Committed Dishonesty: A Systematic Meta-Analysis of the Effect of Social Commitment on Dishonest Behavior

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Abstract

People feel committed to other individuals, groups, or organizations in many contexts in everyday life. Such social commitment can have many positive outcomes, related to job satisfaction or relationship longevity, but there might also be detrimental effects when feeling overly committed. Recent high-profile cases of fraud or corruption in companies such as Enron or Volkswagen are likely based to some degree on strong commitment to the organization or co-workers. While social commitment might increase dishonest behavior, there is little systematic cumulative knowledge on when and how this may occur. In the present project, we reviewed 19,544 articles, while focusing on studies experimentally manipulating social commitment and measuring actual dishonest behavior. We retained 226 effect sizes from 91 articles featuring a total of 40,972 participants across 23 countries. We found no evidence that social commitment increases or reduces dishonest behavior in general, but we did find evidence that the effect depended strongly on the target of the commitment. Feeling commitment to other individuals or groups reduced honest behavior (g = -.22 [-.29, -.14]), while feeling commitment to social norms via oaths or pledges increased honest behavior (g = .27 [.16, .38]). The analysis identified several moderating variables and found evidence for some degree of publication bias across effects. Our findings highlight the diverging effects of different forms of social commitment on dishonest behavior, and suggest a combination of the different forms of commitment as a possible means to combat corruption and dishonest behavior in the organizational context.

keywords: commitment; dishonesty; ethical behavior; cheating; meta-analysis

Public Significance Statement

This meta-analysis reveals that social commitment can impact dishonest behavior in different ways. Feeling committed or interacting with other individuals showed an increase in dishonesty, while committing to a social norm (by for example signing an oath) showed an increase in honesty. These effects were small and subject to different situational moderations.

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Commitment represents a *social glue* holding society together. Commitment has been associated with increased job performance and satisfaction (Cho & Park, 2011; Jaramillo et al., 2005; Mathieu & Zajac, 1990; Meyer et al., 2002; Riketta, 2002), short- and long-term behavioral changes, for example related to pro-environmental behavior (Katzev & Wang, 1994; Lokhorst et al., 2013), as well as increased relationship satisfaction and reduced likelihood of relationship termination (Le & Agnew, 2003). Altogether, feeling committed towards other individuals, organizations, goals, values, or norms reduces fluctuations in attitudes, desires, and intentions, and aids individuals in coordinating their daily social life (Burke & Reitzes, 1991; Kiesler, 1971).

However, there might be detrimental outcomes to feeling committed in certain contexts. Being (overly) committed might make us blind to competing norms or values, abandoning our moral fiber by adopting group- or context-specific moral norms, and potentially increasing dishonest and unethical behavior (Berry et al., 2021). Consider recent revelations about large-scale corruptive practices in international corporations, such as the Volkswagen emissions scandal. There is little doubt that feeling a strong commitment to the organization, amongst other factors, played an important role in facilitating engagement in and delaying the uncovering of fraudulent behavior (Umphress & Bingham, 2011; Castille & Fultz, 2018; Cavico & Mujtaba, 2016; Rhodes, 2016).

Relatedly, commitment is an inherent part of dishonest practices such as bribery, which can only be beneficial if all involved parties commit to remain silent and do not blow the whistle (Jiang et al., 2015). Further, recent studies have highlighted the phenomenon of *corrupt collaboration* (Weisel & Shalvi, 2015) or the so-called *dishonesty shift* in groups (Kocher et al., 2018), suggesting that people in groups behave more unethically than

individuals acting on their own. These findings have important practical implications for individuals, organizations, and society. While work-groups are associated with several positive outcomes, including increased job performance and creativity (Hülsheger et al., 2009; LePine et al., 2008; D. Wang et al., 2014), working together might reduce the social norm of honesty and encourage corrupt behavior (Belle & Cantarelli, 2017). Consider the *four-eyes principle*, the idea that at least two individuals have to execute a certain task, as a means of fighting corruption (Poerting & Vahlenkamp, 1998). Ironically, recent studies indicate that such measures might backfire by introducing collaborative cheating behavior (Bodenschatz & Irlenbusch, 2018; Schikora, 2011).

On the other hand, commitment can of course also lead to positive outcomes. In particular, commitment might be used as a means to highlight moral norms and values by having people sign a pledge or swear an oath prescribing ethical actions or group-related standards of honesty. Examples include the Hippocratic oath taken by physicians, or the MBA oath that has been taken by business administration graduates across various universities (Jacquemet, Luchini, Rosaz, et al., 2018). Importantly, feeling committed to such norms or standards is expected to decrease dishonest actions (Belle & Cantarelli, 2017).

Reflecting this dual nature of commitment, recent empirical studies highlight that the relationship between feeling committed to other individuals and dishonest behavior might strongly depend on different contextual factors, such as who is harmed by the dishonest act (Castillo et al., 2020), and whether competition is introduced (Dannenberg & Khachatryan, 2020; Chui, Kouchaki, & Gino, 2021). On the other hand, feeling committed to social procedures, codes of conduct, norms or rules might in turn reduce dishonesty (Bruin, 2016; Jacquemet, Luchini, Rosaz, et al., 2018). This illustrates that the relationship between commitment and dishonesty might be rather complex, and likely depends strongly on the context, and on the target to whom or to which the commitment is enacted. It is therefore

vital to investigate the conditions under which social commitment might lead to dishonest behavior, and the conditions under which it might not.

Here, we provide a systematic quantitative review of the effect of social commitment on dishonest behavior. Specifically, we focus on experimental studies manipulating commitment and measuring actual dishonest behavior. In addition, we control for several moderating variables that might influence the relationship, such as the number of committed agents, the presence of competition, the beneficiary and victim of the dishonest act, and the specific target of the commitment. Our meta-analysis provides the first systematic overview of the literature on social commitment and dishonesty, revealing several important mechanisms that affect moral behavior under a sense of *social commitment*.

Commitment

There are many definitions of *commitment* in the scientific literature, with some focusing on commitment to certain activities, behavior, or goals (Kiesler, 1971; Klein et al., 2001; Locke et al., 1988; Kiesler & Sakumura, 1966), some focusing on commitment to other individuals (Arriaga & Agnew, 2001), and some focusing on commitment to organizations (Cho & Park, 2011; Reichers, 1985). It has been argued that all of these types of commitment have in common that they characterize commitment as being important to a specific identity and/or as something that provides meaning to the self (Burke & Reitzes, 1991; Burke & Stets, 1999). In the current manuscript, we take a minimal approach in defining the concept. According to our view, *commitment* represents a dispositional state of an agent *Y* that obtains when another agent *X* desires a goal *G,* and needs an external contribution *E* by agent *Y*. *Y*'s tendency to maintain her or his motivation to perform *E* because of *X*'s potential reliance describes *Y*'s sense of commitment (see Michael et al., 2016a), which induces a biased (non-random) corresponding behavior. On this view, commitment can be indexed to specific actions, or it can imply a general state of being disposed to perform actions that other agents

or groups are relying on one to perform. In other words, being committed to specific individuals or groups is to have a tendency to be motivated to perform actions that they are relying on one to perform. It is also important to note that we focus on *social commitment* or *interpersonal commitment* in contrast to *self-commitment* (Michael et al., 2016a). Self-commitment refers to a state in which one is disposed to maintain the motivation to follow through on certain behaviors relevant to the self, and in doing so to resist temptations and distractions, such as being committed to certain goals or resolutions one has set. In these cases, both agents *X* and *Y* would refer to the same individual. In the case of social commitment, on the other hand, the agent *X* one feels committed to might be an individual, a group, an organization, a deity, a country, a regulation, or a rule. Agent *X* and *Y* might have a common goal (i.e., in the case of collaboration), but they do not necessarily need to. Social commitment might be further differentiated into *mutual* and *unilateral* commitment (Clark, 2020). In the former, both agents are committed to some goal, whereas only one agent is committed in the latter case. Mutual commitments can be further differentiated into *complementary* and *joint* commitments. In a complementary situation, *Y* is committed as long as *X* performs some kind of complementary action (e.g., such as paying the person for a certain behavior). In the case of joint commitments, both agents have a shared goal (Clark, 2020).

Social commitment is related to other concepts such as cooperation (Frank, 2001; Kerr & Kaufman-Gilliland, 1994), trust (Morgan & Hunt, 1994), closeness (Tsang et al., 2006), loyalty (Bloemer et al. 2002), or social identity (Charness & Chen, 2020; Ellemers et al., 1999; Postmes et al., 2001). Importantly, however, these concepts should not be treated interchangeably. For instance, although if someone feels committed to another individual, this might result in collaboration with that individual, it does not necessarily do so. On the other hand, collaborating with someone on a task will typically induce a certain sense of

commitment. Similarly, commitment might lead to loyalty, but the latter requires some form of self-sacrifice and going beyond one's self-interests (Duska, 2007; Berry et al., 2021). Thus, these different concepts are related but not identical; they are important to consider when reviewing the literature on social commitment insofar as the experimental paradigms used to probe them may also induce a sense of commitment.

Previous studies have used different procedures to induce social commitment experimentally, including coordination, synchrony, investment, minimal-group, closeness, or oath and promise paradigms (e.g., Burke & Stets, 1999; Finkel et al., 2002; Joule et al., 2007; Joule & Beauvois, 1998; McEllin et al., 2020; McLeish & Oxoby, 2011; Michael et al., 2016b; Székely & Michael, 2018; van Baal et al., 2020; Wang & Katzev, 1990). For example, in one study commitment was manipulated by telling participants that their group partners needed to perform an additional task to the group task that consisted of deciphering either easy or difficult captchas. Knowing about the other's higher investment and effort increased commitment to the partner and persistence in a follow-up group task (Székely & Michael, 2018). Similarly, commitment might be induced by promising an oath or signing a certain code of conduct or pledge (Joule & Beauvois, 1998). For instance, in one study signing a truth-telling oath increased commitment and coordination, as well as subsequent truthful behavior (Jacquemet, Luchini, Shogren, et al., 2018).

While commitment has been related to various positive intra- and interpersonal outcomes (Cho & Park, 2011; Le & Agnew, 2003; Lokhorst et al., 2013), some negative outcomes have also been documented (focusing on *escalating* commitment, e.g., Staw, 1976; Tang, 1988; Whyte, 1986, 1993). One particular behavioral response we focus on in the present review is a possible effect on dishonest behavior.

Dishonesty

Dishonest behavior is a common and widespread phenomenon (Gächter & Schulz, 2016; Gerlach et al., 2019). Based on the Corruption Perceptions Index (CPI) assessing perceptions of public sector corruption worldwide, two thirds of the 180 sampled countries scored below the midpoint of the scale in 2020, and the average score was only 43 out of a possible maximum score of 100 (Transparency International, 2020). The prevalence of dishonest behavior imposes large costs on organizations but also society at large (Mazar & Ariely, 2006). Focusing on the standard economic model of rational actions, it would be expected that people engage in dishonest behavior if the external benefits outweigh the costs (Becker, 1968). Despite these predictions, people typically seem to act less dishonestly than one would expect if one regards them as rational decision makers trying to maximize their payoffs (Abeler et al., 2019; Gerlach et al., 2019). In experimental studies assessing dishonest behavior in private (i.e., without the possibility of being checked or punished), the proportion of brazen liars, who strategically misreport their behavior all the time, is surprisingly low (Abeler et al., 2019). It is suggested that this pattern arises from the fact that individuals in general have a preference for being honest (Abeler et al., 2019) and that performing fewer dishonest acts can be more easily justified as fitting one's self-concept of being an honest person (Mazar et al., 2008). Thus, engaging in only a modest amount of dishonest behavior enables individuals to overcome the internal conflict between, on the one hand, gaining from dishonest behavior, and on the other hand, maintaining a positive self-concept and being regarded as an honest person.

There is no universally agreed-upon definition of what counts and what does not count as dishonesty. Previous studies have defined dishonesty as actions that *violate accepted standards or norms* (Köbis et al., 2019) or specific *moral values* (Weisel & Shalvi, 2015), or have not provided any explicit definition but rather relied on specific experimental operationalizations such as misreporting a die roll (Abeler et al., 2019; Gerlach et al., 2019).

We believe that the definition of dishonesty as a violation of specific standards might be too broad, as it can possibly include many facets of unethical behavior, including killing or incest. Moreover, in highly corrupt contexts, behavior such as bribery might be perceived as the accepted standard and cultural norm (e.g., Tian, 2008).

We define dishonesty as actions which either intentionally communicate false or misleading information, or undermine norms of honesty or transparency for the acquisition of profits or other advantages (see Miller, 2020). It is important to stress that dishonest behavior involves at least some degree of intentionality. For example, you might tell your friend that biking to the station from your house takes 10 minutes, while it actually takes 25 minutes. You might intentionally deceive your friend for specific reasons, which would qualify as dishonesty based on our definition. However, you might just be bad at approximating distances, have an inaccurate sense of time, or have been lucky with traffic lights on your last ride. Thus, merely miscommunicating a true state would not necessarily qualify as dishonest behavior in our view. Similarly, profits in this context might refer to personal profits, but could also relate to other individuals or larger entities such as organizations or countries. In the end, we include concepts such as cheating, lying, deception, corruption, bribery, and stealing when focusing on *dishonesty*.

Previous research has identified several moderating variables affecting whether people engage in dishonest behavior or not (Abeler et al., 2019; Gerlach et al., 2019; Gino & Ariely, 2016; Jacobsen et al., 2018; Köbis et al., 2019; Rosenbaum et al., 2014). For instance, there seems to be some evidence that on average men behave more dishonestly than women (Gerlach et al., 2019), and that dishonesty is more prevalent among younger individuals (Gerlach et al., 2019). Similarly, increasing stakes is positively associated with dishonest behavior (Gerlach et al., 2019) and dishonesty is increased when abstract others are harmed in contrast to concrete others (Köbis et al., 2019). In addition, dishonesty seems to be reduced

when it occurs in public or can possibly be monitored (Schild et al., 2019). Finally, social settings in which behavior takes place seem to play an important role in dishonesty. For example, field studies have shown that cleansing effects within specific cultural settings (Mitkidis et al., 2017) and descriptive social norms (Ayal et al., 2019) also seem to reduce dishonesty. While all of these variables have been found to moderate the intensity and frequency of dishonest behavior, there is no systematic evidence on how social commitment affects the honesty of individuals.

Committed Dishonesty

Does feeling social commitment reduce or increase dishonest behavior? In order to answer this question, it can be helpful to focus on the target of commitment. More specifically, how does people's commitment to other individuals, groups or social norms and rules, such as oaths or codes of conduct, affect their dishonesty?

Commitment to Individuals or Groups. Recent studies have suggested an increase in dishonest behavior when collaborating with other individuals in contrast to acting alone (e.g., Cohen et al., 2009; Kocher et al., 2018; Weisel & Shalvi, 2015). These studies typically let participants complete a task which affords them with the opportunity to misreport their actual performance, such as rolling a die (Fischbacher & Föllmi-Heusi, 2013), while they either have to coordinate or work together with other individual(s), or do not have to do so. In similar paradigms, participants are primed with common identities or closeness, such as shared group membership, and engage in tasks that allow for misreporting (e.g., Cadsby et al., 2016; Gino & Galinsky, 2012; Irlenbusch et al., 2020). In most of these studies, participants primed with shared identities, and thereby commitment to other agents, were found to engage in more dishonest acts.

Several theories aim to explain why dishonest behavior would increase with stronger commitment to other individuals or groups. First, it has been argued that the possibility of

communication with other group members can increase the possibility of justifying the dishonest act (Gino et al., 2012; Kocher et al., 2018; Mazar et al., 2008). When being able to communicate with others, it is possible that more diverse arguments will be exchanged than when working alone, which might relate to justifying likely dishonest acts. Nevertheless, this proposition has not been tested systematically to our knowledge, and there are several studies showing that dishonest acts can increase when people feel committed to each other without having the chance for communication (e.g., Cadsby et al., 2016; Gino & Galinsky, 2012).

Second, another reason for increased dishonesty in commitment settings might be a certain degree of diffusion of responsibility (Behnk et al., 2019; Gross et al., 2018; Mazar & Aggarwal, 2011; O'Leary & Pangemanan, 2007; Wiltermuth, 2011). When feeling committed to other individuals, people can attribute and hand over responsibilities for dishonest acts to other group members, thereby washing their hands of responsibility. Similarly, possible moral costs – including negative emotions, such as guilt, that arise when behaving dishonestly – can be shared in settings with many interaction partners. This proposition would assume that increasing the degree of commitment or number of partners can also increase dishonesty, though probably not in a linear fashion. Empirical evidence for this proposition is scarce. In one study, participants showed more dishonest behavior when interacting in a group of three than when working in dyads (Gino et al., 2013).

Third, another theory argues that commitment to other individuals increases dishonesty because there is a higher probability of exposure to dishonest behavior (Gross & De Dreu, 2020). Reflecting the saying that *one bad apple can spoil the barrel*, being committed to several individuals might increase the chance of interacting with people that behave dishonestly. The action of these *bad apples* might then overwrite group norms and increase dishonest acts because cheating becomes an acceptable social norm (Chui et al., 2021; Paternoster et al., 2013; Gino et al., 2009). As the previous explanation, this

proposition would imply that dishonesty increases with the number of individuals one is committed to, as this raises the chance of exposure to others' unethical behavior. Corroborating this, a recent study found evidence that dishonesty does not increase with commitment if a group consists of a majority of *rule followers* (Gross & De Dreu, 2020).

Fourth, a final proposition highlights that people act more dishonestly when feeling committed to other individuals because they want to compensate for their partner's honest behavior (Soraperra et al., 2017). Based on this view, individuals might want to compensate for the non-profit-oriented actions of the people they are committed to by being more dishonest. However, this explanation is contradicted by the previous proposition suggesting that in groups with more honest people dishonesty is reduced (Gross & De Dreu, 2020).

Commitment to Social Norms. While feeling committed to other individuals or groups might increase dishonest behavior, studies focusing on commitment to social norms or rules seem to suggest the opposite. Several studies have provided evidence that providing an oath or signing a pledge or code of conduct can increase honest behavior (e.g., Beck et al., 2020; Jacquemet, Luchini, Rosaz, et al., 2018; Schild et al., 2019; Shu et al., 2012).[1] In these studies, participants typically complete a verbal honesty pledge or sign a code of conduct before engaging in tasks that allow for misreporting of actual performance. Importantly, we regard such paradigms as inducing social commitment because these oaths or rules are typically social in nature. Moreover, insofar as they invite individuals to rely on others, their nonobservance can have direct negative consequences on other individuals or organizations. In addition, when individuals are more committed to a specific oath or rule, this should in turn also increase commitment towards the organization representing the procedure (e.g., a university's code of conduct). Therefore, such manipulations can be regarded as increasing

[1] Note, that we focus specifically on active commitment to social norms and rules. Recent investigations have questioned the effect of priming moral or social norms without any specific commitment on honest behavior (e.g., Verschuere et al., 2018).

the salience of being committed to the groups, organizations, or societies sharing these rules. Previous research has studied commitment to social norms under the term of *moral commitment*, and while we do not object to this conceptualization, we emphasize that such types of commitment are social and interpersonal in nature.

There are two main explanations why feeling committed to oaths, social norms or rules and the alike would reduce dishonest behavior. First, committing to an oath reduces self-justification processes of being dishonest (Jacquemet et al., 2020). While liars might rationalize their actions through different channels, committing to honesty works as a reminder of one's moral self, thereby reducing the possibility of justifying a lie (Hildreth et al., 2016). Second, committing to honesty directly integrates the desire for being perceived as an honest person (Abeler et al., 2019; Vanberg, 2008), while also appealing to the desire to behave consistently and reducing the possibility of not behaving according to one's words (Bénabou & Jean, 2011; Mazar et al., 2008). Studies suggest that feeling committed to oaths or rules increases decision times (Jacquemet, Luchini, Rosaz, et al., 2018), with individuals possibly using less intuitive and more deliberate decision-making strategies (see Köbis et al., 2019). In addition, such commitments seem to have the greatest effect on people who only act dishonestly occasionally, while dishonesty seems not to be reduced for more chronic liars (Jacquemet et al., 2020), probably because they employ a more varied repertoire of rationalization strategies and are less concerned with appearing honest.

A Cost-Benefit Analysis of Commitment to Norms vs. People. Applying a cost-benefit framework to the relationship between social commitment and dishonesty, one could argue that feeling committed to other individuals is associated with reduced costs of dishonesty. The probabilities of getting caught when cheating in a large group are possibly smaller than when acting individually, as responsibilities get distributed (Whyte, 1991; Wiltermuth, 2011). Similarly, a possible punishment might be perceived as less immediate or

harsh when shared with other group members. On the other hand, making a verbal or written commitment increases the chances of getting caught if records or witnesses exist. Similarly, committing to refrain from engaging in dishonest acts and then nevertheless doing so represents a conflict of behavior. Depending on the strength of the transgression, such dissonances might be hard to rationalize and may make it harder for people to maintain a positive self-concept (Mazar et al., 2008). Therefore, one common mechanism uniting the effects of commitment to individuals or groups and commitment to social norms on dishonesty might be the specific personal responsibility felt for the dishonest action. While people committed to social norms feel a strong responsibility towards their own actions (Jacquemet, Luchini, Rosaz, et al., 2018), there is evidence that people in groups feel less individual responsibility towards the group's actions (e.g., Darley & Latané, 1968; Mynatt & Sherman, 1975). Therefore, it would be expected that commitment to other individuals and groups increases dishonesty, while commitment to social norms decreases dishonesty.

There is at least one recent study comparing the effect of social commitment to other individuals and to an honesty oath on dishonest behavior (Beck et al., 2020). As expected, group decision making increased average payoff in the die-rolling task, indicating dishonest reporting, while moral awareness through signing an oath reduced it. Importantly, the effect of social commitment to other individuals increased dishonesty only to a small (and non-significant) degree from baseline (acting individually). In addition, a recent study explored the effect of an honesty oath when participants were committed to other individuals, and had the opportunity to act dishonestly (Dunaeiv & Khadjavi, 2021). The study thereby combined different forms of social commitment, and it was found that dishonesty was reduced after signing an honesty oath not only in the individual condition, but also in the team condition. However, there was no statistically significant difference between dishonesty for individuals and teams. These and other findings not only suggest that the effect of social commitment on

dishonesty is rather specific with regard to the target of commitment, but also identified other moderating effects, such as who benefits from the dishonest act (Gino & Pierce, 2010; Ayal & Gino, 2011), who is damaged by the dishonest act (Castillo et al., 2020), how familiar the committed agents are with each other (Potipiti & Kingsuwankul, 2020; Irlenbusch et al., 2020), whether the dishonest act occurs in private or not (Halevy, 2018), and whether competition is observed (Dannenberg & Khachatryan, 2020). Further, some studies found no significant effects of social commitment on dishonesty, highlighting that situational, individual, and cultural factors might play an important role in moderating possible effects (e.g., Muehlheusser et al., 2015; Bonfim & Silva, 2019; Chua et al., 2021; Soraperra et al., 2017).

These empirical findings underline the need for a systematic investigation of when and how social commitment affects dishonest behavior. The current study aims to shed light on this question by conducting a quantitative literature review of studies experimentally manipulating social commitment and measuring actual dishonest behavior. Based on recent investigations on selective reporting practices (Dickersin, 1990; Fanelli, 2010), we also tested the robustness of the effects included here using meta-analytical techniques.

Relation to Previous Meta-Analyses

There exist several previous meta-studies on dishonesty (Leib et al., 2021; Abeler et al., 2019; Gerlach et al., 2019; Köbis et al., 2019; Belle & Cantarelli, 2017). With the exception of the study by Leib and colleagues (2021), these previous attempts focused on predictors of dishonesty in general, mainly focusing on specific dishonesty paradigms (Gerlach et al., 2019), demographic variables or attitudes (Abeler et al., 2019), or the importance of intuition (Köbis et al., 2019), but not the effect of social commitment specifically. Belle and Cantarelli (2017) focused on different factors predicting dishonesty and also included *social influence* and *moral reminders* that can be considered as different

forms of social commitment. However, they did not explicitly combine these aspects theoretically and focused on measures going beyond behavioral dishonesty including intentions or attitudes. Leib and colleagues (2021) considered the effects of corrupt collaboration, but focused mainly on different designs and the development of dishonesty in dyadic interactions. The current investigation can be to some degree informed by these previous endeavours, though its main goal goes beyond previous attempts by focusing explicitly on the relationship between social commitment and actual dishonest behavior.

Method

The present meta-analysis was conducted taking into account six practical recommendations for openness and reproducibility of meta-analyses (Lakens et al., 2016): 1.) In our coding scheme, we always quote the relevant text from which effect sizes were calculated and note subjective decisions. 2.) Studies were coded independently by several researchers. 3.) We adhere to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA; Moher et al., 2009). 4.) The protocol, coding scheme, and analysis plan were pre-registered (https://osf.io/7yck4), and deviations are explicitly noted. 5.) All data are openly available in order to facilitate reproducibility (https://osf.io/nd27z/?view_only=8390ba91ab8544608993ac185b6fe04b). 6.) We employ open-source software packages in order to automatize the procedure as much as possible.

In order to select possible studies for inclusion in our quantitative review, we employed a literature search by first generating keywords, then searching different databases, screening titles and abstracts, as well as full texts, and finally coding the retained articles.

Literature Search

Keyword Generation. In order to partially automate keyword selection, we employed the *litsearchr* package for *R* (version 1.0.0; Grames et al., 2019) using co-occurrence networks in order to identify possible keywords. Therefore, we conducted a naïve

search on Web of Science (including all years and all collections) on the 2nd January 2021 using the Boolean search: [“commitment” OR “collaboration”] AND [“dishonesty”]. This search returned 69 articles and 58 unique contributions after removing duplicates. Using the *litsearchr* package, we extracted all keywords and titles from the retained records. After controlling for different stopwords, the list included 481 keywords. Creating a network of these keywords, we retained only keywords that occurred in at least two studies. Afterwards, we applied a cut off of 80% node strength, resulting in 76 final keywords. Finally, we manually checked this list and included nine keywords for the final search. Keywords were dropped because they represented either too broad terms (e.g., “behavior”, “decision”) or were not related to our main research question (e.g., “design”, “model”).

The final nine keywords were: “commitment”, “cooperation”, “trust”, “collaboration”, “dishonesty”, “misconduct”, “cheating”, and “lying”. We added twelve keywords to this list based on previous research investigating the topic and our theoretical focus. First, we added “team work” because it reflects the group processes that we were interested in investigating and has been employed in previous studies (e.g., Gross & De Dreu, 2020; Kocher et al., 2018; Soraperra et al., 2017). Second, we added “joint action”, “coordination”, “synchrony”, “investment”, “effort”, “social identity”, “minimal group paradigm”, and “loyalty” because they have been identified as important aspects of commitment or refer to specific manipulations of commitment (Michael et al., 2016a, 2016b; Michael & Pacherie, 2015). Third, we added “deception”, “bribery”, “stealing”, and “corruption” because they have been used to describe dishonest behavior (Köbis et al., 2019; Weisel & Shalvi, 2015).

Database Search.

Main Search. We conducted a database search from the 1st to the 5th of February 2021. Based on the keyword generation procedure and additional theoretical considerations, we employed the following Boolean search:[2]

[commitment OR collaboration OR cooperation OR coordination OR "joint action" OR synchrony OR "team work" OR trust OR loyalty OR investment OR effort OR "social identity" OR "minimal group paradigm"]
AND
[dishonesty OR cheating OR lying OR deception OR corruption OR misconduct OR stealing OR bribery]

The main search was conducted using three different databases. First, we employed Web of Science (n = 10,000) searching all years available of the Web of Science Core Collection and restricting the search to the first 10.000 articles that were ordered based on relevance. Second, we used GoogleScholar (n = 200) focusing on the first 20 pages. Third, we searched the ProQuest Dissertations and Theses Global database (n = 3538) searching anywhere except for 'full text'. Fourth, we collected articles citing four papers that we identified as *gold standards* a priori (n = 683), as they experimentally manipulated commitment and assessed dishonest behavior (Cohen et al., 2009; Gino & Galinsky, 2012; Kocher et al., 2018; Weisel & Shalvi, 2015). Fifth, we issued a call for published and unpublished data via various associations and mailing lists (n = 18), including the Society for Judgement and Decision-Making (SJDM), the European Association for Decision Making (EADM), Academy of Management – Organizational Behavior (AOM OB), Economic

[2] In this first main search we failed to include possible relevant keywords such as 'oath', 'pledge', or 'promise'. We later conducted an additional search in order to account for these shortcomings.

Science Association (ESA), Society for Personality and Social Psychology (SPSP), European Association for Social Psychology (EASP), and Association for Psychological Science (APS). In total we recorded 14,439 sources. An overview of the identification and screening process is provided in Figure 1.

Additional Searches. After screening and coding articles from the main search, we realized that at least one article that was identified as a *gold standard* a priori was not included in the search. In addition, we supplemented the main search by adding important keywords that were not included in the original search. We also searched articles included in previous meta-analyses focusing on similar research questions. In order to decrease the possibility of missing out on important articles we conducted three types of additional searches. Note, that these searches were not specified in the original preregistration, but considered as a sensible addition to the main search procedure. The additional searches were conducted after the main search was completed and its results had been screened and successfully coded. For the first additional search, we included all references listed in the bibliographies of the 49 final records identified by the main search, as well as articles citing these 49 sources based on GoogleScholar.[3] This search was performed between the 31st of May and 4th of June 2021 and it resulted in 4,877 records. For the second additional search, we searched for the following Boolean search on Google Scholar on the 23rd of August, 2021, focusing on the first 20 pages (thus, including 200 records):

[commitment OR signing OR pledge OR oath OR promise] AND
[dishonesty OR cheating OR lying OR deception OR corruption OR misconduct OR stealing OR bribery]

[3] Note, that we had originally identified 53 articles based on the main search for final inclusion and included the bibliographies and articles citing these articles for the additional search. However, four articles from the original main search were later excluded after an additional check.

For the third type of search, we inspected the included articles of a previous meta-analysis focusing on a similar research question (Belle & Cantarelli, 2017). We focused on the articles included for the factors of social influences and moral reminders (n = 26) and checked whether they had been included in any of the previous search strategies. In addition, we included one additional article that was part of another meta-analysis on a similar question (Leib et al., 2021).[4] Finally, we received one additional article by another researcher based on the original call during the additional search procedure. In total, these additional searches focused on 5,105 different articles.

An overview of the identification and screening process is provided in Figure 1.

Inclusion Criteria. We included studies based on four main criteria. First, we focused on studies reporting nonclinical samples. Second, we included only articles written in English. Third, to study the causal links between commitment and dishonesty we only focused on articles experimentally manipulating commitment and excluded correlational studies. Fourth, for the dependent variable we focused on actual behavioral measurements of dishonesty (see Gerlach et al., 2019), thereby excluding studies focusing on behavioral intentions or attitudes about dishonesty.

Commitment Manipulations. For the independent variable, we focused on (social) commitment manipulations compared to a control condition including either reduced commitment or the absence of such. We did not consider manipulations focusing on the opposite of commitment (i.e., competition) as control conditions. Based on previous studies studying commitment and theoretical perspectives (Michael et al., 2016a, 2016b), we classified commitment manipulations based on five different types:

[4]Note, that we included the article based on a presentation the first author gave on their meta-analysis. We were not able to read or access the full report while conducting the searches.

i. *Joint Action/Coordination* tasks (k = 67) require participants collaborating or working together in dyads or groups (in contrast to working individually) or participants taking turns with another individual(s) (in contrast to no turns; Michael et al., 2016b; Weisel & Shalvi, 2015).

ii. *Synchrony* tasks (k = 0) ask participants acting synchronously with other participants or in a group (in contrast to no/reduced synchrony; McEllin et al., 2020).

iii. *Oath/Pledge*[5] tasks (k = 49) require participants to testify their commitment to certain individual(s)/group(s)/organization(s), for example by using an oath or signing a contract (in contrast to no oath or reduced commitment) or have participants primed with commitment (in contrast to no commitment prime; Finkel et al., 2002).

iv. *Investment/Effort* tasks (k = 36) ask participants to complete a certain amount of work, investment, or effort for another individual/group/organization (in contrast to no/reduced investment/effort; van Baal et al., 2020).

v. *In-Group Formation* tasks (k = 69) use minimal group paradigms, common knowledge paradigms, social identity or closeness priming to induce common knowledge or shared identities with another individual/group/organization (in contrast to private knowledge or

[5] This category was originally called *commitment* tasks. However, we later changed this description to improve clarity and specificity.

sharing no/reduced identities; Burke & Stets, 1999; Gino & Galinsky, 2012; McLeish & Oxoby, 2011).

Manipulations that could not be classified into these five categories were coded as *other* ($k = 5$).

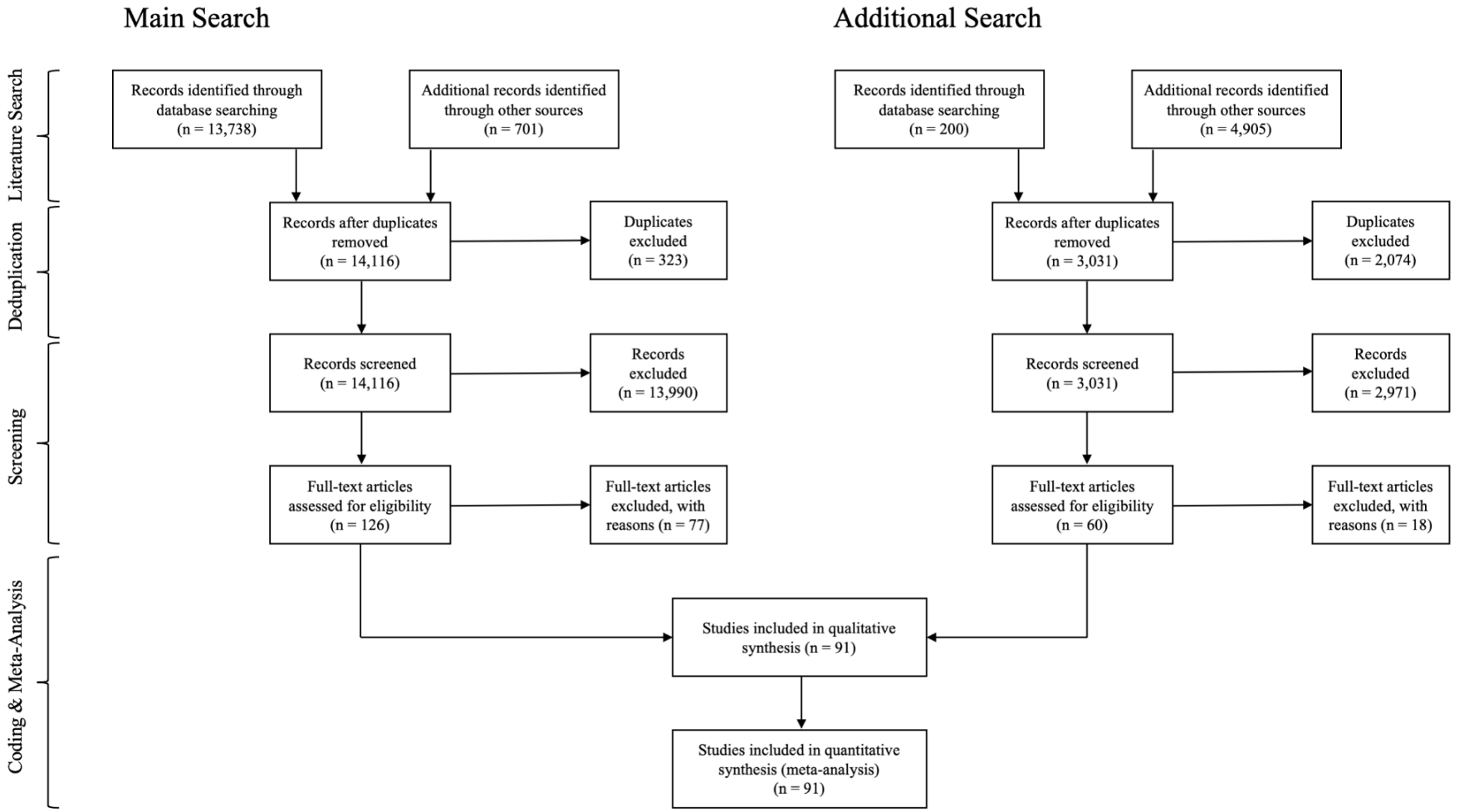

Figure 1. Preferred Reporting Items for Systematic Reviews and Meta-Analysis (PRISMA) flow diagram illustrating the literature search, article screening, and final inclusion

Dishonest Behavior. For dishonest behavior, we classified tasks based on a recent meta-analysis identifying the most common behavioral measures of dishonesty in the lab context (adapted from Gerlach et al., 2019). This resulted in six different types of dishonesty paradigms:

i. *Sender-Receiver* paradigms (k = 18) involve a sender making deceptive or honest offers, which are accepted or rejected by another party (the receiver) affecting both the sender's and receiver's outcomes (Gneezy, 2005; Sutter, 2009)

ii. *Coin Toss* paradigms (k = 6) involve participants tossing a coin and reporting on the outcome, which typically allows for false reports to increase outcomes (Bucciol & Piovesan, 2011)

iii. *Die Roll* paradigms (k = 87) include participants throwing one or more dice and reporting on the outcome, which typically allows for false reports to increase outcomes (Fischbacher & Föllmi-Heusi, 2013)

iv. *Matrix* paradigms (k = 30) involve participants solving one or several mathematical puzzles by finding two numbers in a given matrix that add up to a certain value, typically allowing for the possibility to overreport one's performance (Mazar et al., 2008)

v. *Dot* paradigms (k = 4) ask participants to report on which side of a line dividing a square more/less dots are shown, incentivizing false reports with higher compensation (Gino et al., 2010; Hochman et al., 2016).

vi. *Other* paradigms (k = 81) involve bribery or corruption paradigms (Abbink, 2002) or actual dishonest behavior such as theft or stealing (Pierce & Balasubramanian, 2015).

Article Screening & Coding

Screening Procedure.

Main Search. Article screening was performed using the *revtools* package (version 0.4.1; Westgate, 2019). We first screened all articles for duplicates using the package and identified $n = 323$ instances that were removed, leaving us with a final number of $n = 14116$ before screening. Screening was performed by the same three coders and all articles were distributed among them (the first [40%], second [20%], and third author [40%]). Thirty percent of articles were screened by at least two individuals. In a first step, all titles were screened leaving a final number of 675 articles. Reliabilities among coders were acceptable (ranging between 94.3% - 95.3% of overlap in codings). In a second step, the remaining abstracts were screened leaving 126 articles. Again, reliabilities among coders were acceptable (80.7% - 88.8% of overlap). Out of these, another 77 sources were excluded after full-text screening. Articles were excluded because they did not include a commitment manipulation ($n = 45$), no proper control condition ($n = 2$), no behavioral dishonesty measure ($n = 20$), were non-experimental ($n = 6$), presented a duplicate, e.g., a dissertation or preprint ($n = 2$), because dishonesty was not a voluntary action ($n = 1$) or because there was no correspondence between the commitment manipulation and the dishonesty measure ($n = 1$, see Supplementary Material, Section 4.1).

Additional Search. The first author screened all records identified by the additional searches using the *revtools* package. First, a total of $n = 2,074$ cases were removed as they represented duplicates, resulting in 3,031 additional articles before screening. After screening titles and abstracts a total of 60 articles were retained. Of these, another 18 were excluded after full-text screening. Articles were excluded because they did not include a commitment manipulation ($n = 13$), were non-experimental ($n = 1$), could not be accessed ($n = 1$),

presented a duplicate (n = 2), or contained missing data and were not recommended to be used by the original supervisor (n = 1, see Supplementary Material, Section 4.2).

Coding Procedure. Based on both search procedures, we identified 91 articles for final inclusion (main search: k = 49, additional search: k = 42). Of the articles identified in the main search, twelve from the main search were coded by at least two coders and overlap among the different coders was acceptable (69.29% - 81.03% of complete overlap for all main codes). Differences in codings were resolved by discussion among coders. As main information, we tried to retrieve cell sizes, means, and standard deviations for the control and experimental condition. We included multiple effects per article and study if available and applicable to our inclusion criteria. For example, if a study included manipulations of group size allocating participants either to working in a dyad or a group of three, we coded the comparison between the dyad vs. control and group of three vs. control as separate effects. We included separate effect sizes for every manipulation focusing on a moderator identified in our coding scheme. For sequential paradigms or bribery studies in which several parties could behave dishonestly, we recorded separate effects for first and second movers (or third, fourth etc. movers if applicable). If studies employed longitudinal designs or pre- and post-tests, we only focused on the time the experimental manipulation(s) was enacted. To improve transparency and reproducibility of our codings, we always noted the text quote and page of the statistics from which the final effect size was calculated (Lakens et al., 2016).

If information on cell sizes, means and/or standard deviations was not available we performed the following steps (in that order): 1.) checking whether missing information was presented in Appendix or Supplementary Materials, 2.) employing algebraic methods to derive missing information (e.g., Weir et al., 2018). For example, we calculated missing standard deviations for several articles reporting percentages when cell sizes were known, 3.) trying to locate whether the associated data was publicly available and calculated the needed

information, 4.) checking whether information was provided in earlier meta-analytic projects (e.g., Gerlach et al., 2019; Leib et al., 2021), 5.) contacting the original authors asking for the specific information, 6.) employing approximate algebraic methods (e.g., Hozo et al., 2005; Weir et al., 2018). For instance, if cell sizes were missing, we assumed equal distributions, 7.) excluding the article if the previous steps were not successful. In total, we contacted corresponding authors 18 times and received a response in 83.3% of cases. We calculated the final information based on openly available data for eight articles. For two studies, we employed approximate algebraic methods, and no study was excluded because of missing information.

Moderators. Next to assessing demographic information such as the type of population, number of female participants, or the average age of participants, we included several moderating variables in the coding scheme. These were informed by previous meta-analyses on dishonest behavior (e.g., Gerlach et al., 2019; Köbis et al., 2019) and focused on variables relating to the commitment manipulation, as well as variables relating to dishonest behavior. An overview of all moderators and additional codings is provided in Table 1 and the final coding scheme is available at: https://osf.io/bu9v6/.

Table 1. Overview of main coding variables and possible moderators.

Coding Variables	Description	Coding
Article Metadata		
ID	ID variable	1 to 19544
Author(s)	List of authors	
Title	Title of article	
Journal	Journal name	
doi	Doi of article	
Publication Year	Year of publication	
Publication Status	Status of publication	0 = unpublished 1 = published
Study/Population Metadata		
Study	Study number in article (1 if only one study is reported)	
Country	Country name in which study was conducted	
Region	Region in which study was conducted	UN M49
Subregion	Subregion in which study was conducted	UN M49
Population	Type of population	1 = undergraduates 2 = crowdsourcing site 3 = general 4 = minors (< 18)

		5 = fully representative 6 = mixed
Undergraduates	Type of student population	1 = economics 2 = psychology 3 = mixed 4 = other
N	Total number of participants (after exclusions)	
n females	Number of women in the total sample	
% females	Percentage of women in the total sample	
M_{age}	Mean age of participants	
SD_{age}	Standard deviation of participants' age	
Location	Where was the study conducted?	1 = online 2 = lab 3 = field 4 = other
IV-related codes (Commitment)		
Commitment Manipulation	Type of commitment manipulation	1 = joint action 2 = oath/pledge 3 = synchrony 4 = investment/effort 5 = in-group formation 6 = other
Commitment Design	Whether commitment manipulation is occurring between or within (e.g., in a within design participants are high commited AND not/low commited)	1 = between 2 = within
n Agents	Number of total agents in experimental condition	
Length	Total length of study procedure	Length in minutes
Relationship Prior	Was it possible for people in the experimental condition to know each other from before?	0 = no, 1 = yes
Hierarchy	Type of explicit hierarchy (leader, supervisor, leadership)	1 = equal, 2 = hierarchy
Contact	Type of contact with relationship partner(s)	1 = online (minimal information) 2 = online (audio) 3 = online (audio & video) 4 = in person 5 = other 6 = none
Communication[a]	Was communication possible during the dishonesty task?	0 = no 1 = yes
Equal	Equal sex relationship in commitment manipulation	0 = no 1 = yes
Goal	Common goal in commitment condition (collaborative corruption)	0 = no 1 = yes
Target of Commitment	What is the target of commitment; What type of agent are participants commited to/collaborate with?	1 = individual(s) 2 = organization 3 = country 4 = deity 5 = oath/promise 6 = other
Competition[a]	Does competition occur?	0 = no 1 = yes
DV-related codes (dishonesty)		
Dishonesty Paradigm	Dishonesty paradigm (Gerlach et al., 2019)	1 = sender-receiver 2 = coin toss 3 = die roll 4 = matrix

		5 = dot task 6 = other
Type of Dishonesty	Explicit type of dishonesty	1 = lying 2 = deception 3 = stealing 4 = bribery 5 = other
Private	Occurs dishonesty in private without possibility of being checked?	0 = no 1 = yes
Punishment	Is there a possible punishment for participants acting dishonestly?	0 = no 1 = yes
Dishonesty Design	Dishonesty design	1 = one-shot 2 = repeated
n of Rounds	Number of rounds in task (e.g., in die roll tasks how many dice are rolled. In matrix tasks how many matrices are solved etc.)	
Dishonesty Beneficiary[a]	Beneficiary of dishonest act	1 = participant (individual) 2 = participant's group 3 = participant's organization 4 = other
Dishonesty Victim[a]	Victim of dishonest act	1 = experimenter 2 = another individual 3 = another group 4 = another organization 5 = other 6 = participant's in-group
Dishonesty Measure[a]	Type of dishonesty measure used	1 = performance score 2 = dishonesty score
Order	Order in sequential tasks (e.g., does the effect refer to the first mover or the second mover)	1 = first 2 = second
Incentive Structures	Incentive structure of experiment (e.g., do participants receive the same amount of incentive or different amounts?)	1 = aligned 2 = disentangled
Advantages	Advantage for participants in terms of rewards (i.e., does the participant receive more than the other agent(s)?	0 = disadvantage 1 = equal 2 = advantage
Reward Type	Type of reward	1 = money 2 = other
Maximum Reward	Max reward (in local currency; INCLUDING possible show up fee).	Converted in USD
Fixed Reward	Fixed reward (e.g., show up fee) in local currency (if applicable)	Converted in USD
Effect-Related Codes		
$M_{control}$	Mean of control condition	
$SD_{control}$	SD of control condition	
$n_{control}$	N of control condition	
$M_{experimental}$	M of experimental condition	
$SD_{experimental}$	SD of experimental condition	
$n_{experimental}$	N of experomental condition	
Effect	Effect size (if *M*, *SD*, and *n* cannot be obtained)	
Variance	Variance of effect size	
Type of Effect	Type of the effect size	
Page	Page on which the coded information was obtained (indicate in case of other sources e.g., open dataset, supplement etc.)	
Quote	Specific quote of the relevant effect size statistics from article (refer to a table if not written in text)	
Contact Authors	Indicate whether original authors need to be contacted	0 = no 1 = yes
Coding Metadata		

Coder	Referring to the main coder	
Identifier	String variable identifying specific additional treatments if there exist several effects per study	
Reverse Scored	Indicating whether high values are indicative of high honesty	0 = no 1 = yes

Note. [a]These codings were added during the coding process and were not originally preregistered.

Results

All analyses were performed in *R* (version 4.0.3; R Core Team, 2019) using the following packages: *metafor* (version 2.4-0; Viechtbauer, 2010), *dmetar* (version 0.0.9; Harrer et al., 2019), *meta* (version 4.18-1; Schwarzer, 2007), *metaforest* (version 0.1.3; van Lissa, 2020), *metaviz* (version 0.3.1; Kossmeier et al., 2017), *PublicationBias* (version 2.2.0; Mathur & VanderWeele, 2020), *TOSTER* (version 0.3.4; Lakens, 2017), *weightr* (version 2.0.2; Coburn et al., 2019), and *dplyr* (version 1.0.2; Wickham et al., 2015).

Effect Size Calculation. We calculated effect sizes Hedges' *g*, the standardized mean difference correcting for small sample bias (Hedges & Olkin, 2014), based on the mean, standard deviation, and cell size for the control and experimental treatments using the *metafor* package. Importantly, *g* can be interpreted the same way as Cohen's *d* for larger sample sizes ($n > 20$). A negative effect size indicates increased dishonesty in the experimental treatment, while a positive effect size suggests increased honesty in the experimental treatment compared to the control condition. In some cases, we needed to reverse score effect sizes to fit this interpretation. The majority of effects were derived from a between-subjects design (86.2%), while some studies featured a within-subjects design (12.8%). For all designs, we employed the same way of calculating the final standardized effect size using the pooled standard deviation based on the two groups design with the correction for small sample sizes (Goulet-Pelletier & Cousineau, 2018; Westfall, 2016). This decision was based on two main reasons. First, calculating the standardized mean difference for a repeated measures design requires the cross-measurement correlation that is rarely reported, and therefore might have resulted in excluding more articles. Second, it has been

argued that the standardized mean difference based on the two groups or repeated measures design are rather similar (Westfall, 2016). If any, our final effect sizes might be more conservative. Similarly, more than half of all included studies measured dishonesty using a repeated design, letting participants in several possibilities to misreport the actual outcome (51.8%). However, the majority of studies report final effects based on aggregating repeated ratings per participant, instead of applying multilevel models to account for variability within individuals as has been suggested (Judd et al., 2012). As most of the studies, we therefore focus on the aggregate cell size when calculating the standardized mean difference. Thereby, we lose a certain degree of power associated with the repeated measurement of the dependent variable. Interpreting the precision of individual effect sizes might therefore be misleading in some cases. However, we primarily focus on the overall meta-analytic effect sizes and the fact that we consider the aggregate ratings per subject might result in more conservative estimates, if any.

Analysis Strategy. For the main analyses, we employed a random effects three-level meta-analytic model (also called *multilevel meta-analysis*) due to the fact that we sometimes included effect sizes from the same article and the same study. For the random effects structure, we thus specified random intercepts for individual effects nested in individual studies (controlling for the fact that two or more effects could originate from the same study), which were again nested in article (controlling for the fact that two or more effects could originate from the same article). We examined heterogeneity focusing on I^2 and investigated the distribution of total variance for the different levels using the *dmetar* package. All models were fitted using restricted maximum-likelihood estimation (REML) in the *metafor* package.

Publication Bias. Publication bias arises if, all else equal, non-significant or non-affirmative results have a lower probability of getting published than statistically significant or affirmative results (Dickersin, 1990). As a consequence, overall effect size estimates might

be biased suggesting a possible effect when none exists. There has been considerable debate on how to adequately control for publication bias, and different methods have been suggested (Carter et al., 2019; Dickersin, 1990; Duval & Tweedie, 2000; Mathur & VanderWeele, 2020a; McShane et al., 2016). Based on simulation studies, it seems that performance of these different methods depends highly on the situation and the context of the studies included in the meta-analysis, including the type of model, the amount of so-called questionable research practices, and the false positive rate (Carter et al., 2019). Therefore, we employed several different strategies to quantify and control for publication bias. First, we visualized individual effect sizes using a funnel plot and employed the Egger's test in order to test for possible asymmetry in the funnel plot. This was supplemented using a trim-and-fill method that adjusts for possible publication bias by adding *missing* studies to restore symmetry in the funnel plot (Duval & Tweedie, 2000). Second, we conducted a *p-curve analysis* using the *dmetar* package. A p-curve analysis tests whether statistically significant studies exhibit evidential value by taking into account the fact that p-values are distributed differently for an actual effect and a null effect (Simonsohn et al., 2014). Under the null, p-values are distributed uniformly, while the distribution is expected to be right-skewed for an actual effect (assuming sufficient power).[6] Third, we performed the *three-parameter selection model* (McShane et al., 2016) using the *weightr* package. This procedure compares a model that is generated in the absence of any publication bias and a model that is considered to be based on the publication process. Weighting the likelihood that affirmative results in contrast to non-affirmative studies get published, the model attempts to uncover the parameters under which the generated data are most likely. Fourth, we conducted a sensitivity analysis using the *PublicationBias* package. This approach considers how much more likely

[6] Note, that we needed to recalculate the three-level model using the *meta* package in order to apply the trim-and-fill procedure and the p-curve analysis. Due to different estimation approaches the results from the *metafor* and *meta* package differed slightly (overall *g* of .09 vs. .10).

affirmative results need to be published in order to shift the observed estimate (Mathur & VanderWeele, 2020b). We set *eta* at 3.51 based on findings by Mathur & VanderWeele (2020a) suggesting that affirmative studies are around three point five times more likely to be published than non-affirmative studies.

Quantifying evidence for the null. In order to quantify whether the final meta-analytic estimate shows evidence for the null (or not), we employed equivalence testing using the *TOSTER* package (Lakens, 2017). We set our smallest effect size of interest (SESOI) a-priori at $g = .20$. There were several reasons for choosing this SESOI. Recent studies trying to estimate typical average effect size estimates in disciplines including social psychology reported estimates around $d = .40$ (Lovakov & Agadullina, 2021; Richard et al., 2003). Similarly, focusing on the 25th percentile of effect sizes published in various meta-analyses suggested $d = .15$ (Lovakov & Agadullina, 2021). In addition, an effect size of $g = .20$ means that 92% of the control and the experimental condition will overlap. We consider that effects that are significantly smaller than this SESOI are unlikely to be of any practical significance.

Moderator Analysis. In order to investigate the heterogeneity of effect sizes and explore possible moderators, we performed a random forest approach using the *MetaForest* package (van Lissa, 2020). Random forests represent a supervised machine learning approach and have several strengths compared to classical regression models as they are naïve to the direction of effects, can handle multicollinearity, include higher-order interactions, and are non-parametric (IJzerman et al., 2018). The MetaForest procedure identifies the importance of different moderators for explaining the observed heterogeneity. We considered all variables included in our coding scheme as possible moderators except for redundant variables (i.e., the number and percentage of females, local currency and USD rewards). For rewards, we focused on the maximum reward minus a possible show up fee (describing the reward that can be achieved by engaging in dishonest behavior). In addition, we only

included moderators without missing values as the MetaForest algorithm is not able to handle these. For that purpose, we retained variables that showed less than 20% of overall missing values, removing studies with missing values for these variables for the final moderator analysis. Note, that this decision was not explicitly preregistered.

Following van Lissa (2020), we first checked at what number of trees the MetaForest model converged and then selected variables for which the 50% percentile interval did not include zero. Employing a 10-fold clustered cross-validation, we then determined the optimal tuning parameters for the final model. Finally, we applied the final model and assessed variable importance for each included moderator. We followed up by conducting a subgroup analysis using the *dmetar* package for the five most important moderator variables identified in this approach. We only focused on subgroups including at least $k = 20$ observations. In addition, based on our preregistration, we performed three explicit comparisons no matter whether these moderators were uncovered in the MetaForest model. First, we compared collaborative dishonesty tasks (i.e., joint action/coordination manipulations) with the remaining tasks (this variable was considered as one of the five most important moderator variables). Second, we compared common goal with non-common goal tasks. Third, we compared *prosocial* with *non-prosocial* dishonesty focusing on whether participants earned less, equal, or more in contrast to the other agent(s).

Influence/Outlier Analysis. We conducted an influence analysis using the *dmetar* package in order to quantify the influence of different estimates on the between-study heterogeneity, on the pooled estimate, and in order to determine possible influential effect sizes. We observed several effects ($n = 7$) that showed considerably high effect size estimates ($g >= -2$), and were flagged by different influence diagnostics. As the majority of these included rather small cell sizes ($M = 50.21$ per cell), and showed effect sizes as large as $g = -$

4.95, we decided to remove these seven effects from the final analysis. Sensitivity analyses including these seven effect sizes can be found in the Supplementary Material (Section 2).

Overview of Effects. In total, our literature review identified 226 effect sizes obtained from 91 articles with a total sample size of 40,972 (ranging from 20-5,039 total participants per study, median = 184; see Supplementary Material Table 1 for an overview of all studies). On average, studies included 50.30% females, and showed a mean age of 23.67 years. Note that this information was not available for all studies (information on gender missing for 23.2% of all studies, information on mean age for 37.6%). The majority of participants across the studies were undergraduates (75.2%). A total of 13.2% of all studies included a general population or a crowdsourcing population, and none of the studies featured a representative sample. These studies were conducted in 23 different countries, with the majority coming from Europe (n = 102) followed by the Americas (n = 80), and Asia (n = 44). Most effects were derived from studies conducted in Germany (n = 65) and the US (n = 60). Publication years ranged from 2004 to 2021 (median = 2018), and the majority were published after peer-review (n = 180, 79.64%). After excluding the seven effects identified in the influence analysis, the final amount included 219 estimates from 91 articles.

Pre-registered Analyses.

Overall Effect. We observed an overall small negative effect of g = -.09 [-.16, -.01] across all studies. Studies were characterized by a high amount of heterogeneity, $Q(218)$ = 1116.97, $p < .001$, total I^2 = 84.17%. Focusing on the variance distribution, the highest amount of variance was detected on the third level (48.42%), followed by the second (35.75%) and first (15.83%), suggesting that there was slightly more heterogeneity between studies and articles as there was within. We compared the final three-level model to models removing the second and third level and observed that model fit was significantly better for the full model (see Supplementary Material Section 3.1).

Importantly, this finding is not surprising given the fact that social commitment might influence dishonest behavior in two different directions. The small negative overall effect possibly suggests that more studies focusing on commitment to individuals and groups are included in our meta-analysis, which is indeed the case when looking at the number of studies per type of commitment manipulation. Given the fact that social commitment might lead to less or more dishonesty, the overall effect and the methods attempting to control for publication bias should be interpreted with caution. It is not clear whether the overall effect is reduced due to actual publication bias or mainly because there are less studies included focusing on commitment to social norms. A more helpful analysis will focus on comparing subgroups of specific studies.

Focusing on possible publication bias, the Eggers' test indicated the presence of funnel plot asymmetry and a trim-and-fill method suggested to add 44 studies, resulting in a corrected overall estimate of g = .04 [-.04, .11]. A p-curve model suggested that evidential value was present. The majority of observed values was p <= .01 (94%). A total of 25 studies included a non-significant effect size. Similarly, the sensitivity analysis indicated a non-significant corrected overall estimate, g = .01 [-.05, .07]. Comparing the obtained effect with our SESOI, we observed that the effect was statistically different from zero and statistically equivalent to zero (see Supplementary Material, Section 3.2). Considering the correction methods and the equivalence tests, we conclude that there is no evidence for an overall effect of commitment on dishonest behavior.

Random Forest Moderator Analysis. We observed high heterogeneity among the different effect sizes with individual studies ranging from g = -1.87 to .92. Based on the random forest procedure (see Supplementary Material, Section 3.3), we identified the target of commitment, the year of publication, the type of commitment manipulation, the type of dishonesty design, and the type of dishonesty paradigm as the five most important variables

explaining heterogeneity. Focusing on the target of commitment (Figure 2), we observed a significant moderation effect, $F(1,214) = 10.58$, $p < .001$. Being commitment to individual(s) ($k = 156$) increased dishonest behavior, $g = -.22$ [-.29, -.14], while feeling commitment to oaths or pledges ($k = 46$) increased honest behavior, $g = .27$ [.16, .38]. We observed some indication of publication bias for the effect sizes focusing on commitment to individual(s) and to a smaller degree for the studies testing commitment to oaths or pledges (Figure 2). Both effects were not significantly smaller than our SESOI.

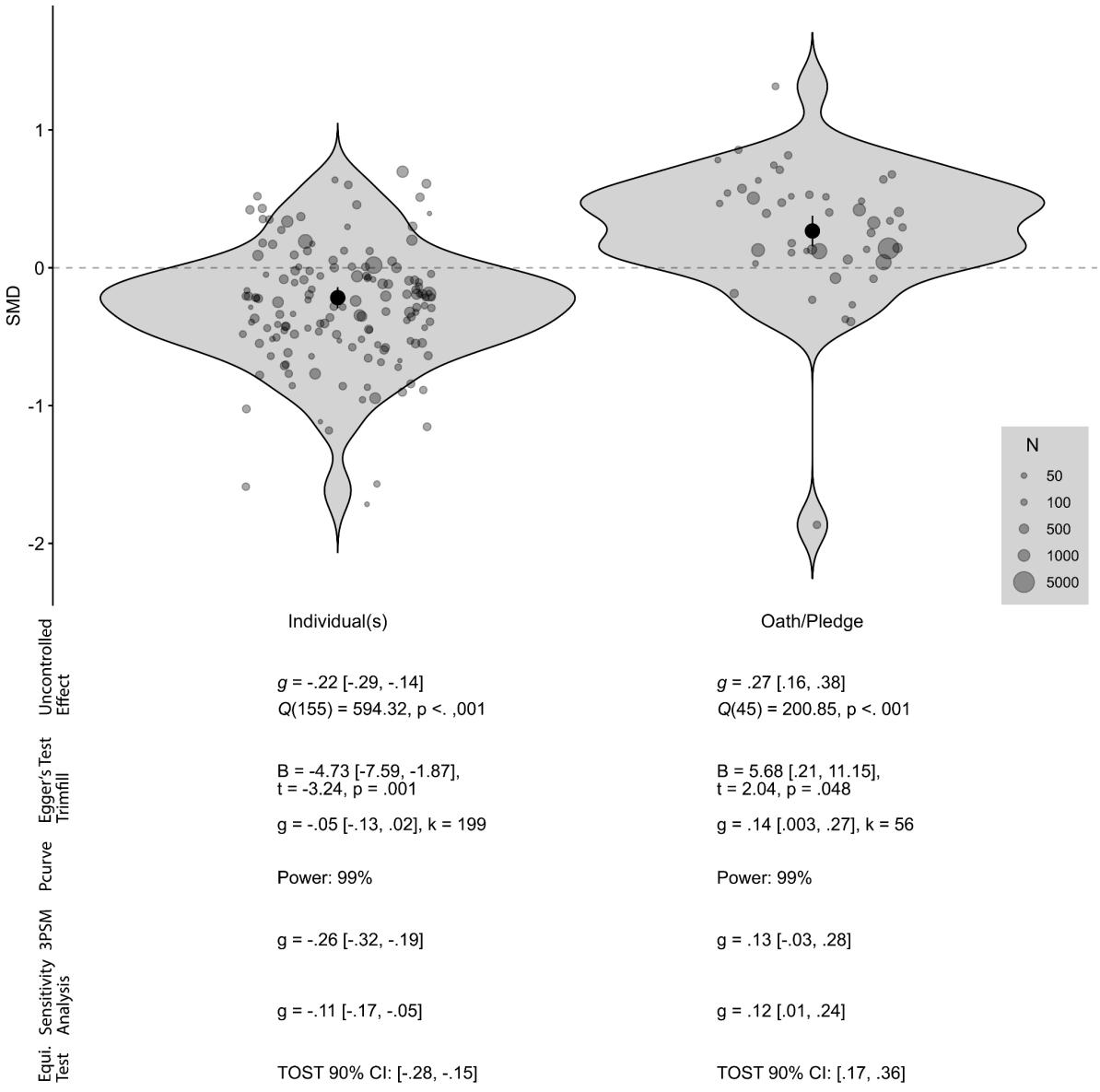

Figure 2. Violin plot of meta-analytical models per type of target. Points represent individual studies, black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects

increased honesty. SMD = standardized mean difference (Hedge's *g*), 3PSM = three parameter selection model, Equi. Test = equivalence testing.

When focusing on the effect of publication year, we observed no statistically significant overall moderation effect, $F(1, 217) = .95$, $p = .331$. We observed a small negative correlation (r = -.09 [-.25, .06]) between publication year and the standardized mean difference (Figure 3, A). From 2004 to 2021, effect sizes became slightly more negative. However, when checking the correlation between the absolute effect size strength (regardless of its direction) and publication year, this relationship was negative (r = -.13 [-.27, .02]). We also explored the relationship between publication year and the variance or precision of effects (Figure 3, B). Similar to the finding that sample sizes have been increasing during recent years in psychological research (Sassenberg & Ditrich, 2019), we found evidence that studies have become more precise with the standard error of effects decreasing over time (r = -.13 [-.31, .04]). When focusing on the relationship between publication year and total sample size we also found a small positive correlation (r = .20 [-.04, .44]). We repeated the analyses for the different targets (individuals vs. oath) in an exploratory manner. Decreases in absolute effect size, standard error with increasing publication year and increases in total N with increasing publication year were stronger for studies focusing on commitment to oaths (Supplementary Material Section 3.4).

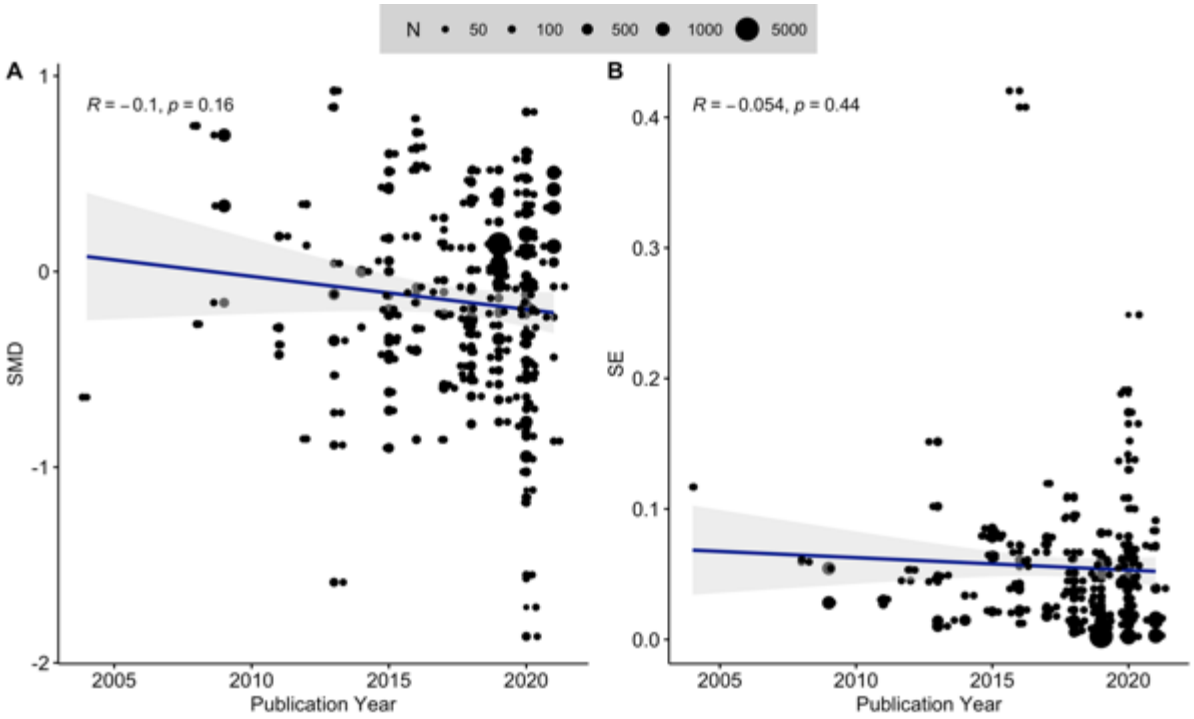

Figure 3. Scatter plot of association between publication year and standardized mean difference (A) and variance (B). Each point represents and individual effect and point sizes represent the total amount of participants per study.

Considering the type of commitment manipulation, we observed a significant overall moderation, $F(4,214) = 10.70$, $p < .001$. Commitment induced via in-group formation tasks ($k = 66$) showed the strongest increases in dishonesty, $g = -.24$ [-.36, -.12], followed by joint action tasks ($k = 65$), $g = -.19$ [-.31, -.07], and investment/effort tasks ($k = 34$), $g = -.16$ [-.31, -.01]. On the other hand, commitment induced via oath/pledge tasks ($k = 49$) increased honesty, $g = .26$ [.15, .37]. An overview is provided in Figure 4. We observed some evidence of publication bias for the joint action/coordination, investment/effort, and oath/pledge paradigms, while there was little evidence of possible publication bias for the in-group formation tasks. All effects were not significantly smaller than our SESOI.

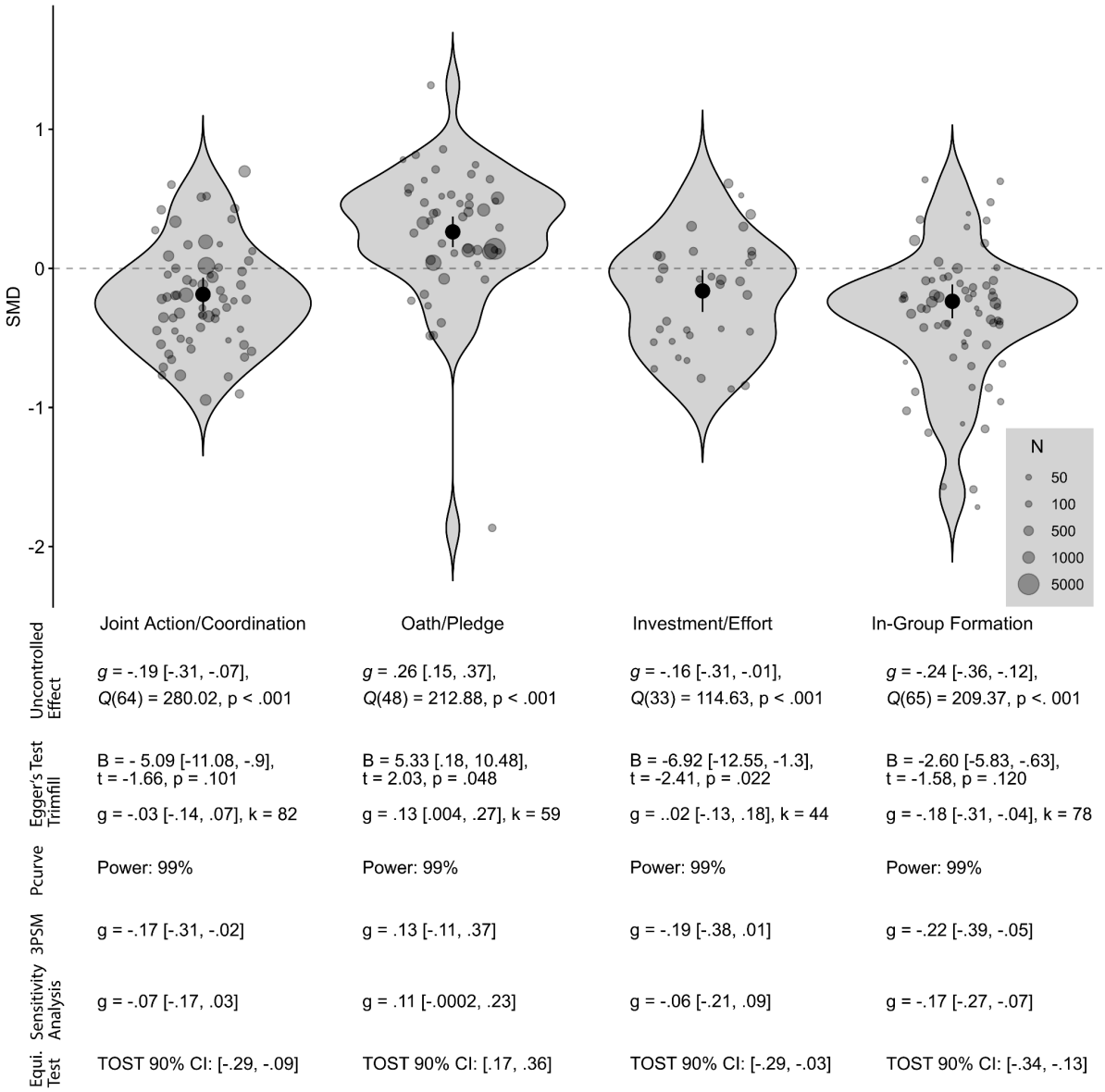

Figure 4. Violin plot of meta-analytical models per commitment manipulation. Points represent individual studies, black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects increased honesty. SMD = standardized mean difference (Hedge's *g*), 3PSM = three parameter selection model, Equi. Test = equivalence testing.

Focusing on the type of dishonesty design we found no overall significant moderation effect, $F(1, 217) = .20$, $p = .653$. There were no significant differences whether participants played a one-shot dishonesty task (e.g., throwing a dice only once, ($k = 105$, $g = -.10$ [-.20, .01]), or a repeated paradigm ($k = 114$, $g = -.06$ [-.17, .05], Supplementary Figure S5). There were signs of publication bias for both type of effects, suggesting even smaller overall effects

(Supplementary Figure, S5). We also found that the effects were significantly smaller than our SESOI. Because direction of the effects might depend strongly on the type of manipulation, we explored differences in effects of type of dishonesty design for each type of commitment manipulation separately. We found similar effects for one-shot and repeated designs for joint action (one-shot: g = -.20 [-.35, -.05] vs. repeated: g = -.16 [-.35, .02]), and in-group manipulations (one-shot: g = -.21 [-.41, -.01] vs. repeated: g = -.23 [-.33, -.14]). In contrast, effects were stronger for repeated designs for the oath/pledge manipulations (one-shot: g = .18 [.01, .34] vs. repeated: g = .35 [.19, .50]). For the investment/effort manipulation, one-shot designs reduced dishonesty (g = .24 [.02, .45]), while repeated designs increased dishonesty (g = -.28 [-.41, -.15]). Importantly, caution should be applied when interpreting this difference as the number of effects for each group is quite small.

Finally, when focusing on the type of dishonesty paradigm, we found no significant overall moderation effect, $F(2,188)$ = .15, p = .858. Die-roll, g = -.12 [-.22, -.02]), and *other* tasks, g = -.12 [-.25, .01]), showed stronger dishonesty effects than matrix tasks, g = .002 [-.30, .31] (see Supplementary Figure S6). The effect for matrix tasks was significantly smaller than our SESOI.

We also investigated the moderating effect of whether participants had a common goal, and whether they earned less, equal, or more when engaging in dishonest behavior compared to other individuals as outlined in our pre-registration. The results suggest that having a common goal increases the overall effect of commitment on dishonesty (g = -.19 [-.30, -.09]), compared to sharing no common goal (g = -.13 [-.26, .003], Figure 5). Notably, these effects were not statistically significantly different from each other. We also observed evidence of publication bias for the non-common goal effect and to a lesser degree for the common goal effect. Considering the incentive structure, we found that the effect of commitment on dishonesty was strongest when higher stakes were included (g = -.23 [-.51,

.05]) in contrast to equal (g = -.17 [-.35, .02]) or lower stakes (g = -.12 [-.33, .10]). Importantly, these effects were not significantly different from each other and the number of effect sizes were small for each group (see Supplementary Figure S7).

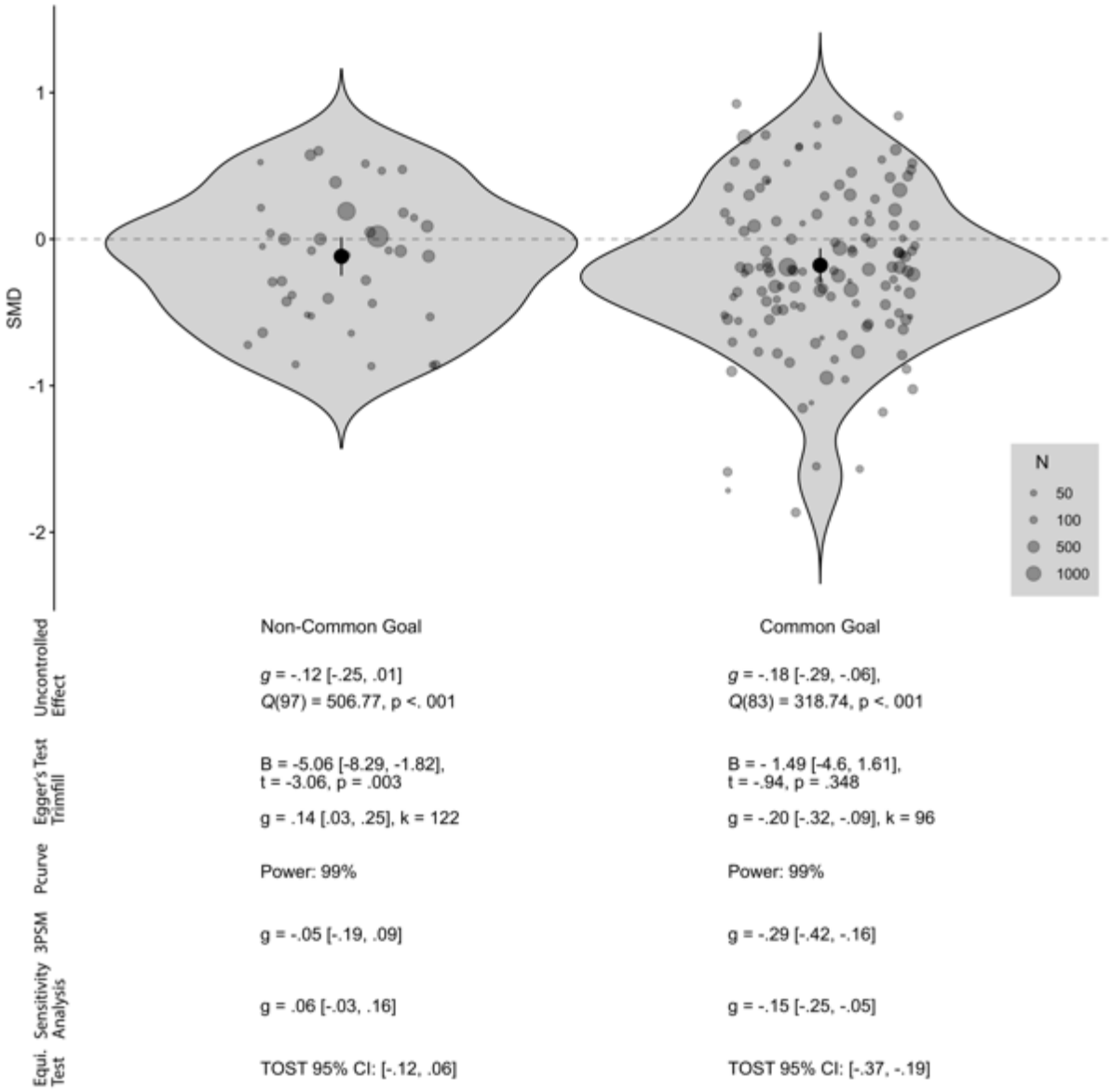

Figure 5. Violin plot of meta-analytical models per type of goal. Points represent individual studies, black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects increased honesty. SMD = standardized mean difference (Hedges' g), 3PSM = three parameter selection model, Equi. Test = equivalence testing.

Exploratory Analyses.

Additional Moderator Analyses. We explored whether the effect of commitment on dishonesty was moderated by the number of agents interacting (Figure 7). Our results suggest

that feeling committed to a no additional individual (i.e. one agent, without interaction) slightly increases honesty (g = .16 [.03, .29]), while feeling committed to another individual (i.e. two agents) increases dishonesty (g = -.19 [-.27, -.10]), and this effect does not further increase when three agents interact (g = -.22 [-.43, -.01]). When focusing on the effect of competition, we observed that competition increased the effect of commitment on dishonesty (g = -.37 [-.56, -.17]) in comparison to no competition (g = -.05 [-.13, .03], Figure 8). We further explored whether this effect differed per type of manipulation, and found that competition increased dishonesty for all types of paradigms - even oath/pledge manipulations (see Supplementary Material, Section 3.6). Importantly, caution needs to be applied as only a small number of reviewed studies introduced competition. In addition, we focused on the question whether possible communication affected dishonest behavior. We observed slightly stronger dishonesty for studies including possible communication (g = -.17 [-.32, -.02]), in contrast to no communication (g = -.06 [-.14, .03], Figure 9). Further exploring this effect per manipulation type, we observed small differences for joint action and in-group manipulations and a stronger effect for oath/pledge manipulations (see Supplementary Material, Section 3.7).

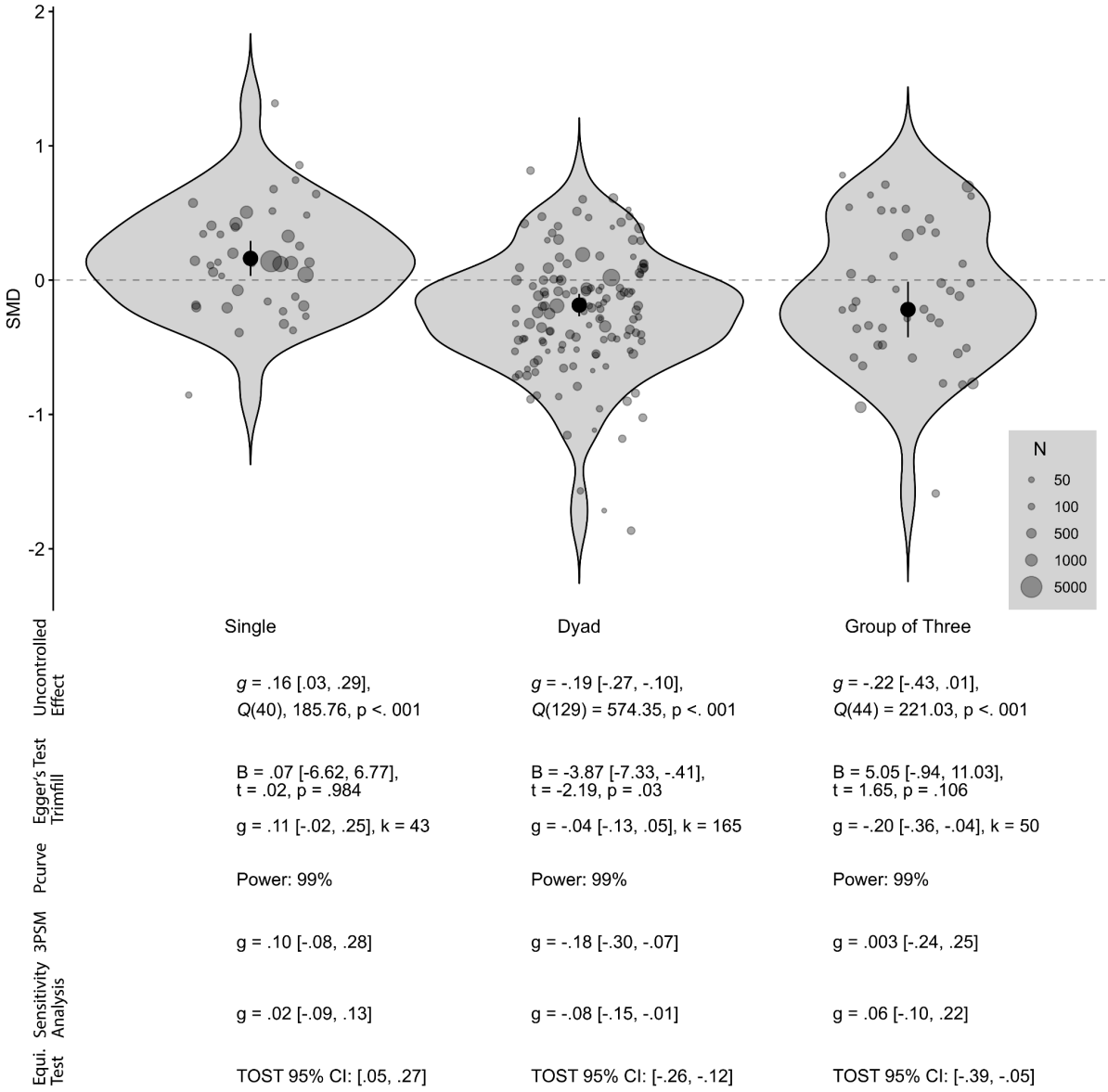

Figure 7. Violin plot of meta-analytical models per number of agents. Points represent individual studies, black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects increased honesty. SMD = standardized mean difference (Hedges' *g*), 3PSM = three parameter selection model, Equi. Test = equivalence testing.

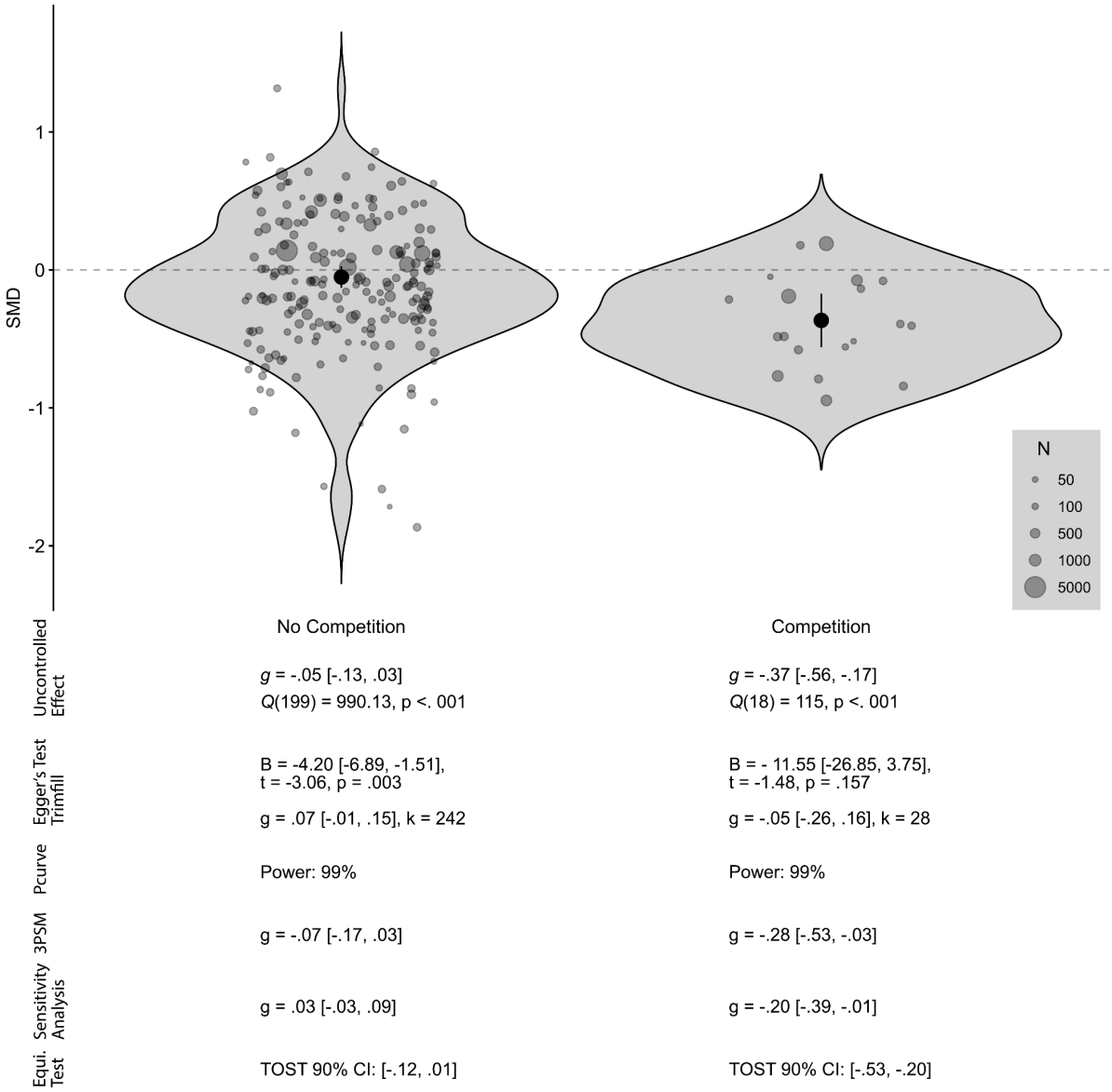

Figure 8. Violin plot of meta-analytical models per competition. Points represent individual studies, black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects increased honesty. SMD = standardized mean difference (Hedges' *g*), 3PSM = three parameter selection model, Equi. Test = equivalence testing.

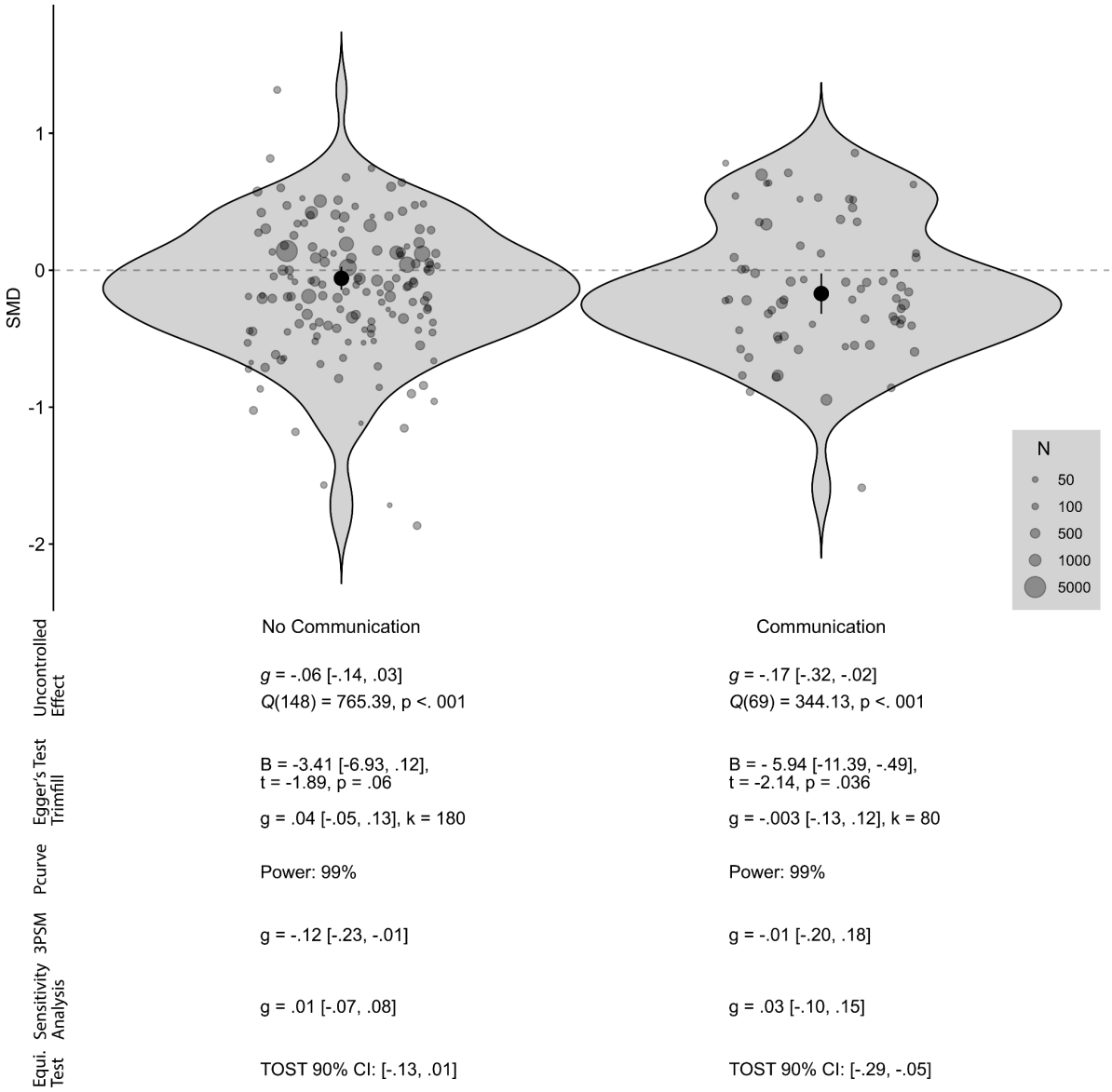

Figure 9. Violin plot of meta-analytical models per communication possibility. Points represent individual studies, the black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects increased honesty. SMD = standardized mean difference (Hedges' *g*), 3PSM = three parameter selection model, Equi. Test = equivalence testing.

Contrary to previous studies, we only found limited evidence that making decisions fully in private increased the effect on dishonesty (g = -.12 [-.20, -.04]) in contrast to being possibly checked by the experimenter or other participants (g = -.05 [-.18, .08], see Supplementary Figure S8). Similarly, we did not obtain strong evidence that punishment reduces the effect of dishonesty (g = -.04 [-.25, .16]) in contrast to no possible punishment (g

= -.10 [-.18, -.02], see Supplementary Figure S9). Yet, we included only a limited number of studies that featured possible punishments.

Finally, we explored whether increasing stakes or rewards has an effect on the relationship between commitment and dishonesty. Interestingly, we did not find an overall strong correlation (r = .04 [-.17, .24]). When focusing on the non-linear relationship, we first observed that committed dishonesty increased with increasing rewards until around 10 USD when commitment leads to more honesty with increasing rewards. This seemed to change again around a maximum reward of 20 USD (see Supplementary Figure S10). Importantly, few studies paid possible higher maximum rewards than 20 USD.

Random Forests For Type of Commitment. We also repeated the random forest moderator analysis for the different types of commitment. As we predicted that different types of social commitment would affect dishonest behavior in opposite directions, and since this factor showed the strongest variable importance for the overall effects, exploring moderators related to feeling committed to other individuals and norms separately seemed warranted. An overview of the results is reported in Figure 10 and Figure 11 (see Supplementary Material, Section 3.7, for more information on the random forest procedure).

For feeling committed to other individuals, we identified the number of rounds for the dishonesty task, the dishonesty paradigm, the location of the experiment, the type of dishonesty, and whether dishonesty occurred in private or public as the most important moderator variables. Effects were strongest in matrix (g = -.37 [-.82, .07] and other (i.e., miscellaneous) tasks (g = -.35 [-.49, -.21]), followed by die roll (g = -.16 [-.25, -.07]) and sender-receiver paradigms (g = -.06 [-.31, .19], Figure 10, B). Dishonesty was strongest for field studies (g = -.46 [-.71, -.21]), followed by lab studies (g = -.20 [-.28, -.12]), and smallest for online studies (g = -.10 [-.32, .12], Figure 10, C). Effects studying bribery showed the strongest effect (g = -.31 [-.62, -.01]), followed by lying behavior (g = -.22 [-.30, -.14]), and

deception (g = -.13 [-.26, .005]; Figure 10, D). Effects decreased with increasing number of rounds played for the dishonesty task (r = -.13 [-.32, .07]), signaling increased dishonesty (Figure 10, E). Surprisingly, dishonest behavior was stronger when it did not occur in private and could be checked by the experimenter (g = -.29 [-.43, -.15]), than if checking actual performance was not possible (g = -.19 [-.28, -.10], Figure 10, F).

For feeling committed to oaths or pledges, we observed the strongest variable importance for the number of rounds, communication, publication status, number of agents, and subregion (Figure 11, A). Effects increased with increasing number of rounds played for the dishonesty tasks (r = .31 [.01, .61], suggesting increased honesty over time (Figure 11, B). In addition, being able to communicate with other participants in the experiment increased honesty (g = .50 [.32, .67]) more so than no possible communication (g = .20 [.08, .32], Figure 11, C). Effects were slightly stronger when participants were not committed to other agents (g = .22 [.10, .34]) in contrast to being committed to another agent in a dyad (g = .15 [-.54, .85], ,Figure 11, D). Effects were stronger for published studies (g = .32 [.21, .43]) in comparison to unpublished studies (g = .08 [-.19, .35], Figure 11, E). Finally, participants from Northern America showed stronger effects (i.e., increased honesty, g = .40 [.22, .58]) than participants from Western Europe (g = 14 [.0003, .29], Figure 11 F).

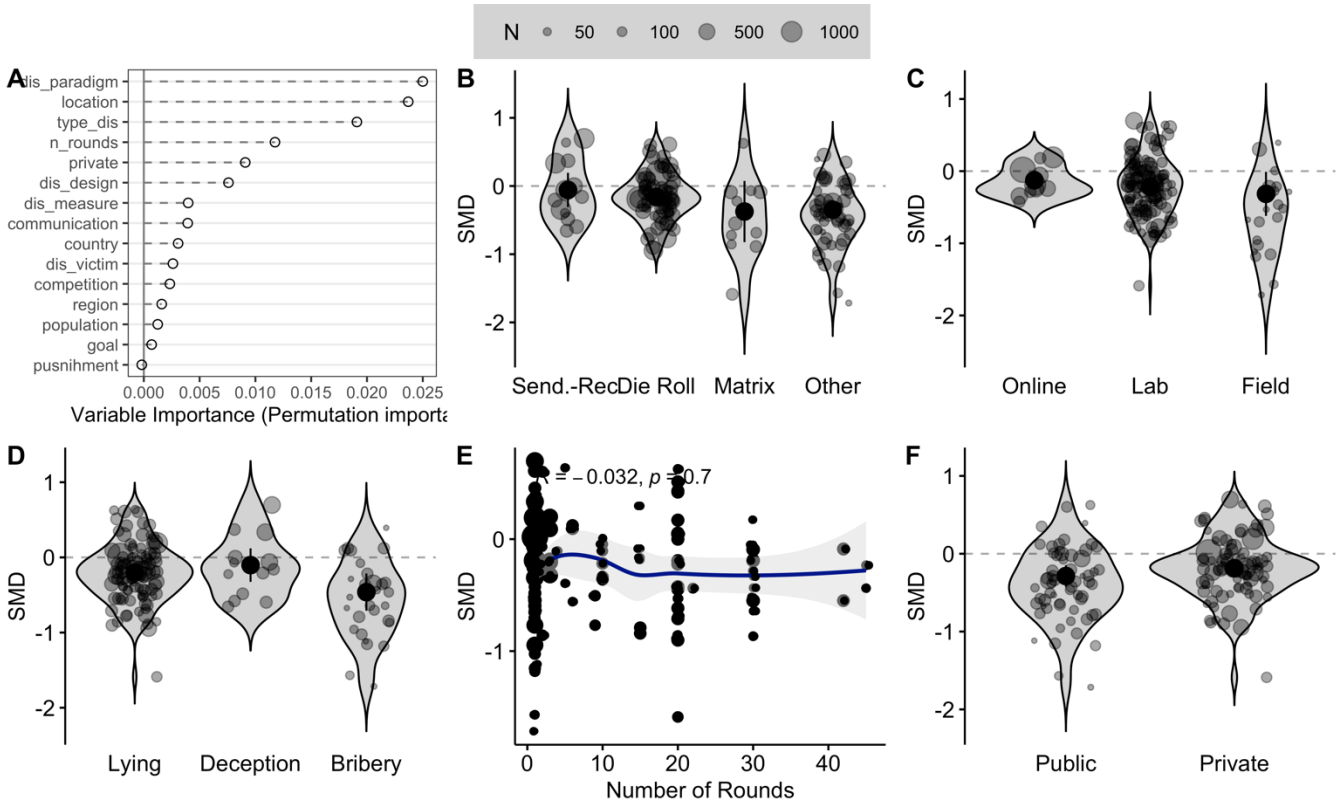

Figure 10. Overview of variable importance plot (A) based on the random forest procedure for feeling committed to other individuals and violin plots of meta-analytical models for the five most important variables: dishonesty paradigm (B), location (C), type of dishonesty (D), number of rounds (E), and private (F). Points represent individual studies, the black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects increased honesty. SMD = standardized mean difference (*g*).

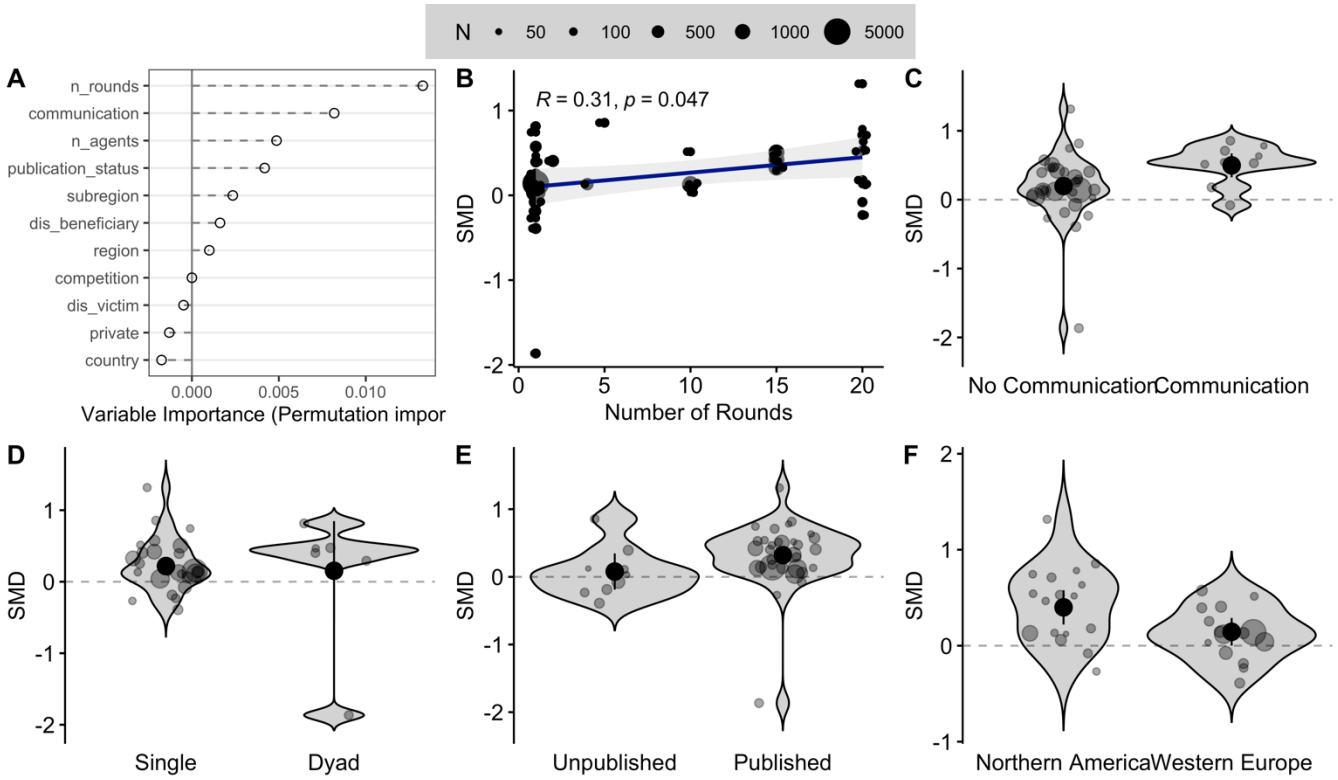

Figure 11. Overview of variable importance plot (A) based on the random forest procedure for feeling committed to norms and violin plots of meta-analytical models for the five most important variables: number of rounds (B), communication (C), number of agents (D), publication status (E), and subregion (F). Points represent individual studies, the black point and lines represent the meta-analytical effect size and its 95% confidence interval. A negative score reflects increased dishonesty, while a positive score reflects increased honesty. SMD = standardized mean difference (*g*).

Discussion

Across 226 effect sizes from 91 articles, representing a total of 40,972 individuals, we investigated the impact of social commitment on dishonest behavior. Specifically, we focused on feeling committed to other individuals, groups, organizations, or social norms, oaths, and pledges. Altogether, we observed an overall slightly negative effect (indicating dishonesty) of social commitment that was not significantly stronger than our smallest effect size of interest. Thus, we suggest that a generic effect of social commitment on dishonest behavior, without knowledge about the target of the commitment (i.e., commitment to a specific individual or group vs. commitment to a social norm), cannot meaningfully be interpreted. More specifically, we observed that the target to whom or to which the social commitment is

enacted, and relatedly the specific type of commitment manipulation, moderated this effect - commitment to individuals or groups increased dishonest behavior, while commitment to norms and regulations via oaths or pledges increased honest behavior overall.

Commitment to Individuals or Groups

Feeling committed to other individuals increased dishonest behavior overall. The effect size is best described as *small* based on conventional guidelines (Sawilowsky, 2009), but possibly reflects a more common and *medium* effect size in psychological science (Lovakov & Agadullina, 2021). This effect was also smaller than findings on social influence in a previous meta-analysis that focused on studies including intentions or attitudes towards dishonesty (Belle & Cantarelli, 2017). Previous studies already highlighted the possibility that some effects in studies on corrupt collaboration have been overestimated (Wouda et al., 2017), and the current investigation highlights a high heterogeneity in the effects of feeling committed to other individuals or groups on dishonest behavior. We identified the type of commitment manipulation as one possible moderating factor. In-group formation tasks, i.e., belongingness to or identification with a specific group, showed stronger effects than joint action and coordination or investment and effort tasks. Differences between these manipulations were relatively small, and possibly also depended on the fact that we included a smaller number of investment and effort tasks. Increasing the salience of an in-group might activate group-dependent norms or strengthen prosociality concerns more easily than merely working or coordinating together. None of the reviewed studies compared such paradigms directly, so the specific mechanisms favoring specific induction methods remain unknown.

We also found that dishonesty in response to commitment to individuals or groups was strongest for bribing behavior, in contrast to lying or deceptive behavior. By definition, bribery, as a social exchange in which one with power makes an illegal request to another individual, involves the coordination of at least two individuals, which might strengthen

commitment in comparison to lying tasks, in which one can misreport a die roll and potentially attribute the outcome to group-specific norms (Zheng et al., 2020; Abbink, 2002). Furthermore, field studies showed the strongest dishonesty effects, while smaller effects were found for laboratory and online studies. These observations are in contrast to findings reported by Gerlach et al. (2019), who found similar dishonesty ratings for online and laboratory studies, and reduced outcomes for field experiments. One possibility for the diverging findings might be constraints in inducing commitment to other individuals in online tasks compared to laboratory and field experiments, in which face-to-face participation or coordination is more easily implemented.

Similar to Gerlach and colleagues (2019), we found that the different dishonesty paradigms result in different rates of dishonest behavior. However, in contrast to this report, we observed the lowest dishonesty rates for sender-receiver paradigms and the highest for matrix tasks. This difference might be reflective of the effect of social commitment on dishonesty compared to Gerlach et al. (2019) who focused on dishonesty in general and only one-shot paradigms, but not repeated possibilities. Similarly, the paradigms might represent different types of dishonesty. Whereas dishonest behavior in the die roll task is related to randomness of the actual response and dishonesty in the sender-receiver game reflects active deception, performance in the matrix task is related to effort (Gerlach et al., 2019). It is possible that dishonesty about the groups' effort when feeling committed to other individuals is more acceptable than deceiving others. Notably, we included only a few studies for the matrix paradigms, and the precision of the effect was low. Additionally, in the sender-receiver task, we did not explicitly consider the target to whom or to which people felt commitment. While in some studies people felt committed to another person with whom they decided which message to send, it is possible that in some studies people felt commitment to others they could potentially deceive, which would explain the reduced dishonesty rate.

Surprisingly, we also observed increased dishonesty for instances in which the experimenter or other third parties were able to check the actual performance, in contrast to situations in which participant's responses were truly private. This finding is inconsistent with previous meta-analyses and studies (Gerlach et al., 2019; Abeler et al., 2019; Köbis et al., 2019; Schild et al., 2019) and the theoretical idea that people want to appear as honest (Mazar et al., 2008; Abeler et al., 2019). One possible explanation could be that feeling committed to other individuals or acting as part of the group reduces the possibility of punishment or reputational damage, as the decision might be perceived as group based, and not as individual behavior. For the overall effects, we observed the effect in the expected direction - dishonesty increased if it was not possible to verify the actual response. However, this difference was particularly small, raising the question whether increased dishonesty in private might be influenced by additional moderating variables.

Finally, we found some indication that dishonesty increased with repeated possibilities of dishonest behavior, by throwing a die several times or completing several matrices. Importantly, this influence seems to stabilize at around 10-20 repetitions, not substantially increasing dishonesty with even more repetitions. The importance of this effect is likely small, since we found no significant difference between one-shot and repeated dishonesty designs for the overall effects.

Why would feeling committed to other individuals or groups increase dishonest behavior? We are able to test different theoretical propositions at least indirectly with the current meta-analysis. Some have argued that dishonesty in groups increases because of diffusion of responsibility (Behnk et al., 2019; Gross et al., 2018; Mazar & Aggarwal, 2011; O'Leary & Pangemanan, 2007; Wiltermuth, 2011) or the possible exposure to others' dishonest actions (the *bad apple* explanation; Gross & DeDreu, 2020). In both cases, it is expected that dishonesty would increase with the increasing number of agents one feels

committed to. In the current investigation, we observed that dishonest behavior increased overall when people were committed to *one other* agent. However, this effect did not increase further when people felt committed to two other agents. This observation fails to support both the diffusion of responsibility and the *bad apple* explanation. However, we only included a handful of studies investigating interactions or commitment of more than three individuals or agents. Therefore, we cannot make any definite claims about these specific theories. Future studies would need to more systematically vary the number of committed agents, going beyond the typical dyad as the level of analysis (see Gross & DeDreu, 2020 for recent advances). Another theoretical explanation highlights that the possibility of communication when interacting with other individuals plays a key-role in increased dishonest behavior (Gino et al., 2012; Kocher et al., 2018; Mazar et al., 2008). When looking at the overall effects, we did indeed find some support for this idea, observing that dishonesty was increased for studies in which communication was possible. However, this analysis also considered commitment to social norms via an oath or pledge, and communication played a less important role when trying to identify possible moderators of the effects of commitment to other individuals. Therefore, this finding should be interpreted with caution.

Commitment to Social Norms

In contrast to commitment to other individuals, commitment to social norms increased honesty overall. This effect was smaller than the effect of moral reminders from a previous meta-analysis that also focused on intentions and attitudes towards dishonesty (Belle & Cantarelli, 2017). We also observed high heterogeneity across effects, though not as strong as for feeling commitment to other individuals.

The number of repetitions in the dishonesty tasks represented the most important moderator. Honesty increased with an increasing number of trials. It is possible that a repeated design strengthens the effect of committing to the social norm of honesty, by

making the moral norm salient and providing repeated opportunities to act in line with one's previous commitment. Similarly, we observed some evidence that increasing the number of trials of a task increased dishonesty to some degree when people felt commitment to individuals. These findings have some interesting practical implications, suggesting that it is not only dishonesty that might work as a *slippery slope* leading people to adapt gradually to increasingly severe dishonest acts (Engelmann & Fehr, 2016; Garrett et al., 2016, though see Köbis et al., 2017), but that moral reminders might also gradually increase honest behavior when dishonesty is possible (see Ma et al., 2018).

Interestingly, we also found that communication moderated the effect of commitment to social norms on dishonesty. Being able to communicate with others about social norms strengthened honest behavior (see Zhang, 2008). This could be due to the fact that discussing or communicating an honesty norm makes it more salient or prescriptive (Hildreth et al., 2016). Importantly, we only found a small number of studies focusing on social commitment to norms and communication, and one effect that included no communication possibly influenced the overall effect as it was in the opposite direction.

Reflecting the publication bias estimates, we observed that publication status moderated the relationship between social commitment to norms and dishonesty. Effects were stronger for published studies, which could be interpreted as evidence that non-significant studies might have a harder time at entering the publication record. It is also possible that the unpublished studies reviewed in the current analysis differed on some specific design characteristics. It should be noted that only a handful of unpublished studies were included for this relationship, so we should be cautious in drawing any definite conclusions. Importantly, we also found evidence that absolute effect sizes and standard errors of effects have been decreasing for studies focusing on social commitment to norms, but to a much smaller degree for articles studying social commitment to individuals.

Finally, we found that the specific subregion moderated the relationship between social commitment to norms and dishonesty. As we only included a few studies outside of Northern America and Western Europe, we were only able to compare these two regions, observing that honesty was stronger for the former. This finding could be related to actual cultural differences in perceiving and processing specific oaths or pledges. For instance, as many of the reviewed studies focused on undergraduates, codes of conduct or specific ethical guidelines might be more common or salient at US than at European universities. As of now, there is limited evidence on cross-cultural variability of the effectiveness of oaths or pledges on honesty and future studies would be needed to study such effects more systematically.

Limitations

As in previous studies (Belle & Cantarelli, 2017; Gerlach et al., 2019), we observed some indication of publication bias for the main effects, depending on the specific type of estimation. Few of the reviewed effects were based on pre-registered studies or registered reports, and the actual effect of social commitment to other individuals or social norms was possibly overestimated. Evidence for possible publication bias was more pronounced for joint action/coordination and investment/effort tasks than for in-group tasks. Similarly, publication bias was generally smaller for studies focusing on commitment to social norms, but we identified publication status as a moderator for this factor, suggesting a possible overestimation. Future studies would need to employ pre-registration methods more extensively in order to provide a more valid estimation of actual effects, especially registered multi-lab projects (Kvarven et al., 2020). Our analysis revealed that over the years absolute reported effect sizes have been decreasing, while precision and sample size have been increasing at the same time. This provides some first evidence that recent developments related to replicability have changed the focus in the right direction.

It is also questionable how generalizable the reviewed effects are. As with most psychological studies (Henrich et al., 2010; Rad et al., 2018), the diversity of the reviewed samples was limited. By far the majority of participants were undergraduates (78.82%), mostly from countries in Northern America or Western Europe. Notably, more than half of all studies (55.31%) were conducted in just two countries - the US and Germany. These characteristics weaken the generalizability of our findings. For example, previous studies have provided some evidence that the relationship between commitment to individuals and dishonesty is stronger in more collective cultures (Mazar & Aggarwal, 2011). Similarly, we found some indication that the effectiveness of commitment to social norms via oath and pledges differed across countries, possibly reflecting some cultural norms. Some studies have explicitly considered differences across cultures (Akbari et al., 2020; Jiang et al., 2015; Jiang, 2014), but future research would need to investigate the effects of social commitment on dishonesty more systematically across different cultures.

Another obstacle to generalizability could be the employment of different dishonesty paradigms, which have been found to vary in their effects on dishonest behavior both in the current and other meta-analyses (Gerlach et al., 2019). Previous studies have shown that dishonesty in such economic games translates to dishonest behavior in the real-world showing a high external validity (List & Levitt, 2005; Cohn & Maréchal, 2017; Dai et al., 2018). However, specific mechanisms differ across various dishonesty paradigms (Gerlach et al., 2019), reflecting different types of dishonesty such as lying, deceptive behavior or bribery. Similarly, recent critics have raised the issue of demand effects in dishonesty paradigms, and suggested that specifically the matrix task might be ill-suited to study actual dishonest behavior (Heyman et al., 2020).

We hypothesized that one main mechanism of explaining differences in social commitment might be the degree of felt responsibility for one’s actions. In contexts in which

decisions can be attributed to other individuals or groups, personal responsibility might be lower, leading to increased dishonest behavior. By contrast, perceived responsibility might be stronger, and lead to increased honesty, when explicit commitment to norms of honesty occurs. We were not able to test this proposition directly in the present meta-analysis, as many studies did not measure or manipulate felt responsibility. Future studies are needed to systematically test this proposition, and determine whether this variable affects the diverging effects of social commitment on dishonest behavior.

Practical Implications

Commitment has been considered a *social glue* holding society together (Michael et al., 2016). As mentioned earlier, commitment has positive effects on different organizational related outcomes. Feeling more committed to one's organization (Cho & Park, 2011; Jaramillo et al., 2005; Mathieu & Zajac, 1990; Meyer et al., 2002; Riketta, 2002), as well as feeling more committed to work teams (Foote & Tang, 2008) can increase job satisfaction and performance.

In light of this, our finding that increased commitment can foster dishonest behavior has important practical implications and even suggests some possible interventions. Since work teams have several positive effects on organizational behavior (Hülsheger et al., 2009; LePine et al., 2008; D. Wang et al., 2014), abandoning team work in order to reduce dishonest or corruptive behavior seems unfeasible. Our review suggests that another form of social commitment, commitment to social norms via oaths or pledges, works in the opposite direction, and might counter the effects of corrupt collaboration. Previous research has highlighted the importance of code of ethics or oaths in the organizational context (Meine & Dunn, 2013; Svara, 2014). Emphasizing the commitment to honesty norms in work team settings might therefore be helpful in reducing dishonest behavior.

At least one recent small empirical study provides some pilot evidence for this proposition. Dunaeiv and Khadjavi (2021) found that having teams (and individuals) signing an honesty oath reduced overall cheating behavior in a matrix task. While this finding is based on a particularly small sample size, it provides preliminary evidence that combining different forms of social commitment might nullify possible dishonest behavior or even increase honesty. Future studies would need to test this proposition systematically across different designs, situations, cultures, and also conduct field experiments in order to assess its applicability.

Conclusion

Recent revelations about large-scale corrupt practices in organizations such as the Volkswagen emissions scandal have shown that too much commitment might be detrimental. In the present meta-analysis, we found that the effect of social commitment on dishonest behavior depends strongly on the target of commitment. Feeling committed to other individuals, organizations, or groups increased dishonest behavior, while feeling committed to social norms via oaths or pledges decreased dishonesty and fostered honest behavior. These effects were highly dependent on specific situational characteristics, and we observed small to medium evidence of a publication bias, suggesting that many of the effects are overestimated. A combination of both types of commitment might be important in achieving good teamwork while curtailing detrimental dishonest behavior.

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