Reward-driven distraction: A meta-analysis

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Abstract

People have a strong tendency to attend to reward cues, even if these cues are irrelevant to their current goal or their current task. When reward cues are goal-irrelevant, their presence may impair cognitive performance. In this meta-analysis, we quantitatively examined the rapidly growing literature on the impact of reward-related distractors on cognitive performance. We included 91 studies (*N* = 2,362) that used different cognitive paradigms (e.g., visual search, conflict processing) and reward-related stimuli (e.g., money, attractive food). Overall, results showed that reward-related distractors impaired cognitive performance across different tasks and stimuli, with a small effect size (Standardized Mean Change = .347). Between-study heterogeneity was large, suggesting that researchers can plausibly expect to sometimes find reversed effects (i.e., reward-related distractors boosting performance). We further showed that the average reward-driven distraction effect was robust across different reward-learning mechanisms, contexts, and methodological choices, and that this effect existed regardless of explicit task instructions to ignore distractors. In sum, the findings of this meta-analysis support the notion that cognitive processes can be thwarted by reward cues. We discuss these findings against the background of distraction-related phenomena as they are studied in clinical, educational, and work psychology.

Keywords: incentives; attentional bias; test anxiety; classroom management; interruptions

Significance statement

We show meta-analytic evidence that the human mind prioritizes rewards, even if this prioritization is counterproductive to people's current goals or to performance on their current task. We propose that this basic psychological process may underlie performance impairments in real-life settings, for instance at school and at work.

Introduction

The human mind has evolved to work harder when rewards – such as food, drink, sex, or money – are available in the environment. As a result, when people can earn rewards, they learn faster, think harder, and perform better. Indeed, the past decades of psychological research have shown that rewards generally boost performance on a broad range of physical and cognitive tasks (Atkinson & Birch, 1978; Braver et al., 2014; Garbers & Konradt, 2014; Geen, 1995; Locke et al., 1988; Padmala & Pessoa, 2011; Wigfield & Eccles, 2000). Paradoxically, though, *attending* to rewards – or to stimuli associated with rewards – can be counterproductive in real-life settings. For example, smartphone notifications signal that social rewards are available, but these notifications impair people's ability to safely drive their car. At the office, the smell of brownies signals that rewards are near, but this smell distracts attention away from work. At home, Netflix playing in the background signals the possibility of entertainment, but it takes attention away from doing homework. Daily life thus offers some situations in which attending to goal-irrelevant rewards may cause negative outcomes. But how do these negative outcomes arise?

In recent years, psychological experiments have discovered a phenomenon that can explain why attending to rewards can be counterproductive. This phenomenon is called *reward-driven distraction*[1] and it refers to the temporary impairment of cognitive performance after the onset of task-irrelevant, reward-related stimuli. In many cases, reward-driven distraction stems from the visual system's tendency to prioritize reward-related information (Chelazzi et al., 2013). This prioritization process presumably operates continuously: it does its job both when

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[1]Note: this phenomenon sometimes has been referred to as *value-driven attentional capture* (Anderson et al., 2011b) or *value-modulated attentional capture* (Le Pelley et al., 2016). In the current paper, we use the term *reward-driven distraction* because we will examine a broad range of cognitive processes – beyond attention – that are influenced by goal-irrelevant rewards.

information is useful (in many or most situations, rapid orienting to reward-related stimuli is advantageous), but also when it is not. When the visual system prioritizes information that is not relevant to the current task, reward-driven distraction ensues.

An early demonstration of reward-driven distraction was reported by Anderson, Laurent, & Yantis (2011b; but also see Della Libera & Chelazzi, 2009; Della Libera, Perlato, & Chelazzi, 2011; Hickey, Chelazzi, & Theeuwes, 2010a, 2010b; Theeuwes & Belopolsky, 2012) who found that distractors that were associated with earning money captured participants' attention – even when they were explicitly instructed to ignore these distractors. This finding was striking for two reasons. First, at the time, researchers typically assumed that there were only two main influences on people's attention; attention could be driven by people's current goals (*top-down*) or by the physical salience of stimuli (*bottom-up*). The Anderson et al. (2011b) study suggested a third possibility, namely that attention can also be driven by the reward value of stimuli, which challenged the classic bottom-up/top-down dichotomy (Awh et al., 2012). Second, Anderson et al. (2011b) inspired a large wave of conceptual replications (around 100 studies), most of which reported similar findings to the original paper (for reviews, see Anderson, 2013, 2016a, 2018; Failing & Theeuwes, 2017; Le Pelley, Mitchell, Beesley, George, & Wills, 2016). So, over the past 8 years, an increasing body of research has established the idea that people attend to reward-related cues, even if these cues are not serving their current goals.

Although evidence for reward-driven distraction appears strong at this point, there are several open questions and controversies, troubling the interpretation of this rapidly growing literature. To address these questions and controversies, this paper presents a meta-analysis, with three aims. First, we aim to establish the magnitude and the scope of the reward-driven distraction phenomenon. Second, some recent, critical reviews have raised doubt about whether findings reported in prior work actually demonstrate reward-driven distraction, or whether something else (not related to reward) is driving the observed behavior (Le Pelley et al., 2016; Sha & Jiang, 2016). Our analysis will address this pressing controversy. Third, we aim to systematically investigate several key methodological aspects of reward-driven distraction. Specifically, we (a) assess the general health of the literature (e.g., publication bias, researcher degrees of freedom) and (b) test whether certain methodological choices (e.g., reward-learning or reward value) modify the magnitude of the effect.

Our introduction starts with a brief overview of existing findings on reward-driven distraction (for recent, comprehensive reviews, see Anderson, 2016a; Failing & Theeuwes, 2017; Le Pelley et al., 2016). Then, we discuss the three aims in separate sections. For each aim, we first lay out the theoretical background, then discuss our methodological approach.

Reward-Driven Distraction: A Brief Review

In one of the most influential studies on reward-driven distraction (Anderson et al., 2011b), participants first performed a reward learning task (i.e., a *training phase*, see Figure 1A), in which they learned to associate stimulus features with the receipt of rewards. Specifically, on each trial six circles appeared in a 'search display', and participants' task was to find the *target*, which was either a red or green circle. If participants responded rapidly and accurately, they received a monetary reward; critically, the size of this reward was determined by the color of the target on that specific trial. If the target appeared in the *high-reward color* (e.g., red), participants would typically earn a large reward (specifically, on 80% of such trials they received 5 cents, and on the remaining 20% they received 1 cent); if the target appeared in the *low-reward color* (e.g., green), participants would typically earn a small reward (80% of such trials earned 1 cent, and 20% earned 5 cents). After a long training session (e.g., 1,008 trials), participants completed the test phase (Figure 1B). During this task, participants again had to find a target, which was now defined as the unique shape in the display (e.g., a diamond among circles). Importantly, on the

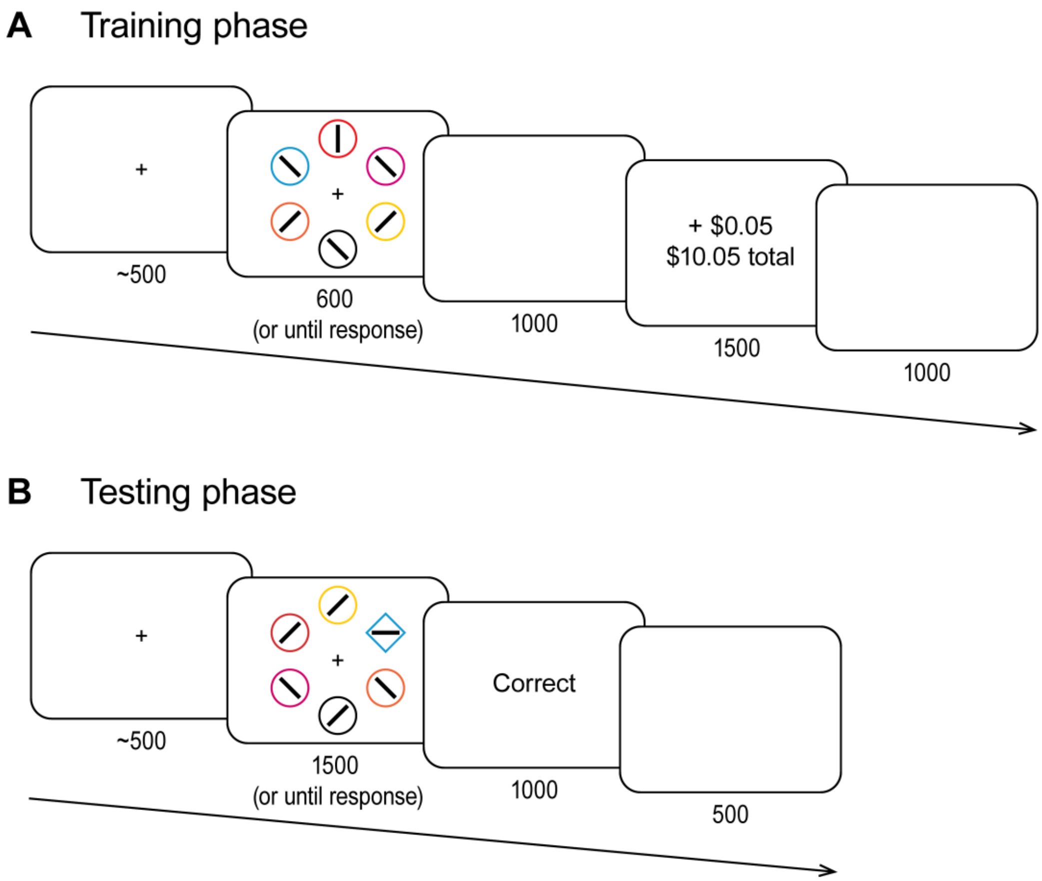

Figure 1. Sequence of events of the training phase and testing phases of Anderson et al.'s (2011b) task. Numbers refer to screen durations in milliseconds. (A) During the training phase, participants had to search for either a red or a green circle (there was always exactly one), and then indicated whether the line in that circle was vertical or horizontal. If their response was correct, they received monetary reward feedback. One of the colors predicted high reward feedback; the other color predicted low reward feedback. Thus, participants learned to associate different colors with different levels of reward. (B) During the testing phase, participants no longer had to search for red or green circles. Instead, they had to search for a unique shape. Also, they could no longer earn money. On some trials, a red or a green circle (which were previously paired with low or high reward) re-appeared as one of the distractors.

majority of trials, one of the non-target circles had either the high-reward or the low-reward color from the training phase. During the test phase, these colored circles were no longer relevant to the task – that is, they were *distractors* – and participants were instructed to ignore them. Despite these instructions, results showed that participants were significantly slower to respond to the target when the search display contained a distractor previously associated with high-reward, compared to when there were no previously reward-associated colors present. The authors concluded that attention can be modulated by the value of (task-irrelevant) rewards. Interestingly, this type of attentional modulation is distinct from top-down or bottom-up mechanisms: it is neither modulated by ongoing goals (participants knew the target would never be colored in the test phase, and hence there was no reason to attend to the colored distractors, suggesting the attentional shift is not voluntary), nor by physical salience (because reward-related cues were no more physically salient than other cues; Anderson et al., 2011b).

Anderson et al.’s (2011b) findings inspired a new wave of studies, many of which adopted the original study’s empirical approach (i.e., using a *reward learning phase*, then a *testing phase*). In this wave of studies, researchers studied reward-driven distraction from many different angles. Although the majority of studies have been conducted on the allocation of spatial attention (Anderson et al., 2011a; Anderson, Folk, et al., 2016; Anderson, Laurent, et al., 2014; Anderson & Yantis, 2012, 2013; Gong et al., 2017; Jahfari & Theeuwes, 2016; Jiao et al., 2015; Laurent et al., 2015; MacLean et al., 2016; MacLean & Giesbrecht, 2015; Miranda & Palmer, 2013; Qi et al., 2013; Rajsic et al., 2016; Roper et al., 2014; van Koningsbruggen et al., 2016), reward-driven distraction has also been shown to impair performance on other tasks. That is, reward-associated distractors were found to disrupt temporal attention (Failing & Theeuwes, 2015; Le Pelley et al., 2017), conflict processing (Krebs et al., 2010, 2011, 2013), visual memory (Gong et al., 2016; Infanti et al., 2015; Klink et al., 2017), and decision-making (Hopf et al., 2015; Itthipuripat et al., 2015). Altogether, these findings imply that reward-driven distraction may be a domain-general process (Braver et al., 2014) – and thus may have consequences in many areas of life.

There is another reason why reward-driven distraction may have broad consequences: there are many circumstances under which stimuli can acquire reward value. First, reward-driven distraction could reflect instrumental learning. That is, when people learn that a response to a cue (e.g., attending to red circles) is rewarded, then this may reinforce that response and make it more likely to be enacted in future, even when it becomes inappropriate (e.g., during the test phase of Anderson et al.'s study). Other evidence suggests that reward-driven distraction can also result from Pavlovian reward learning; that is, learning that a stimulus is a reliable *signal* of the availability of reward. These studies demonstrate that stimuli that merely co-occur with reward (vs. stimuli that do not co-occur with reward), regardless of people's actions, also become more likely to distract people (e.g., Bucker, Belopolsky, & Theeuwes, 2014; Bucker & Theeuwes, 2016b; Le Pelley, Pearson, Griffiths, & Beesley, 2015; Mine & Saiki, 2015; Pearson, Donkin, Tran, Most, & Le Pelley, 2015). Moreover, the stimuli that can cause reward-driven distraction come in many shapes and sizes. Although the majority of studies used very basic, low-level stimulus features (such as colors) as reward-associated distractors, recent studies show that other, more complex stimulus dimensions (e.g., spatial location, shape, picture, or semantic meaning) may acquire reward value and disrupt later performance (Anderson, 2015a, 2016a; Failing & Theeuwes, 2015; Hickey et al., 2015). Interestingly, one of those dimensions appears to be auditory, as some studies show that reward-associated sounds may also cause reward-driven distraction (Anderson, 2016c; Asutay & Vastfjall, 2016; Pooresmaeili et al., 2014), suggesting that the phenomenon is not confined to the visual domain. All in all, these findings together suggest that reward-driven distraction is generalizable across many different contexts.

Aim 1: To Establish the Magnitude and Scope of Reward-Driven Distraction.

Although the literature reviewed above supports the existence of reward-driven distraction, the *magnitude* of the phenomenon is not yet well-established (Failing & Theeuwes, 2017). Because the literature on reward-driven distraction has quickly become substantial, the time is ripe for a systematic investigation of how strong and consistent the effect is across different paradigms, contexts, and stimuli. If it is true that reward-driven distraction is a clear and reliable effect, it likely has implications for applied research areas, such as work psychology and educational science. Therefore, with this meta-analysis, we estimate the general magnitude of reward-driven distraction.

In addition, we also aim to address the *scope* of reward-driven distraction. Specifically, as mentioned above, past research on reward-driven distraction has been mainly restricted to visual search tasks (Anderson, 2016b). Nevertheless, some recent studies have examined whether reward-driven distraction impacts performance on other types of tasks, such as cognitive control tasks (Anderson et al., 2012; Krebs et al., 2011). These studies are important, because they may indicate that reward-driven distraction is generalizable across different performance situations. Indeed, in real life – e.g., at work and at school – many performance situations require people to interact with information in complex ways, rather than to merely locate target stimuli (as in visual search). We reasoned that if reward-driven distraction generalizes to tasks that rely on many different cognitive operations, then being exposed to these reward-related distractors is likely to harm performance in daily life.

On this latter point, we note that there are existing studies that have investigated how reward-driven distraction relates to real-life settings. However, these studies mainly focused on reward-driven distraction's role in the development of psychopathologies, such as substance abuse (Albertella et al., 2017; Anderson, Kronemer, et al., 2016) and depression (Anderson, Leal, et al., 2014). It remains an open question, though, whether these findings extend to healthy individuals in educational and occupational performance settings – an area that is unexplored in the reward-driven distraction literature.

Our methodological approach. In order to identify the *magnitude* of reward-driven distraction, we aggregated data from individual studies and calculated an overall effect size estimate. In particular, we examined the effect size for the difference in cognitive performance when a distractor associated with high versus low (or neutral) reward was present in the task.

In order to define the *scope* of reward-driven distraction, we explored whether certain study characteristics modify the magnitude of reward-driven distraction. To do this, we coded three characteristics of each study for moderator analyses. First, we coded the *nature of performance interference*, that is, which paradigm was used to assess distraction by reward-associated cues. By comparing different paradigms, we can test whether reward-driven distraction is mainly important for visual attention or also extends to other forms of (non-visual) interference, such as conflict processing or maintaining task relevant information, which are very important for optimal performance at work and education.

Second, we coded the *difference between training and test phases*. That is, we coded whether the training and testing phases of the experiment (where applicable) were the same task (e.g., both visual search) or different tasks that target distinct cognitive processes (e.g., reward training was a visual search task and testing was a working memory task). One possibility is that reward-driven distraction is harmful only when learning and testing take place in a similar context (e.g., similar task and/or stimuli). That is, context may evoke a set of learned stimulus-reward associations that have been experienced specifically within that context, and it may be these contextually specific reward associations that impair cognitive operations (Anderson, 2015b). However, it is also possible that the negative impact of reward-associated cues can transfer across contexts and cognitive tasks (Anderson et al., 2012). If transfer turns out not to be strongly dependent on context, this would imply greater relevance to real-life educational and work-related performance situations.

Third, we coded *task instructions*. That is, we coded whether participants were explicitly instructed during the task to ignore reward-associated distractors, or whether there were no such explicit instructions. With this moderator we examined what role voluntary control processes play in overriding a reward-based distraction effect. It is important to investigate whether conscious preparation to discount distracting events helps to reduce performance decrements (e.g., via pro-active control; Braver, 2012). If external instructions to ignore distractors turn out to diminish reward-driven distraction, this would imply that the negative impact of reward-associated cues might be easily prevented in applied settings. In particular, it would suggest that effective interventions may need to target only the current task (e.g., by using instructions to concentrate, or by making tasks more interesting or engaging), and that it is not necessary to remove the sources of distractions altogether (e.g., by banning smartphones from the classroom).

Aim 2: To Examine an Alternative Explanation: Have We Been Studying Selection-Driven Distraction Instead of Reward-Driven Distraction?

Researchers use language that suggests they are confident in the existence of reward-driven distraction. Nevertheless, recently, some researchers have raised doubts about whether prior experiments on the topic have demonstrated a *reward-driven* process after all (Anderson & Halpern, 2017; Le Pelley et al., 2015; Sha & Jiang, 2016). Their doubts stemmed from the way reward-driven distraction is usually operationalized. In particular, in many previous experiments, the existence of reward-driven distraction is inferred from a comparison between two conditions: (a) a condition in which a high-reward distractor was present and (b) a condition in which no previously-trained distractor was present (Anderson, 2015a; Anderson et al., 2011b; Sali et al., 2014). Problematically, this particular comparison leaves room for an alternative explanation: it is possible that high-reward distractors hurt performance not because they signal higher rewards, but because they have an extensive history of having been searched for, and selected, in the preceding training phase. Recent studies indeed suggest that these two mechanisms of distraction can be dissociated (Kim & Anderson, 2019a, 2019b).

Recall that in the training phase of many previous experiments (see Figure 1), participants were instructed to search for a shape that had a specific color (e.g., a red circle among circles of other colors). When participants repeatedly perform this search, they develop a strong, top-down attentional set for the target color, which facilitates future searches. This attentional set might then carry over to the test phase, such that the previously selected target (now distractor) color keeps drawing attention, even after it is no longer task-relevant. The consequence would be that, when comparing trials with (a) an often-selected distractor color versus (b) trials with no such distractor, performance would be impaired in the former case. Notably, this account makes no reference to a specific effect of reward. So, an alternative explanation for many previous findings is that results were due to *selection-driven* distraction rather than reward-driven distraction.

To address this pressing problem, researchers have developed more rigorous ways to test whether reward-driven distraction exists. First, rather than comparing participants' performance on high-reward versus no-distractor trials, one can test whether high-reward distractors hurt performance more strongly than *low-reward* (or *neutral*[2]) distractors (Le Pelley et al., 2016). For example, in the paradigm introduced by Anderson et al. (2011b) there are two target colors (e.g., red and green), each of which is associated with a different reward magnitude (e.g., red with 5 cents, green with 1 cent). Crucially, these target colors are equally likely to be selected in the training phase, because both served as targets; they differ only in the magnitude of reward they signal. Therefore, comparing performance in the presence of high-reward versus low-reward

[2] In some studies, researchers use neutral distractors instead of low-reward distractors (e.g., Le Pelley et al., 2017). A neutral distractor is a distractor that was paired with the delivery of 0 cents/0 points. So, it carries no reward value; however, it has the same selection history as the high-reward distractor.

distractors allows us to isolate the effect of reward value—rather than selection history—and hence provides a direct test for reward-driven distraction. Unfortunately, this more stringent test of the effect of reward on attention is not always reported among the results of the relevant studies.

Another way to rule out selection-driven accounts is to use a design without a separate learning phase. In this design, participants do not learn to select certain stimuli more than others. Instead, this approach uses just one testing session, in which the presence of certain distractor stimuli signals rewards. For example, in a study by Le Pelley et al. (2015; see also Bucker & Theeuwes, 2016b), participants performed a visual search task, in which they had to make an eye-movement to a shape-singleton target (e.g., a diamond among circles) as quickly as possible. Importantly, in some trials, one of the non-target circles was a color-singleton (e.g., a red circle among gray circles), with the color of this singleton signaling whether a high or low-reward was available on that trial. So, even though these colors signaled reward, they were never the target that participants were required to respond (or attend) to in order to earn that reward. That is, participants were never required to look at the reward-signaling distractors; in fact, the task was arranged so that if they *did* look at the colored distractor, the reward that would otherwise have been delivered on that trial was cancelled. Findings showed that participants still sometimes fixated on the colored distractor, even though this was counterproductive. More importantly, this happened most often when this distractor signaled high reward (versus low reward). This finding shows further support for a pure reward-driven distraction effect, ruling out the alternative, selection-related explanation.

Taken together, it is now clear that comparing high-reward versus low-reward distractors is the most accurate test of reward-driven distraction. Still, several past studies have compared high-reward distractors to no distractors at all, and then concluded that distraction was reward-driven, despite the confound of differences in selection history. The use of this suboptimal comparison is potentially important when considering the robustness of reward-driven distraction, if confidence in the effect is mainly based on tests that do not represent a true reward-driven account. To address this issue, the current meta-analysis tested whether the magnitude of reward-driven distraction is robust across different ways of operationalizing the phenomenon.

Our methodological approach. To investigate this issue, we tested whether the magnitude of reward-driven distraction depends on the type of comparison that is made. Following the line of reasoning addressed above, we calculated effect sizes separately for two different comparisons (a) our original effect size estimate: high-reward distractors versus low-reward (or neutral) distractors and (b) high-reward versus no distractors present. We are particularly interested in estimating the effect size for the former comparison, which we consider the cleanest test of reward-driven distraction, even though it is not always reported in empirical papers. If the latter comparison (which is less clean) yields a higher effect size than the former comparison, this suggests that selection-driven distraction is part of the explanation for many previously reported findings.

Aim 3: To Provide Methodological Guidelines for Studying Reward-Driven Distraction.

Researchers have become increasingly worried about the replicability of many findings in psychological science: many ‘classic’ and highly influential findings (e.g., ego-depletion, unconscious priming) simply could not be replicated (e.g., Doyen, Klein, Pichon, & Cleeremans, 2012; Hagger et al., 2016; Open Science Collaboration, 2015). One of the major contributors to replicability issues is *publication bias* (Renkewitz & Keiner, 2018), which refers to the idea that significant findings are more likely to be published in scientific journals than non-significant findings (Borenstein et al., 2009). Consequently, positive findings may be over-represented in the published literature, with negative findings consigned to the “file drawer” (Spellman, 2012).

Additionally, even if a study, at first, does not yield significant effects, researchers can take a lot of liberty in trying to reach significance. This behavior is often referred to as *p-hacking* or *researcher degrees of freedom* and entails practices such as carefully choosing which items to include in analyses, or which participants to exclude (Simonsohn et al., 2014a). The consequence of publication bias and p-hacking is that a meta-analysis of a certain effect (if anything) tends to overestimate the actual size of the effect – as the negative findings are not available to include in the meta-analysis, and the findings of positive studies may be artificially inflated (Renkewitz & Keiner, 2018; Simonsohn et al., 2014b).

Statistical tools have been developed in order to detect and estimate the influence of publication bias and p-hacking on effect sizes in psychological science. In this meta-analysis, we deployed these tools to assess whether the magnitude of reward-driven distraction is likely to have been overestimated in the literature, and more generally how much trust we can place in this effect.

Additionally, we conducted a methodological assessment of the literature. That is, studies testing reward-driven distraction make various methodological choices about stimuli and study design. For instance, some studies used money as reward cues (Anderson et al., 2011b), while others used the smell of chocolate (Pool et al., 2014). We currently do not know whether these methodological choices modify the magnitude of reward-driven distraction. Our meta-analysis therefore tested whether these methodological choices influence the magnitude of reward-driven distraction. Results from this test can inform theory on the reliability of reward-driven distraction across different methodological approaches, but also can make suggestions for future researchers on how to design their studies.

Our methodological approach. First, we tested whether the reward-driven distraction literature shows evidence of publication bias. We constructed and interpreted a funnel plot

(Duval & Tweedie, 2000; Peters et al., 2008; Sterne et al., 2011). Moreover, we corrected for potential publication bias using weight-function modelling (Vevea & Hedges, 1995). Second, to test whether there is evidence for p-hacking, and to assess the evidential value for reward-driven distraction, we conducted a *p-curve* analysis (Simonsohn et al., 2014a).

In order to test whether methodological choices impact the magnitude of reward-driven distraction, we coded seven moderators. First, we coded *type of learning*. That is, we coded whether participants had to *select* certain stimuli to gain rewards (i.e., instrumental learning), or instead whether stimuli merely signaled the magnitude of the available reward, but participants were not required to respond to these stimuli (i.e., Pavlovian learning). Second, we coded *type of reward*, that is, the reward that was associated with distractors. While some studies used primary rewards that have higher biological relevance, such as food (Piech et al., 2010) or odor (Pool et al., 2014), others used secondary rewards that have acquired their value through socialization, such as money, points, or people’s own names (Ljungberg et al., 2014). Third, we coded the *ratio of low versus high*, that is, the ratio between how much money participants received on low versus high-reward trials (note: this moderator was only coded for studies that used money or points). In some studies, the difference between the reward value of low and high was small (e.g., 1 cent vs. 5 cents; e.g., (Anderson et al., 2011b), whereas in other studies this difference was larger (e.g., 25 cents vs. 1.5 dollars; Anderson, Kuwabara, et al., 2016). Fourth, we coded *length of training*, that is, the duration of the training phase (number of trials) to test whether longer training leads to stronger interference as a consequence of the greater opportunity for reward learning. Fifth, we coded *stimulus features*, that is, what specific stimulus feature was associated with rewards (e.g., low level features like color and location, or higher-level features like semantic category). Sixth, we coded *measure*, that is, whether the study used a direct (e.g., eye movements) versus indirect (e.g., reaction time [RT] or accuracy) measure to assess reward-driven distraction. Seventh, we coded *physical salience*, that is whether reward-associated distractors were physically salient (e.g., a single red shape among a set of grey shapes) or not (e.g., a single red shape among a set of uniquely colored shapes).

Method

This research was approved by the Ethics Committee of the Social Science Faculty of Radboud University. We aimed to make our meta-analysis reproducible. Based on guidelines from recent work (Lakens et al., 2016), we disclose all decisions we made in coding effect sizes, all meta-analytic data, and our analysis script online on the Open Science Framework (OSF; https://osf.io/rgeb6/). In addition to providing transparency, this will also facilitate future meta-analyses of the same data-set using different techniques as new theoretical approaches or statistical methods emerge (Lakens et al., 2016).

Inclusion Criteria

We selected studies that met the following criteria:

1. The article was written in English.
2. The article contained original data that were collected from healthy human populations and were published before August 2017.
3. The study used a task that measures any type of cognitive performance (e.g., visual search, conflict processing, etc.).
4. The dependent variable in the study was either a behavioral measure (e.g., RT or accuracy) or eye-gaze behavior.
5. The study contained distractor stimuli that were associated with any type of reward (e.g., a primary reward, such as food, or a secondary reward, such as money).
6. The study measured our *contrast of interest*, that is, it examined the effects of distractor stimuli associated with *high* versus *low (or neutral)* reward. We also included studies that examined the

effects of distractor stimuli associated with *high-reward* versus *no task-irrelevant stimuli present*. Studies that compared high-reward distractor stimuli with anything other than low-reward or neutral distractor stimuli (e.g., reward vs. loss, erotic pictures vs. mutilated pictures) were not included in this meta-analysis.

7. The contrast of interest was tested with a within-subject design.

8. The effect size of the contrast of interest could be calculated from descriptive statistics (mean and standard deviation for each condition).

Systematic Search Strategy

We searched for published articles in three ways (see Figure 2). First, we searched for all available records on Web of Knowledge until August 2017. For this, we used the following search terms: TS = ((value OR reward) AND (irrelevant OR incidental) AND (attention* OR performance OR cognit*) AND (stimul* OR cue*)). This search yielded 292 hits. Second, we systematically consulted the two most recent narrative reviews and their reference sections (Anderson, 2016b; Le Pelley et al., 2016), which yielded 440 articles. Third and last, we searched for articles that cited the key publication (Anderson et al., 2011b), which yielded 294 articles. Overall, these three searches yielded 868 papers, after deleting duplicates.

We applied our inclusion criteria in two steps. First, one rater read the abstracts of all articles and checked whether they met the Step-1 criteria (inclusion criteria 1-5 above). In case it was not clear whether a study fulfilled an inclusion criterion, a second rater also read the abstract, and the two raters made a mutual decision. This phase of the systematic search yielded 119 articles.

Second, two raters read the remaining 119 articles in full and checked the Step-2 criteria (inclusion criteria 6-8 from the list above). We assessed inter-rater variability by comparing the decisions of the two raters. We found that agreement was moderate (Cohen's k = .79). Then, the

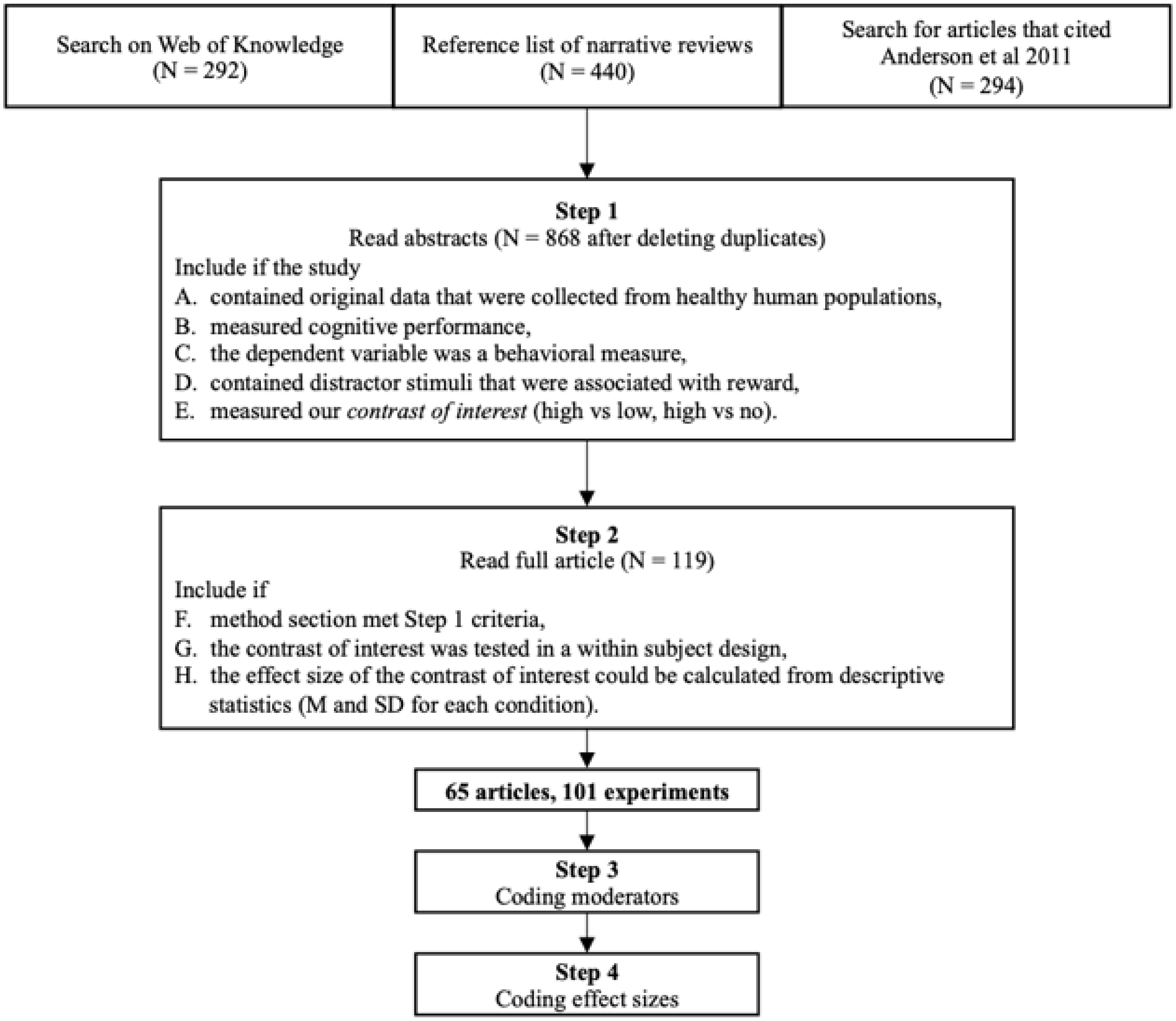

Figure 2. Selection procedure of articles.

two raters discussed the disagreements and came to a consensual decision. This search phase yielded 65 articles, which reported a total of 101 original experiments.

Calculating Effect Sizes

As discussed above, we were mainly interested in the difference between high versus low or high versus no reward-associated distractors. However, these comparisons (and the corresponding effect sizes) were not always explicitly reported in the articles. Instead, many articles reported an omnibus test (e.g., *F* test) that compared three conditions: high, low, and no reward-associated distractors. An effect size of this omnibus test (i.e., partial eta squared) is not particularly informative, as it does not reveal (a) which conditions differ from each other and (b) what the direction of the effect is. Thus, in many cases, the statistics reported in the articles were not sufficient to test our specific contrasts of interest.

To solve this issue, we calculated effect sizes of the specific comparisons from descriptive statistics with the *escalc* function of the metafor package, (version 2.0-0; Viechtbauer, 2010) in R (R Core team, 2015). This solution was beneficial for two reasons. First, we did not have to rely on what was reported in the articles but we could calculate effect sizes for all possible comparisons (high vs. low or neutral and high vs. no distractor present). Second, it ensured that every effect size included in this meta-analysis was calculated in the same way. This is especially important because researchers, in general, tend to rely too much on statistical software packages that may not provide the optimal effect size for the specific design that was used (Lakens, 2013). For the effect size calculation, we extracted descriptive statistics from each study. More specifically, we coded means (*M*) and standard deviations (*SD*) for each of the conditions in the study (i.e., high-reward, low or neutral-reward, and no distractor present). In some cases, these statistics were reported in the text or in tables. In other cases, however, descriptive statistics were not reported in text, but were visualized in plots. In these cases, we digitized plots and extracted statistics based on guidelines from Parmar, Torri, and Stewart (1998) with a plot digitizer (Rohatgi, 2011). In these cases, we converted the standard error of the mean to *SD*. Finally, when no descriptive statistics were available in the articles, we contacted the authors to provide us with *M* and *SD* in each condition. If the authors did not respond to this original email, or to two subsequent reminders, we excluded the article from the analysis (n = 2).

After extracting these descriptive statistics, we calculated effect sizes for all possible comparisons (i.e., high-reward vs. low-reward/neutral and high-reward vs. no distractor present). Based on guidelines on meta-analysis (Morris, 2000; Viechtbauer, 2016), the optimal effect size for within-subject designs and quantitative dependent variables was *standardized mean change* using change score standardization (SMC). The change score had to be *standardized* because we had one dependent variable that was measured in different ways: that is, our dependent variable was performance, but it could be measured in terms of response times, accuracy, eye-gaze, etc. (Borenstein et al., 2009). Thus, in the current meta-analysis, SMC refers to the mean change in response time or accuracy when participants were exposed to low (or neutral) versus high-reward distractors during the task.

We gave SMC a positive sign when performance was worse in the high-reward distractor condition compared to the low (or neutral) reward distractor condition. When performance was better in the high-reward distractor condition compared to the low-reward distractor condition, we gave SMC a negative sign.

Importantly, calculating an effect size for within-subject comparisons is critically dependent on the correlation between conditions (i.e., in this case, correlation between performance on high-reward and low-reward/neutral distractor trials). In general, when the correlation is large, the error tends to be smaller and thus the test statistics are larger. When the effect size is calculated from such test statistics, it is often overestimated (Dunlap et al., 1996). Thus, to correct for this potential overestimation, we included the correlation coefficient between high versus low/neutral condition in our effect-size calculation. Unfortunately, the correlation between conditions is almost never reported in published research. To solve this issue, we calculated an average correlation coefficient from raw data that were available from typical studies that are a part of this meta-analysis (Feldmann-Wustefeld et al., 2016; Le Pelley et al., 2015). With this procedure, we estimated the average correlation between performance with high-reward versus low-reward/neutral distractors to be .90.

Decisions about Effect Sizes

While coding effect sizes, we made the following decisions:

1. In order to be consistent in effect size calculation (Lakens et al., 2016), we included only studies in which the *simple effect of reward* (high-reward vs. low-reward/neutral; high-reward vs. no distractor present) could be calculated. Based on this decision, we had to exclude studies that measured our contrast of interest with an *interaction effect* between reward and other characteristics (Asgeirsson & Kristjánsson, 2014; Asutay & Vastfjall, 2016; Hickey et al., 2010b, 2010a; Hickey & van Zoest, 2013).
2. When the study contained a condition with any sort of clinical symptom, we coded statistics only for the healthy control condition (e.g., Anderson, Faulkner, Rilee, Yantis, & Marvel, 2013; Anderson et al., 2014).
3. There were several studies that reported descriptive statistics of high-reward distractors, low-reward/neutral distractors, and no distractor present conditions *across* the levels of an additional factor (e.g., invalid vs. valid, congruent vs. incongruent). In order to be consistent, in all of these studies, we decided to select only one condition, which suited our inclusion criteria best. More specifically, when the study used a spatial cueing task (Bourgeois et al., 2016, 2017; Bucker & Theeuwes, 2016a; Munneke et al., 2015; Pool et al., 2014; Rutherford et al., 2010), we selected the exogenous, invalid condition. When the study used a Stroop task (Krebs et al., 2010, 2013), we selected the incongruent, no reward condition. When the study used a flanker task (Anderson et al., 2012; Nikolaou et al., 2013), we selected the incongruent condition. In some other cases, when it was impossible to choose one

condition that suited our inclusion criteria (e.g., when descriptive statistics were presented by different blocks of trials; e.g., Le Pelley et al., 2015), we contacted authors for descriptive statistics. For a more detailed selection procedure, see "Inclusion decisions" file on https://osf.io/rgeb6/).

4. We coded descriptive statistics for both response times and accuracy (when reported). For the final effect size calculation, we included the one that was the most meaningful in the design of the study (e.g., response time in search tasks, accuracy in rapid serial visual presentation [RSVP]). When only eye-tracking measures were reported, we coded response latency and time to fixate the target as response times and proportion of trials on which gaze fell on the distractor as accuracy. For a more detailed selection procedure, see "Inclusion decisions" file on https://osf.io/rgeb6/).
5. Before we started coding, we expected to encounter papers that reported several analyses (and thus, several effect sizes) that are relevant to our contrast of interest. We anticipated that, in these cases, it could sometimes be difficult to determine which of the analyses was the most relevant analysis. Thus, we decided that in such cases (in which it was debatable which analysis was the most relevant analysis), we would code the first analysis that was presented in the paper. However, in the vast majority of papers, we had no difficulties identifying and selecting the most relevant analysis; we used this decision rule only in one instance (Anderson & Yantis, 2012).

Moderators

Two raters coded moderators (Table 1). We calculated inter-rater variability by comparing the decision of two raters on 15 articles. Cohen's k varied between .86 and 1 across all moderators with an average of .96. Thus, overall inter-rater variability was high. In case of disagreements, the coding decisions were discussed, and the two raters came to an agreement.

Table 1

List of moderators grouped by the three major aims.

Moderator	Levels (k)
Aim 1	
1a. Type of performance task	visual search (59), spatial cueing (8), RSVP (7), judgment (7), conflict processing (5), visual memory (5).
1b. Type of performance task	spatial (66), non-spatial (25)
2. Difference between training and test phases	same (36), different (22), no learning phase (33)
3. Task instructions	no instructions (53), ignore distractors (38)
Aim 3	
4. Type of learning	instrumental (55), Pavlovian (36)
5. Type of reward	money (52), points (32), own name (2), odor (2), social (1), food (1), alcohol (1)
6. Ratio between low and high reward	money/points assigned to low value divided by money/points assigned to high value [continuous; M = .15, SD = .08] (71*)
7. Length of training	number of trials [continuous; M = 619, SD = 696] (58†)
8. Stimulus features	color (65), picture (10), orientation (6), sound (5), shape (5)
9. Type of distraction measure	indirect (79), direct (12)
10. Physical salience of distractor	physically salient (32), physically non-salient (59)

k = number of studies. *coded only for studies in which reward value was quantifiable and in which the low-value reward was greater than zero cents/points. †coded only for studies that included a separate training phase with a fixed number of trials.

Details about moderators coded for each study can be found on our OSF page (“Final data” on https://osf.io/rgeb6/). In total, we tested three theory-based moderators (#1–3 in Table 1) and seven moderators related to methodological variations (#4–10 in Table 1). We examined how these moderators affected the effect size of reward-driven distraction (i.e., the effect of high reward vs. low reward/neutral distractors).

Results

Meta-Analytical Procedures

We conducted the meta-analytic computations in R (R Core team, 2015), using the metafor package (version 2.0-0;Viechtbauer, 2010). In order to calculate summary effect sizes, we ran a random-effects model. For moderator analyses, we used mixed-effects for meta-regression models (Borenstein et al., 2009).

Aim 1: To Establish the Magnitude and Scope of Reward-Driven Distraction.

Magnitude. In our main meta-analysis, we tested whether performance is more impaired by high-reward compared to low-reward (or neutral) distractors. This analysis indicated a significant and small effect across studies (k = 91, n = 2362), showing that performance is, on average, systematically more impaired by high-reward than low-reward (or neutral) distractors (*Standardized Mean Change [SMC]* = 0.347, *CI* = [0.257, 0.437], Z = 7.54, p < .001); Figure 3 shows a forest plot for this meta-analysis.

There was substantial between-studies variation in the magnitude of reward-driven distraction. Specifically, SMC for individual studies ranged from –2.00 to 1.84. Although part of this variation can be explained by within-study error, this variation also reflected variation in true effects, $Q(90)$ = 353.9, p < .001. The estimated variance of the true effects, T^2, was 0.142, which yielded a prediction interval of [-0.396, 1.091]. The *prediction interval* refers to the range of true effects that we can expect in 95% of hypothetical, future studies (IntHout et al., 2016). In other

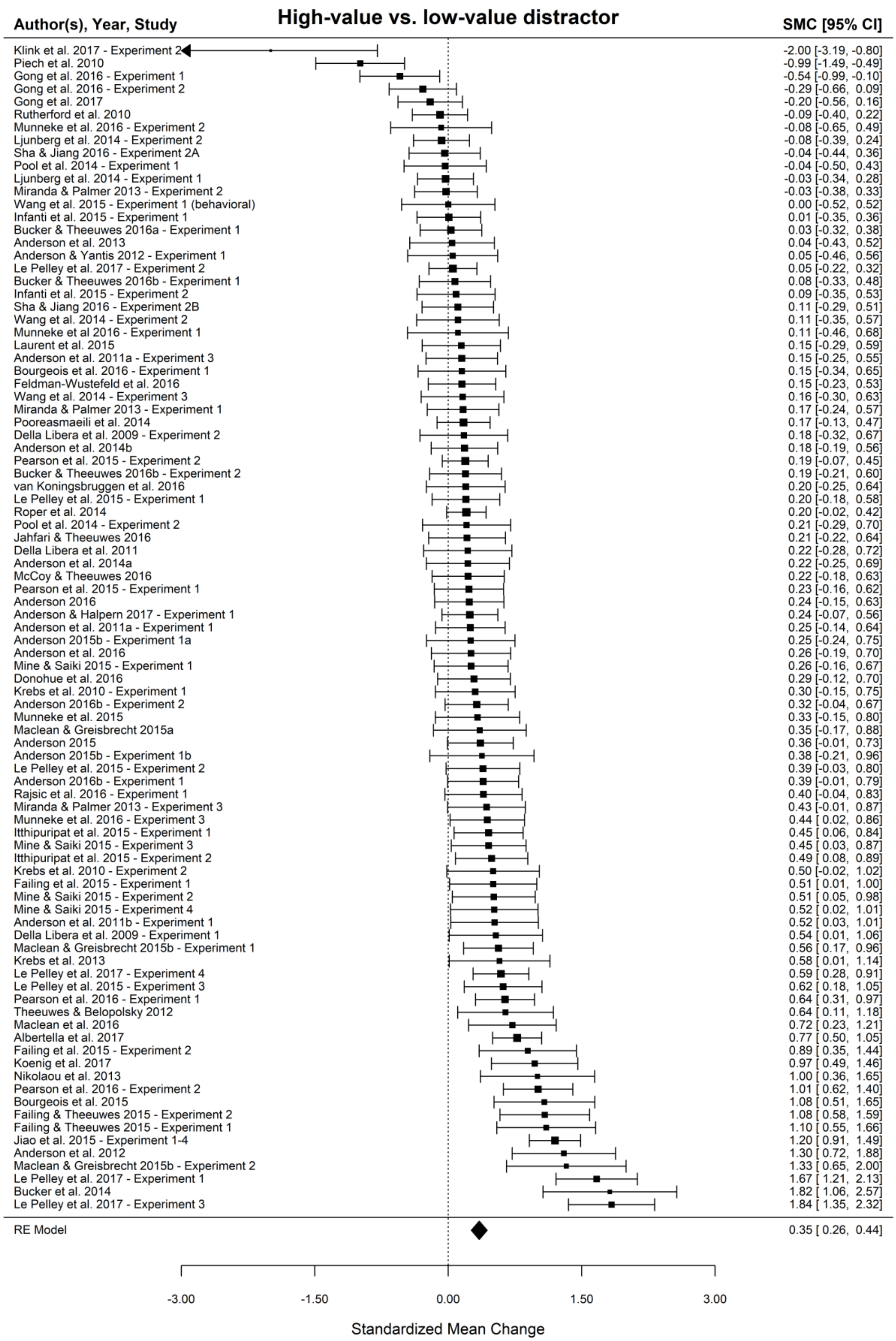
High-value vs. low-value distractor
Author(s), Year, Study
SMC [95% CI]
Klink et al. 2017 - Experiment 2 -2.00 [-3.19, -0.80]
Piech et al. 2010 -0.99 [-1.49, -0.49]
Gong et al. 2016 - Experiment 1 -0.54 [-0.99, -0.10]
Gong et al. 2016 - Experiment 2 -0.29 [-0.66, 0.09]
Gong et al. 2017 -0.20 [-0.56, 0.16]
Rutherford et al. 2010 -0.09 [-0.40, 0.22]
Munneke et al. 2016 - Experiment 2 -0.08 [-0.65, 0.49]
Ljunberg et al. 2014 - Experiment 2 -0.08 [-0.39, 0.24]
Sha & Jiang 2016 - Experiment 2A -0.04 [-0.44, 0.36]
Pool et al. 2014 - Experiment 1 -0.04 [-0.50, 0.43]
Ljunberg et al. 2014 - Experiment 1 -0.03 [-0.34, 0.28]
Miranda & Palmer 2013 - Experiment 2 -0.03 [-0.38, 0.33]
Wang et al. 2015 - Experiment 1 (behavioral) 0.00 [-0.52, 0.52]
Infanti et al. 2015 - Experiment 1 0.01 [-0.35, 0.36]
Bucker & Theeuwes 2016a - Experiment 1 0.03 [-0.32, 0.38]
Anderson et al. 2013 0.04 [-0.43, 0.52]
Anderson & Yantis 2012 - Experiment 1 0.05 [-0.46, 0.56]
Le Pelley et al. 2017 - Experiment 2 0.05 [-0.22, 0.32]
Bucker & Theeuwes 2016b - Experiment 1 0.08 [-0.33, 0.48]
Infanti et al. 2015 - Experiment 2 0.09 [-0.35, 0.53]
Sha & Jiang 2016 - Experiment 2B 0.11 [-0.29, 0.51]
Wang et al. 2014 - Experiment 2 0.11 [-0.35, 0.57]
Munneke et al 2016 - Experiment 1 0.11 [-0.46, 0.68]
Laurent et al. 2015 0.15 [-0.29, 0.59]
Anderson et al. 2011a - Experiment 3 0.15 [-0.25, 0.55]
Bourgeois et al. 2016 - Experiment 1 0.15 [-0.34, 0.65]
Feldman-Wustefeld et al. 2016 0.15 [-0.23, 0.53]
Wang et al. 2014 - Experiment 3 0.16 [-0.30, 0.63]
Miranda & Palmer 2013 - Experiment 1 0.17 [-0.24, 0.57]
Pooreasmaeili et al. 2014 0.17 [-0.13, 0.47]
Della Libera et al. 2009 - Experiment 2 0.18 [-0.32, 0.67]
Anderson et al. 2014b 0.18 [-0.19, 0.56]
Pearson et al. 2015 - Experiment 2 0.19 [-0.07, 0.45]
Bucker & Theeuwes 2016b - Experiment 2 0.19 [-0.21, 0.60]
van Koningsbruggen et al. 2016 0.20 [-0.25, 0.64]
Le Pelley et al. 2015 - Experiment 1 0.20 [-0.18, 0.58]
Roper et al. 2014 0.20 [-0.02, 0.42]
Pool et al. 2014 - Experiment 2 0.21 [-0.29, 0.70]
Jahfari & Theeuwes 2016 0.21 [-0.22, 0.64]
Della Libera et al. 2011 0.22 [-0.28, 0.72]
Anderson et al. 2014a 0.22 [-0.25, 0.69]
McCoy & Theeuwes 2016 0.22 [-0.18, 0.63]
Pearson et al. 2015 - Experiment 1 0.23 [-0.16, 0.62]
Anderson 2016 0.24 [-0.15, 0.63]
Anderson & Halpern 2017 - Experiment 1 0.24 [-0.07, 0.56]
Anderson et al. 2011a - Experiment 1 0.25 [-0.14, 0.64]
Anderson 2015b - Experiment 1a 0.25 [-0.24, 0.75]
Anderson et al. 2016 0.26 [-0.19, 0.70]
Mine & Saiki 2015 - Experiment 1 0.26 [-0.16, 0.67]
Donohue et al. 2016 0.29 [-0.12, 0.70]
Krebs et al. 2010 - Experiment 1 0.30 [-0.15, 0.75]
Anderson 2016b - Experiment 2 0.32 [-0.04, 0.67]
Munneke et al. 2015 0.33 [-0.15, 0.80]
Maclean & Greisbrecht 2015a 0.35 [-0.17, 0.88]
Anderson 2015 0.36 [-0.01, 0.73]
Anderson 2015b - Experiment 1b 0.38 [-0.21, 0.96]
Le Pelley et al. 2015 - Experiment 2 0.39 [-0.03, 0.80]
Anderson 2016b - Experiment 1 0.39 [-0.01, 0.79]
Rajsic et al. 2016 - Experiment 1 0.40 [-0.04, 0.83]
Miranda & Palmer 2013 - Experiment 3 0.43 [-0.01, 0.87]
Munneke et al. 2016 - Experiment 3 0.44 [0.02, 0.86]
Itthipuripat et al. 2015 - Experiment 1 0.45 [0.06, 0.84]
Mine & Saiki 2015 - Experiment 3 0.45 [0.03, 0.87]
Itthipuripat et al. 2015 - Experiment 2 0.49 [0.08, 0.89]
Krebs et al. 2010 - Experiment 2 0.50 [-0.02, 1.02]
Failing et al. 2015 - Experiment 1 0.51 [0.01, 1.00]
Mine & Saiki 2015 - Experiment 2 0.51 [0.05, 0.98]
Mine & Saiki 2015 - Experiment 4 0.52 [0.02, 1.01]
Anderson et al. 2011b - Experiment 1 0.52 [0.03, 1.01]
Della Libera et al. 2009 - Experiment 1 0.54 [0.01, 1.06]
Maclean & Greisbrecht 2015b - Experiment 1 0.56 [0.17, 0.96]
Krebs et al. 2013 0.58 [0.01, 1.14]
Le Pelley et al. 2017 - Experiment 4 0.59 [0.28, 0.91]
Le Pelley et al. 2015 - Experiment 3 0.62 [0.18, 1.05]
Pearson et al. 2016 - Experiment 1 0.64 [0.31, 0.97]
Theeuwes & Belopolsky 2012 0.64 [0.11, 1.18]
Maclean et al. 2016 0.72 [0.23, 1.21]
Albertella et al. 2017 0.77 [0.50, 1.05]
Failing et al. 2015 - Experiment 2 0.89 [0.35, 1.44]
Koenig et al. 2017 0.97 [0.49, 1.46]
Nikolaou et al. 2013 1.00 [0.36, 1.65]
Pearson et al. 2016 - Experiment 2 1.01 [0.62, 1.40]
Bourgeois et al. 2015 1.08 [0.51, 1.65]
Failing & Theeuwes 2015 - Experiment 2 1.08 [0.58, 1.59]
Failing & Theeuwes 2015 - Experiment 1 1.10 [0.55, 1.66]
Jiao et al. 2015 - Experiment 1-4 1.20 [0.91, 1.49]
Anderson et al. 2012 1.30 [0.72, 1.88]
Maclean & Greisbrecht 2015b - Experiment 2 1.33 [0.65, 2.00]
Le Pelley et al. 2017 - Experiment 1 1.67 [1.21, 2.13]
Bucker et al. 2014 1.82 [1.06, 2.57]
Le Pelley et al. 2017 - Experiment 3 1.84 [1.35, 2.32]
RE Model 0.35 [0.26, 0.44]
-3.00
-1.50
0.00
1.50
3.00
Standardized Mean Change

Figure 3 (on previous page). Forest plot showing the estimated standardized mean change of cognitive performance in high-reward distractor trials compared to low-reward or neutral distractor trials. The left column shows the authors, year, and experiment numbers. The middle column shows the individual study effect sizes (black squares), with estimated 95% confidence intervals (error bars). The size of the square reflects the sample size of the study. The right column shows these effect sizes and CIs numerically. The arrow at the left side of the error bars indicates that the CI is large and that it continues beyond the size of the graph. RE model = Random-effects model.

words, although future studies can plausibly expect to find that high-reward distractors strongly impair performance (up to *SMC* = 1.091, a very large effect), it is also plausible that studies may find results indicating that high-reward distractors may *boost*, not hurt, performance (with up to *SMC* = -0.396, a small-to-moderate effect). We further observed that I^2 was 76.3%. I^2 gives an indication of what proportion of the observed variance (see Figure 3) would remain if we were able to remove all sampling error (note that I^2 is not an abolute measure of heterogeneity; Borenstein, Higgins, Hedges, & Rothstein, 2017).

We further explored the origins of the substantial heterogeneity that we found in our dataset. First, we constructed and visually inspected a Baujat plot (Baujat, Mahé, Pignon, & Hill, 2002; see Supplementary Material). Interpreting this plot, it turned out that five studies made an overly strong contribution to heterogeneity, potentially affecting the results. Thus, as a sensitivity analysis, we repeated our main analysis after excluding these five studies. Results were similar to results from our main analysis. In particular, *SMC* = 0.321, *CI* = [0.235, 0.408], T^2 = 1.18, I^2 = 72.5%. Moreover, the prediction interval was [-0.357, 1.000], which still suggested that medium-sized reverse effects (i.e., high-value distractors increasing performance) are to be expected. So, the heterogeneity in our dataset does not seem to be driven by a limited set of atypical studies.

As noted in the Method, when calculating effect sizes, we assumed that the average correlation between within-subject conditions was r = .90. Although we believe this assumption is realistic, we conducted two sensitivity analyses in which we assumed lower correlations between conditions (i.e., r = .80 and r = .70). These analyses suggested that the meta-analytic effect size decreased, but remained significant, while heterogeneity decreased as well. For r = .80, *SMC* = 0.254, *CI* = [0.187, 0.322], T^2 = 0.06, I^2 = 59.1%, prediction interval = [-0.232, 0.741]. For r = .70, *SMC* = 0.212, *CI* = [.155, 269], T^2 = 0.03, I^2 = 44.6%, prediction interval = [-0.148, 0.572]. Finally, to account for the possibility that experiments reported in the same article may be more similar to one another than they are to other experiments, causing dependencies within our data, we carried out a further sensitivity analysis in which we included a random effect for each article. This sensitivity analysis yielded estimates that were similar to those from the main meta-analysis reported above, *SMC* = .325, *CI* = [0.225, 0.425], $Q(90)$ = 353.9, T^2 = 0.119, prediction interval = [-0.359, 1.009].

Scope. In order to explore the scope of reward-driven distraction, we tested (a) whether reward-driven distraction is moderated by the type of the performance task, (b) whether training and test phases used different methods, and (c) whether participants were explicitly instructed to ignore distractors. First, we included all three predictors in our mixed-effects meta-regression model. This overall model was significant, Q_M (8) = 23.8, p = .003, which suggested that a significant proportion of the heterogeneity in the true effect can be explained by at least one of these moderators.

We next examined the effect of each moderator separately. Inspection of Table 2 shows that only the type of performance task moderated the magnitude of reward-driven distraction significantly. So, some performance tasks are more conducive to reward-driven distraction than others. More specifically, we found significant reward-driven distraction effects only for visual

Table 2

Summary results of the moderator analysis.

Moderator	k	*SMC*	*95% CI*	Q_M	Q_E	T^2	I^2
Aim 1							
1a. Type of performance task				22.6***	297.2***	0.115	72.1
Visual search	59	0.349***	[0.246, 0.452]				
Spatial cueing	8	0.268	[-0.016, 0.552]				
RSVP	7	0.732**	[0.431, 1.032]				
Judgment	7	0.235	[-0.057, 0.526]				
Conflict processing	5	0.710***	[0.325. 1.095]				
Visual memory	5	-0.315	[-0.689, 0.058]				
1b. Type of performance task				< 1	353.8***	0.144	76.5
Spatial	66	0.336***	[0.230, 0.443]				
Non-spatial	25	0.378***	[0.204, 0.552]				
2. Difference between training and test phases				< 1	166.0***	0.084	64.5
Same	36	0.265***	[0.145, 0.384]				
Different	22	0.304***	[0.149, 0.460]				
3. Task instructions				< 1	353.2***	0.144	76.6
No instructions	53	0.341***	[0.222, -0.461]				
Ignore distractors	38	0.356***	[0.216, 0.497]				
Aim 3							
4. Type of learning				< 1	352.4***	0.143	76.5
Instrumental	55	0.320***	[0.202, 0.437]				
Pavlovian	36	0.388***	[0.246, 0.531]				
5. Type of reward				4.6	334.6***	0.136	75.4
Money	52	0.391***	[0.274, 0.509]				
Points	32	0.347***	[0.198, 0.496]				
Other	7	0.020	[-0.301, 0.340]				
6. Ratio between low and high reward				< 1	224.5***	0.099	69
7. Length of training				< 1	165.7***	0.084	64.6
8. Stimulus features				5.9	337.5***	0.139	75.9
Color	65	0.345***	[0.239, 0.451]				
Picture	10	0.614**	[0.344, 0.884]				
Orientation	6	0.209	[-0.149, 0.567]				
Sound	5	0.150	[-0.210, 0.510]				
Shape	5	0.216	[-0.179, 0.610]				
9. Type of distraction measure				7.8**	328.8	0.129	74.4
Indirect	79	0.298***	[0.205, 0.392]				
Direct	12	0.663***	[0.424, 0.902]				
10. Physical salience of distractor				< 1	353.1	0.144	76.5
Physically salient	32	0.388	[0.235, 0.541]				
Physically non-salient	59	0.326	[0.213, 0.438]				

Note: k = number of studies, *SMC* = standardized mean change, Q_M = test of moderator, Q_E = residual heterogeneity (measure of weighted squared deviations), T^2 = between-studies variance, I^2 = the ratio of true heterogeneity to total observed variation.
* $p < .05$, ** $p < .01$, $p < .001$

search tasks, rapid serial visual presentation (RSVP) tasks, and conflict processing tasks. The other two moderators (difference between training and test phases; task instructions) did not significantly affect reward-driven distraction. As in other forms of null-hypothesis significance testing, absence of evidence does not imply evidence of absence. In other words, the non-significant findings here do not show evidence against effects of these two moderators.

We next aimed to further understand the significant effect of type of task. First, to explore the range of to-be-expected effects, we computed prediction intervals separately for all task types. For visual search tasks, spatial cuing tasks, judgment tasks, conflict processing tasks, and visual memory tasks, the prediction interval contained zero (prediction intervals were [-0.323, 1.020], [-0.454, 0.990], [-0.490, 0.959], [-0.057, 1.477], and [-1.077, 0.446], respectively), indicating that reverse effects—i.e., high-reward distractors being associated with increased performance—can plausibly be expected, at least sometimes. In fact, for visual memory tasks, reverse effects should be expected by default. For RSVP tasks, the prediction interval did not include zero, [0.003, 1.460], suggesting that it should be surprising if future research finds a reverse effect on this task type.

Second, in the preceding analyses, we examined each moderator separately, treating our data as if all moderators were independent of one another. However, it is likely that specific levels of some moderators tend to co-occur with specific levels of some other moderators (e.g., it may be the case that visual search tasks often use explicit instructions to ignore the distractor). So, next, we explored all potential dependencies between moderators. A full report of this exploration is presented in the Supplementary Materials. Here, we report only how the type of performance task—the only moderator for which we found a significant effect—was associated with the other moderators: (a) Type of task was associated with whether different tasks were used in the training vs. test phases. Compared to all other task types, studies that used visual search tasks in the test phase, often used the same task in the training phase. (b) Type of task was associated with the ratio between low and high reward. Studies that used judgment tasks tended to use a smaller relative difference between low vs. high reward; studies that used spatial cueing, visual memory, and visual search tasks used a larger difference. (c) Type of task was associated with the number of training trials. Studies that used judgment tasks used many training trials; studies that used conflict tasks and spatial cueing tasks used relatively few training trials. (d) Type of task was associated with which stimulus features were rewarded. Studies that used visual search tasks predominantly used color as the rewarded dimension; studies that used RSVP tasks often used pictures. Note that none of the moderators that was associated with type of task (i.e., difference between training and test phases, ratio between low and high reward, length of training, and stimulus features), yielded a significant effect on reward-driven distraction on its own (Table 2). So, there is no evidence that these other moderators can account for the effect of type of task on reward-driven distraction.

Third, in order to get more insight into how the type of the performance task influences the magnitude of reward-driven distraction, we grouped the paradigms into two distinct categories. Specifically, visual search and spatial cueing were categorized as conducive to a spatial form of distraction, in which distractors appear at different locations than targets. By contrast, conflict processing, RSVP, judgment, and visual memory were categorized as assessing a non-spatial form of distraction, in which distractors and targets appear at the same location. We again conducted a mixed-effects meta-regression model, but now with this dichotomous variable (type of performance task: spatial vs. non-spatial) as the only moderator. Table 2 shows that the test of this moderator was not significant. So, there is no evidence that the reward-driven distraction effect is stronger when reward-related distractors appear in different locations than task-relevant target stimuli.

Aim 2: To Examine an Alternative Explanation: Have We Been Studying Selection-Driven Distraction Instead of Reward-Driven Distraction?

In the introduction, we reasoned that the most accurate test of reward-driven distraction is the comparison between how high-reward distractors and low-reward (or neutral) distractors influence performance. Yet, researchers often report different comparisons. Most notably, prior work has often compared the effects of high-reward distractors to no previously-trained distractors. The problem with this comparison is that differences in performance may stem from a selection-driven process, rather than a reward-driven process (see Introduction; Anderson & Halpern, 2017; Le Pelley et al., 2016; Sha & Jiang, 2016). Therefore, we assessed whether the estimated magnitude of reward-driven distraction changes based on the type of comparison researchers make.

To examine the effect of choice of comparison, we calculated an effect-size estimate that reflected the difference between when there were *high-reward distractors* present versus when there were *no distractors* present. Note that some studies used physically salient reward distractors (e.g., distractors were color singletons; Le Pelley et al., 2015). In these studies, any difference between high-reward distractors versus no distractors could stem from the impact of physical salience on attention (Theeuwes, 1992), not just from selection-driven and reward-driven processes. To rule out this alternative explanation, in this analysis, we included studies that used distractors that were not physically salient (e.g., shapes in the critical test phase were heterogeneously colored, and hence distractors were not color singletons). Results are summarized in Figure 4. As before (when we compared high-value vs. low-value distractors), results from this meta-analysis (k = 45, n = 1,140) showed that high-reward distractors systematically impair performance compared to when there are no distractors present, SMC = 0.493, CI = [0.301, 0.685], p < .001. Heterogeneity was even higher than in the main analysis,

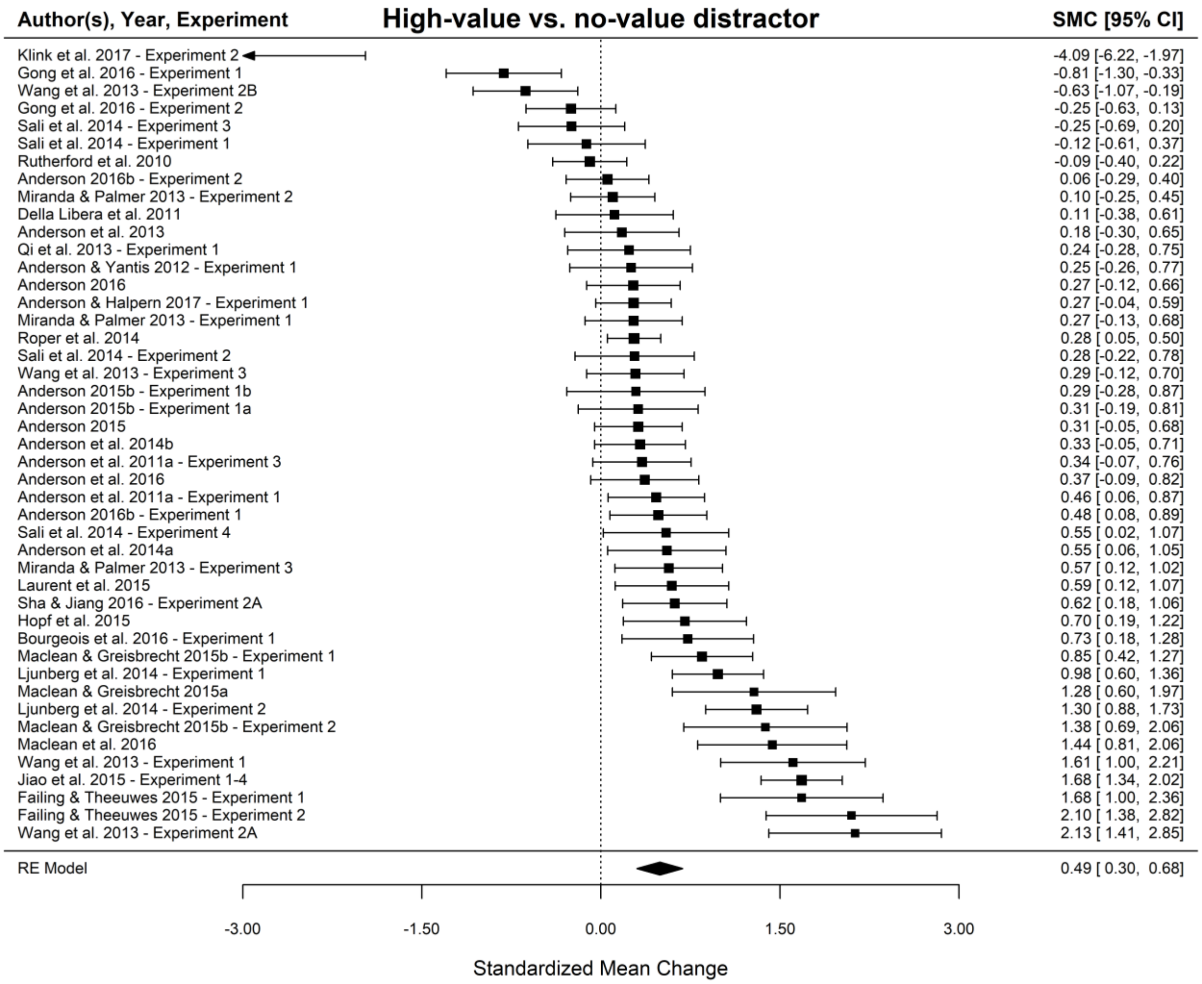

Figure 4. Forest plot showing the estimated standardized mean change of cognitive performance in high-reward distractor trials compared to no-distractor trials. The left column shows the authors, year, and experiment numbers. The middle column shows the individual study effect sizes (black squares), with estimated 95% confidence intervals (error bars). The size of the square reflects the sample size of the study. The right column shows these effect sizes and CIs numerically. RE model = Random-effects model.

indicating that there was large variation between studies, $Q(44) = 297.1$, $p < .001$, $T^2 = 0.366$, $I^2 = 88.0\%$, prediction interval = [-0.708, 1.694]. Importantly, the effect size in this analysis, SMC = 0.493, was 1.4 times larger than our original effect size estimate, SMC = 0.347. We should note, though, that the confidence intervals from the two estimates overlap. With some caution, this difference suggests that it is plausible that published effect sizes do not just stem from a reward-driven process, but also from a selection-driven process, and that previous studies based on the “high-reward versus no distractor” contrast may have overestimated the magnitude of reward-driven distraction.

Aim 3: To Provide Methodological Guidelines for Studying Reward-Driven Distraction.

Publication bias. First, we visually assessed the possibility of publication bias by inspecting a *funnel plot* (Figure 5), which presents the effect size of individual studies against the standard error (*SE*) of those same effect sizes. As noted in the Method section, not all published articles report effect sizes for the same comparison. So, to be consistent across studies, we calculated effect sizes ourselves, based on means and standard deviations, for the comparison we consider most appropriate (i.e., high value vs. low value or neutral distractors). In the funnel plot, we distinguish between studies that reported this specific comparison (filled circles) versus those that reported a different comparison (empty circles). In general, one would expect funnel plots to be tent-shaped, such that more precise studies (i.e., studies with small *SE*s) produce effect size estimates that are closest to the meta-analytic effect size (solid, red, vertical line in Figure 5).

We note three observations from the funnel plot (Figure 5). First, on the one hand, the cloud looks somewhat asymmetrical: studies with a large, positive effect size and a large standard error seem to be somewhat overrepresented. This pattern could be an indication of publication bias (Egger, Smith, Schneider, & Minder, 1997). Second, there seems to be a sharp drop-off in density at the transition from positive to negative effects. This pattern could indicate that negative effects are less likely to be published than positive effects. Third, there are several studies for which the statistical test (of the appropriate low vs. high comparison) is not significant at the $p < .05$ level; these are the studies that fall in the area in between the two shaded areas of the

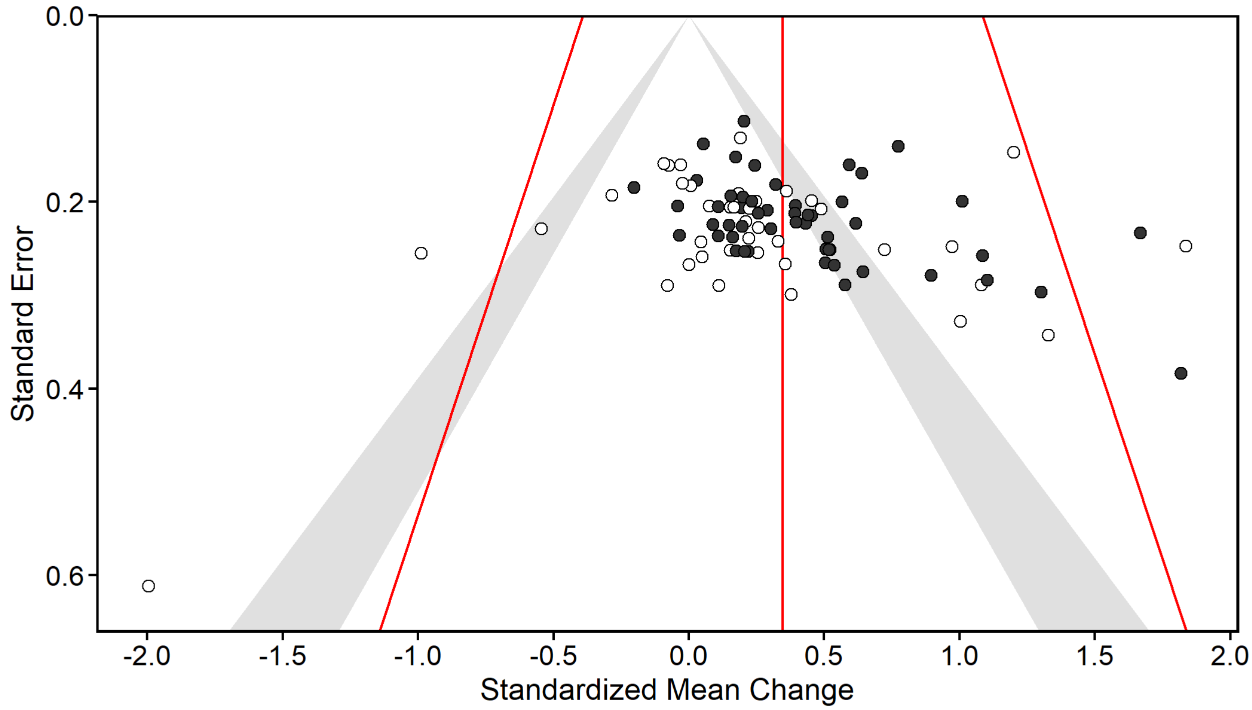

Figure 5. Funnel plot. Observed effect sizes (x-axis) are plotted against corresponding standard error (y-axis). Full circles represent studies that explicitly reported the most appropriate effect size (i.e., for high vs. low reward); empty circles represent studies that did not explicitly report the most appropriate effect size. Solid, red lines mark the area in which we would expect 95% of studies to fall, based on our random-effects model. Shaded, grey areas reflect the area in which effect sizes should be statistically significant with $.01 < p < .05$. The tent-shaped area in between the two shaded, grey areas reflects the area in which effect sizes should not be statistically significant, with $p > .05$. The areas outside the shaded, grey areas reflect the area in which effect sizes should be significant, with $p < .01$. We use to phrase "should (not) be statistically significant" (rather than "was [not] significant") to reflect the fact that we computed effect sizes ourselves (from means and standard deviations; see main text), rather than relying on the values reported by the authors.

funnel plot (Figure 5; see Peters, Sutton, Jones, Abrams, & Rushton, 2008). On first sight, this observation may suggest that publication bias is not a big problem in this body of research—after all, non-significant findings apparently still find their way into the published literature. Yet, it may also be the case that researchers who find no evidence when doing the most appropriate statistical test, may subsequently test less appropriate comparisons (see Aim 2), and then draw conclusions mainly from these. This situation, of course, would be an instance of selective reporting. We return to this issue in the Discussion. With all these three observations, it is important to note that visual interpretation of funnel plots is difficult and subjective (e.g., Terrin et al., 2005). It is clear, however, that the plot warrants further investigation of publication bias.

To provide such a further investigation, we used weight-function modeling (Coburn & Vevea, 2019; Vevea & Hedges, 1995). In weight-function modeling, the original meta-analytic model is compared to an alternative model. The alternative model, but not the original model, takes into account that some findings may be more likely to be published than others, depending on these findings' *p* values. In particular, in the alternative model, we assumed that the prospect of publication differs for (a) $p < .05$ vs. (b) *p* between .05 and .50 vs. (c) $p > .50$. In the alternative model, our effect size estimate dropped, but did not get close to zero, SMC = 0.222 (vs. SMC = 0.347 in the original model). Importantly, the alternative model fit the data better than the original model, as was evident from a significant likelihood ratio test, $\chi^2(2) = 13.7$, $p = .001$. Interpreting these findings, this test provides support for the existence of publication bias in this literature, and suggests that our meta-analytical effect size estimate may be an overestimation of the true effect. However, these findings also suggest that the reward-driven distraction effect survives correction for publication bias[3].

Evidential value. To assess the strength of the evidence for reward-driven distraction in the published literature, we conducted a *p-curve analysis* (Simonsohn et al., 2014a). A *p*-curve

[3] When using alternative cut-points (.10 and .50; .01, .05, and .50; .50 only), the likelihood ratio test remained significant, *ps* < .001. Thus, assuming the existence of publication bias robustly improved model fit. However, the effect size estimate did not always drop. It is thus somewhat less clear whether (and if so, by how much) the meta-analytical effect size estimate is inflated by publication bias.

analysis examines the distribution of statistically significant *p* values in a set of published studies (Simonsohn et al., 2014a). If a set of published studies is examining a true effect, it is likely that most *p* values in this set of studies will be lower (e.g., $p < .001$) rather than higher (e.g., *p* values between .01 and .05; Lehmann & Romano, 2014; Wallis, 1942). So, when the distribution of *p* values in a set of studies is right-skewed (a large proportion of low *p* values), this suggests that this set of studies is healthy, in that it has high evidential value. When the distribution of *p* values in a set of studies is flat, this suggests that this set of studies has low evidential value. When the distribution of *p* values in a set of studies is left-skewed (with a relatively large proportion of *p* values close to $p = .05$), this suggests that this set of studies not only has low evidential value, but also that it contains studies that have been *p-hacked* (e.g., researchers may have considered different ways of dealing with outliers, analyzed only a subset of participants, computed dependent variables in multiple ways, included different covariates, etc.). Indeed, assuming that p-hacking is aimed at reaching the significance criterion ($p < .05$), and that it is less likely to go further in the pursuit of very low (e.g., $p < .001$) *p* values (Simonsohn et al., 2015), a set of studies in which p-hacking is common should produce a left-skewed distribution of *p* values. We acknowledge that p-hacking may well happen without malicious intent (Gelman & Loken, 2013); it does not imply fraudulent behavior.

In our *p*-curve analyses, we included only studies that explicitly reported our contrast of interest. That is, the study had to report a test (t-test or F-test) of cognitive performance in high-reward distractor compared to low-reward distractor or neutral distractor conditions. Of the 58 studies that reported such a test, 43 yielded a significant *p* value. These 43 studies were included in the *p*-curve analysis (see "p-curve data" file on https://osf.io/rgeb6/). We entered the relevant tests of the contrast of interest into the p-curve web application (www.p-curve.com/app/). Results, plotted in Figure 6, showed that the distribution of *p* values was significantly right-

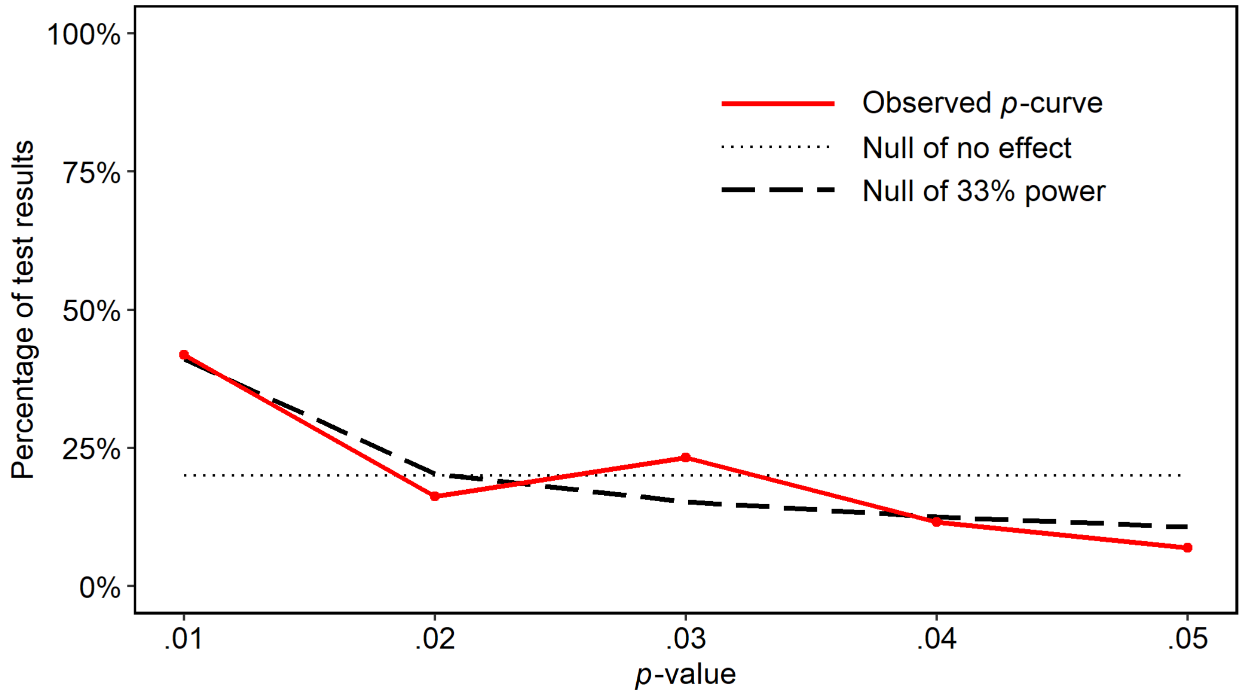

Figure 6. *P*-curve plot. The red, solid line indicates the distribution of significant *p* values across the experiments in the reward-driven distraction literature. The black, dotted line indicates the expected distribution of *p* values if there is no true effect. The black, dashed line indicates the expected distribution of *p* values under 33% statistical power. The *p*-curve is right skewed (there are more studies that report $p < .025$ than studies that report $p > .025$), which indicates that there is high evidential value for reward-driven distraction.

skewed, $Z = -5.4$, $p < .001$, which suggests this set of studies contains evidential value for reward-driven distraction. It provides no evidence for p-hacking.

The distribution of *p* values can further be used to estimate the statistical power typically used in these studies. The estimated statistical power from this *p*-curve (Figure 6) is 52%. More precisely, if we were to take many sets of 43 significant studies that all had 52% power, half of these sets would probably show a flatter *p*-curve than ours; the other half of these sets would probably show a more right-skewed *p*-curve than ours. Thus, 52% should be considered merely as a rough estimate of the power of the typical study in this area.

Methodological variations across studies. We assessed whether the methodological parameters of the experiments influenced the magnitude of reward-driven distraction. To this end, we conducted a mixed-effects meta-regression model with predictors: type of learning, type of reward, length of training, stimulus format, type of measure and physical salience (note: the moderator ratio between low and high was not included in this model, as this moderator was calculated only for studies that used either money or points as a reward; we report this moderator separately in Table 2). This model was significant, Q_M (28) = 42.6, p = .034, indicating that at least part of the heterogeneity in the true effect is related to one of these moderators.

Next, we examined these moderators separately (Table 2, Aim 3). Most notably, reward-driven distraction was stronger when it was assessed using direct measures of attention (eye movements) rather than indirect measures (manual response times or accuracy). For direct measures, the prediction interval was [-0.079, 1.406]; for indirect measures, the prediction interval was [-0.411, 1.007]. Thus, for direct measures, it would be surprising to find reverse effects (i.e., high-value distractors increasing performance) in future studies, unless such reverse effects are very small. For indirect measures, one can expect to find anything between medium-sized reverse effects to very large reward-driven distraction effects.

Interestingly, other methodological differences did not significantly moderate reward-driven distraction. We highlight three non-significant findings here. First, we found no evidence that the use of explicit monetary incentives is necessary to produce reward-driven distraction; the use of points (that can later be converted to money) produced reward-driven distraction as well. A small number of studies (n = 7) used rewards other than money or points (e.g., alcohol cues, food cues). These studies did not yield significant reward-driven distraction effects (Table 2).

Second, we found no evidence that longer reward learning sessions were associated with stronger evidence for reward-driven distraction. Third, the relative difference in value between the low-value and high-value rewards—operationalized as the low/high ratio—did not significantly affect the magnitude of reward-driven distraction.

With regard to the latter non-significant finding, we realize that there are alternative ways of computing the relative difference in value between low-value and high-value rewards. To explore whether an alternative operationalization would yield different results, we computed the absolute difference between low-value and high-value rewards, and we tested whether this variable moderated the magnitude of reward-driven distraction. We found no significant effect, either when we considered only the studies that used money as rewards, $Q_M(1) = 0.3$, $p = .588$, $Q_E(50) = 156.9$, $T^2 = 0.10$, $I^2 = 68.6\%$, or when we considered all studies that used either money or points as rewards, $Q_M(1) = 2.4$, $p = .12$, $Q_E(79) = 283.6$, $T^2 = 0.13$, $I^2 = 73.9\%$. Furthermore, as another alternative operationalization, we explored whether the magnitude of reward-driven distraction depended on whether the low-value reward had some value (e.g., 1 cent or point) vs. no value at all (i.e., 0 cents or points). We found no support for this possibility, $Q_M(1) = 1.5$, $p = .213$, $Q_E(79) = 299.9$, $T^2 = 0.13$, $I^2 = 74.7\%$.

It should be noted that, in our moderator analysis, some of the categories included relatively few studies (Table 1). Thus, low statistical power is a likely explanation for the non-significant effects that pertain to these moderators. Specifically, for the moderator *type of reward*, there were only 7 studies in the ‘other’ category; for the moderator *stimulus features*, there were only 6 studies in the ‘orientation’ category, 5 studies in the ‘sound’ category, and 5 studies in the ‘shape’ category. To avoid potential problems related to low statistical power, we again examined these two moderators, but now without including any categories that included fewer than 10 studies. These sensitivity analyses led to the same statistical conclusion. Specifically, when excluding the ‘other’ category, there still was no significant effect of reward type, $Q_M(1) = 0.2$, $p = .652$, $Q_E(82) = 307.8$, $T^2 = 0.13$, $I^2 = 74.6\%$. Similarly, when excluding the ‘orientation’, ‘sound’, and ‘shape’ categories, there still was no significant effect of stimulus features, $Q_M(1) = 2.98$, $p = .084$, $Q_E(73) = 321.8$, $T^2 = 0.17$, $I^2 = 79.1\%$.

Finally, we explored whether different laboratories differ in the effect sizes they typically report. By examining the author lists of the included papers, we identified three labs that contributed more than 10 studies each (i.e., Anderson’s, Le Pelley’s, and Theeuwes’ labs; Table 3). Then, we conducted another mixed-effects meta-regression model with lab as a predictor. Results indicated that studies from the different labs produced systematically different effect sizes, $Q_M(3) = 10.71$, $p = .013$, $Q_E(87) = 320.5$, $T^2 = .125$, $I^2 = 73.9$. Specifically, Anderson’s studies had *SMC* = 0.297, *CI* = [0.103, 0.490], prediction interval = [-0.423, 1.016]; Le Pelley’s studies had *SMC* = 0.681, *CI* = [0.462, 0.900], prediction interval = [-0.046, 1.408]; Theeuwes’ studies had *SMC* = 0.309, *CI* = [0.106, 0.512], prediction interval = [-0.413, 1.031]; Studies from other labs had *SMC* = 0.271, *CI* = [0.145, 0.397], prediction interval = [-0.433, 0.976]. Thus, on average, Le Pelley’s laboratory produced larger effect sizes than other laboratories. A plausible explanation for this finding is that this laboratory often uses RSVP tasks and often uses direct measures of distraction (i.e., eye tracking; see Supplementary Material); both of these practices are associated with large effect sizes (Table 2). Other than that, the different laboratories’ effect sizes were roughly in the same range.

Discussion

With the current meta-analysis, we synthesized the rapidly-growing literature on reward-driven distraction. We had three major goals. First, we aimed to establish the magnitude and scope of reward-driven distraction. Second, based on recent critical remarks (Le Pelley et al., 2015; Sha & Jiang, 2016), we tested whether existing findings reflect the operation of a reward-driven process, as is usually assumed, or if an alternative explanation is viable. Third, we assessed the general state of the literature by assessing potential publication bias, assessing the literature's evidential value, and examining whether methodological choices influence the magnitude of reward-driven distraction. Below, we discuss findings related to these three aims. Then, going beyond these three aims, we will discuss how our findings relate to existing work on distraction in the broader literature.

Aim 1: To Establish the Magnitude and Scope of Reward-Driven Distraction.

In order to establish the magnitude of reward-driven distraction, we systematically compared the impact of high-reward distractors to low-reward (or neutral) distractors on cognitive performance across 91 studies (*N* = 2,362). These studies used a wide variety of cognitive tasks, such as visual search and conflict processing, and a wide variety of rewards, such as money, food, and people's own names. The meta-analysis yielded a significant though small effect size (SMC = .347), which implies that, on average, the presence of a high-reward distractor impairs performance by just over one third of a standard deviation compared to low-reward or neutral distractors. This result indicates that high-reward distractors impair people's cognitive performance compared to low-reward/neutral distractors across different paradigms and across different reward cues.

This finding has an important theoretical implication. As noted in the Introduction, a traditional view sees attention as being modulated via two routes: a top-down route (in which current goals determine attentional selection) and a bottom-up route (in which physical salience determines attentional selection; Theeuwes, 2010a). Along with recent work (for reviews, see Anderson, 2016a; Failing & Theeuwes, 2017; Le Pelley et al., 2016), this meta-analysis supports the existence of a third route, by showing that attention can be modulated by rewards independently of people's current goals, and independently of the physical salience of stimuli.

Going beyond previous work, our analysis reveals that attention can be modulated by rewards in a broad collection of performance situations. In these situations, reward-related stimuli may distract people from the current task, impeding their performance.

It is important to emphasize that the latter conclusion refers to the reward-driven distraction effect that is to be expected *on average*. Our meta-analysis also shows that there is substantial heterogeneity in reward-driven distraction. That is, our findings suggest that researchers should not be surprised if their studies indicate that distractors associated with rewards boost, rather than hurt, performance. Moreover, this heterogeneity cannot be fully explained by examining the potential role for currently-known moderators. It is thus a priority for future work to develop theory that can predict and explain when task-irrelevant reward stimuli have the potential to distract, and when they have the potential to boost performance (Rusz et al., 2018). It is plausible that such work needs to take into account not just the type of task value, but also the stimulus strength and the timing of the reward-related stimulus (e.g., Zedelius, Veling, & Aarts, 2011, 2013).

We will now further discuss our moderator analysis. Reward-driven distraction occurred on three (out of six) task categories; it occurred regardless of whether there was a clear task difference between learning and testing phases; and it occurred regardless of explicit instructions to ignore the distractors. These three findings suggest that reward-driven distraction has a broad scope. We will now discuss these findings in greater detail.

In most previous experiments on reward-driven distraction, researchers used visual search tasks (Anderson, 2016b). These studies consistently show that visual attention is modulated by reward cues, even when these reward cues are irrelevant to the current search task (i.e., even when reward cues are not search targets). However, there is surprisingly little research on whether and how reward-driven distraction impacts other cognitive processes, beyond visual search (e.g., Failing & Theeuwes, 2015; Krebs et al., 2010; Le Pelley et al., 2017). In our meta-analysis, we found that reward-driven distraction was reliably present not only in visual search, but also in rapid serial visual presentation tasks and conflict processing tasks. This finding suggests that at least three types of cognitive processes can be perturbed by rewards. First, rewards guide the deployment of spatial attention, even when this potentially hampers task performance. Second, rewards modulate the time course of attention, potentially causing temporary blind spots (attentional blinks) immediately after rewards are presented. Third, rewards bias cognitive control, increasing the strength of potential response conflicts. Collectively, these three ways in which rewards may harm performance may emerge in many performance domains.

We found no systematic evidence for reward-driven distraction in spatial cueing, judgment, and visual memory tasks. The most likely and most parsimonious explanation for this non-significant finding is that we had lower statistical power for these categories of tasks, as our meta-analysis included relatively few of these studies ($k \leq 8$ per category), especially when compared to visual search ($k = 59$). With this caveat in mind, we will nevertheless speculate about the possibility that these non-significant findings might reflect true null effects.

Such speculation is especially warranted for spatial cuing tasks. After all, spatial cueing tasks should be sensitive to the same reward-driven processes that also affect visual search (Bourgeois et al., 2017; Munneke et al., 2015)—that is, reward drawing spatial attention away from task-relevant stimuli. Although the non-significant finding for spatial cuing tasks is most likely due to low statistical power, it could also be due to the difference in the timing of the distractor in the two tasks: while in search tasks the distractor is presented simultaneously with the target, in cueing tasks it is typically presented *before* the target. If the effect of reward on attention is rapid and short-lived, then it may dissipate by the time the target appears in a cueing task. Moreover, we should mention that in spatial cuing studies, researchers often quantify the reward-driven distraction effect in terms of its effect on certain trial types (specifically: on exogeneous/invalid trials) *relative to* other trial types, as reflected in a statistical interaction. As we did not and could not take such more nuanced patterns into account (we consistently coded our contrast of interest as the high vs. low reward main effect, see Lakens et al., 2016), it is possible that our meta-analysis underestimated the strength of reward-driven distraction in spatial cuing studies[4]. Future research that focuses specifically on spatial cuing should examine this possibility.

An outstanding theoretical question with regard to reward-driven distraction is whether reward-associated stimuli affect only shifts of spatial attention, which can quickly be corrected, or whether reward-related cues can also produce performance impairments via other, non-spatial cognitive processes, such as temporal attention (Anderson et al., 2012; Failing & Theeuwes, 2015; Le Pelley et al., 2017; Sha & Jiang, 2016). Thus, we categorized tasks according to whether distraction was spatial (i.e., visual search, spatial cueing) or not (conflict processing, RSVP, judgment, visual memory), and tested whether this dichotomous moderator influenced reward-driven distraction. The results from this analysis show no evidence that the magnitude of reward-driven distraction depends on whether the distraction is spatial or non-spatial in nature.

Another finding that strengthens the idea that reward-driven distraction has a broad scope is that reward-driven distraction did not seem to be tied to one context. In particular, we found

[4]To take into account the possibility that our coding procedure (i.e., consistently coding the main effect of high vs. low reward) underestimated the effect of reward-driven distraction in spatial cuing tasks, we checked if our overall meta-analytic effect size estimate would be different if we excluded all 8 spatial cuing experiments. The difference was negligible, $SMC = .355$, $p < .001$, $CI = .259 – .451$, Q (82) = 331.7, $T^2 = .15$, $I^2 = 77.23$.

reward-driven distraction regardless of whether the training and testing phases took place in the same context (e.g., both training and testing phases are visual search tasks) or different contexts (e.g., the training phase is a visual search task, but the testing phase is a flanker task). Thus, we found meta-analytic evidence for the idea that stimulus-reward associations learned in one task can generalize to another, qualitatively different task. This finding implies that reward associations that underlie reward-driven distraction may affect performance in new situations and stimulus contexts (Anderson et al., 2012).

In further support of the idea that reward-driven distraction has a broad scope, we found that reward-driven distraction was present regardless of whether people were explicitly instructed to ignore distractors. Based on the dual-mechanisms framework of cognitive control (Braver, 2012; Meiran et al., 2015), we reasoned that explicit instructions may induce proactive control in participants, which is characterized by strong maintenance of goal-relevant information and less susceptibility to distraction. On this account, we expected that explicit instructions would weaken the impact of reward-associated distractors. However, reward-driven distraction existed even when people were instructed to ignore the distractors. This finding implies that reward-associated distractors are capable of penetrating strong, top-down processes, substantiating findings from previous studies (Munneke et al., 2015, 2016; L. Wang et al., 2014; L. H. Wang et al., 2015).

Together, these findings point towards the idea that reward-driven distraction is a domain-general and adaptable mechanism that is not restricted to certain specific experimental procedures (Anderson, 2016b)—and thus, has a wide scope. Thus, there is reason to speculate about the idea that reward-driven distraction will exert meaningful effects in real-world situations. We will further explore this idea in the section titled ‘Relationship to research from clinical, educational, and work psychology’.

Aim 2: To Examine an Alternative Explanation: Have We Been Studying Selection-Driven Distraction Instead of Reward-Driven Distraction?

Recent studies have raised doubt about whether prior work has been measuring a reward-driven process after all (Anderson & Halpern, 2017; Le Pelley et al., 2015; Sha & Jiang, 2016). In particular, several past studies operationalized reward-driven distraction by comparing cognitive performance when there was a high-reward distractor to when there was no distractor present. However, as we laid out in the introduction, this operationalization has a major shortcoming: we cannot distinguish whether distraction here was indeed driven by rewards, or by selection history (Anderson & Halpern, 2017; Le Pelley et al., 2016; Sha & Jiang, 2016). Notably, recent studies suggest that reward-driven distraction vs. selection-driven distraction may stem from different mechanisms (Kim & Anderson, 2019a, 2019b). This raises the possibility that the ‘reward-driven distraction’ literature may have established an effect that may not be explained by a pure reward-driven mechanism after all.

To examine this issue, we systematically tested whether the use of this suboptimal comparison—high reward versus no distractors present—matters for the conclusion researchers tend to draw about the robustness of reward-driven distraction. We compared the effect sizes from studies that measured high-reward versus no distractors present (including only studies that used non-physically-salient distractors), to the effect sizes from studies that measured high-reward versus low-reward or neutral distractor (our original meta-analytic effect size estimate). We found that when reward-driven distraction is assessed with the suboptimal comparison that has alternative explanations (i.e., high-reward vs. no distractor present), the effect size of reward-driven distraction may well be overestimated (potentially by 40%). We also found that using this suboptimal comparison increases heterogeneity even further. That is, when researchers use this suboptimal comparison in future work, they should not be surprised by any finding ranging from large reverse effects (i.e., task-irrelevant reward stimuli increasing performance) to large distraction effects.

Based on these findings, we suggest that the way researchers operationalize reward-driven distraction in their studies matters. With caution, we conclude that many previous studies could have overestimated the magnitude of reward-driven distraction, as these studies' findings may, in part, result from a selection-driven process. In order to avoid alternative explanations in future studies, and to increase the predictability of findings in this research area, researchers should aim for selecting an operationalization (i.e., comparing high-reward vs. low-reward distractors) that does not allow for alternative explanations (Anderson & Halpern, 2017; Le Pelley et al., 2015, 2016; Sha & Jiang, 2016).

Aim 3: To Provide Methodological Guidelines for Studying Reward-Driven Distraction.

The general state of the literature. Publication bias is a prominent problem for meta-analysis because it leads to an overestimation of true effect sizes (Borenstein et al., 2009; Renkewitz & Keiner, 2018). Therefore, in this meta-analysis, we assessed whether publication bias is likely to be a problem in the reward-driven distraction literature. Our visual inspection of a funnel plot suggested that publication bias may exist. We then estimated, and corrected for, publication bias using a weight-function model (Vevea & Hedges, 1995). This model suggested (a) that publication bias is present in this literature, (b) that publication bias may result in an overestimation of effect size, but (c) that the reward-driven distraction effect survives corrections for publication bias (i.e., after correction, the effect is still significantly greater than zero). Next, we assessed the evidential value in favor of reward-driven distraction. We did this by conducting a *p*-curve analysis (Simonsohn et al., 2014b, 2014a, 2015). Results from this analysis showed that was no evidence for *p-hacking* in the reward-driven distraction literature. Moreover, this analysis yielded strong evidential value for reward-driven distraction. Together, these findings suggest that the literature on reward-driven distraction is in reasonably good health, and that we can have confidence in the existence of reward-driven distraction.

However, while we were coding all articles for this meta-analysis, we did notice a clear shortcoming of this body of literature, which is that the reporting of tests and effect sizes of reward-driven distraction is far from consistent across articles. For example, some papers report an omnibus test of the difference between high-reward, low-reward, and no distractors; other papers report only the comparison between high-reward versus low-reward distractors; and yet further papers zoom in on some other comparison (see Aim 2). In addition to the problems laid out in the previous section, this inconsistency in reporting is problematic in itself. In particular, in the absence of a clear reporting standard, it is impossible for readers to assess which specific analysis was used to test the main hypothesis (Simonsohn et al., 2014a). In other words, in these studies, readers cannot be sure whether a finding was specifically predicted a priori, or whether it was found only after (at least some) data exploration. In general, a lack of clarity regarding what analyses were planned has been argued to have contributed to the 'replicability crisis' in psychological science (Munafò et al., 2017). A potential way to circumvent this issue would be using *preregistration* (Forstmeier et al., 2017). Preregistering the hypotheses and the specific analysis plan for a study would help cleanly distinguish between confirmatory and exploratory analyses, which is an important aspect of open science (Munafò et al., 2017; Nosek et al., 2015; Nosek, Ebersole, DeHaven, & Mellor, 2018; Wagenmakers & Dutilh, 2016).

Methodological choices. We assessed whether the methodological choices made by researchers influence the magnitude of reward-driven distraction. To summarize our main findings: we found that the magnitude of reward-driven distraction is bigger when it is being assessed with a more direct measure of attention (eye movements) rather than an indirect measure (response times, accuracy). We found no evidence that the type of learning, type of reward, the ratio between high versus low-reward, the length of the training phase, type of stimulus feature that was paired with reward, or physical salience of the distractor influenced the magnitude of reward-driven distraction.

In line with a growing body of prior work (Donohue et al., 2016; Failing et al., 2015; Koenig et al., 2017; Le Pelley et al., 2017; Maclean & Giesbrecht, 2015; McCoy & Theeuwes, 2016; L. H. Wang et al., 2013), we found meta-analytic evidence for the idea that reward-associated distractors have an immediate, rapid influence on the visual system. In particular, reward-driven distraction was much stronger when measured with a direct response (i.e., eye movements) rather than an indirect response, in which a skeletal muscle response is also required (i.e., manual response times). This finding is consistent with the idea that reward-related cues exert a relatively early and involuntary influence on the attentional system (cf. Theeuwes, 2010), such that their effect is more apparent in rapid, ‘online’ measures (like eye gaze), as compared to slower, downstream measures (like manual responses) which represent the end-point of a longer chain of cognitive processes. On this account, the influence of reward-associated distractions can be suppressed by voluntary, top-down control processes, but these processes take some time to operate and hence have a greater impact on slower forms of behavior. Consistent with this idea, recent studies of eye movements have demonstrated that reward-related distraction is most pronounced for the very fastest eye movements that people make, and is weaker for slower responses (Failing et al., 2015; Pearson et al., 2016). Reward-associated distractors, then, seem most harmful when they can impact early visual selection processes; at later stages, their impact is weakened.

We found no evidence that other methodological choices mattered. The length of the training phase did not significantly moderate the magnitude of reward-driven distraction. This nonsignificant finding is consistent with the idea that people learn reward-stimulus associations quite rapidly and that these associations have a persistent effect on attentional priority and therefore on cognitive performance (Anderson et al., 2011b). Moreover, the ratio of high-reward versus low-reward magnitude also did not significantly moderate reward-driven distraction. One plausible interpretation of this nonsignificant effect is that participants encode reward magnitudes in relative terms, effectively as ‘large’ versus ‘small’, and largely regardless of their absolute magnitudes (Vlaev et al., 2011). Future studies could investigate this issue further by comparing performance for three or more reward levels in the same study.

Further, our meta-analysis found that the stimulus features of the reward-related distractor did not significantly moderate reward-driven distraction either. That said, it is potentially interesting that reward-driven distraction was significant only when the reward-related feature of the distractor was a color or picture, but not when it was orientation, sound, or shape. This could reflect a fundamental difference in the encoding processes for different stimulus features or, more likely, could be a consequence of the rather low sample sizes for the latter categories (k = 5–6). Overall, reward-driven distraction seems to exist across many methodological alterations and these results fit well with our suggestion (see Aim 1) that reward-driven distraction is robust across tasks, stimuli, and contexts.

Results further showed that reward-driven distraction was significant regardless of whether the experiment used an instrumental or Pavlovian reward learning procedure. Specifically, reward-driven distraction existed when participants previously had to respond to a cue to earn rewards, but also when the presence of a cue merely signaled the magnitude of the available reward (e.g., Bucker & Theeuwes, 2016b, 2016a; Failing et al., 2015; Le Pelley et al., 2015). These results demonstrate that reward-associated distractors can capture attention regardless of how their reward value was acquired. Reward-driven distraction thus does not depend on a trained response to stimuli and seems to be independent of strategic attentional control (Bucker & Theeuwes, 2016b).

Additionally, we found that reward-driven distraction existed when distractors were physically salient, but also when distractors were not physically salient. An implication of this finding is that reward and physical salience exert independent effects on attention, potentially by making additive contributions to the activity of a stimulus's representation on a common attentional priority map (Anderson & Kim, 2019; Awh et al., 2012; Pearson et al., 2016).

Lastly, reward-driven distraction existed when either money or points served as incentives during the task. In typical experiments on reward-driven distraction, participants can earn some sort of reward during the training phase. These rewards are often money cues, but recently, studies have increasingly used points (perhaps because they are more cost efficient) that are later translated into some monetary bonus. As money cues are strong motivators (Bijleveld et al., 2012; Pessoa & Engelmann, 2010; Zedelius et al., 2014), one would assume that being directly presented with the amount of money during the experiment would lead to stronger cue-reward associations. Nevertheless, we found that points, when they are used in a more symbolic way, can also drive reward-driven distraction. This finding is important for future studies, as it suggests that researchers can potentially conduct experiments in more cost-efficient ways.

Relationship to research from clinical, educational, and work psychology

In this section, we discuss how our findings on reward-driven distraction are related to other research lines. The primary goal of this discussion is to facilitate cross-fertilization between different research areas. The secondary goal is to speculate about how reward-driven distraction may play a role outside the laboratory. With regard to this secondary goal, we should add that it is currently not certain whether the core findings from the present meta-analysis are applicable. After all, we found a small effect size; also, whether any (meta-analytic) finding can serve as a basis for meaningful interventions depends on several factors (e.g., the target group, the costs of the intervention, the existence of competing interventions; e.g., Kraft, 2018; Lipsey et al., 2012). Thus, we do not wish to claim that our findings are ready to be applied. We do think, however, that our findings may be interesting to researchers outside cognitive psychology.

Attentional bias in addiction

People who suffer from addiction often develop a powerful attentional bias towards substance-related stimuli. Specifically, among people who suffer from addiction, substance-related stimuli can acquire extremely high attentional priority, causing these stimuli to grab the user's attention (for reviews, see Anderson, 2016d; Field & Cox, 2008). From a clinical perspective, it is important to understand this specific attentional bias, as this bias contributes to *cue-triggered 'wanting'* (Robinson & Berridge, 2008), a process via which mere exposure to substance-related stimuli can trigger relapse. Attentional bias plays a role in various addictions, including addictions to alcohol, cocaine, heroin, smoking, and gambling (Ciccarelli et al., 2016; Cox et al., 2002; Hester et al., 2006; Waters et al., 2003, 2012).

Over the past years, it has become clear that reward-driven distraction (among healthy people) can improve our understanding of attentional bias (among people with addiction)—and vice versa. For example, animal studies have shown that animals, such as rats, differ in their tendency to engage with reward-related stimuli after Pavlovian conditioning. Importantly, this tendency, called *sign-tracking*, predicts susceptibility to impulse control disorders (Colaizzi et al., 2020), including addiction (Flagel et al., 2008, 2009). Translating this idea to humans, researchers have recently hypothesized that individual differences in reward-driven distraction can be seen as an instance of sign-tracking (Albertella, Watson, et al., 2019). In line with this hypothesis, individual differences in reward-driven distraction (during visual search) have been found to correlate with behaviors that stem from deficits in impulse control (e.g., compulsive behaviors; Albertella, Le Pelley, et al., 2019; hazardous drinking; Albertella et al., 2017; Albertella, Le Pelley, et al., 2019).

Findings from our meta-analysis further support the existence of similarities between attentional bias in addiction and reward-driven distraction (Anderson, 2016d). First, attentional bias in addiction is known to persist even when it is no longer frequently rewarded (e.g., in people who have learned to abstain; Stormark, Field, Hugdahl, & Horowitz, 1997). Consistent with this finding, many studies in our meta-analysis include a training phase and a test phase; rewards are typically omitted in the test phase, yet reward-driven distraction persists (see Della Libera & Chelazzi, 2009). Second, attentional bias in addiction involves rapid, involuntary eye movements (e.g., Lochbuehler et al., 2011). Consistent with this finding, our meta-analysis shows that reward-driven distraction is especially pronounced when it is measured directly, with eye tracking. Third, attentional bias in addiction is resistant against conflicting goals (e.g., the goal to abstain; Marhe, Luijten, Van De Wetering, Smits, & Franken, 2013). Consistent with this finding, our meta-analysis shows that reward-driven distraction can emerge despite instructions to ignore distractors. Taken together, in line with previous theorizing (Albertella, Watson, et al., 2019; Anderson, 2016d), our meta-analysis illustrates that the literature on reward-driven distraction can provide effective tools to better understand attentional bias in addiction. In future research, these tools may prove helpful to examine other deficits in impulse control as well (see Albertella, Le Pelley, et al., 2019).

Test anxiety and related phenomena

Although our meta-analysis focused on distractions that stem from external stimuli, it is well-established that distractions can also arise from internal mental content—i.e., from task-irrelevant thoughts and worries. Specifically, internal mental content is thought to be involved in at least three types of performance impairments, each of which has been shown to be relevant to real life. First, *test anxiety* refers to the psychological and physiological processes that are triggered by concern for failure in testing situations. Often conceptualized as a trait variable (e.g., Wine, 1971), test anxiety negatively correlates with test scores (Chapell et al., 2005). Second, *math anxiety* is often defined as the feeling of tension, apprehension, or fear that some people experience when they are in a situation that requires them to solve math problems (Ashcraft, 2002; Ramirez et al., 2018). Akin to test anxiety, math anxiety is often conceptualized as a trait variable; it correlates with worse performance at math, with a stronger tendency to avoid math altogether, and with worse educational and career achievements. Third, *choking under pressure* occurs when people perform below the level that would be expected given their ability, when pressure to perform is very high (Beilock et al., 2004). Choking under pressure is a well-known phenomenon in competitive sports and in the performing arts (Jordet, 2009; Yoshie et al., 2009).

Even though test anxiety, math anxiety, and choking under pressure emerge in different circumstances, research suggests that they share a core mechanism (e.g., Maloney, Sattizahn, & Beilock, 2014). Using terminology from this research area, this mechanism is as follows: high-stakes performance situations trigger task-irrelevant thoughts and worries; in turn, these thoughts and worries use up working memory capacity. As a result, in high-stakes situations, less working memory capacity remains for the task at hand, causing people to perform worse than would be expected given their ability, especially during cognitively-demanding tasks (Cassady & Johnson, 2002; Eysenck et al., 2007; J. H. Lee, 1999; Maloney et al., 2014; Wine, 1971).

An intriguing parallel between these three phenomena vis-à-vis the studies in our meta-analysis, is that the task-irrelevant thoughts and worries—that hamper good performance in test anxiety, math anxiety, and choking under pressure—are often found to be about the outcomes of the performance situation (i.e., about the rewards that can be earned; about the punishments can be averted). For example, a qualitative study among competitive athletes showed that, during choking under pressure, distracting thoughts are often about the performance situation's immediate consequences, such as winning, letting team mates down, getting criticized by the audience, and getting selected (Hill & Shaw, 2013). Similarly, self-report instruments that measure test anxiety invariably include items such as "during tests I find myself thinking about the consequences of failing" (Spielberger, 1980) and, for children, "I worry about what my parents will say" (Wren & Benson, 2004). Thus, even though the locus of the distraction is different in these three phenomena (mainly internal) vs. our meta-analysis (exclusively external), the two literatures are remarkably consistent in that both point to prospective rewards as potential sources of performance impairment.

Building on this consistency, we see two avenues for cross-fertilization. First, the literatures on test anxiety, math anxiety, and choking under pressure are rich with interventions. For example, expressive writing may guard against test anxiety (Ramirez & Beilock, 2011); computer-adaptive practice may guard against math anxiety (Jansen et al., 2013); re-appraisal strategies may guard against choking under pressure (Balk et al., 2013). If reward-driven distraction is similar to test anxiety, math anxiety, and choking under pressure, it seems worthwhile to consider whether these interventions may also serve to curb reward-driven distraction; perhaps, they have broader potential than is currently assumed. Second, the literatures on test anxiety, math anxiety, and choking under pressure have generally not considered the possibility that external reward-related stimuli may initiate distraction (for exceptions, see Belletier et al., 2015; Bijleveld, Custers, & Aarts, 2011; Zedelius, Veling, & Aarts, 2011). Based on the current meta-analysis, it seems plausible that the physical environment—specifically, the reward associations of stimuli in that physical environment—may play an activating role in test anxiety, math anxiety, and choking under pressure. Further research is needed to clarify the nature of this putative, stimulus-dependent process.

Understanding distraction in the classroom

Over the past two centuries, classroom teaching has become a cornerstone of educational practice. Compared to one-on-one instruction, classrooms are efficient; also, classrooms offer the possibility for students to learn from each other. Nevertheless, classroom teaching comes with many challenges (Good & Lavigne, 2007), including the challenge of creating a learning environment that is conducive to on-task behavior—i.e., an environment in which distractions are kept under control (Behnke et al., 1981; Berry & Westfall, 2015). The behaviors that teachers use to create and maintain such learning environments, are collectively referred to as *classroom management* (Emmer & Sabornie, 2015; Emmer & Stough, 2001). Previous research identified some general principles of effective classroom management. For example, classroom management works best when it is proactive rather than reactive, focusing on expected behavior (e.g., at the beginning of the school year; Emmer, Evertson, & Anderson, 1980), rather than dealing with problem behavior after the fact. Also, classroom management works best when it clearly identifies and teaches desirable behaviors, e.g., by making use of well-rehearsed routines (taking attendance, introducing lessons; Emmer & Stough, 2001).

By showing that distractions may stem from a reward-driven process, our meta-analysis further supports two existing ideas from this research domain. First, successful classroom managers have been shown to go to great lengths to clarify task goals (Emmer et al., 1980). This practice is in line with studies that show that distraction can be prevented by increasing motivation on the primary task (e.g., Krebs et al., 2010; but see Rusz et al., 2018). Second, classroom management is sometimes approached from an Applied Behavior Analysis framework (Landrum & Kauffman, 2015). In this approach, teachers make sure on-task behavior is followed by immediate rewards, whereas off-task behavior is not. It seems plausible that this approach is successful (e.g., it works well in classrooms for children with behavioral problems; Pfiffner, Rosén, & O'Leary, 1985), because of how it systematically does not reward distraction.

Potentially, our meta-analysis may inform future research that aims to improve classroom management. Our meta-analysis suggests that it is necessary to first identify what external stimuli are associated with reward (e.g., in a given classroom), and then to intervene to reduce the reward value of these stimuli, diminishing their impact. Prior research has successfully used re-training to reduce the reward value of palatable food (Chen et al., 2018), sometimes using gamified computer tasks (Stice et al., 2017). Other prior research suggests that the social value of smartphones, which are common distractors in classrooms, is lower in conditions of non-deprivation (e.g., after a 1-hour period of smartphone use; Johannes, Dora, & Rusz, 2019). To increase on-task behavior in the classroom, it may be worthwhile to experiment with techniques such as these (re-training, preventing deprivation), to provide an initial test of whether it is feasible to target reward-distraction in interventions.

Distractions and micro-breaks at work

We will now explore the possibility that reward-driven distraction is a mechanism that contributes to distraction in workplaces. In work and organizational psychology, researchers distinguish between different subtypes of interruptions at work (e.g., Jett & George, 2003). Three of these subtypes, *microbreaks*, *distractions*, and *intrusions* are potentially relevant for present purposes. That is, consistent with the studies in our meta-analysis, in all of these subtypes, people divert attention away from their current task, in order to attend to external stimuli that are potentially associated with reward. First, *microbreaks* are short respite activities (e.g., standing up to drink some water) that people undertake voluntarily between episodes of on-task behavior (Kim et al., 2017). Second, in work psychology and ergonomics, *distractions* are often defined as psychological reactions triggered by external stimuli or secondary activities that interrupt focused concentration on a primary task (Jett & George, 2003). Third, *intrusions* are similar to distractions, but involve another person bringing the work to a temporary halt (Jett & George, 2003). So, while all these work-related interruptions involve brief episodes of attention to external stimuli, a key difference between microbreaks vs. distractions and intrusions is that microbreaks are self-initiated, whereas distractions and intrusions are not.

Prior research has explored the consequences of microbreaks, distractions, and intrusions. This research shows that microbreaks generally have positive outcomes. For example, although microbreaks do not necessarily boost or harm performance, they have been shown to reduce the physical discomfort associated with working behind a computer (McLean et al., 2001). Similarly, in a diary study, several types of microbreaks (specifically: drinking water, having a snack, stretching the legs, looking out the window, getting some fresh air), decreased feelings of fatigue or increased feelings of vitality (Zacher, Brailsford, & Parker, 2014; see also Kim et al., 2017). Comparable positive effects have been reported for brief exposures to nature-related stimuli (e.g., sounds, Largo-Wight, O'Hara, & Chen, 2016; scenery, Lee, Williams, Sargent, Williams, & Johnson, 2015).

By contrast, *distractions* and *intrusions* often harm work performance. Perhaps most strikingly, several observational studies among health care workers show that distractions and intrusions are associated with errors and delays. In a study among pharmacists, for example, distractions and intrusions were found to be associated with dispensing errors (e.g., mislabeling medication; Flynn et al., 1999). In a study among anesthetists, 22% of all distractions and intrusions during surgery were found to have negative consequences for the patient (e.g., less smooth induction of anesthesia; Campbell, Arfanis, & Smith, 2012). In a study among gynecologists, distractions and intrusions during surgery were associated with prolongations of the total operating time (Yoong et al., 2015). Similar reductions in safety and efficiency have been found in other work domains, such as aviation (for a review, see Loukopoulos et al., 2009).

Reflecting on the latter discussion, we suggest that the studies in our meta-analysis (from cognitive psychology) are closely related to research on distractions and intrusions (from work psychology and ergonomics). Specifically, the general conceptualization of distraction is similar in both research domains; at the same time, both research domains emphasize the potential for negative effects on human performance. Thus, in our view, it is plausible that that reward-driven distraction can explain (at least some) instances of suboptimal performance at work. Akin to our discussion of attentional bias in addiction, findings on reward-driven distraction may help understand the mechanisms—related to learning, reward, and attention—that cause distractions at work.

Limitations

As mentioned previously, our meta-analysis relied heavily on studies that used visual search paradigms (k = 59). This emphasis on one type of task raises the question of whether we missed some studies that used different tasks, a possibility that we cannot exclude. Indeed, it is possible that our search terms may have been more likely to pick up on papers on visual search, as compared to other task domains (e.g., we screened papers that referred to Anderson et al., 2011, which is a paper about visual search). As a result, we recommend that future meta-analyses about this topic use a search strategy that is specifically designed to detect papers from a broader range of task domains. Regardless of our search strategy, it is clear that visual search is currently the most prominent way of studying reward-driven distraction. To further extend this literature, and to increase its relevance to other areas, we also recommend that future research should systematically examine reward-driven distraction in task paradigms that go beyond visual search. It seems especially promising to intensify the use of conflict processing and RSVP tasks.

Conclusion

In line with recent theoretical developments (Anderson, 2013, 2016b; Failing & Theeuwes, 2017; Le Pelley et al., 2016), we show meta-analytic evidence for the hypothesis that cognitive processes can be modulated by rewards, independently of current goals and the physical salience of stimuli. Thus, our meta-analysis supports the idea that we have to think beyond the top-down and bottom-up theoretical dichotomy when we want to explain attentional processes (Awh et al., 2012). Furthermore, the reward-driven distraction literature suggests a promising avenue for understanding the underpinnings of impulse control disorders, including addiction (Colaizzi et al., 2020). Finally, the finding that reward-driven distraction was robust across many different performance situations suggests that it has implications for real-life settings. We suggest that insights from the reward-driven distraction literature may be used to further our understanding of distractions as they occur in the workplace and in educational settings.

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