2016a)	1	Exp. 5	Thinking of money	Imagine counting money. List five aspects of money	Selfishness	Four items from interpersonal helping scale	Perception	88	0.00

(Continues)

TABLE 1 (Continued)

Authors and year	Pub	Study	IV category	Specific IV[a]	DV category	Specific DV	DV type	N	d
Mok and De Cremer (2016a)	1	Exp. 2	Thinking of money	Imagine counting money and list aspects	Selfishness	Amount of words entered for a sick coworker	Behavior	156	−0.30
Mok and De Cremer (2016b)	1	Exp. 4	Thinking of money	Rate a money photo on three quality dimensions	Selfishness	Whether participants typed their email indicating they would help recruit people	Perception	78	−0.50
Mok and de Cremer (2018)	1	Exp. 1	Thinking of money	List five aspects of money	Selfishness	Number of images typed up to help a colleague	Behavior	172	0.00
Mok and de Cremer (2018)	1	Exp. 2	Thinking of money	List five aspects of coins	Selfishness	Dichotomous indication of volunteering intention	Perception	93	0.25
Mok and de Cremer (2018)	1	Exp. 3	Thinking of money	List five aspects of coins	Motivation	Self-reported feelings of energy (state self-control capacity scale)	Behavior	105	0.00
Moran (2018)	0	Exp. 1	Subconscious concept of money	Scrambled sentence task	Motivation	Attainable rating 1–11 of selected goal	Perception	126	0.31
Moran (2018)	0	Exp. 2	Subconscious concept of money	Scrambled sentence task	Self-sufficiency	Questions to measure feelings of efficacy on a 5-point Likert scale	Perception	66	−0.80
Moran (2018)	0	Exp. 3	Subconscious concept of money	Scrambled sentence task	Performance	Performance relative to the given goal	Behavior	119	0.00
Moran (2018)	0	Exp. 4	Subconscious concept of money	Image of money in the corner of survey screen	Motivation	Rating 1–11 of motivation to perform	Perception	79	0.00
Moran (2018)	0	Exp. 5	Subconscious concept of money	Image of money in the corner of survey screen	Motivation	Rating 1–11 of willing to self-handicap	Perception	123	0.00
Moran (2018)	0	Exp. 6	Subconscious concept of money	Scrambled sentence task	Motivation	Time spent on the task	Behavior	104	0.00
Mukherjee et al. (2013)	1	Exp. 2	Subconscious concept of money	Background photo of currency on survey form	Self-sufficiency	10-item general self-efficacy scale	Perception	88	0.48
Mukherjee et al. (2015)	1	Exp. 1	Subconscious concept of money	Images	Motivation	Speediness to complete task	Behavior	54	0.67

TABLE 1 (Continued)

Authors and year	Pub	Study	IV category	Specific IV[a]	DV category	Specific DV	DV type	N	d
Mukherjee et al. (2015)	1	Exp. 2	Subconscious concept of money	Images	Motivation	Speediness to complete task	Behavior	36	0.63
Park et al. (2015)	0	Exp. 1	Subconscious concept of money	Screensaver	Self-sufficiency	Intent not to socialize	Perception	79	0.31
Park et al. (2015)	0	Exp. 2	Thinking of money	Handling money	Self-sufficiency	Solitary activity preference	Perception	40	0.72
Park et al. (2015)	0	Exp. 3	Subconscious concept of money	Screensaver	Selfishness	Generosity	Behavior	74	0.35
Pfeffer and DeVoe (2009)	1	Exp. 2	Subconscious concept of money	Scrambled sentence task	Selfishness	5-item willingness to volunteer	Perception	260	0.34
Piechowska et al. (2016)	0	Exp. 1	Thinking of money	Counting coins in a photo	Selfishness	Amount intention to spend on a birthday gift for best friend	Behavior	50	1.19
Piers et al. (2014)	0	Exp. 1	Subconscious concept of money	Images	Selfishness	Amount of donation	Behavior	114	0.38
Piers et al. (2014)	0	Exp. 2	Subconscious concept of money	Scrambled sentence task	Selfishness	Willingness to volunteer	Perception	260	0.29
Sarial-Abi and Vohs (2015)	0	Exp. 1	Subconscious concept of money	Images	Self-sufficiency	Persistence	Behavior	54	1.24
Sarial-Abi et al. (2015)	0	Exp. 2	Subconscious concept of money	Scrambled sentence task	Performance	Performance	Behavior	49	0.63
Sarial-Abi et al. (2015)	0	Exp. 3	Subconscious concept of money	Images	Performance	Performance	Behavior	48	0.65
Sarial-Abi et al. (2015)	0	Exp. 5	Subconscious concept of money	Images	Performance	Performance	Behavior	32	2.08
Teng et al. (2016)	1	Exp. 2	Subconscious concept of money	Scrambled sentence task	Self-sufficiency	(lack of) Approach intention for helpful target	Perception	200	−0.10
Teng et al. (2016)	1	Exp. 3	Subconscious concept of money	Scrambled sentence task	Self-sufficiency	(lack of) Approach intention for helpful target	Perception	182	0.00
Teng et al. (2016)	1	Exp. 4	Thinking of money	Rate a money photo on three quality dimensions	Self-sufficiency	(lack of) Approach intention for helpful target	Perception	248	−0.10
Trzcińska and Sekścińska (2016)	1	Exp. 1	Exposure to money	Counting money	Self-sufficiency	Time spent trying solving puzzle before seeking help	Behavior	61	0.80

(Continues)

TABLE 1 (Continued)

Authors and year	Pub	Study	IV category	Specific IV[a]	DV category	Specific DV	DV type	N	d
Trzcińska and Sekścińska (2016)	1	Exp. 2	Thinking of money	Counting coins	Self-sufficiency	Waiting until tomorrow and getting two stickers	Behavior	46	0.70
Vohs (2015b)	0	Exp. 1	Subconscious concept of money	Scrambled sentence task	Performance	Task persistence	Behavior	129	0.59
Vohs et al. (2006)	1	Exp. 1	Subconscious concept of money	Scrambled sentence task or money posters	Self-sufficiency	Time spent before asking for help	Behavior	52	0.78
Vohs et al. (2006)	1	Exp. 3	Subconscious concept of money	Scrambled sentence task	Selfishness	Number of data spreadsheets willing to code	Perception	39	0.68
Vohs et al. (2006)	1	Exp. 4	Subconscious concept of money	Scrambled sentence task	Selfishness	Time spent helping the confederate	Behavior	44	0.66
Vohs et al. (2006)	1	Exp. 5	Thinking of money	Imagine a future with abundant finances	Selfishness	Pencils picked up	Behavior	24	0.75
Vohs et al. (2006)	1	Exp. 6	Subconscious concept of money	Scrambled sentence task	Selfishness	Dollars donated to U. student fund	Behavior	44	0.69
Vohs et al. (2006)	1	Exp. 7	Subconscious concept of money	Screensaver photo of money	Self-sufficiency	Physical distance put between self and partner	Behavior	24	−0.80
Vohs et al. (2006)	1	Exp. 8	Subconscious concept of money	Poster of money photos t in experiment room	Self-sufficiency	Preference for solitary activities	Perception	61	0.75
Vohs et al. (2006)	1	Exp. 9	Subconscious concept of money	Screensaver photo of money	Self-sufficiency	(lack of) Desire to work with a peer	Perception	37	1.04
Weng et al. (2014)	0	Exp. 1	Thinking of money	Handling money	Motivation	Social perception (competence)	Perception	60	0.25
Weng et al. (2014)	0	Exp. 2	Subconscious concept of money	Pictures of money	Motivation	Social perception (competence)	Perception	86	0.29
Weng et al. (2014)	0	Exp. 3	Subconscious concept of money	Pictures of money	Motivation	Social perception (competence)	Perception	59	0.54
Zhou et al. (2009)	1	Exp. 3	Exposure to money	Counting money	Self-sufficiency	Southampton Social Self-Esteem scale (reporting less social distress)	Perception	84	0.68

TABLE 1 (Continued)

Authors and year	Pub	Study	IV category	Specific IV[a]	DV category	Specific DV	DV type	N	d
Zhou et al. (2009)	1	Exp. 4	Exposure to money	Counting money	Self-sufficiency	9-point scale to rate how painful immersion of fingers into hot water was (reverse coded)	Perception	96	0.83
Zhou et al. (2009)	1	Exp. 5	Thinking of money	List monetary expenditures for the past 30 days	Self-sufficiency	Southampton Social Self-Esteem scale (reporting less social distress)	Perception	108	−1.20
Zhou et al. (2009)	1	Exp. 6	Thinking of money	List monetary expenditures for the past 30 days	Self-sufficiency	9-point pain scale following immersion of fingers in hot water (reverse coded)	Perception	96	−1.10

[a]All independent variable manipulations involved supraliminal priming.

they often examine them in unison, for example, completing more puzzles (performance) in less time (due to motivation/effort) (Boucher & Kofos, 2012; El Hazzouri, 2014; Gasiorowska et al., 2016).

Selfishness including self-sufficiency

Money priming research also finds that after being primed by money individuals feel more self-sufficient, and, thus, they engage in selfish behaviors (Beus & Whitman, 2017). Vohs et al. (2006, p. 1154) defined self-sufficiency as "… an insulated state wherein people put forth effort to attain personal goals and prefer to be separate from others." This definition is in line with the description of selfishness as "… attention to personal pursuits and inattention to group interests …" (Wagner, 1995, p. 154). Thus, we categorize studies in the "selfishness" group if they captured intentions or behaviors in pursuit of one's own interest without the assistance of others. Examples of selfishness include physically distancing (not because of COVID-19) from others (Vohs et al., 2006, 2008), embracing solitary activities (Park et al., 2015), withholding assistance from those in need (Vohs et al., 2006, 2008), and lying to maximize individual outcomes at the expense of others (Kouchaki et al., 2013).

For example, Gasiorowska and Hełka (2012) primed participants with money by asking them to count coins and found that people primed by money gave less money away in a sharing game compared to participants in the control group (who counted candy). Ekici and Shiri (2018) similarly found that after exposure to a transparent jar of money, in contrast to an opaque jar, people donated less money to charity (see also Piers et al., 2014). Priming money also decreases helping behaviors (Gasiorowska et al., 2016; Vohs et al., 2006). Mok and de Cremer (2018) primed participants by asking them to list five aspects of money and then asked them to help their colleague. Compared to participants who were asked to list characteristics of cardboard, those primed with money volunteered less to help their coworkers.

2.2.2 | Behavioral versus perceptual measures of performance and selfishness

In money priming research, perceptual and behavioral responses are commonly used measures of performance and selfishness. Studies that use perceptions examine reactions in the form of rating a willingness to perform an action, completing a self-report scale, or expressing an intention (Ekici & Shiri, 2018; Jiang et al., 2014; Zhou et al., 2009). Behavioral responses have been measured as points scored, dollars donated, objects picked up, speed, or number of puzzles solved (Beus & Whitman, 2017; Gasiorowska et al., 2016; Moran, 2018). Dijksterhuis and Bargh (2001) argue that the primes that affect behavior do not necessarily affect perception. Bargh et al. (1996) found that "rudeness" and "politeness" primes caused participants to be ruder and politer toward an experimenter, but these same primes did not influence a perception that the experimenter was rude or polite. Similarly, in Bargh et al. (2001, Experiment 3), priming achievement strengthened behavior until the outcome was attained,

but the impact of the same primes on perception waned during the same period. One explanation is that people do not always deliberate what to do but respond to stimuli in the moment, that is, automatically. Thus, behavior does not always proceed perception nor does perception always manifest in behavior.

2.2.3 | Moving forward with a meta-analysis

In the preceding review, we laid out the land on what we know about money priming and focused attention on organization-relevant outcomes of performance and selfishness. In the proceeding section, we use a meta-analysis to contribute to the lively debate by examining the following research questions: (1) Does money priming have a significant effect on outcomes when examined across available studies (this addresses the current controversy in the field about effectiveness of money priming); (2) is there within-group homogeneity of effect sizes across available studies (in which case all studies have the same effect size and differ only by random error) or does performance and selfishness moderate this relationship (in which case single estimates differ by systematic variance as manifested through this moderator), and (3) whether the way in which performance and selfishness are measured explains systematic variance further.

3 | METHOD

Through its statistical foundations, conjoint with theory guiding hypothesis development, meta-analysis is a means of analyzing disparate research designs and conflicting results and discovering consistencies in a seemingly inconsistent literature. Conducting a meta-analysis is following a process of interrelated stages that progressively build upon one other. The three main steps are data collection, primary meta-analysis, and moderator meta-analysis, as described next.

3.1 | Study collection and selection criteria

We collected available studies on the effects of primed money on performance and selfishness. Figure 1 shows 10 progressive steps pertaining to identification of studies, inclusion criteria, and exclusion criteria. Here, we describe further the rationale behind some of the steps.

We looked for unpublished studies because of what is known as a "file drawer" problem and its impact on publication bias. The "file drawer" problem is manifested by studies filed away in researchers' file cabinets. Because results from such studies are not available for inclusion in a meta-analysis that can affect meta-analytic average estimates (Rosenthal, 1979). The issue here is not that studies are missing randomly, which would result in less data and wider confidence intervals, because randomly distributed omissions would not systematically affect average effect size. Rather, the file drawer problem refers to instances in which unpublished studies have been excluded systematically from meta-analysis and that creates a systematic publication bias. The assumption is that published studies most likely reported statistically significant results, whereas unpublished studies did not (Cooper, 2010). If only published studies are in a meta-analysis, it moves estimates upward, and readers are presented with biased conclusions. Our final study count, broken down in Table 1, is comprised of 61.76% published and 38.24% unpublished studies.

We conducted an ancestry approach (Lefebvre et al., 2009) because disciplinary database sources focus on different records. Thus, we followed electronic search with a manual search of the article reference sections, as well as those of the review articles on money priming.

Inclusion criteria specify which categories of the variable of interest qualify for inclusion in meta-analysis (e.g., type of outcomes and their organizational relevance), and exclusion criteria stipulate which study characteristics will be excluded. On occasion, these two criteria are inversely related (e.g., if organization-relevant outcomes qualify, fear of death does not). A meta-analysis might also heed exclusion criteria that relate to methods. We excluded studies without a control group, such as studies that compared one priming money priming intervention (e.g., counting money) to another (e.g., looking at money), but not primed money to a control group. On occasion, exclusion might be based on unintended "misinformation," for example, an article mentions the "analysis" in the abstract, but the article was not empirical. Similarly,
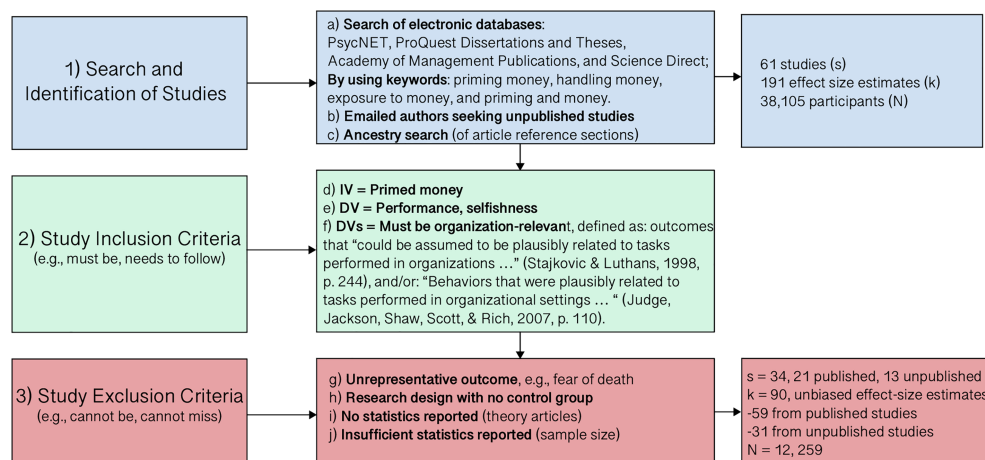

FIGURE 1 Data collection and selection criteria

some statistics might be reported in the article, but if the sample size was not reported, it becomes practically impossible to conduct meta-analytic calculations based on the extant equations.

The next decision was how to treat multiple effect sizes from the same sample because such estimates are stochastically dependent (Gleser & Olkin, 2009). Averaging, or treating them as if they were drawn from independent samples, just averages the bias, as well as violates the chi-square distribution properties (Rosenthal, 1991), which is the foundation of the homogeneity tests. Thus, we used equations for adjusting dependent effect size estimates provided by Hedges and Olkin (1985, p. 212), allowing us to obtain an independent effect size from each sample.

In primary and moderator meta-analyses, we followed Hedges and Olkin (1985) meta-analytic method. Hedges and Olkin (1985) provide comprehensive meta-analytic procedures, underscoring homogeneity/heterogeneity statistical testing and nested moderator analysis.

3.2 | Primary meta-analysis

We first calculate an average effect size of money priming on the outcomes we examine. Then, we statistically examine whether this average effect size occurred by chance. Hedges and Olkin (1985) recommend estimating single effect sizes in the form of index (g). Because for small sample sizes, (g) has a slight tendency to over-estimate the population effect size, single estimates of (g) were multiplied with the correction factor, resulting in an unbiased single effect size estimate (d) (Hedges, 1981; Hedges & Olkin, 1985). Hedges and Olkin (1985) equations use weighted averages, incorporating variances (v_i) to (v_k) for each (d_i) to (d_k), where each effect size is weighted by the inverse of its variance. Once we estimated average effect size ($d.$) and its variance ($v.$), we next test for statistical significance by comparing the ratio of ($d.^2/v.^2$) to the χ^2 distribution for $df = 1$. We intend to show if there was a significant main effect for money priming across (i) to (k) studies. Calculating an average effect is necessary but not sufficient. We next empirically examine publications bias (stability of the obtained average effect size) using three procedures (Duval & Tweedie, 2000; Orwin, 1983; Rosenthal, 1979).

We conclude the primary meta-analysis by testing for within-group homogeneity of effect sizes, which determines if a model can be unambiguously represented by one average effect. If the average effect is significant, and k effect sizes that comprise it do not deviate from one another beyond chance, such average effect size is considered a representative estimate of the population effect size. Heterogeneity of k effects indicates that magnitude differences between at least two of them are large enough to reject the homogeneity assumption that all single effects sizes are drawn from the same

population. In other words, there is significant moderation, in which case a model of average effect cannot be interpreted unambiguously.[2]

Within-group homogeneity is critical for unambiguous interpretation of results, especially in light of the inconsistent findings in the extant money priming literature being meta-analyzed. As Hunter and Schmidt (1995, p. 112) point out regarding the implications of homogeneity in meta-analysis, "if the chi square is not significant [is homogeneous], this is strong evidence that there is no true variation across studies." Hedges and Olkin (1985, p. 147) warn about the same point because if "the underlying (population) effect sizes are not identical in all of the studies, the representation of the results of a set of studies by a single estimate of effect magnitude can be misleading." Cooper (1997, p. 177) adds his take by emphasizing that "Homogeneity analysis provides the most complete guide to making inferences about a research literature."

3.3 | Moderator meta-analysis

If within-group heterogeneity of single effects size was significant, that would indicate moderation, but not necessarily as a confirmation of the way we specified it. Thus, these tests are needed to examine the two-level nested moderation: (1) between-group homogeneity of average effect sizes among two first-level moderator groups (performance and selfishness), (2) homogeneity of single effect sizes within each of the two moderator subgroups (performance and selfishness), (3) repeat 1 and 2 for the second level of moderation (behavioral measure, perception measure), and (4) orthogonal comparisons in the second level of moderation for each of the four groups as in 2 (performance, selfishness) × 2 (behavioral measures, perceptual measures) design.

4 | RESULTS

4.1 | Primary meta-analysis

All studies included in the meta-analysis reported estimates of effect (cf., correlations) sizes, defined as a mean difference between an experimental group and control group (Rosenthal, 1994; Woodward, 2003). Effect size shows an effect of a dichotomous variable (e.g., money priming) on a continuous variable (e.g., number of puzzle pieces solved) or at what percentile in the control group an average person in the treatment group is. For example, an effect size of .8 means that the score of the average person in the treatment group is .8 standard deviations above the score of an average person in the control group. We converted the estimates in the original studies to a common effect size d, as described above. The average effect size of primed money on organization-relevant outcomes taken together was $d = .35$, $p < .001$, 95% CI [.24, .45], indicating that the effect of primed money is significant across all k estimates.

We further examined the stability of this average effect and its significance by evaluating publication bias. First, we tested for a difference in average effects between the published studies $d = .34$,

(k = 59), p < .001, 95% CI [.21, .48] and the unpublished studies, d = .34, (k = 34), p < .001, 95% CI [.20, .49]. Both average effects had the same value, d = .34, $Q(1)$ = 0.00, p = .99. Second, we analyzed the sensitivity of the average effect to changes in included studies. Using the trim-and-fill procedure,[3] we looked for missing unpublished studies to the left and right of the mean. This resulted in zero identification of missing unpublished studies to the left of the mean and nine missing studies to the right of the mean (see OS, Figure S2, Panels A and B). Plotting observed and imputed studies and rerunning the analysis resulted in an increase in the weighted average effect size of primed money from d = .35 to d = .46, p < .001, 95% CI [.34, .58].

Third, we also ran the classic fail-safe N, which reports how many studies of zero effect size would need to be found filed away in draws and then added to our meta-analysis to drive what we found to a not significant average effect size. The classic fail-safe N analysis reported that 4990 unpublished studies of zero effect size would need to be missing. Likewise, Orwin's fail-safe-N identified that 123 unpublished missing studies of zero effect size would need to be found and included in meta-analysis to bring the weighted average effect size to a value of .10. Together, data upon which we derived an average effect size appear robust to publication bias.

These tests also paved the way for a cleaner examination of within-group homogeneity of effect sizes by a Q_w test (Hedges & Olkin, 1985) because we know that publication bias did not systematically affect the average effect size. Considering the sensitivity of the χ^2 distribution-based test to detect the slightest deviations from homogeneity, we also performed the Higgins and Thompson (2002) homogeneity test supplement by I^2 statistic, showing how much of the total variability can be attributed to within-group heterogeneity comprising the average effect. Given the diverse attributes of the studies across k effects and combined outcomes, the within-group heterogeneity of effect sizes was significant, $Q_w(89)$ = 703.69, p < .001, I^2 = 87.07%.

The conclusions we can draw based on the results so far are as follows. An average effect size of money priming on organization-related outcomes taken as a whole is significant, and it appears robust to publication bias. However, this average effect is also comprised of single effect sizes that significantly differ from one another, that is, estimate different population parameters. Thus, we cannot specify a model of one average effect size of money priming unambiguously. To account for the significant variation, as demonstrated by significant heterogeneity of within-group effect sizes, we proceed with the theory-driven, two-level nested moderator meta-analysis.

4.2 | Two-level nested moderator meta-analysis

All individual effect sizes were split into two groups, performance or selfishness. Primed money significantly affected performance, d = .38,

(k = 26), p < .001, 95% CI [.19, .57] and selfishness, d = .33, (k = 64), p < .001, 95% CI [.21, .46]. Homogeneity testing within each of the two moderator groups showed significant heterogeneity in each group, for performance, $Q(25)$ = 145.61, p < .001, I^2 = 82.83% and for selfishness, $Q(63)$ = 545.12, p < .001, I^2 = 88.44%. To account for this variation, we conducted a second-level nested moderator analysis in which performance and selfishness groups were further split into behavioral and perceptional measures.

Primed money was a significant predictor of both performance and selfishness behaviors, but less so of perceptions. The strongest effect of primed money was on behavioral selfishness, d = .61, (k = 29), p < .001, 95% CI [.36, .87], and the average effect size of primed money on behavioral performance was d = .48, (k = 18), p = .001, 95% CI [.20, .75] (see OS, Figures S3 and S4 for forest plots). By contrast, the average effect size of primed money on perceptual selfishness was d = .12, (k = 35), p = .07, 95% CI [−.01, .25], and on perceptual performance was, d = .16, (k = 8), p = .02, 95% CI [.03, .30]. See Figure S5 in OS for a distribution of the effect sizes from a meta-regression including moderator variables. Hence, we focus on behavioral measures of performance and selfishness.

Because the splits in moderator analyses were not random, sensitivity analyses for publication bias are pertinent again. Thus, as in prior analyses, we conducted the trim-and-fill and fail-safe N procedures for the results at the second level of moderation. For behavioral selfishness, trim-and-fill resulted in the identification of six missing unpublished studies to the left and zero to the right of the mean (see OS, Figure S6, Panels A and B). Plotting observed and imputed studies and rerunning the analysis resulted in a decrease in the weighted average effect size of primed money from d = .61 to d = .38, p < .001, 95% CI [.11, .65]. The classic fail-safe N reported that 1252 unpublished studies of zero effect size would need to be missing to move the effect sizes at the second level of moderation to zero. Orwin's fail-safe N identified that 119 unpublished missing studies of zero effect size would need to be found and added to bring the average effect size down to a small effect size value of .10. For behavioral performance, trim-and-fill identified zero missing unpublished studies to the left and three missing unpublished studies to the right of the mean (see OS, Figure S7, Panels A and B).

The classic fail-safe N reported that 345 unpublished studies of zero effect size would need to be missing to bring our effect sizes to zero. Orwin's fail-safe N showed that 58 unpublished studies of zero effect size would need to be found and added to bring the average effect size down to a value of .10. Rerunning the analysis after imputing values for the three unpublished missing studies resulted in an increase in the average effect on behavioral performance from d = .48 to d = .59, p < .001, 95% CI [.32, .86]. Though heterogeneity values decreased from the first moderator (performance, selfishness) to the second (behavior, perceptions), significant heterogeneity of single effect sizes was still present in each group, behavioral performance, $Q(17)$ = 131.59, p < .001, I^2 = 87.08%, and behavioral selfishness, $Q(28)$ = 249.71, p < .001, I^2 = 88.79%.

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[3]See online supplement (OS) for funnel plots, meta-regression visual, and forest plots.

4.3 | Post hoc analyses

4.3.1 | Empirical validity of theoretical groupings

We conceptualized why we combined performance and motivation in one group and selfishness and self-sufficiency in another. Here, we examine the empirical validity of these combinations. Parsing the first group, similar effect sizes for behavioral performance, $d = .54$, ($k = 7$), $p = .01$, 95% CI [.13, .96] and behavioral motivation (e.g., effort), $d = .51$, ($k = 3$), $p < .01$, 95% CI [.21, .82] resulted. These two average effect sizes were not significantly different from each other, $Q(1) = .01$, $p = .91$. Parsing the second group, effect sizes were for behavioral selfishness $d = .65$, ($k = 20$), $p < .001$, 95% CI [.30, 1.01] and behavioral self-sufficiency, $d = .79$, ($k = 6$), $p < .001$, 95% CI [.58, 1.00]. These two average effect sizes were not significantly different from each other, $Q(1) = .44$, $p = .51$. Aside from repeating a caveat

that more groups split k further, these results support our categorization of these four variables into two groups.

4.3.2 | Outlier analyses

Researchers disagree on the role of outlier analyses in meta-analysis. Most meta-analytic authors (Hedges & Olkin, 1985; Hunter & Schmidt, 1990, 1995) recommend outlier analyses, as do we, because of the undue influence large sample sizes and large effect magnitudes would have in deriving an average effect size. If an average effect size, which is composed of many single effect sizes, is mostly influenced by one or two studies with large samples and/or large effect size magnitudes, generalizability of the results across k estimates is lessened. An equally valid argument in the opposite direction is that results from large-sample studies are exactly the ones to which we should pay the

TABLE 2 Summary of results

Primary meta-analysis	k	d	p	95% CI
All studies	90	.35	.00	.24, .45
Published studiesdy	59	.34	.00	.21, .48
Unpublished studies	34	.34	.00	.20, .49
Trim and fill	99	.46	.00	.34, .58
Two-level nested moderator analysis	**k**	**d**	**p**	**95% CI**
Performance	26	.38	.00	.19, .57
Selfishness	64	.33	.00	.21, .46
Behavioral performance	18	.48	.00	.20, .75
Trim and fill: Behavioral performance	20	.59	.00	.32, .86
Behavioral selfishness	29	.61	.00	.36, .87
Trim and fill: Behavioral selfishness	23	.38	.00	.11, .65
Perceptual performance	8	.16	.02	.03, .30
Perceptual selfishness	35	.12	.07	−.01, .25
Post hoc analyses	**k**	**d**	**p**	**95% CI**
Outlier removal primary meta-analysis	65	.35	.00	.28, .42
Outlier removal two-level nested moderator analysis				
Behavioral performance	14	.33	.00	.16, .50
Behavioral selfishness	22	.72	.00	.59, .85
Sample size outlier sensitivity analysis				
All studies	76	.41	.00	.27, .55
Behavioral performance	17	.49	.00	.16, .82
Behavioral selfishness	28	.63	.00	.36, .90
Additional moderator analysis of the first-level moderator				
Performance behavior	7	.54	.01	.13, .96
Motivation behavior	3	.51	.00	.21, .82
Difference: Q(1) = .01, p = .91				
Selfishness behavior	20	.65	.00	.30, 1.01
Self-sufficiency behavior	6	.79	.00	.58, 1.00
Difference: Q(1) = .44, p = .51				

most attention. Regarding effect size magnitudes, the Q statistic has excessive power to detect even the negligible variability for the large number of k/studies, as is the case in meta-analysis. Statistical power is welcome. But it raises concern if identified differences in the form of within-group statistically significant heterogeneity of effect sizes have little practical meaning or send researchers to an unfruitful chase for further moderators.

The results reported so far are based on data without outlier analyses. Next, we show how the results might be impacted in light of sample size and effect magnitude outliers. Our aim with these analyses is to offer a "sensible description of the remaining data" (Behrens, 1997, p. 145).

Sample size outliers

For the studies in the behavioral performance group, there was one sample size outlier. The range of the sample sizes was $n = 32$ to $n = 166$, excluding one study with a sample size of $n = 615$. Excluding this study slightly changed the original effect size of $d = .48$ ($k = 18$) to $d = .49$ ($k = 17$), which remained statistically significant, $p = .001$, 95% CI [.16, .82]. For the studies included in the behavioral selfishness group, there was also one sample size outlier. The range of the sample sizes was $n = 24$ to $n = 172$, excluding one study with a sample size of $n = 257$. Excluding this study also slightly changed the original effect size of $d = .61$ ($k = 29$) to $d = .63$ ($k = 28$) while maintaining statistical significance, $p < .001$, 95% CI [.36, .90]. In both of these analyses, removing outlier sample sizes increased the average effect size, though slightly, and the significance remained. Said differently, not much has substantively changed.

Magnitude outliers

After the second-level nested moderator meta-analysis, both performance and selfishness groups remained with significant within-group heterogeneity of effect sizes. This leaves the story of both groups without a (statistically "clean") ending, in that we cannot make unambiguous conclusions about each of these average effect sizes. If the upper bound confidence interval for an effect is lower than the lower bound for the pooled effect, or if the lower bound confidence interval for an effect is higher than the upper bound pooled effect confidence interval, then such effect magnitude may be considered an outlier (Viechtbauer & Cheung, 2010). The following is a post hoc adjustment, that is, if within-group homogeneity was obtained at the second level of moderation, what would the results look like?

Two high and two low heterogeneity outliers were removed from the performance group, and homogeneity was achieved, $Q(12) = 19.49$, $p = .08$, $I^2 = 38.44\%$. Five low and two high heterogeneity outliers were removed from the selfishness group to achieve homogeneity, $Q(20) = 27.64$, $p = .12$, $I^2 = 27.63\%$. Following Behrens (1997), we reviewed each study from which outliers came and identified no systematic variance that might have explained the heterogeneity. Outliers varied in terms of how money was primed, and there were outliers from published and unpublished studies. A majority of outliers pertained to self-sufficiency or selfishness, two to performance, and none to motivation; however, this aligns with the

quantities of studies that measured these variables. Outliers included both perceptions ($n = 14$) and behaviors ($n = 11$). This indicated that heterogeneity values were most likely randomly inconsistent with the rest of the data (Barnett & Lewis, 1994). The adjusted average effect sizes kept the same trend as before adjustments with the strongest effect size for primed money on behavioral selfishness, $d = .72$, ($k = 22$), $p < .001$, 95% CI [.59, .85], and a weaker effect size for primed money on behavioral performance, $d = .33$, ($k = 14$), $p < .001$, 95% CI [.16, .50]. The average effect sizes for behavioral performance and behavioral selfishness were significantly different from each other, $Q(1) = 13.31$, $p < .001$. The results from all analyses performed are summarized in Table 2.

5 | DISCUSSION

Across philosophy (Simmel, 1978), anthropology (Maurer, 2006), sociology (Baker & Jimerson, 1992), psychology (Doyle, 1992; Mead & Stuppy, 2014), and motivation literature (Lawler, 1990), to say nothing of economics (Heyman & Ariely, 2004), money is recognized as an important determinant of thought and action. Research on the effects of primed money has arisen more recently (Vohs et al., 2006), and the results are mixed. Though it was unclear what to make of the inconclusive findings for the workplace, the potential of primed money having a role in organization-relevant outcomes is considerable for most people make money at work.

We meta-analytically revisited the money priming debate, and our findings are at a variance with unsettled prior inferences. Namely, when examined across available studies, the effects of primed money on organization-relevant outcomes—performance and selfishness—are statistically significant. It is not unusual to find significant results in meta-analysis despite the null findings in single studies, for all the reasons specified by Hunter and Schmidt (1995, 2004). Hunt (1997) also discussed examples of this scenario in medical and education research. When we further accounted for the type of measurement of the two outcomes, primed money still significantly affected performance and selfishness behaviors but not perceptual assessments.

These new meta-analytic findings can provide a foundation upon which a psychological theory of money priming in organizations can be further developed. That is, we hope to steer the research focus from the general question of whether money priming is effective to more specific inquiries regarding the nature of the relationships between primed money and organization-relevant outcomes, such as performance and selfishness. In this vein, we proceed as follows.

First, we discuss what theories could explain how money priming affects performance and selfishness at work. Because money priming is, ostensibly, an atheoretical technique, theory guiding analyses has not been at a forefront in this research. For example, in the preregistered replication, Caruso et al. (2017) stated that their study provided "a systematic exploration of manipulations, moderations, and mechanisms of money priming effects" (p. 1). Yet, no theory

differentiated among treatments, constructs, and processes.[4] To complicate matters further, primed money causes unharmonious effects on performance and selfishness concurrently. This obfuscates the disentanglement of these effects because the same decision frame—money matters—causes contradictory behaviors (further discussed below). Second, we offer several research questions stemming from our findings for both performance and selfishness and propose designs to facilitate empirical inquiry. Finally, given the evidence, we discuss practical implications of money priming for performance and selfishness at work. Following the structure of the meta-analysis, we first present these segments for performance.

5.1 | Money priming and performance: Theory, future research, and implications

5.1.1 | What theories might explain money priming effect on performance

We start by considering how reinforcement theory and goal-setting theory might explain money priming effect on performance. These two conceptual frameworks have dealt extensively with money, in terms of history of reinforcement and a goal to make money, at work.

Reinforcement theory

This conceptual framework (also known as operant conditioning, behavior modification) postulates that behavior is a function of its contingent consequences (Bandura, 1969; Komaki, 1986; Rogers & Skinner, 1956; Scott & Podsakoff, 1985). Put simply, behaviors must be contingently reinforced to drive desired outcomes, and money has been one of the most researched and practically utilized reinforcers at work (O'Hara et al., 1985; O'Reilly & Pfeffer, 2000; Pfeffer, 1995). Work motivation research has evidenced positive effects of money on criteria in field settings (Haynes et al., 1982; Komaki, 1986; Komaki et al., 1978; Latham et al., 1978; Luthans et al., 1981;Saari & Latham, 1982; Yukl & Latham, 1975) and laboratories (Komaki et al., 1982; Riedel et al., 1988), as well as across levels of analyses (Pritchard et al., 1988) and national cultures (Welsh et al., 1993).

Strategic use of money, that is, as a competitive advantage that is not easily replicated, was spearheaded by Lawler (1981, 1990) and colleagues (Larkin et al., 2012; Martocchio, 2016). Compensation work in human resource management followed by focusing on ways to administer money effectively via different plans (Gerhart & Rynes, 2003) and on the differences between organizational performances based on the compensation plans instituted (Gerhart & Milkovich, 1990). This research documents the effectiveness of money even in

relation to topics with prior mixed findings, such as creativity and intrinsic motivation (Gerhart & Fang, 2015).

The psychological mechanisms of money have been explained within the framework of social cognitive theory (Bandura, 1969; Stajkovic & Luthans, 2001). First, the utility of money is high for it leads to payoffs (Bandura, 1986). In this way, the motivation value of money is found in its exchange function, as money can be easily exchanged for many goods and services (Luthans & Stajkovic, 1999). Second, the informative content of money is moderate; money is most often only a dichotomous representation of whether the task was performed well or not. The informative content increases to an ordinal level if amounts received correspond to different performance levels. Finally, money also regulates behavior semiotically, a perceptual process in which people attach symbolic meaning (e.g., status) to money (Lea & Webley, 2014).

How a money–performance history of reinforcement link is created and stored? We next discuss how money–behavior associations are created in the subconscious based on history of reinforcement and how this association can be primed by money-related cues into automatic behaviors. A common way to administer money in organizations is via pay-for-performance (PFP) (Durham & Bartol, 2000), often accompanied by schedules of reinforcement (Saari & Latham, 1982; Yukl & Latham, 1975) and delivered within performance management systems (Aguinis, 2019). PFP is defined as "paying individuals predetermined amounts of money for each unit produced" (Lawler, 1990, p. 57). When employees understand clearly what leads to their performance and how such behaviors are measured and contingently reinforced, then compensation administered as PFP has a positive effect on myriad outcomes (Lawler, 1987; Luthans et al., 1998; Newman et al., 2017).

Because most adults in the United States earn money from employment (Cappelli, 2012), a history of PFP creates associations between money as a reinforcer and behaviors to earn it. Money priming research is, however, vague on the nature of this automation (cf., Kahneman & Treisman, 1984; Smith & Lerner, 1986). To address how money–outcome associations are formed over time and unfold automatically when primed, we rely on Bargh's (1989) concept of post-conscious automaticity that "requires some form of conscious processing first but produces an automatic outcome at the end" (p. 7). In this causal chain, priming money works after prior conscious processing of the money–behavior link has been gradually automated (Bargh, 1984).

Once automated, primed money activates its associated mental representation, which then automatically guides subsequent behavior accordingly (Bargh, 1990). Capturing this sequence, money cues reside in the environment, a money representation is primed in the subconscious, and the result is a measurable behavioral effect, all without awareness of the causal connections involved (Boucher & Kofos, 2012). Juxtaposing reinforcement theory on money as a contingent reinforcer with a money priming literature, we argue that they both draw from the same theory well; primed money triggers history of reinforcement as a meaningful mental representation.

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[4]We wrote to the authors about it, and the first author responded "although we agree that this previous work has left unanswered questions about the specific processes through which money primes might operate, the lack of clarity was largely beside the point of our paper. We did not claim nor aim to distinguish between these constructs" (correspondence available upon request).

Goal-setting and goal priming

Employees pursue many goals and one of them is a goal to make money (Locke & Latham, 2013). We doubt that most people consciously reset a goal to make money daily; instead, the link between a goal to make money and receiving money more likely gets automated over time. We next review goal theory, discuss goal priming in OB, and explain how the connection between a goal to make money and a behavior gets automated.

Goal-setting theory explains how goals impact organizationally relevant outcomes, where a goal is as "a regulatory mechanism for monitoring, evaluating, and adjusting one's behavior" (Locke & Latham, 2009, pp. 19–20). Goal-setting effects have been supported over the past five decades, with the main finding being that setting a specific and difficult goal leads to better performance than "do your best" and easy goals (Locke & Latham, 2013). Just as conscious goals are effective regulators of performance, burgeoning research in OB has been showing recently that primed goals improve performance in ways similar to conscious goals (Latham et al., 2010; Shantz & Latham, 2009, 2011; Stajkovic et al., 2006, 2019). Together, approximately 20 experiments with organizational relevance have shown a causal effect of primed goals on performance, as a recent meta-analysis reaffirmed (Chen et al., 2021), and about two hundred studies in psychology have reported behavioral effects of primed goals (Weingarten et al., 2016).

Formation and pursuit of primed goals

As individuals pursue repeatedly a conscious goal in a similar context, they gradually encode associations among the goal (e.g., make money), environment (e.g., boardroom), and behaviors (e.g., assertiveness). Over time, these associations turn automatic. When the environment with a priming cue is reencountered, it triggers a goal, which activates behavior that has been associated with it. This process is automatic, unfolding without awareness of the context–goal–behavior associations (Custers & Aarts, 2010). Thus, money-relevant cues in the organizational/social environment could inadvertently prime a goal to make money, leading to behaviors associated with it, without awareness of these links.

Boundary conditions of theory frameworks

Boundary conditions, pertaining both to money priming on the basis of history of reinforcement and priming a goal to make money, are as follows. Neither priming money nor priming a goal to make money imparts new concepts; they only trigger the associations already automated and stored in the unconscious. It follows that if money has not been perceived as a meaningful concept in one's past (see Mitchell & Mickel, 1999), then its effects might not be supported. Examples would include preference for intrinsic motivation, weak associations between money and performance (e.g., noncontingent/salaried pay), negative personal experiences related to money (e.g., pay discrimination), and broader societal concerns (e.g., income inequality). Somewhere in between is a scenario in which an individual might value money, has had positive experience with it, and endorses a goal

to make money, but does not embrace what it takes (e.g., sacrificing family time) to go for it.

5.1.2 | Future research on money priming and performance

Additive effects

An attractive research question concerns potential additive effects in organizations; can real money effects be enhanced with primed money? Given that both real and primed money affect performance, the two together might generate additive effects. This idea draws from research on goal priming as it relates to work motivation (see Latham et al., 2010, for a review). This research demonstrates that assigned and primed goals have additive effects on performance both in the laboratory (Stajkovic et al., 2006) and the field (Shantz & Latham, 2009, 2011; Stajkovic et al., 2018). To test if this additive effect is present for money, a laboratory experiment could be designed first to examine if primed money augments the effectiveness of real money, controlling for threats to internal validity.

For example, a 2 (primed money, neutral prime) × 2 (contingent reinforcement by real money, control group) analysis-of-variance (ANOVA) experimental design could be used. Following prior research, money could be primed by asking participants in the experimental group to write a story that included money prime words (e.g., Molinsky et al., 2012) or to unscramble sentences with money words embedded (e.g., Boucher & Kofos, 2012). Those in the control group would write a story or unscramble sentences with neutral words. Performance could be assessed in a variety of ways. For example, creative performance could be measured with a brainstorming task, such as listing uses for a common object (Latham et al., 2010; Stajkovic et al., 2006). Contingent reinforcement by money can be manipulated by offering pay for each legitimate use listed by a participant. Those in the control group would not receive money; they would simply be asked to perform the same task. Performance could also be assessed with management simulations. This would enable effects to be compared cross in-role behaviors (assigned tasks), extra-role behaviors (discretionary organizational citizenship behaviors), and prosocial behaviors (Farmer & Van Dyne, 2017). Such a design could provide insight into nuanced research questions, such as whether additive effects are found across all performance types, or if these effects might be contingent on dimension or the aim of the task. For instance, if extra-role behaviors are aimed at helping a supervisor (e.g., volunteering extra hours and taking on extra tasks) in comparison to the organization (e.g., serving on onboarding teams), would this moderate any additive effects, and why (see Lennard & Van Dyne, 2018)?

Field quasi-experiments

To bolster external validity, field experiments could be conducted next, for example, in an organization that uses a PFP system. Baseline performance could be measured a week prior to the experiment.

During the experimental week, money could be primed, for example, by changing employees' desktop background (e.g., Itzchakov & Latham, 2020) either to money or a neutral image (e.g., Vohs et al., 2006). Priming money could also follow the primed achievement goal manipulation used in Stajkovic et al. (2019) field study. In that experiment, a CEO inserted achievement prime words in an email message sent Monday morning to the goal prime experimental group and goal-neutral words in an email message to employees in the control group. Performance throughout the week could be measured and compared between groups. A within-group analysis could also be performed by comparing performance to the pre-and-post experimental weeks.

Another field study might examine the extent to which money priming effect on performance is moderated by the perceptions of how fair the PFP system is. If employees believe their organization's PFP system is fair, they may be more susceptible to the effects of money priming compared with those working in organizations where the PFP system is perceived as unfair or insufficiently tied to performance. Stagnant salaries might also lead to a disassociation of pay with performance. To test these questions, measures of fairness of employees' PFP could be obtained and employees could then by primed by money, ideally in a time-lagged setting to separate measurement of PFP fairness from the priming, and measures of performance could be obtained. Effects of money priming could also be compared by conducting a field experiment in both an organization that uses PFP and a similar organization that does not use PFP. Covariates should, at a minimum, include tenure with an organization and previous experience with PFP.

Intermittent effects

Budget constraints can limit an organization's financial resources to consistently administer PFP. Thus, we pose a research question as to whether primed money could be used as an intermediary reinforcer to help sustain the effects of real money on performance. For example, when employees keep performing without intermittent reinforcement, they can struggle to stay motivated (Woolley & Fishbach, 2016). Money priming could help sustain effort toward reaching a longer term PFP goal by keeping the goal of making money active in the subconscious so that it continues to be pursued. This could be tested in the field with a diary-based measure to track employees' performance over time.

For example, participants could enter their long-term performance goal with the expectation that once it was completed, they would receive monetary reinforcement. An online or app-based diary could be used to have participants track progress toward their goal. In the money priming condition, pictures of money could be embedded supraliminal in the diary entry screen. This longitudinal design could be altered to test for saturation effects by manipulating the amount of exposure to the money prime. For instance, some employees could be exposed to the money prime daily, others twice a week, and others once a week. Differences across conditions would indicate whether money priming works like a trip-wire (i.e., once primed the strength of the effect is the same) or whether more consistent priming enhances (or saturates) the effects.

We mentioned one way to prime money in the application-based design, but the concept of money could also be primed in an organization by reminding employees of an approaching performance review and renewal (or not) of annual contracts, discussions of salary, examination of company profits and losses, or reading financial statements. Another interesting angle from which monetary priming might ensue is organizational culture because it is "tacit and turns automated over time, eventually dropping out of daily awareness" (Schein, 2009, p. 19).

5.2 | Implications of money priming effect on performance

Priming money, or a goal to prime money, to augment effects of real money is an easier sell in organizations than the argument in the opposite direction. That is, we next discuss the implications of the replacement effect or whether primed money or a goal to prime money can replace some of the effects of real money in organizations. We place this polemic in this section instead of future research because more sifting and winnowing is needed of this complicated idea with broad social implications before proposing a research design to test how that might come about in organizations. We note at the outset that by asserting the replacement effect, we do not espouse insensitivity to income inequality but evoke it as of yet an unprecedented conversation.

In terms of the comparative strength of the associations between primed money, real money, and behavioral performance, a primed money effect size we found of $d = .31$ is not as strong as meta-analytic effect estimates for real money in organizations, $d = .99$ found by Stajkovic and Luthans (2003). Stajkovic and Luthans (1997) found an average effect size of real money in service sector of $d = .42$, which is closer to the effects size of primed money we found. Can these comparisons be contextualized? For example, the annual US GDP is ~\$20 trillion, and its growth is measured by more work hours and productivity. Both of these GDP growth inputs receive monetary compensation through wages or salaries. If the effect of real money on performance is $d = .99$ and of primed money $d = .31$, could a third of the financial cost be saved if real money effects are replaced with primed money effects? On the first glance, this idea does not seem palpable, and the reactions to it would likely be split; employers might favor it, but employees will oppose it (if they knew about it). However, because the economic upside of some replacement effect is too substantial to be entirely unnoticed, difficult conversations might arise.

5.3 | Money priming and selfishness: Theory, future research, and implications

5.3.1 | What theories might explain money priming effect on selfishness

Money priming literature draws from theories of market-pricing mindsets and business decision frames to explain why priming money

might cause individuals to act in selfish ways. We describe these two conceptual frameworks in that order for the latter builds upon the former.

Market-pricing mindset

Fiske (1992) proposed "market pricing" as one of the four cognitive models humans use to understand and organize the world around them. This model orients people to boil down the world to a single yardstick—money. When in this mindset, social behavior becomes a function of an interpersonal cost–benefit analysis. Individuals predominantly view interactions with others as exchange opportunities to accrue benefits for themselves (Beus & Whitman, 2017). Given that money is a standard medium for market-pricing exchanges, Vohs et al. (2008, p. 209) elaborated on the connections in this sequence including money priming:

> … market pricing underlies cost/benefit analysis, in that a person considers what he or she will receive in return before enacting a given behavior. Because money is the most typical form of market pricing, over time, the mere presence of money should elicit a market-pricing orientation toward the world.

Because "putting yourself first" in a social exchange under the market-pricing mindset can lead to money, it follows that when money is primed, individuals would pursue their own interest, even at the expense of others. The proportionality in social exchange under the market-pricing mindset is reduced to ratios and rates of the single utility metric, money, and, therefore, chances of building emotional connectedness are reduced (Levine, 1985). If attending to the needs of others does not produce a monetary computational benefit to oneself, it is unlikely to be pursued and vice versa. This framework offers a plausible set of conceptual connections explaining why priming money could lead to selfish behaviors, such as being insensitive to the needs of others (Gasiorowska et al., 2016; Mogilner, 2010), socially distant (Mogilner, 2010), less compassionate (Molinsky et al., 2012), and even immoral (Kouchaki et al., 2013).

The effects of primed money on selfishness have been revealed in the United States, Europe, and Asia, in samples of children, elderly, and managers (Vohs, 2015a). As a silver lining, this only appears to be a transitory state brought on by money cues and not a permanent change in morals. One reason money priming might not affect selfishness is if market pricing is not a prevalent decision frame guiding an individual's behavior and adaptation (Mead & Stuppy, 2014).

Business decision frame

Extending the notion of money-pricing mindset, Kouchaki et al. (2013) examined whether priming money activates a business decision frame. Tversky and Kahneman (1981, p. 452) define a decision frame as "the decision-maker's conception of the acts, outcomes, and contingencies associated with particular choice," or an interpretative cognitive structure. Kouchaki et al. (2013) suggest that money cues influence the way decisions are framed in the following way. When primed with money, people are more likely to objectify others in a cost–benefit analysis in which self-interest rains above interests of others (Kouchaki et al., 2013). Unlike a money-pricing mindset, where it is

possible to consider the interests of others if they produce tangible benefits to oneself, in a business decision frame, social bonds are not considered, potentially undermining an individual's morality according to this research (Kouchaki et al., 2013). Thus, if the workplace is characterized by weak morals, priming money could activate the business decision frame, leading to not only selfish but even immoral actions.

5.3.2 | Future research on money priming and selfishness

Conceptual replications

The finding that primed money causes selfishness is arguably at cross-purposes with research on teams, which magnets concern. Given that teams are a hallmark of contemporary organizations (DeShon et al., 2004; Mathieu et al., 2000), putting personal interests above team's interests, due to exposure to money, could be examined in conceptual replications. Virtually every study that has examined team cooperation, emotion contagion, cohesion, coordination, communication, identify, efficacy, and trust (Barsade, 2002; Barsade & Knight, 2015; DeJong et al., 2016; Dirks, 2000; DeShon et al., 2004; Knight & Eisenkraft, 2015; Lee et al., 2016; Lumsden et al., 2009; Mathieu et al., 2000; Prussia & Kinicki, 1996; Stajkovic et al., 2009; Zhang & Peterson, 2011) could be retested to examine if these indicators of team camaraderie, broadly speaking, diminish when money is primed. For example, a team could be asked to perform a money priming manipulation (e.g., go over the firm revenues; discuss company profit statement) and then the same outcomes as in the studies cited above could be measured and compared to the levels in the original research.

Selfishness and individual and team performance

Another research avenue could examine if primed money undermines team-level performance due to reduced cooperation among team members. A decline in willingness to cooperate could be used as a manifest variable for selfishness, as done in prior research (Pfeffer & DeVoe, 2009; Vohs et al., 2006). However, whereas previous research examined effects of primed money, selfishness, and performance at an individual level of analysis, these variables could be examined at both an individual and group levels of analyses to match the underlying theory. In particular, future research could examine if money individually primed boosts individual performance while simultaneously undermining team-level cooperation/coordination/cohesion in a cross-level (interaction) contagion effect design (e.g., Sergent et al., 2021).

Selfishness could also be proxied with other work-related behaviors, such as withholding organizational citizenship behaviors or engaging in counter-productive tasks (e.g., surfing the Internet and showing up late). Beyond team cooperation/cohesion, selfishness could be examined by manipulating money primes and comparing interpersonal interactions between employees and their supervisors. This design would provide insight into how primed money affects selfishness at the micro level, which could help explicate daily consequences on interpersonal interactions.

Another fruitful avenue for future research would be to investigate individual differences or contextual factors that moderate the priming effects. For example, the Big Five Personality traits (Stajkovic et al., 2018) could be examined as moderators of money priming on selfishness (e.g., does conscientiousness attenuate the effect of money priming on selfishness). Likewise, cognitive interventions, such as mindfulness training (Dahl & Davidson, 2019), could be examined as moderators (e.g., does mindfulness play a role in whether priming effects last longer or shorter periods). Finally, situational differences, such as leadership style, might impact primed money effects on selfishness, especially in the context of teams or employee–supervisor relationships (e.g., would selfish behaviors triggered by primed money be more pronounced under exploitative leadership (Guo et al., 2021) in comparison to transformational leadership (Eagly et al., 2003)).

5.3.3 | Implications of money priming effect on selfishness

The effect of primed money on selfish behaviors was not only significant but about twice as strong as effect on performance. All things considered, the implications of this finding are wide and multilayered. The idea that real money causes selfishness is perhaps unsurprising and is colloquially explained away with "nothing personal (about my selfishness), it's just business." The common distaste for this type of discourse arguably stems from the societal disapproval of severing personal bonds merely for money. Moreover, to some extent, selfishness also signals lack of empathy for others, which is at an all time high in the United States (Hall & Schwartz, 2019; Zaki, 2019).

As philosophers remind us, survival of humans as social beings enmeshes adhering to some standards of conduct evolved in a social context (de Waal, 2006; Haidt, 2008). As Haidt and Kesebir (2010, p. 800) explain, "moral systems are interlocking sets of values, virtues, norms, practices, identities, institutions, technologies, and evolved psychological mechanisms that work together to suppress or regulate *selfishness* [emphasis added] and make life possible." Selfishness is not one of these enabling standards, to say nothing of its subterranean effects at work. That is, boosting one's performance while causing detriments by selfish pursuits outside of awareness at the expense of group effort is of concern to organizations, military units, sport teams, and classrooms. The sooner we research cross-level (individual/group), cross-outcome (performance, selfishness) subconscious contagion, the better.

5.3.4 | Limitations and ethical concerns

In terms of limitations through which the new findings we report might be weighed, the customary criticism of meta-analyses, that is, garbage in–garbage out, is a possibility given the empirical controversy in the money priming literature. Though we paid particular attention to the thoroughness of analyses, if the input studies are flawed, then our findings could be interpreted in the similar light. However, we narrowed down this varied literature to form a meaningful group of studies that examined two organization-relevant outcomes—performance and selfishness—and used rigorous meta-analytic methods to examine research questions. The input studies incorporated a range of money priming manipulations, and we report their characteristics in a comprehensive table. After the analytic corrections and adjustments, our meta-analytic findings indicate that primed money still significantly affects work-related performance and selfishness behaviors. With this perspective in mind, we believe findings from this meta-analysis contribute to the cumulative building of a psychological theory of money in organizations.

Priming money in the field could raise ethical concerns for it involves stimuli that impact employees independent of their will. A related concern is lack of transparency when priming is used to modify behavior, even if it benefits both the organization and the employees. These considerations have been discussed in detail elsewhere (Latham & Ernst, 2006), and it has been shown recently that priming can even mitigate unethical behavior (Welsh & Ordóñez, 2014). We reiterate the main points of contention. First, some opacity in organizational research is to be expected. For example, organizations use personality assessments for hiring without informing candidates what is measured and how their scores influence the likelihood of offers. In response to freewill concerns, priming activates chronic constructs that are already present and valued by the person; it does not infuse them (Papies et al., 2014). In two organizational studies in which primed goals were examined, debriefing did not reveal employee discontent. Shantz and Latham (2009) found that employees expressed bewilderment that priming could work, but no dissatisfaction at being unobtrusively primed to perform better. Stajkovic et al. (2019) solicited human resources to follow up with employees to discern if there was discontent. That study reports all the responses received from the employees, and none of the reactions were in the direction of apparent displeasure at the priming treatment they had received.

6 | CONCLUSION

Work motivation research has said little theoretically about the role primed money might have on organizationally relevant outcomes. Social psychology research has examined effects of primed money, but contradictory results have been found. To date, this intriguing research stands with mixed findings, failed replications, and a lack of theory framework. Yet, its implications for organizations can hardly be understated because money plays a lead role in many stories at work and has ubiquitous presence in the business world. We scoured the money priming literature for the effects of primed money on organization-relevant outcomes of performance and selfishness and conducted a meta-analysis to obtain average effect estimates. We examined both perceptual and behavioral measures of these outcomes. We reported meta-analytic results and used them to propose a new platform for future OB research. It appears that priming money

could offer new alternatives for managers looking for viable options to improve employee functioning.

DATA AVAILABILITY STATEMENT

Data is available on request from the authors.

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SUPPORTING INFORMATION

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