

Manipulating belief in free will and its downstream consequences:

A meta-analysis

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Coding manual, data, and analysis scripts are open accessible at the Open Science Framework (OSF; <https://bit.ly/2L69prl>)

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Abstract

Whether free will exists is a longstanding philosophical debate. Cognitive neuroscience and popular media have been putting forward the idea that free will is an illusion, raising the question of what would happen if people stopped believing in free will altogether.

Psychological research has investigated this question by testing the consequences of experimentally weakening people's belief in free will. The results of these investigations have been mixed, with successful experiments and unsuccessful replications. This raises two fundamental questions that can best be investigated with a meta-analysis: First, can free will beliefs be manipulated and, second, do such manipulations have downstream consequences?

In a meta-analysis across 146 experiments (95 unpublished) with a total of 26,305 participants, we show that exposing individuals to anti-free will manipulations decreases belief in free will, $g = -0.29$, 95% CI = $[-0.35, -0.22]$, and increases belief in determinism, $g = 0.17$, 95% CI = $[0.09, 0.24]$. In contrast, we find little evidence for the idea that manipulating belief in free will has downstream consequences after accounting for small sample and publication bias. Together, our findings have important theoretical implications for research on free will beliefs and contribute to the discussion of whether reducing people's belief in free will has societal consequences.

Keywords: free will; determinism; belief; meta-analysis; morality; cheating; social behavior; punishment

Public Significance Statements:

This meta-analysis indicates that beliefs related to free will can be altered by experimental manipulations. However, these manipulations do not have downstream consequences.

From morality to politics, public policy, intimate relationships, and punishing behavior—“most of what is distinctly human about our life depends upon our viewing one another as autonomous persons, capable of free choice” (Harris, 2012, p. 1). Thus, unsurprisingly, most people believe that they have free will (Baumeister et al., 2009; Nahmias et al., 2005). Whether free will actually exists, however, is a longstanding philosophical debate (e.g., Dennett, 2015; Van Inwagen, 1983). This debate has reached an extremely high level of sophistication outlining different theoretical positions that span free will skepticism to complete libertarianism (for an overview see Dennett, 2015). However, these philosophical arguments rarely left academic circles and therefore had limited impact outside academia.

In the last decades, cognitive neuroscientists and psychologists entered the debate by claiming humans’ perception of free will is nothing more than an illusion (e.g., Crick, 1994; Harris, 2012; Wegner, 2002) just arising from unconscious brain activity (Hallett, 2007; Libet et al., 1983; Soon et al., 2008). A seminal study supporting this view is the study by Libet and colleagues (1983), who measured neural activity while participants made voluntary finger movements. After each movement, participants indicated on a clock the time at which they perceived their first urge to initiate a movement. Libet and colleagues found an increase in neural activity several hundred milliseconds before participants reported being aware of this urge. These and similar findings (Libet et al., 1983; Libet et al., 1993) have often been used as an argument for the claim that free will does not exist.

Despite criticisms of this argument (Brass et al., 2019; Saigle et al., 2018), anti-free will viewpoints have become in vogue not only in academia (e.g., Greene & Cohen, 2004), but also, and perhaps even more so, in popular media (e.g., Chivers 2010; Griffin, 2016; Wolfe, 1997). This raises fundamental questions of whether reading anti-free will viewpoints pushes people towards a deterministic world view and lowers their belief in free will. Moreover, the question arises what impact this has on society.

Some philosophers argue that undermining people's belief in free will saps the basis for moral behavior and would therefore have catastrophic consequences (e.g., Smilansky, 2000, 2002). In contrast, other philosophers argue that disbelief in free will might instead have positive effects, as it would cause people to abandon retribution-based morality and illusory beliefs in a just world (e.g., Caruso, 2014; Greene & Cohen, 2004; Nadelhoffer, 2011; Pereboom, 2006). Research in social and cognitive psychology as well as in neuroscience has tested these theories empirically by experimentally manipulating belief in free will. These studies provided evidence for the idea that free will beliefs indeed have societal consequences. There have also been a number of failures to replicate some of these findings, however. As a result, it remains unclear whether exposing individuals to anti-free will viewpoints has behavioral and societal consequences. In this article, we address this controversy by first reviewing the literature on belief in free will, its experimental manipulations, and its downstream consequences. Then, we test in a meta-analysis (1) whether beliefs related to free will can be manipulated and (2) whether these manipulations have downstream consequences.

Belief in Free Will and Determinism

Philosophical definitions regarding the concept of free will are rather diverse and complex (Carey & Paulhus, 2013). Because of this, researchers often assess laypeople's concepts of free will and to which degree laypeople believe in these concepts (e.g., Nichols, 2006). These analyses indicate that free will beliefs in laypeople are metacognitive judgments about the extent to which individuals intentionally guide their thoughts and actions (Frith, 2012). Specifically, belief in free will reflects the belief that people are responsible for their actions (Carey & Paulhus, 2013), because they can decide and control their own behavior (Paulhus & Carey, 2011).

Although conceptually related to constructs such as internal locus of control (Rotter, 1966) and self-efficacy (Bandura, 1977), there are important differences between these

constructs that set free will beliefs apart. Internal locus of control shares the emphasis on internal causal attribution, but represents a personality dimension, whereas free will belief assesses an attitudinal orientation (Waldman et al., 1983). Self-efficacy differs from belief in free will in that it reflects metacognitive judgments about one's specific skill or ability (e.g., "Can I execute this successfully?"). In contrast, belief in free will reflects a much broader belief about choice and freedom (e.g., "Do I have a choice? Can I freely choose to do otherwise?").

Related to the belief in free will is the belief in determinism. Philosophers typically define determinism as: given the past and the laws of nature, there is only one possible future at any moment in time (e.g., Van Inwagen, 1983). Whether free will and determinism are the endpoints of the same continuum or separate constructs is part of a debate in philosophy and psychology. For instance, incompatibilists see free will as the exact opposite of determinism. Such a view suggests that the more a person believes in free will, the less they believe in determinism (Rakos et al., 2008; Viney et al., 1982). In contrast, compatibilists see free will and determinism as independent constructs. Based on a compatibilistic view, people believe that if determinism were true, people could nevertheless be free. Past research has shown a positive correlation between belief in free will and belief in determinism (Wisniewski et al., 2019) and suggested that a compatibilistic view is more widespread in the general public than philosophers and psychologists may have traditionally assumed (e.g., Monroe & Malle, 2010a; Murray & Nahmias, 2014; Nadelhoffer et al., 2014; Nahmias et al., 2006; Nichols, 2004, 2006; Nichols & Knobe, 2007; Rose & Nichols, 2013; Shepard & Reuter, 2012; Shepherd, 2012).

Research on Manipulating Belief in Free Will

To test the consequences of (dis)believing in free will, researchers have developed various approaches to experimentally manipulate people's belief in free will. The first who did so were Vohs and Schooler (2008). In one experiment, participants either read a passage

from Francis Crick's (1994) book "the Astonishing Hypothesis", arguing against the plausibility of free will or a passage from the same book that did not mention free will. In another experiment, the authors followed a Velten-like technique (Velten, 1968), in which participants either read and pondered anti-free will or pro-free will statements. In both experiments, participants presented with anti-free will viewpoints reported lower beliefs in free will and were more willing to cheat on a test than control participants.

These findings inspired researchers all around the world to start investigating the consequences of experimentally reducing belief in free will (for a review, see Ewusi-Boisvert & Racine, 2018). Using a wide variety of different manipulations, this research suggests that weakening belief in free will increases anti-social behavior, such as prejudice (Zhao et al., 2014) or aggressiveness towards others (Baumeister et al., 2009), and decreases pro-social behavior, such as helping (Baumeister et al., 2009) or cooperation (Protzko et al., 2015). At the same time, however, exposing people to anti-free will viewpoints can also lead to reduced retributive punishment (Shariff et al., 2014).

In addition, anti-free will manipulations have been found to increase conformity (Alquist et al., 2013) and feelings of alienation (Seto & Hicks, 2016), and to decrease causal attributions of other people's actions (Genschow et al., 2017a), the perceived meaningfulness of life (Crescioni et al., 2016; Moynihan et al., 2019), perceived gratitude (MacKenzie et al., 2014), counterfactual thinking (Alquist et al., 2015), and risk taking behavior (Schrage et al., 2016). Finally, a last line of research suggests that experimentally reducing people's belief in free will may even influence fundamental neurocognitive processes such as intentional action preparation (Rigoni et al., 2011), deliberate motor inhibition (Lynn et al., 2013; Rigoni et al., 2012), and the processing of performance errors (Rigoni et al., 2015; Rigoni et al., 2013).

In sum, there is a large body of research suggesting that manipulating belief in free will affects societally relevant behaviors such as cheating (Vohs & Schooler, 2008), retributive punishment (Shariff & Vohs, 2014), and anti-social behavior (Baumeister et al.,

2009; Zhao et al., 2014), as well as basal neurocognitive mechanisms (Rigoni & Brass, 2014). As a result, it has been argued that care should be used in the manner in which anti-free will and deterministic viewpoints are presented to society, because it may change the way people interact with each other. For example, some scholars suggested that encountering anti-free will viewpoints in the popular press may “move judges and jurors toward being less punitive and less retributive in general” (p. 1569, Shariff et al., 2014) or “provide the ultimate excuse to behave as one likes” (p. 54, Vohs & Schooler, 2008).

Failed Replications

Despite the mounting evidence that manipulating belief in free will influences behavior, a number of studies have reported difficulties in replicating some key results (Crone & Levy, 2019; Eben et al., 2020; Genschow, Hawickhorst, et al., 2020; Giner-Sorolla et al., 2016; Monroe et al., 2017; Nadelhoffer et al., 2020; Open Science Collaboration, 2015; Schooler et al., 2014; Shariff & Vohs, 2014; Zwaan, 2014). For example, Monroe et al. (2017) found no effect of diminishing participants’ belief in free will on moral behavior, judgments of blame, or punishment decisions. Similarly, Nadelhoffer et al. (2020) found that manipulating free will beliefs in a robust way is more difficult than implied by previous work, and that the proposed link with immoral behavior, such as cheating for instance, might be similarly tenuous (for similar findings, see Crone & Levy, 2019; Giner-Sorolla et al., 2016; Zwaan, 2014).

Although these failed replications call into question the societal relevance of belief in free will, it is not yet clear what caused them. Before we can draw conclusions about the role of free will beliefs in society, it is imperative to understand why some findings failed to replicate as well as which mechanisms underlie free will belief manipulations. In principle, three explanations could account for the replication failures reported in the literature. First, it could be that the failed replications are false negatives. That is, they were not able to detect an effect that actually is real. Second, it might be that free will beliefs cannot be manipulated,

and that successful studies in the literature are therefore false positives. Third, it could be that manipulations of belief in free will successfully affect free will beliefs, but these manipulations are not causally related to other behaviors, and thus have no downstream consequences. In the present article, we put these explanations to the test by analyzing all available evidence in a meta-analysis.

Previous Reviews and Meta-Analyses

To the best of our knowledge, this is the first comprehensive meta-analysis on the effectiveness of manipulations related to free will beliefs. There are, however, two articles related to our analysis. First, Ewusi-Boisvert and Racine (2018) published a qualitative review of the literature on free will belief manipulations. Overall, the authors report a substantial amount of methodological diversity and a lack of replication studies in the published literature. Moreover, the review suggests that the studied samples are heavily constituted of women, students, and younger participants, and contain little information about the representation of ethnic minorities. This review does not allow accurately estimating the effect size of free will belief manipulations and its downstream consequences, however, as the researchers provided a descriptive review of the literature and did not include unpublished data.

Second, Genschow and colleagues (2017) tested the effectiveness of one specific manipulation (i.e., the Crick manipulation) in a mini meta-analyses involving 9 published and unpublished experiments that were conducted in their own research group ($N = 625$ participants). Overall, the authors found that participants who read the anti-free will text had a significantly lower belief in free will than participants who read the control text. However, this analysis does not allow drawing strong conclusions regarding the general effectiveness of free will belief manipulations, because only one manipulation type, a small number of experiments, one measure, and data from only one research team was investigated. In addition, this analysis did not explore potential reasons for why the manipulation sometimes

works and sometimes fails, nor did it investigate whether the manipulation has any downstream consequences.

The Present Meta-Analysis

The present meta-analysis aims to build on and considerably extend previous work by including both published and unpublished evidence and addressing two main research questions: (1) Can belief in free will be experimentally manipulated, and (2) does this have any downstream consequences?

Research Question 1: Can Belief in Free Will Be Manipulated?

In the first part of the meta-analysis, we investigate whether it is possible to experimentally manipulate beliefs related to free will and explore the conditions under which the manipulations are effective. To this end, we investigated different moderators.

Beliefs

The two most often used measures to test the effectiveness of free will belief manipulations are belief in free will scales and belief in determinism scales (Ewusi-Boisvert & Racine, 2018). It is generally assumed that anti-free will manipulations decrease free will beliefs and increase beliefs in determinism (for an overview, see Ewusi-Boisvert & Racine, 2018). This assumption, however, has never been systematically tested. In the present meta-analysis, we test whether the applied manipulations decrease the belief in free will, increase the belief in determinism, or both.

Scales

Several validated scales have been developed to measure belief in free will and determinism. The most often used scales are the Free Will and Determinism Scale (FWD; Rakos et al., 2008), the Free Will and Determinism Scale (FAD; Paulhus & Carey, 2011), and the Free Will Inventory (FWI; Nadelhoffer et al., 2014). In addition, researchers have sometimes also used self-made rating scales (e.g., Baumeister et al., 2009; Moynihan et al., 2019). Less often used scales measuring beliefs related to free will and determinism include

the Belief in Genetic Determinism scale (BGD; Keller, 2005), the Belief in Social Determinism scale (BSD; Rangel & Keller, 2011), and the Wif scale (Melcher, 2019).

However, while a wide range of scales has been used, it remains unknown whether some scales are more sensitive in picking up effects of the manipulation than others. To address this question, the present meta-analysis tests whether the effects of free will belief manipulations differ across scales. We focus on the FWI and the FAD, as these are the only two scales that tease apart belief in free will and belief in determinism. Moreover, they are also the two most commonly used scales and the only scales for which sufficient data is available to reliably compare them to each other.

In addition to the sensitivity of different scales, little is known about whether free will belief manipulations specifically affect beliefs in free will and determinism or also influence other, related beliefs. Therefore, we investigate if the influence of free will belief manipulations extends to other scales that are related to free will beliefs and are part of free will questionnaires, such as belief in dualism, fatalistic determinism, and unpredictability.

Type of Manipulation

Not only the scale but also the type of manipulation differs across studies. Specifically, four types of manipulations can be distinguished. First, some researchers let participants read a text—either a control text or a text arguing against the plausibility of free will. Often-used anti-free will texts include a passage of Francis Crick’s (1994) book “The Astonishing Hypothesis” (e.g., MacKenzie et al., 2014; Rigoni et al., 2011; Shariff et al., 2014; Vohs & Schooler, 2008), or articles featuring neuroscientific experiments (e.g., Harms et al., 2017; Protzko et al., 2016; Shariff et al., 2014), among others. Second, researchers presented participants with several statements using a Velten-like technique (Velten, 1968). That is, participants were either presented with a variety of anti-free will or control statements (e.g., Rigoni et al., 2012; Stillman et al., 2010; Vohs & Schooler, 2008). Third, other researchers used a combination of text and statements (e.g., Seto & Hicks, 2016). Fourth,

participants watched videos related to (anti-)free will viewpoints (e.g., Highhouse & Rada, 2015). To reliably manipulate belief in free will, it is important to know which of these manipulations are most effective. Therefore, we directly compare them in the present meta-analysis.

Participant Involvement

Whether the manipulation is effective may, however, not only depend on the type of manipulation but also on participants' involvement in the manipulation. While some researchers merely presented participants with anti-free will viewpoints (e.g., Baumeister et al., 2009; Goodyear et al., 2016; Rigoni et al., 2012; Shariff et al., 2014; Stillman et al., 2010), others engaged participants more strongly by letting them, for instance, summarize or rewrite the presented messages (Harms et al., 2017; Highhouse & Rada, 2015; Moynihan et al., 2019; Rigoni et al., 2011; Seto & Hicks, 2016; Vonasch et al., 2017). Yet other researchers told participants they had to attend the presented messages carefully as they had to summarize them at the end of the experiment (e.g., Genschow et al., 2017a). An interesting question is how deeply participants have to process the information provided for the manipulation to succeed. To answer this question, we investigate to which degree the effectiveness of the manipulation depends on whether participants have to engage with the task (e.g., by summarizing or rewriting presented messages) before free will beliefs are measured.

Baseline Condition

Another factor that may influence the manipulations' effectiveness is the baseline condition. Past research has shown that most people believe in free will (Baumeister et al., 2009; Nahmias et al., 2005). As a result, a tacit assumption in the literature is that beliefs in free will can only be diminished, but not increased. Hence, a substantial amount of research has focused on manipulations made to reduce free will. Yet, some experiments have also compared these anti-free will messages not only with neutral messages, but also with pro-free will messages (e.g., Baumeister et al., 2009; Clark et al., 2017; Highhouse & Rada, 2015;

Moynihan et al., 2019; Schrag et al., 2016; Seto & Hicks, 2016; Vohs & Schooler, 2008).

This allows us to test the effectiveness of anti-free will messages both with respect to control messages and with respect to pro-free will messages, allowing us to investigate whether belief in free will can also be experimentally increased, in addition to decreased.

Measurement Moment

A final moderator that may influence the effectiveness of the manipulation is the moment at which free will beliefs are measured. While some experiments measured participants' beliefs directly after the manipulation (Baumeister et al., 2009; Monroe et al., 2017; Seto & Hicks, 2016; Shariff et al., 2014; Vohs & Schooler, 2008; Vonasch et al., 2017), others measured them after participants performed a secondary task, often included to explore potential downstream consequences (Clark et al., 2017; Genschow et al., 2017a; Harms et al., 2017; Highhouse & Rada, 2015; Protzko et al., 2016; Rigoni et al., 2012; Schrag et al., 2016). Measuring beliefs at the end of the experiment assumes the manipulation has a relatively long-lasting effect. This may, however, not necessarily be the case. For instance, it could also be that free will belief manipulations do not fully change people's beliefs but rather prime an anti-free will mindset. Such priming effects may not be as long-lasting, and measuring free will beliefs at the end of the experiment may therefore make it more difficult to confirm that the manipulation worked. To test this hypothesis, we investigate if the effect of the manipulation differs depending on whether beliefs were measured immediately after the manipulation or at the end of the experiment (i.e., after completing another task).

Secondary Moderator Analyses

In addition to the above-mentioned moderators, we also consider the influence of participant age, gender, continent in which the study was conducted (US vs. Europe), test location (i.e., online vs. laboratory), and sample type (students vs. non-students) as secondary moderators.

Research Question 2: Do Free Will Belief Manipulations Have Downstream Consequences?

In the second part of the meta-analysis, we investigate whether experimentally reducing free will beliefs influences attitudes, behavior, and cognition. An interesting characteristic of the literature is that a large variety of dependent variables have been studied (for an overview, see Table 1). While this illustrates the breadth of the field, it also makes it difficult to quantify the evidence for downstream consequences, because for some dependent variables only one or a few experiments exist. In this respect, one solution could be to group the variables into broad categories such as “behavior” or “attitudes”. However, this would involve pooling together studies with vastly different dependent variables under the same denominator and would therefore run the risk of making the meta-analysis uninterpretable, a problem that is well-known as the “apples and oranges” critique of meta-analysis (Carpenter, 2020). To deal with this issue, we proceed in three steps.

In a first step, we run a *p*-curve analysis across all dependent variables. Whereas the aim of estimating a population effect size makes meta-analysis unsuited to evaluate diverse sets of dependent variables, this is not the case for *p*-curve. Rather than estimating a population effect size, *p*-curve investigates if a set of statistically significant findings contains evidential value by testing whether the distribution of *p*-values is consistent with the existence of an effect (Simonsohn et al., 2014). Importantly, if confirmed, this does not mean that all included studies have evidential value. Instead, it merely implies that at least some studies do (Simonsohn et al., 2014). As such, *p*-curve can be applied to diverse findings, as long as they form a meaningful whole (Simonsohn et al., 2015).

In a second step, we run meta-analyses on internally coherent sets of dependent variables. Upon reviewing the literature, one clear set arose—namely anti-social vs. pro-social behavior (for an overview, see Table 1). Hence, we pool together the studies in this set and subject them to a meta-analysis testing whether manipulating belief in free will influences

social behavior. However, pro- and anti-social behavior is still a relatively broad and unspecific dependent variable. Therefore, in a third and final step, we also run meta-analyses on three specific dependent variables that have been used in at least five experiments: conformity, punishment, and cheating.

Table 1

Overview Over All Published Experiments Included in the P-Curve Analysis and/or the Meta-Analyses Assessing the Influence of Free Will Belief Manipulations on Downstream Consequences

Experiment	DV	Included in anti-social meta-analysis	Included in <i>p</i> -curve analysis
Vohs & Schooler (2008); Exp. 1	cheating	yes	yes
Vohs & Schooler (2008); Exp. 2	cheating	yes	yes
Baumeister et al. (2009); Exp. 1	helping	yes	yes
Baumeister et al. (2009); Exp. 3	aggression	yes	yes
Stillman & Baumeister (2010); Exp. 1	learning	no	yes
Stillman & Baumeister (2010); Exp. 2	learning	no	yes
Rangel & Keller (2011); Exp. 6	in-group preference	no	yes
Rigoni et al. (2011); Exp. 1	preconscious motor preparation	no	yes
Alquist et al. (2013); Exp. 2	conformity	no	yes
Alquist et al. (2013); Exp. 3	conformity	no	yes
Evans (2013); Exp. 1	willingness to help	yes	yes
Rigoni et al. (2013); Exp. 1	action monitoring	no	yes
MacKenzie et al. (2014); Exp. 2	gratitude	no	yes
MacKenzie et al. (2014); Exp. 3	gratitude	no	yes
MacKenzie et al. (2014); Exp. 4	gratitude	no	yes
Shariff et al. (2014); Exp. 2	punishment	no	yes
Shariff et al. (2014); Exp. 3	punishment	no	yes
Zhao et al. (2014); Exp. 2	prejudice	yes	yes
Zhao et al. (2014); Exp. 3	prejudice	yes	yes
Open Science Collaboration (2015); Exp. 1	cheating	yes	no
Alquist et al. (2015); Exp. 1	counterfactual thinking	no	yes
Alquist et al. (2015); Exp. 2	counterfactual thinking and intention	no	yes
Plaks and Robinson (2015); Exp. 4	perception	no	yes
Plaks and Robinson (2015); Exp. 4	moral judgments	no	yes
Rigoni et al. (2015); Exp. 1	error detection	no	yes
Crescioni et al. (2016); Exp. 3	meaningfulness of life	no	yes
Crescioni et al. (2016); Exp. 4	meaningfulness of life	no	yes
Goodyear et al. (2016); Exp. 1	assigned moral responsibility	no	yes
Protzko et al. (2016); Exp. 1	cooperation	yes	yes
Schrag et al. (2016); Exp. 1	risk behavior	no	yes
Seto and Hicks (2016); Exp. 1	self-alienation	no	yes
Seto and Hicks (2016); Exp. 2	reported authenticity	no	yes
Caspar et al. (2017); Exp. 1	immoral behavior	yes	yes
Clark et al. (2017); Exp. 4	punitive distress	no	yes
Genschow et al. (2017); Exp. 3a	correspondence bias	no	yes
Genschow et al. (2017); Exp. 3b	correspondence bias	no	yes
Hannikainen et al. (2017); Exp. 4	preference for autonomy vs. welfare	no	yes
Hannikainen et al. (2017); Exp. 5	approval of paternalistic policies	no	yes
Monroe et al. (2017); Exp. 1	punishment, cheating, moral judgments	yes	no
Monroe et al. (2017); Exp. 3	punishment and moral judgments	no	no
Vonasch et al. (2017); Exp. 6	addiction and self-control	no	yes
Ching & Xu (2018); Exp. 1	prejudice	yes	yes
Moynihan et al. (2019); Exp. 1	meaninglessness	no	yes
Nadelhoffer et al. (2019); Exp. 3	cheating	yes	no
Nadelhoffer et al. (2019); Exp. 4	cheating	yes	no
Genschow & Vehlow (2019); Exp. 1	victim blaming	yes	yes

Note. Nadelhoffer et al. (2019) and Genschow & Vehlow (2019) were included in the meta-analysis as unpublished studies but were published later as Nadelhoffer et al. (2020) and Genschow and Vehlow (2021) after data-analysis had already been completed. Experiments were included in the *p*-curve analysis if the effect of the manipulation on the DV was significant. Articles were included in the meta-analyses on anti-social behavior, conformity, punishment, and cheating irrespective of whether the effect was significant or not.

Method

Search Strategy

The literature search for published articles was initiated in July 2018 and includes studies published between January, 2008 and July 2019, based on the publication date of the first experiment that experimentally manipulated belief in free will (Vohs & Schooler, 2008). To collect published studies, we entered the following search terms in Web of Science, PubMed, and PsycINFO: (“Free will” AND “belie*”) OR (“Free will” AND “manipulat*”) OR (“Free will” AND “experiment*”) OR (“Free will” AND “group”) OR (“Free will” AND “induc*”) OR (“Free will” AND “reduc*”) OR (“Free will” AND “threat*”) OR (“Free will” AND “undermin*”) OR (“Free will” AND “weak*”) OR (“Determinis*” AND “belie*”) OR (“Determinis*” AND “manipulat*”) OR (“Determinis*” AND “experiment*”) OR (“Determinis*” AND “group”) OR (“Determinis*” AND “induc*”) OR (“Determinis*” AND “encourag*”) OR (“Determinis*” AND “increas*”) OR (“Determinis*” AND “enhanc*”).

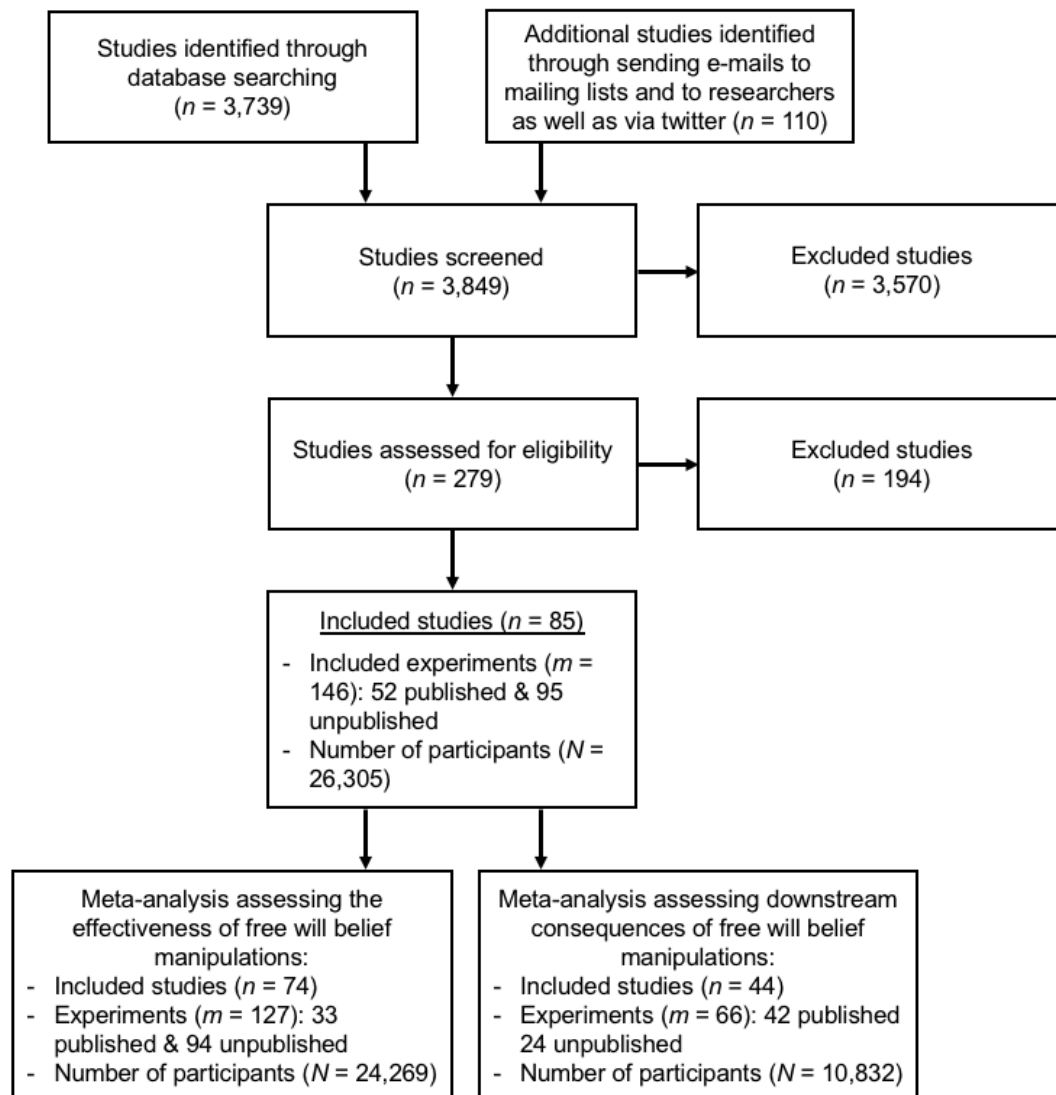
In addition to this Boolean search, we also looked for studies that cited the Vohs and Schooler (2008) paper. Furthermore, we included studies that were cited in the recent review paper on belief in free will by Ewusi-Boisvert and Racine (2018). Third, unpublished data were collected by sending requests to authors who had previously published articles using free will belief manipulations. Fourth, we sent around requests for unpublished data via different national and international mailing lists including the mailing list of the German Psychology Association (DGPs), the mailing list of the Belgian Association for Psychological Science (BAPS), and the mailing list of the European Society for Cognitive and Affective Neuroscience (ESCAN). Finally, we asked for unpublished data via Twitter and different online discussion fora of the Society of Personality and Social Psychology (SPSP). The search for unpublished data was terminated on August 15, 2019. The above search strategy resulted in 3849 unduplicated results.

Screening Process

We screened titles and abstracts from 3739 records obtained from the literature search. In addition, 110 unpublished experiments were included. Studies that were clearly not eligible based on the criteria described below were excluded (i.e., $n = 3570$). The remaining studies were then evaluated in more detail by screening the full-text articles. 279 full-text articles were assessed for eligibility. This procedure resulted in a database of 85 eligible studies containing 145 experiments (50 published; 95 unpublished) involving 26,112 participants. The criteria used to screen the search results are described below. The numbers of excluded and included studies can be seen in the PRISMA flow chart in Figure 1.

1. The studied population had to involve healthy adults.
2. The study had to include an experimental manipulation with the aim of reducing or increasing belief in free will or belief in determinism.
3. For the research question investigating the influence of free will belief manipulations on free will beliefs, but not for the research question investigating downstream consequences, the study had to contain a manipulation check administered after the manipulation—that is a measure of belief in free will, determinism, or both. We focused on belief in free will and belief in determinism because these are the two most commonly measured beliefs. Importantly, we only included measures that considered free will and determinism as separate constructs and not measures that considered them as two opposite endpoints of the same scale because the latter approach does not allow us to distinguish between both beliefs¹.
4. Sufficient statistical information had to be available to calculate the necessary effect sizes. The required information had to be either reported in the paper or obtained from the authors by e-mail.

¹ Based on this approach we had to exclude only two published and two unpublished experiments.

Figure 1*Flowchart of the Screening Process*

Note. The number of included or excluded studies (n), experiments (m), and participants (N) are denoted. See text for additional detail.

Coding and Reliability

Research Question 1: Can Belief in Free Will be Manipulated?

The eligible studies were first coded by the first author. To evaluate the reliability of the coding, the third author was trained to use the coding manual and subsequently recoded 17 randomly selected published studies containing 27 experiments in total. To further identify errors, the initial coding was also checked by the third author. Disagreements and inconsistencies that arose during coding were resolved by the first three authors via

discussion. Coding reliability was assessed with intraclass correlation coefficients (ICC) for continuous variables and with kappa coefficients for categorical variables. The reliability for continuous variables was high (all $ICC \geq 0.99$). Likewise, the average reliability for categorical variables was high ($\kappa = 0.96$) and varied from 0.74 to 1.00. The coding manual is open accessible at the Open Science Framework (OSF; <https://bit.ly/2L69prl>). The following variables were coded:

Publication Status. We coded whether an experiment was published ($m = 51$) or unpublished ($m = 95$). An experiment was considered published when it appeared in an academic journal or book. Dissertations, preprints (if not published elsewhere), conference posters, and raw data were all considered unpublished. Experiments that were initially unpublished but then later published before the meta-analysis was completed remained coded as unpublished experiments to account for potential differences between the original and published results (e.g., additional data collection, different exclusion criteria, different analytical choices).

Beliefs. We coded whether belief in free will or belief in determinism was measured. Effect sizes obtained with instruments measuring belief in free will and belief in determinism as opposite ends of a scale (e.g., the FWD) were not included. Effect sizes were coded as measuring belief in free will when they were obtained with the free will subscales of the FWI, the FAD-Plus, or the FAD. In addition, we also included self-made scales and individual items designed to measure belief or disbelief in free will. The disbelief scales were reverse coded.

Effect sizes were coded as measuring belief in determinism when they were obtained with the following scales: belief in determinism of the FWI, biological determinism of the WiF, genetic determinism of the BGD, social determinism of the BSD, and scientific determinism of the FAD-Plus or the FAD scale. In addition, as for free will beliefs, we also included self-made scales and individual items designed to measure either belief or disbelief

in determinism. The disbelief items were reverse coded. We did not include the FAD/FAD-Plus' fatalistic determinism subscale, as it does not capture determinism as construed by the other scales (i.e., the belief that future events are completely determined by prior events) but rather measures belief in 'destiny' or 'fate'.

Scales. To compare the sensitivity of different scales, we coded which of the following scales was used to measure belief in free will and/or determinism: the FAD/FAD-Plus, the WiF, the BGD, the BSD, or self-made scales. The coding indicated that not enough experiments (i.e., $m < 3$) had used the WiF, BGD, and BSD to draw reliable conclusions. Moreover, as the self-made scales did not form an internally coherent cluster, we restricted the scales analysis to the FWI and FAD/FAD-Plus.

For the FWI and FAD/FAD-Plus, we also coded not only the free will and determinism subscales but also all other subscales to investigate whether the influence of free will belief manipulations is specific to belief in free will and belief in determinism or also extends to other, related beliefs. More specifically, we coded the dualism subscale of the FWI and the fatalistic determinism and unpredictability subscales of the FAD/FAD-Plus.

Type of Manipulation. To compare different types of manipulations, we coded whether a text, statements, combination of text and statements, or a video was used to manipulate belief in free will.

Participant Involvement. For some studies, participants were asked to attend the provided information carefully as they had to summarize it at the end of the experiment. In other studies, participants had to process the provided information directly after the manipulation by writing an essay or rewriting attended messages. Yet in other studies, participants merely attended the provided information and did not have to reproduce any information. For participant involvement, we coded whether participants (1) had to summarize or rewrite the attended information directly after the manipulation, (2) had to summarize or rewrite the information at the end of the experiment, or (3) simply had to attend

the provided information, such as by merely reading a text or watching a video. If participant involvement plays a crucial role, one would expect the manipulation to have the strongest effects when participants have to reprocess the information directly after the manipulation.

Baseline Condition. To test whether beliefs in free will and determinism can also be increased, in addition to decreased, we coded whether the baseline condition consisted of neutral messages or pro-free will messages.

Measurement Moment. To investigate how long-lasting the free will belief manipulations are, we coded whether belief in free will or determinism was assessed directly after the manipulation or after the primary dependent variable was assessed.

Secondary Moderators. We also coded several secondary moderators, namely age, gender, continent, test location, and sample type. Age was coded as the mean age of participants in the sample. Gender was coded as the proportion of female participants. Continent refers to whether the sample was collected in the US or on US-based platforms such as MTurk versus in Europe or on Europe-based platforms such as Prolific (not enough experiments were conducted on other continents to draw reliable conclusions). Test location refers to whether the experiment was conducted in the laboratory or online. Finally, sample type refers to whether the participants were students or panel participants (e.g., MTurk or Prolific).

Research Question 2: Do Free Will Belief Manipulations Have Downstream Consequences?

In a first step, the first author coded all studies measuring downstream consequences of manipulating belief in free will. The following variables were coded: the dependent variable, whether the study included a manipulation check, whether the manipulation check was significant, and whether the experiment was published. A manipulation check was considered significant when at least one of the measured beliefs (i.e., free will or determinism) reached a p -value of $p < .05$. In a second step, the second author checked the

first author's coding for errors. Disagreements and inconsistencies that arose during coding were resolved through discussion between both authors. The pooling of dependent variables into the clusters described below was done through mutual discussion.

P-Curve. Rather than estimating a population effect size, *p*-curve investigates if a set of *p*-values from statistically significant findings follows a flat or skewed distribution (Simonsohn et al., 2014). The *p*-curve analysis assessing the overall evidence for downstream consequences of free will manipulations included 35 studies with a total of 49 experiments (see OSF; <https://bit.ly/2L69prl>). 39 of these experiments were published in academic journals or books. The remaining studies were unpublished datasets or theses. 31 of the included experiments were also included in the meta-analysis investigating the influence of free will belief manipulations on free will beliefs. The remaining experiments ($m = 18$) were not included in the meta-analysis on free will beliefs because they did not include a manipulation check (i.e., a measure of free will or determinism belief). *P*-values to use in the *p*-curve analysis were coded according to the guidelines laid out in Simonsohn et al. (2014). If a study reported multiple relevant tests, we coded only the first test. We also report robustness analyses checking whether using the second instead of the first test changes the results.

Anti-Social Behavior Meta-Analysis. Upon reviewing the literature on downstream consequences, one clear set of variables arose—namely anti-social vs. pro-social behavior. Hence, we pooled together the studies in this set and subjected them to a meta-analysis testing whether manipulating belief in free will influences social behavior. Fifteen studies ($m = 21$) investigated the influence of anti-free will manipulations on pro- or anti-social behavior or attitudes. The pro-social dependent variables were helping and positive attitudes towards minority. The anti-social dependent variables were immoral behavior, cheating, prejudice, malicious envy, and victim blaming. Studies measuring pro-social behavior or pro-social attitudes were reverse coded.

Conformity, Punishment, and Cheating Meta-Analyses. Three dependent variables were measured in at least five experiments: cheating ($n = 5$; $m = 8$), conformity ($n = 4$, $m = 6$), and punishment ($n = 3$; $m = 9$).

Meta-Analytic Procedures

Robust Variance Estimation

Analyses were performed in R (v3.5.1) using the metafor (Viechtbauer, 2010) and robumeta (Fisher & Tipton, 2015) packages. The data was analyzed using random effects models because we considered heterogeneity to be likely and because random effects models converge on fixed effects models in the absence of heterogeneity (Field & Gillett, 2010). A critical assumption of such random effects models is that the included effect sizes are statistically independent (Lipsey & Wilson, 2001) and violating this assumption is known to inflate the false-positive rate (Tanner-Smith et al., 2016). In the current meta-analysis, however, many studies measured multiple dependent variables or included multiple baseline conditions and therefore yielded more than one relevant effect size. As a result, the included effect sizes were not independent. To control for this dependency, we decided to empirically estimate standard errors that do not assume independent effect sizes using robust variance estimation (RVE; Fisher & Tipton, 2015; Hedges et al., 2010; Tanner-Smith et al., 2016; Tanner-Smith & Tipton, 2014).

In meta-analyses, effect sizes are typically weighted by their standard error. Within the RVE framework, two ways to calculate such weights have been proposed: hierarchical effects weights and correlated effects weights. The former is most appropriate when dependency originates mainly from effect sizes being nested in larger units (e.g., research groups), whereas the latter is more appropriate when dependency originates mostly from a single study providing multiple effect sizes. While RVE provides asymptotically unbiased standard errors regardless of how the weights are calculated, the choice of weights does influence statistical efficiency (Hedges et al., 2010). As we expected dependency to arise mainly from studies

providing multiple effect sizes, we used correlated effects weights (Fisher & Tipton, 2015). These weights are inverse variance weights, where the denominator of the weight assigned to each effect size in an experiment is determined by the average variance in the experiment multiplied by the number of effect sizes provided by that experiment. Importantly, this ensures that the total weight of each experiment does not depend on the number of effect sizes it provides (e.g., Cracco et al., 2018). Of note, to estimate variability, a parameter ρ representing the correlation among the effect sizes has to be specified (Fisher & Tipton, 2015). This parameter is assumed to be the same for all experiments and typically has a negligible influence on the results. In the current meta-analysis, we used the default value of the *robumeta* package (i.e., $\rho = 0.80$).

Importantly, one problem with RVE is that it has an inflated false positive rate when the number of studies is moderate to small or when skewed or unbalanced moderators are included (Tipton, 2015; Tipton & Pustejovsky, 2015). As a solution, small-sample corrections (i.e., small number of studies within a meta-analysis) have been proposed for both *t* tests (Tipton, 2015) and *F* tests (Tipton & Pustejovsky, 2015). As it is difficult to know when these corrections should be implemented and when not, it has been recommended to implement them for all RVE analyses, regardless of the meta-analytical sample size (Tipton, 2015). In the current meta-analysis, we follow this recommendation. Importantly, corrections to the *t* test (but not the *F* test) are only valid when $df \geq 4$ (Tipton, 2015). As a result, we only report *t* tests with more than 4 degrees of freedom. In addition, for continuous moderators (e.g., age), we removed outlier values exceeding the weighted mean by more than 3 *SDs* because such outliers strongly reduce the available degrees of freedom and hence statistical power (Tanner-Smith et al., 2016). Weighted means were calculated by dividing a weight of 1 equally among the different values provided by the same experiment.

Effect Size Estimation

Hedge's g was used as a measure of effect size. Effects were coded so that positive effect sizes corresponded to stronger beliefs in free will/determinism (research question 1) and larger values on the outcome measures (research question 2) in the anti-free will condition than in the reference condition. Hedge's g was calculated by first calculating Cohen's d and then correcting these values using the *escalc* function of the metafor package in R (Viechtbauer, 2010). When means and standard deviations were reported, Cohen's d was calculated as:

$$d_s = \frac{M_{anti} - M_{ref}}{\sqrt{\frac{(n_{anti}-1)SD_{anti}^2 + (n_{ref}-1)SD_{ref}^2}{n_{anti} + n_{ref} - 2}}} \quad (1)$$

With *anti* referring to the anti-free will condition and *ref* referring to the reference (i.e., baseline) condition. When standard errors were provided instead of standard deviations, these were transformed to standard deviations and Cohen's d was calculated using (1). When insufficient information was reported to use (1), we instead calculated Cohen's d from the t or F -value as:

$$d_s = t \sqrt{\frac{1}{n_{anti}} + \frac{1}{n_{ref}}} \text{ or } \sqrt{F} \sqrt{\frac{1}{n_{anti}} + \frac{1}{n_{ref}}} \quad (2)$$

When we could not retrieve sufficient information to calculate an effect size, we contacted the corresponding author for the necessary information. Nevertheless, despite our efforts, we could not calculate the effect size for all coded studies. Specifically, for the first research question, we could not calculate any of the effect sizes for 4 experiments from 3 studies and only part of the effect sizes for 3 experiments from 3 studies. These effects were therefore not

included in the meta-analysis. For the second research question, we were not able to code 5 experiments from 5 studies.

Outliers

Outliers were defined as effect sizes exceeding the weighted mean effect size, calculated as before, by more than 3 *SDs*. For the first research question, this procedure identified 1 outlier for free will beliefs ($g = -2.89$) and 1 outlier for determinism beliefs ($g = 4.00$). These outliers were replaced by the effect size 3 *SDs* above the weighted mean effect size for free will beliefs ($g = -1.64$) and determinism beliefs ($g = 2.09$). There were no outliers for the second research question. In addition to replacing outliers, we also tested the influence of each individual effect size on the average effect size using a leave-one-out cross-validation procedure. This indicated that leaving out individual effect sizes did not strongly influence the average effect size and mostly influenced it similarly in both directions for both the first (free will beliefs: $\Delta g_{\min} = -0.01$, $\Delta g_{\max} = 0.01$; determinism beliefs: $\Delta g_{\min} = -0.02$, $\Delta g_{\max} = 0.01$) and the second research question (antisocial behavior: $\Delta g_{\min} = -0.03$, $\Delta g_{\max} = 0.04$; cheating: $\Delta g_{\min} = -0.11$, $\Delta g_{\max} = 0.12$; conformity: $\Delta g_{\min} = -0.04$, $\Delta g_{\max} = 0.11$; punishment: $\Delta g_{\min} = -0.05$, $\Delta g_{\max} = 0.05$).

Moderator Correlations

To control for confounded moderators (Field & Gillett, 2010; Lipsey & Wilson, 2001), we computed the weighted associations, calculated as before, between the different moderators included in the analysis of the first research question with r for continuous–continuous and continuous–dichotomous pairs, with multiple R for continuous–polytomous pairs, and with Cramér’s V for dichotomous–dichotomous, dichotomous–polytomous, and polytomous–polytomous pairs (Cracco et al., 2018). For all these measures, 0 means no relationship and 1 means a perfect relationship. When two moderators correlated > 0.50 , we tested whether the moderator effects (if any) remained after controlling for the confounded moderator. When the contingency table of two confounded moderators did not contain empty

cells, we controlled for their confounding influences by including both moderators into the same meta-regression model. Using sum coding, this allowed us to test the average effect of each moderator across the levels of the other moderator (for categorical moderators). When the contingency table did contain empty cells, we controlled for confounding by restricting the analysis of moderator A to the level of moderator B where the levels of moderator A were most balanced, to maximize statistical power (Tipton, 2015; Tipton & Pustejovsky, 2015).

Table 2*Weighted Correlations Among Moderator Variables*

	Published	Scale	Type	Involvement	Moment	Baseline	Age	Sex	Continent	Location	Sample
Published	1.00	0.43	0.32	0.25	0.06	0.00	0.16	0.16	0.25	0.08	0.03
Scale	0.28*	1.00	0.40	0.31	0.52*	0.15	0.27	0.38	0.29	0.28	0.26
Type	0.20	0.36***	1.00	0.59***	0.55**	0.75***	0.23	0.27	0.51***	0.10	0.27
Involvement	0.19	0.37***	0.42***	1.00	0.39*	0.69***	0.14	0.25	0.50**	0.21	0.36*
Moment	0.08	0.44***	0.43***	0.44***	1.00	0.01	0.11	0.04	0.38**	0.06	0.05
Baseline	0.13	0.25	0.66***	0.59***	0.19	1.00	0.05	0.20	0.24	0.07	0.09
Age	0.04	0.30	0.34*	0.10	0.17	0.01	1.00	0.42**	0.25	0.84***	0.75***
Sex	0.09	0.18	0.39**	0.23	0.10	0.08	0.55***	1.00	0.36**	0.43**	0.66***
Continent	0.27**	0.21	0.29*	0.26*	0.01	0.10	0.24*	0.43***	1.00	0.42**	0.69***
Location	0.02	0.18	0.18	0.18	0.08	0.06	0.82***	0.44***	0.41***	1.00	0.86***
Sample	0.15	0.23	0.33*	0.46***	0.06	0.12	0.81***	0.63***	0.50***	0.72***	1.00

Note. The lower half of the matrix shows correlations for free will beliefs and the upper half of the matrix shows correlations for determinism beliefs. Relations between continuous–continuous variables and between continuous–dichotomous variables were assessed with Pearson’s r . Relations between continuous–polytomous variables were assessed with multiple R . Relations between dichotomous–dichotomous, dichotomous–polytomous, and polytomous–polytomous variables were assessed with Cramér’s V . Correlations > 0.50 are indicated in bold font. These correlations were controlled for whenever one of the two involved moderators produced a significant effect. * $p < .05$, ** $p < .01$, *** $p < .001$.

Publication Bias

For both research questions, we tested for publication bias and other small study effects using a hierarchical Egger's regression test (Sterne & Egger, 2005). More specifically, we ran an RVE meta-regression predicting effect sizes from their standard error (Rodgers & Pustejovsky, 2020). Publication bias leads to a positive relationship between effect sizes and standard errors because only large effect sizes are statistically significant in studies with a large standard error (small N). When Egger's regression test yielded a significant result, we followed up on this test with a PET-PEESE analysis (Stanley & Doucouliagos, 2014). That is, we took the intercept of two RVE meta-regressions respectively regressing effect sizes onto their standard error (PET) and variance (PEESE) as an estimate of the bias-corrected effect size. PET and PEESE have complementary strengths and weaknesses: whereas PET tends to be overly conservative when there is a true effect, PEESE tends to be overly liberal when there is no true effect (Stanley & Doucouliagos, 2014). Therefore, we report both measures, as has recently been recommended (Carter et al., 2019). Importantly, in some cases, PET and PEESE overcorrect effect sizes in the opposite direction. As these overcorrections are not meaningful (Carter et al., 2019), we report the corrected effect size as 0 when this occurs. Finally, in addition to using bias correction methods, we also compare published with unpublished studies and test if the effects remain significant if only unpublished studies are considered.

Sensitivity Analyses

To assess the robustness of our main results to different analytical choices, we ran several sensitivity analyses for the publication bias analyses and for the analyses testing the main effect of the manipulation. Specifically, we conducted three sensitivity analyses. First, we repeated the analyses with different values for the ρ parameter of the RVE models by varying this parameter from 0 to 1 in increments of 0.2. Second, we repeated the analyses after computing effect sizes preferentially from the reported statistics instead of computing

them preferentially from the means and standard deviations. That is, sometimes studies report both test statistics and descriptive information. Although this information should, in principle, lead to the same effect size, this is not always the case. Therefore, we decided to test the robustness of our results to changes in the information used to calculate effect sizes. Finally, we repeated the analyses with a standard meta-analytical approach instead of RVE. While RVE has already been well-validated (Fisher & Tipton, 2015; Hedges et al., 2010; Tanner-Smith et al., 2016; Tanner-Smith & Tipton, 2014), it is still a relatively new technique. Therefore, we investigated whether similar results are obtained if a conventional random effects model was used (Viechtbauer, 2010). Note that for this analysis, we averaged all effect sizes coming from the same experiment to ensure statistical independence (Lipsey & Wilson, 2001).

Results

Research Question 1: Can the Belief in Free Will Be Manipulated?

Effect on Belief in Free Will and Belief in Determinism

We first tested the overall effectiveness of the manipulation for both free will and determinism beliefs. This indicated that exposing participants to anti-free will manipulations decreased belief in free will ($g = -0.29$, $t(110) = -8.72$, $p < .001$, 95% CI = $[-0.35, -0.22]$, $m = 118$, $k = 148$), and increased belief in determinism ($g = 0.17$, $t(46.5) = 4.33$, $p < .001$, 95% CI = $[0.09, 0.24]$, $m = 53$, $k = 61$). Next, we compared the influence of the manipulation on these two beliefs by reverse-coding the determinism belief effect sizes and comparing them with the free will belief effect sizes. This revealed a borderline non-significant difference, with the manipulation having a slightly stronger effect on free will beliefs than on determinism beliefs ($t(66.1) = -1.89$, $p = .063$, $m = 123$, $k = 209$). Finally, we looked at the I^2 coefficients, which

indicated that a substantial portion of the variance for both belief in free will ($I^2 = 79\%$), and belief in determinism ($I^2 = 67\%$), was due to heterogeneity in the effect sizes².

In sum, these analyses indicate that anti-free will manipulations reduce participants' belief in free will and increase their belief in determinism.

Publication Bias and Small-Study Effects

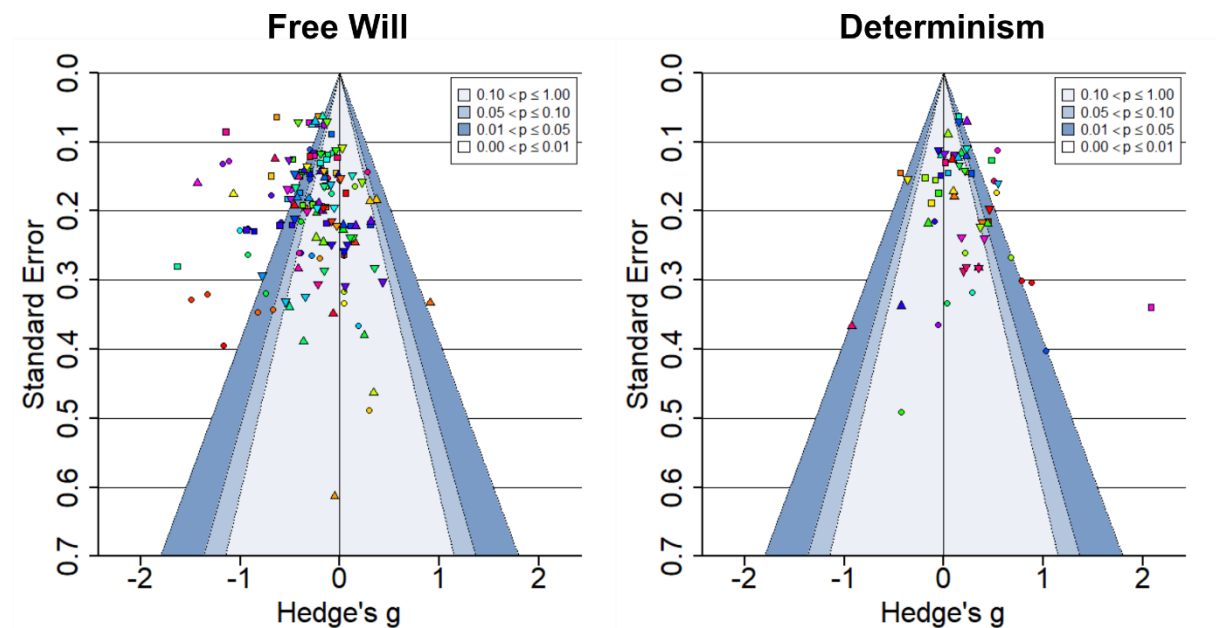
A visual inspection of the funnel plots (see Figure 2) revealed a largely symmetrical effect size distribution for both free will and determinism beliefs. In line with this visual inspection, a hierarchical Egger test found no relationship between the standard error and the effect size for either free will ($t(41.1) = 0.60, p = .553$), or determinism beliefs ($t(19.4) = 0.76, p = .457$). Importantly, however, this does not necessarily mean that there is no publication bias in the literature. Indeed, our analysis included a large number of unpublished studies both for the free will ($m = 92, k = 116$) and for the determinism analysis ($m = 39, k = 44$), potentially masking the presence of publication bias. In support of this hypothesis, an analysis including publication status as a moderator showed that the effect of the manipulation was larger in published than in unpublished studies for both belief in free will ($t(37) = -3.42, p = .002$; see Table 3), and belief in determinism ($t(18.9) = 2.14, p = 0.046$; see Table 4). Importantly, however, the effect size of both free will beliefs ($t(84.9) = -6.58, p < .001$), and determinism beliefs ($t(34.6) = 2.73, p = .010$), remained significant even when only unpublished studies were included in the analysis.

Together, these analyses indicate that there is evidence for publication bias, but this cannot explain the effect of the manipulation on free will or determinism beliefs.

² Note that RVE uses a simplistic method to estimate I^2 and that this should therefore be seen as a rough indicator of heterogeneity rather than as a precise estimate (Tanner-Smith et al., 2016).

Figure 2

Funnel Plots for Belief in Free Will (Left) and Belief in Determinism (Right)



Note. Effect sizes from the same study are plotted in the same color with the same symbol.

Primary Moderators

Scales. We first investigated whether the effectiveness of the manipulation depended on whether free-will and determinism were measured using the FWI scale or using the FAD scale. This revealed no significant difference between both scales for free-will beliefs ($t(74.9) = 1.53, p = 0.130$; see Table 3), but a stronger effect when belief in determinism was measured with the FAD than with the FWI ($t(21.8) = 2.29, p = 0.031$; see Table 4). However, scale correlated highly with measurement moment for determinism beliefs (see Table 2). In particular, studies that used the FWI tended to measure beliefs at the end of the experiment rather than right after the manipulation, whereas studies using the FAD were more balanced with respect to measurement moment. To control for this confound, we fitted a model including both scale and measurement moment. This revealed that the difference between the FWI and FAD for determinism beliefs was no longer significant ($t(15.8) = 1.43, p = .172$),

when measurement moment was controlled, even though the numerical pattern went in the same direction as before.

Next, we investigated whether free will manipulations also influenced the other subscales of the FWI and FAD. The FWI analysis indicated that the manipulation influenced not only belief in free will ($t(25.9) = -3.73, p < 0.001$), and belief in determinism ($t(26.9) = 2.27, p = 0.031$), but also belief in dualism ($t(25.8) = -6.56, p < 0.001$). This indicates that belief in free will and belief in dualism decreased, whereas belief in determinism increased following anti-free will manipulations. A direct comparison of the size of these three effects revealed a significant main effect of subscale ($F(2, 31.6) = 4.43, p = .020$), with significantly larger effect sizes for dualism than for determinism ($t(27.7) = 2.87, p = .008$), but no significant difference between free will and determinism ($t(34.4) = 1.86, p = 0.072$), or between free will and dualism ($t(33.9) = 0.57, p = 0.571$; see Table 5).

The FAD analysis likewise showed that the manipulation changed not only belief in free will ($t(33) = -2.88, p = 0.007$), and belief in scientific determinism ($t(7.8) = 5.26, p < 0.001$), but also belief in fatalistic determinism ($t(5.3) = 3.10, p = 0.025$). More specifically, belief in free will decreased and belief in scientific and fatalistic determinism increased following anti-free will manipulations. No effect was found on belief in unpredictability ($t(8.3) = 0.67, p = 0.522$). Comparing the effectiveness of the manipulation across subscales again revealed a significant main effect ($F(3, 9.8) = 6.51, p = .011$). Post-hoc tests showed that the effect on scientific determinism was stronger than on all other subscales (all $ps \leq .007$), and that the effect on unpredictability was significantly weaker than the effect on the other subscales (all $ps \leq .045$; see Table 5).

In sum, the analysis comparing the different scales indicated that the manipulations had similarly strong effects irrespective of whether belief in free will and belief in determinism were measured with the FWI or with the FAD. Interestingly, the results also showed that the effects of the manipulation were not specific to belief in free will and belief

in scientific determinism, but also extended to belief in dualism and belief in fatalistic determinism, albeit not to belief in unpredictability.

Type of Manipulation. We first compared the effectiveness of the different manipulations (i.e. text, statements, text and statements, or video). This revealed a significant effect of manipulation type on free will beliefs ($F(3, 26.3) = 6.70, p = .002$; see Table 3), but not on determinism beliefs ($F(2, 15.7) = 1.83, p = .193$; see Table 4).

A further analysis of the free will belief effect showed that manipulations combining a text with statements were more effective than manipulations only presenting statements ($t(11.4) = -3.26, p = 0.007$), only presenting a text ($t(9.5) = -4.17, p = 0.002$), or only showing a video ($t(14.6) = -2.14, p = 0.050$). None of the other conditions differed significantly from each other (all $ps \geq .057$; see Table 3). Next, we also compared the effectiveness of the Crick text, which has been used most often in the literature, with the effectiveness of other texts that have been used. This revealed no difference for free will beliefs ($t(13.2) = -0.71, p = 0.493$; see Table 3). The determinism analysis did not have enough degrees of freedom (i.e., $df = 2.15$) to interpret.

Importantly, however, manipulation type correlated strongly with baseline condition for free will beliefs (Table 3). In particular, a closer look at the coding revealed that studies combining a text with statements and studies using videos always had a pro-free will baseline. Therefore, to test if this could explain the effect of manipulation type, we did a control analysis restricting the analysis to those studies using a pro-free will baseline. While this changed the main effect of manipulation type from significant to marginally significant ($F(3, 21.9) = 2.86, p = .060$), it did not substantially change the pattern of results. In particular, manipulations combining a text with statements were still significantly more effective than manipulations only using statements ($p = 0.016$), and manipulations only using a text ($p = 0.016$), and were marginally more effective than manipulations using a video ($p = 0.061$).

Taken together, attempts to manipulate beliefs in free will are most effective when combining texts arguing against free will with statements that help strengthen the message. They are more effective than only using statements, texts, or videos.

Participant Involvement. For participant involvement, there was a significant main effect on free will beliefs ($F(2, 69.5) = 10.9, p < .001$; Table 3), but not on determinism beliefs ($F(2, 29.3) = 0.61, p = .549$; see Table 4). A further analysis of the free will effect showed that the manipulation was more effective when participants had to report on the content of the manipulation immediately after the manipulation compared with when they had to report on the content at the end of the experiment ($t(63.8) = -4.67, p < 0.001$), or not at all, ($t(61.9) = -2.79, p = .007$). The latter two conditions, however, did not differ, ($t(58.5) = 1.19, p = .240$). Importantly, like manipulation type, participant involvement also correlated highly with baseline condition for free will beliefs. Visual inspection showed that most studies requiring participants to report on the content of the manipulation immediately after the manipulation also used a pro-free will baseline, whereas other studies tended to use a neutral baseline. To control for this confound, we fitted a model including both participant involvement and baseline condition, which revealed that participant involvement remained significant ($F(2, 50) = 4.71, p = .013$).

In conclusion, experimental manipulations showed the strongest effects when experimental procedures were rehearsed or verified directly after the manipulation suggesting that participant involvement increases the effectiveness of free will belief manipulations.

Measurement Moment. For measurement moment, anti-free will manipulations had a larger influence on free will beliefs ($t(91.4) = -2.3, p = .024$; see Table 3) when the manipulation check was conducted before compared with after assessing the primary dependent variable (i.e. downstream consequences). The same statistical test was not significant for determinism beliefs ($t(33.2) = 0.95, p = .349$; see Table 4).

In sum, experimental manipulations showed the strongest effects when free will beliefs were measured directly after the experimental manipulation instead of at the end of the study. This suggests the effects of manipulations on beliefs in free will start to fade during the experimental session.

Baseline Condition. Anti-free will manipulations had a larger effect on free will beliefs when the baseline was a pro-free will condition than when it was a neutral condition, ($t(105.5) = -4.16, p < .001$; Table 3). A similar effect was also apparent for determinism beliefs, but did not reach statistical significance ($t(25) = 1.05, p = .304$; see Table 4). As discussed above, however, baseline condition correlated highly with manipulation type and manipulation task. Since there were no studies in some cells of the baseline condition x manipulation type combination, we decided to control for manipulation type by looking at the effect of baseline condition for the manipulation type where baseline condition was most balanced (i.e., statements) to optimize power (Tanner-Smith et al., 2016). This revealed that even when restricting the analysis to those studies using statements, anti-free will messages were more effective when the baseline condition was a pro-free will condition than when it was a neutral condition ($p = .032$). To control for participant involvement, we fitted a model including both baseline condition and participant involvement, which did not change the results ($p = .024$).

Taken together, experiments comparing anti-free will manipulations with pro-free will conditions resulted in larger effect sizes than experiments comparing anti-free will conditions with neutral conditions. This suggests that belief in free will cannot only be reduced but can also be increased by experimental manipulations.

Secondary Moderators

Age. The mean age of the sample did not influence the effectiveness of the manipulation for either free will beliefs ($t(28.2) = 0.27, p = .793$; see Table 3), or determinism beliefs ($t(19.1) = 0.21, p = .838$; see Table 4).

Sex. The proportion of female participants in the sample did not influence the effect of the manipulation on either free will beliefs ($t(31.5) = 0.53, p = .598$; see Table 3), or determinism beliefs ($t(17.3) = -0.14, p = .893$; see Table 4).

Continent. The manipulation had a stronger effect on samples collected in the US or on US-based platforms (e.g., MTurk) than on samples collected in Europe or on Europe-based platforms (e.g., Prolific) for free will beliefs ($t(101) = -2.54, p = .013$; see Table 3), but not for determinism beliefs ($t(45.2) = 0.99, p = .329$; see Table 4).

Test Location. Whether the experiment was conducted in the lab or online did not influence the effect of the manipulation on either free will beliefs ($t(77.2) = 0.45, p = .654$; see Table 3), or determinism beliefs ($t(42.6) = -0.55, p = .586$; see Table 4).

Sample Type. The effectiveness of the manipulation did not differ between student samples and samples collected via online platforms for either free will beliefs ($t(60.9) = 0.88, p = .383$; see Table 3), or determinism beliefs ($t(28.7) = 0.62, p = .543$; see Table 4).

Sensitivity Analyses

To test the robustness of our main results, we ran 3 sensitivity analyses. First, we varied the ρ parameter between 0.1 and 1.0. Second, we calculated effect sizes preferentially from the test statistics rather than from the means and standard deviations. Finally, we used a standard meta-analytical approach instead of RVE. However, none of these changes significantly impacted the results (for more details, see OSF; <https://bit.ly/2L69prl>).

Summary on the Effectiveness of Free Will Belief Manipulations

In sum, our analyses indicate that manipulations were able to successfully affect the belief in free will and related beliefs. The strongest effects occurred when reading an anti-free will text (e.g., the Crick text) was paired with anti-free will statements. Manipulation effects were strongest when assessed directly after the manipulation, and group differences were largest when the anti-free will condition was compared to a pro-free will condition.

Effects were strongest on free will and belief in determinism scales measured with the FWI or the FAD. There was no moderation by the demographics investigated here.

Table 3*Free Will Moderator Statistics*

Moderator	g/β	SE	95% CI	m	k	F/t (df)	p
Publication Status						3.42 (37.0)	.002
Unpublished	-0.23 _a	0.04	[-0.30, -0.16]	92	116		
Published	-0.51 _b	0.07	[-0.66, -0.36]	26	32		
Questionnaire						1.53 (74.9)	.130
FWI	-0.23 _a	0.04	[-0.31, -0.14]	39	40		
FAD	-0.13 _a	0.04	[-0.22, -0.05]	46	59		
Manipulation Type						6.70 (3, 26.3)	.002
Text	-0.18 _a	0.03	[-0.24, -0.12]	58	63		
Statements	-0.29 _a	0.06	[-0.41, -0.17]	35	54		
Text and Statements	-0.79 _b	0.14	[-1.12, -0.45]	8	9		
Video	-0.41 _a	0.11	[-0.64, -0.17]	12	12		
Text Type						0.71 (13.2)	.493
Crick	-0.17 _a	0.03	[-0.23, -0.11]	48	51		
Other	-0.22 _a	0.06	[-0.35, -0.08]	10	12		
Involvement						10.90 (2, 69.5)	<.001
None	-0.22 _a	0.06	[-0.34, -0.10]	33	43		
Report Before	-0.46 _b	0.06	[-0.59, -0.34]	44	53		
Report After	-0.14 _a	0.03	[-0.20, -0.09]	32	39		
Measurement Moment						2.30 (91.4)	.024
Before	-0.38 _a	0.07	[-0.52, -0.25]	51	68		
After	-0.21 _b	0.03	[-0.27, -0.15]	48	58		
Baseline						4.16 (105.5)	<.001
Control	-0.18 _a	0.03	[-0.23, -0.12]	72	80		
Pro-Free Will	-0.42 _b	0.06	[-0.54, -0.31]	63	67		
Age	0.04	0.14	[-0.25, 0.32]	64	80	0.27 (28.2)	.793
Sex (% Female)	0.09	0.16	[-0.24, 0.42]	66	82	0.53 (31.5)	.598
Continent						2.54 (101.0)	.013
Europe	-0.19 _a	0.05	[-0.29, -0.09]	54	62		
USA	-0.36 _b	0.05	[-0.45, -0.27]	61	83		
Test Location						0.45 (77.2)	0.654
Lab	-0.29 _a	0.05	[-0.40, -0.18]	42	49		
Online	-0.26 _a	0.05	[-0.36, -0.15]	57	69		
Sample						0.88 (60.9)	0.383
Students	-0.30 _a	0.05	[-0.39, -0.21]	50	63		
Panel	-0.38 _a	0.08	[-0.54, -0.22]	29	32		

Note. Different subscripts indicate $p < .05$. g/β = Hedge's g /beta coefficient; SE = standard error; 95% CI = 95% confidence interval; m = number of experiments; k = number of effect sizes. All statistical tests are unsigned.

Table 4*Determinism Moderator Statistics*

Moderator	g/β	SE	95% CI	m	k	F/t (df)	p
Publication Status						2.14 (18.9)	.046
Unpublished	0.12 _a	0.05	[0.03, 0.21]	39	44		
Published	0.30 _b	0.07	[0.15, 0.44]	14	17		
Questionnaire						2.29 (21.8)	.032
FWI	0.11 _a	0.04	[0.02, 0.19]	30	31		
FAD	0.30 _b	0.07	[0.14, 0.47]	16	18		
Manipulation Type						1.83 (2, 15.7)	.193
Text	0.09 _a	0.04	[0.02, 0.17]	30	32		
Statements	0.26 _a	0.08	[0.08, 0.43]	15	20		
Video	0.26 _a	0.15	[-0.12, 0.64]	7	7		
Involvement						0.61 (2, 29.3)	.549
None	0.23 _a	0.07	[0.08, 0.37]	16	19		
Report Before	0.18 _a	0.11	[-0.05, 0.41]	14	17		
Report After	0.14 _a	0.04	[0.06, 0.22]	21	22		
Measurement Moment						0.95 (33.2)	.349
Before	0.22 _a	0.08	[0.05, 0.39]	18	22		
After	0.13 _a	0.05	[0.03, 0.23]	28	29		
Baseline						1.05 (25.0)	.304
Control	0.13 _a	0.03	[0.06, 0.20]	40	43		
Pro-Free Will	0.25 _a	0.10	[0.02, 0.47]	18	18		
Age	0.03	0.13	[-0.25, 0.30]	42	49	0.21 (19.1)	.838
Sex (% Female)	-0.03	0.19	[-0.43, 0.38]	45	52	0.14 (17.3)	.893
Continent						0.99 (45.2)	.329
Europe	0.13 _a	0.06	[0.01, 0.25]	30	35		
USA	0.21 _a	0.05	[0.10, 0.31]	23	26		
Test Location						0.55 (42.6)	.586
Lab	0.19 _a	0.06	[0.07, 0.32]	27	33		
Online	0.15 _a	0.05	[0.04, 0.26]	22	23		
Sample						0.62 (28.7)	.543
Students	0.19 _a	0.06	[0.07, 0.31]	31	37		
Panel	0.15 _a	0.05	[0.03, 0.26]	14	15		

Note. Different subscripts indicate $p < .05$. g/β = Hedge's g /beta coefficient; SE = standard error; 95% CI = 95% confidence interval; m = number of experiments; k = number of effect sizes. All statistical tests are unsigned.

Table 5*FWI and FAD Statistics*

Moderator	g/β	SE	95% CI	m	k	F/t (df)	p
FWI						4.43 (2, 31.6)	.020
Free Will	0.25 _{a,b}	0.07	[0.11, 0.38]	39	40		
Determinism	0.11 _a	0.05	[0.01, 0.21]	30	31		
Dualism	0.29 _b	0.05	[0.20, 0.39]	28	29		
FAD						6.52 (3, 9.8)	.011
Free Will	0.12 _a	0.04	[0.04, 0.21]	46	59		
Scientific Determinism	0.40 _b	0.08	[0.22, 0.57]	16	18		
Fatalistic Determinism	0.13 _a	0.04	[0.02, 0.23]	13	14		
Unpredictability	-0.03 _c	0.05	[-0.13, 0.07]	12	13		

Note. Different subscripts indicate $p < .05$. g/β = Hedge's g /beta coefficient; SE = standard error; 95% CI = 95% confidence interval; m = number of experiments; k = number of effect sizes. Effect sizes are coded such that larger effects in the expected direction lead to more positive effect sizes. All statistical tests are unsigned.

Research Question 2: Do Free Will Belief Manipulations Have Downstream**Consequences?*****P-Curve Analysis***

A p -curve is considered to contain evidential value if the half p -curve, considering only p -values $< .025$, is significantly right skewed at $p < .05$, or if the full p -curve, considering all p -values $< .05$, and half p -curve are both right-skewed at $p < .10$. Similarly, a p -curve is considered to signal the absence of evidential value if the half p -curve is significantly flatter than the curve expected with 33% power at $p < .05$ or if both the full and half p -curve are flatter than 33% power at $p < .10$ (Simonsohn et al., 2015).

The p -curve across 49 experiments testing the influence of anti-free will manipulations on behavior, attitudes, and cognition revealed that both the full ($z_{\text{full}} = -3.54$, $p_{\text{full}} < .001$), and half p -curve ($z_{\text{half}} = -1.38$, $p_{\text{half}} = .084$), had a p -value $< .10$, and hence that the set of studies contained evidential value. However, a visual inspection of the p -curve revealed that it was almost entirely flat (see Figure 3). Although the flatness test did not find evidence for the absence of evidential value ($z_{\text{half}} = 5.86$, $p_{\text{half}} > .999$), a robustness check, as recommended by

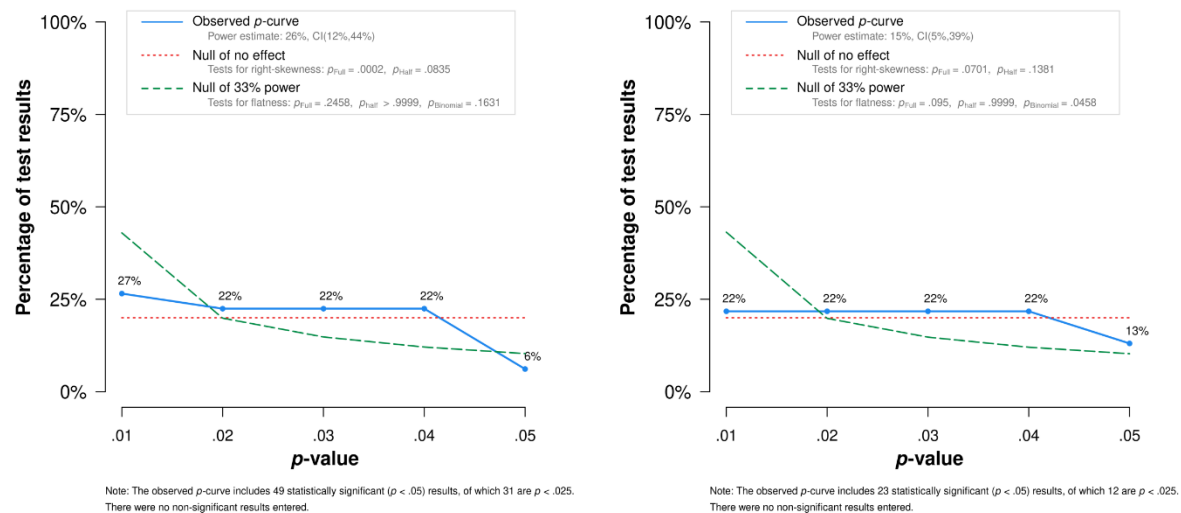
Simonsohn et al. (2015), indicated that removing the single most significant p -value across all included studies (i.e., Ching & Xu, 2018) was sufficient to render the test of evidential value non-significant ($z_{\text{half}} = 0.70$, $p_{\text{half}} = .243$). A second robustness check indicated that evidential value was stronger ($z_{\text{full}} = -4.57$, $p_{\text{full}} < .001$, $z_{\text{half}} = -2.49$, $p_{\text{half}} = .006$), when the second rather than the first result was used from studies reporting multiple relevant results. However, also here, removing the three most significant tests was sufficient to render the test of evidential value non-significant ($z_{\text{half}} = -0.86$, $p_{\text{half}} = .196$).

A potential explanation for this lack of clear evidence for downstream effects could be that some of the included studies were not able to confirm that their manipulation changed participants' belief in free will. That is, a study cannot be expected to have downstream consequences if it did not successfully manipulate the belief in free will in the first place. To test this hypothesis, we ran a second p -curve analysis including only those studies with a statistically significant manipulation check (i.e., $p < .05$). Across 23 tests, this revealed no evidence for either the presence ($z_{\text{half}} = -1.09$, $p_{\text{half}} = .138$) or absence ($z_{\text{half}} = 3.66$, $p_{\text{half}} = .999$), of evidential value (Figure 3). A robustness check using the second instead of the first test reported in studies with multiple relevant tests did indicate the presence of evidential value ($z_{\text{full}} = -1.87$, $p_{\text{full}} = .031$, $z_{\text{half}} = -1.48$, $p_{\text{half}} = .069$), but removing the single most significant test was again sufficient to make this evidence disappear ($z_{\text{half}} = -0.71$, $p_{\text{half}} = .240$).

Taken together, the p -curve analysis finds little evidence for the hypothesis that manipulating belief in free will has downstream consequences on behavior, attitudes, and cognition, after removing only the single largest effect. Moreover, there is equally little evidence for downstream consequences when considering only studies for which the effectiveness of the manipulation could be confirmed. At the same time, there was also no conclusive evidence for the absence of an effect. Hence, the p -curve found no clear evidence for, but also not against the hypothesis that free will belief manipulations have downstream consequences.

Figure 3

P-curve on the Entire Dataset (Left) and on the Studies With a Significant Manipulation Check (Right)



Note. See the OSF folder (<https://bit.ly/2L69prl>) for the evidence tables.

Anti-Social Behavior

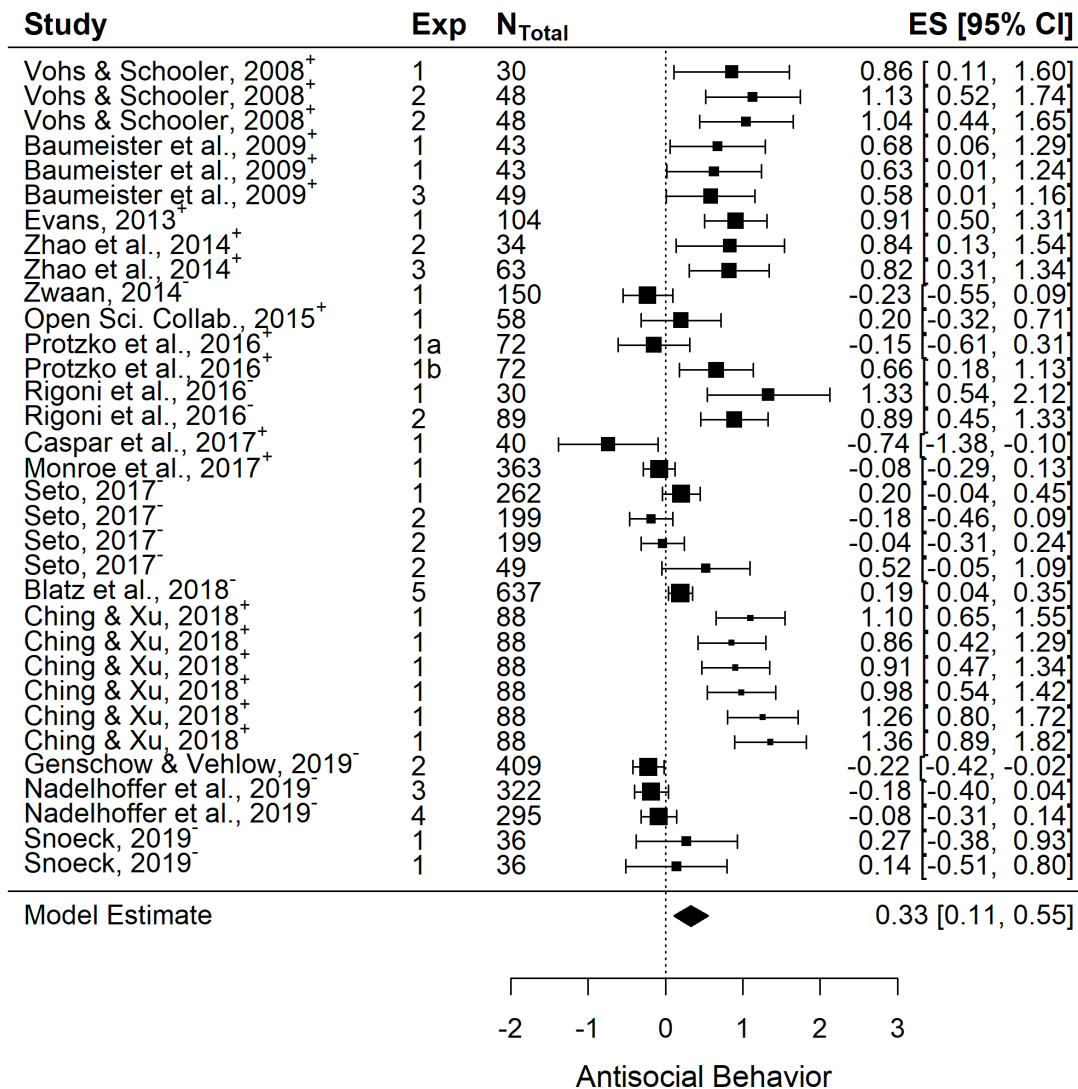
A random effects meta-analysis with robust variance estimation indicated an effect size of $g = 0.33$ for the influence of anti-free will manipulations on anti-social behavior, ($t(21.1) = 3.10$, $p = .005$, 95% CI = [0.11, 0.55], $m = 23$, $k = 33$)³. However, a visual inspection of the forest plot (see Figure 4) suggested that this significant effect was mostly driven by studies with smaller sample sizes, whereas studies with larger sample sizes tended to report null results. This was confirmed by a hierarchical Egger test, indicating a significant positive relationship between the effect size and the standard error ($\beta = 3.01$, $t(12.8) = 3.52$, $p = .004$). In other words, while the meta-analysis suggested an effect of the manipulation on anti-social behavior, additional analyses indicated that this effect may have been driven by

³ Note that after data collection had already been completed, Many Labs 5 was published, containing 5 replications of Vohs and Schooler's (2008) Experiment 1 (Buttrick et al., 2020). While these 5 experiments are not included in the meta-analysis, we did check whether adding them changed the results. For antisocial behavior, the estimated effect size with these 5 experiments included was $g = 0.29$ ($m = 28$, $k = 38$, $p = .004$). For cheating, it was $g = 0.27$ ($m = 13$, $k = 14$, $p = .082$). In both cases, a hierarchical Egger test found evidence for small study bias (both $p = .004$) and both PET and PEESE indicated that the corrected effect size was $g = 0$. In other words, the antisocial behavior and cheating meta-analyses yielded very similar results with and without these 5 experiments.

small-study bias. Correcting the effect size for this bias using PET ($g = 0.00$) or PEESE ($g = 0.00$) resulted in substantially reduced and non-significant effect sizes. Likewise, even though there was no significant difference between published ($g = 0.50$) and unpublished ($g = 0.15$) studies ($t(19.6) = 1.76, p = .095$), the effect was descriptively larger for published studies and was no longer significant if only unpublished studies were included in the analysis ($t(8.6) = 1.16, p = .279$). Taken together, this indicates that there is little evidence that anti-free will manipulations influence anti-social behavior.

However, including all studies in a meta-analysis of this sort may be problematic because studies that did not change the belief in free will (e.g., failed at the manipulation check) could not hope to have downstream consequences on antisocial behavior. That said, a moderator analysis comparing effect sizes in studies with a significant manipulation check ($g = 0.30, m = 10, k = 13$) to effect sizes in studies with no manipulation check or a non-significant manipulation check ($g = 0.37, m = 13, k = 20$) revealed no significant difference ($t(19.4) = -0.34, p = .736$). If anything, the effect size was even numerically smaller in the set of studies reporting a successful manipulation check, and this effect size was not significant ($t(8.7) = 2.11, p = .065, 95\% \text{ CI} = [-0.02, 0.61]$).

In sum, the analyses on anti-social behavior indicate that when controlling for publication and small sample biases, there is no evidence in favor of the idea that anti-free will manipulations affect anti-social behavior. Even when including only studies with significant effects of the manipulation on belief in free will, we found no evidence for an effect on anti-social behavior.

Figure 4*Forest Plot of the Effect of the Manipulation on Anti-Social Behavior.*

Note. Pro-social effect sizes are reverse coded. Published studies are annotated with “+” and unpublished studies with “-”. Nadelhoffer et al. (2019) and Genschow & Vehlow (2019) were included in the meta-analysis as unpublished studies but were published later as Nadelhoffer et al. (2020) and Genschow & Vehlow (2021) after data-analysis had already been completed.

Cheating, Conformity, and Punishment

No effects of anti-free will manipulations were found on either cheating ($g = 0.39$, $t(6.8) = 1.81$, $p = .114$, 95% CI = [-0.12, 0.91], $m = 8$, $k = 9$), conformity ($g = 0.26$, $t(4.7) = 1.63$, $p = .168$, 95% CI = [-0.16, 0.68], $m = 6$, $k = 9$), or punishment ($g = -0.15$, $t(7.5) = -1.60$, $p = .151$, 95% CI = [-0.36, 0.07], $m = 9$, $k = 9$). In line with the anti-social behavior meta-

analysis, visual inspection of the forest plots (see Figures 5-7) indicated that studies with smaller sample sizes tended to report significant effects, whereas studies with larger sample sizes tended to report null effects. This was confirmed by a hierarchical Egger test, showing significant relationships between the effect size and the standard error for all three dependent variables (all $p \leq .014$). Correcting the effect sizes for these small-study biases using PET (all $g = 0.00$) or PEESE (all $g = 0.00$) eliminated all possible evidence. However, the Egger and PET/PEESE results should be interpreted with care, as the limited dfs (all ≤ 4.1) likely led to an inflated false positive rate (Tipton, 2015). Similarly, for cheating and conformity, there were not enough studies to reliably test if the effect remained significant when including only the unpublished studies (both $df \leq 3.9$), but in both cases, effect sizes were numerically smaller for unpublished ($g = 0.23, 0.04$) than for published studies ($g = 0.69, 0.90$). For punishment, effect sizes were numerically smaller for unpublished studies ($g = -0.10$) than for published studies ($g = -0.23$), but this difference did not reach significance ($t(6.1) = -0.61, p = .563$).

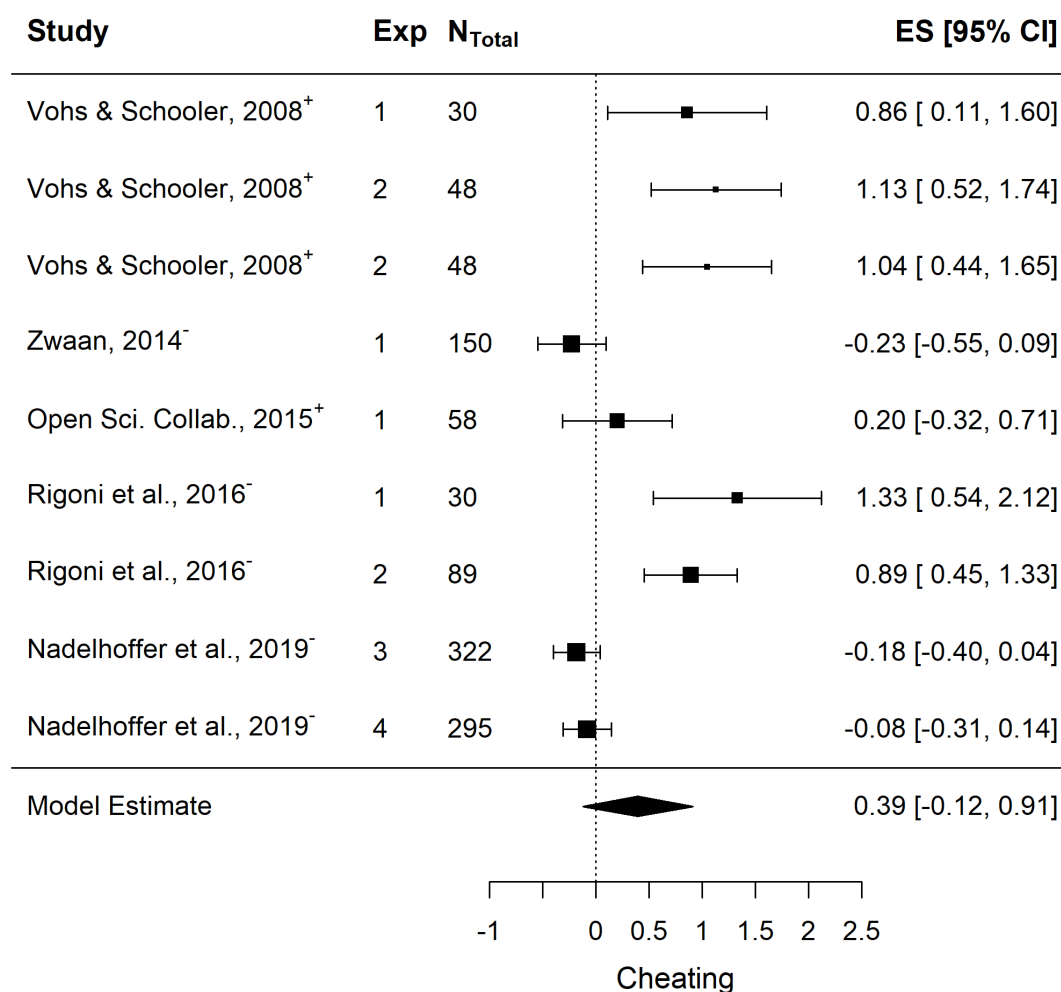
Finally, we investigated if the effect was modulated by whether or not a significant manipulation check was reported and if it was significant when including only those studies reporting a significant manipulation check. However, for cheating and conformity, there were insufficient studies to test either of these two hypotheses (all $df \leq 3.77$). Although the cheating effect size was descriptively larger for studies reporting a significant manipulation check ($g = 0.94, m = 3, k = 4$) than for studies not reporting a significant manipulation check ($g = 0.02, m = 5, k = 5$), this difference should not be taken at face value, given the limited number of studies and given that 3 of the 4 included effect sizes with a significant manipulation check came from the same paper (i.e., Vohs & Schooler, 2008). The conformity effect size was descriptively smaller for studies reporting a significant manipulation check ($g = 0.10, m = 3, k = 4$) than for studies not reporting a significant manipulation check ($g = 0.49, m = 3, k = 5$). For punishment, the difference between studies that did or did not report a significant

manipulation check was not significant ($t(6.0) = 0.24, p = .821$). There were not enough studies to test if the effect remained significant when only including studies reporting a significant manipulation check ($df = 3.9$). However, if anything, the effect was numerically smaller in studies that reported a significant manipulation check ($g = -0.13, m = 5, k = 5$) than in studies that did not report a significant manipulation check ($g = -0.18, m = 4, k = 4$).

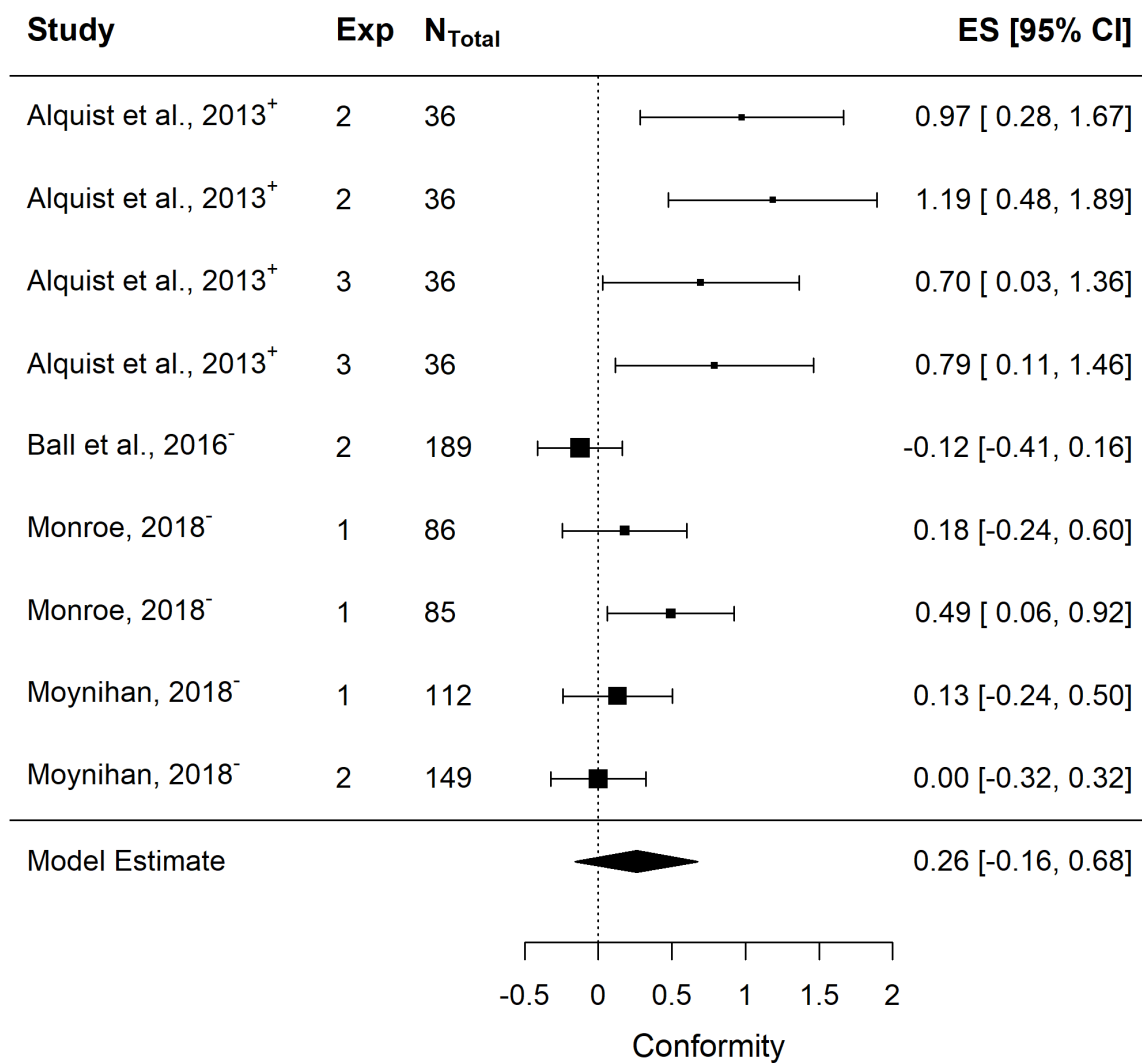
In conclusion, when controlling for small-study biases we found no evidence in favor of the idea that anti-free will manipulations affect either cheating, conformity, or punishment.

Figure 5

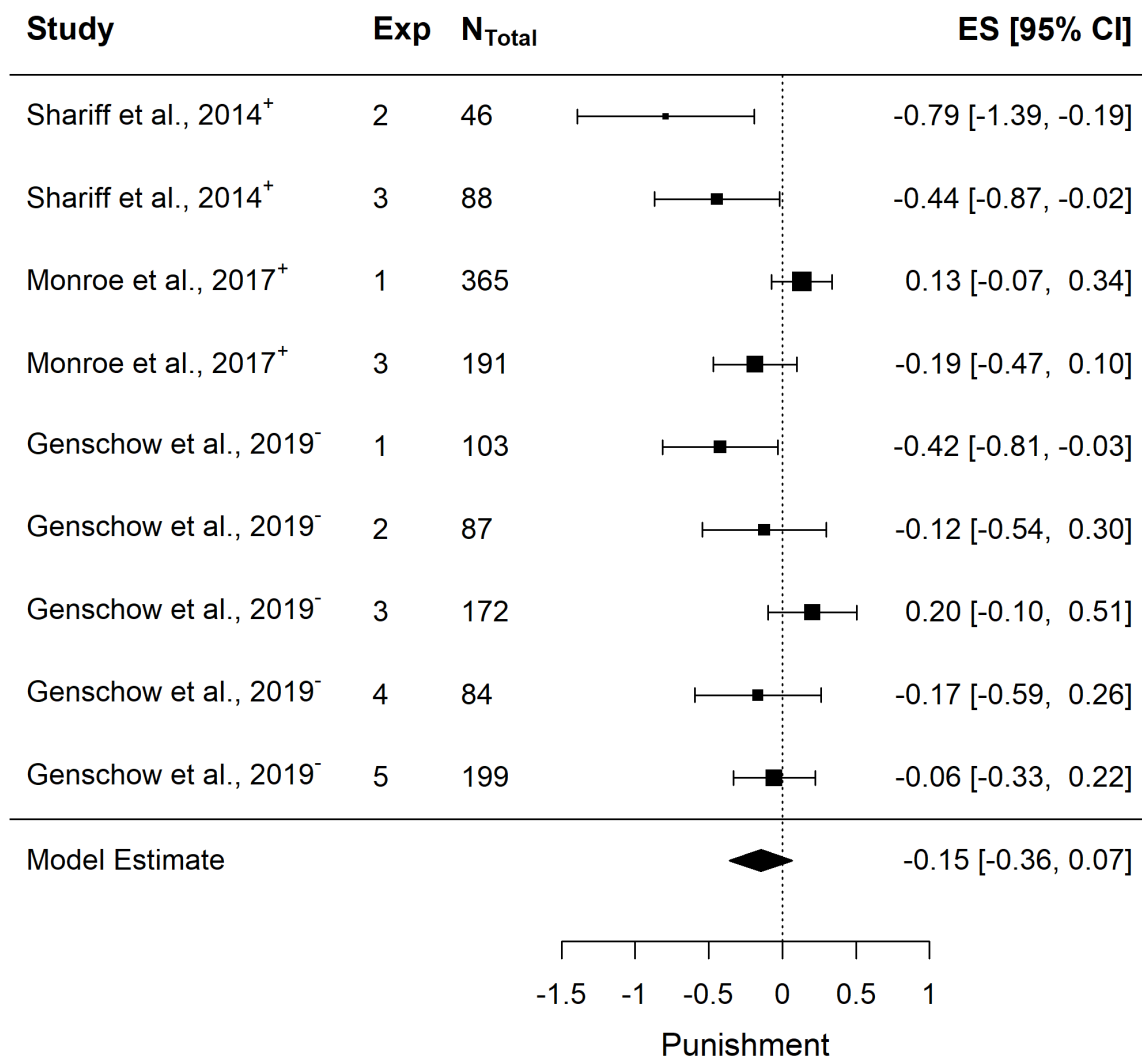
Forest Plot of the Effect of the Manipulation on Cheating



Note. Published studies are annotated with “+” and unpublished studies with “-”. Nadelhoffer et al. (2019) and Genschow & Vehlow (2019) were included in the meta-analysis as unpublished studies but were published later as Nadelhoffer et al. (2020) and Genschow and Vehlow (2021) after data-analysis had already been completed.

Figure 6*Forest Plot of the Effect of the Manipulation on Conformity*

Note. Published studies are annotated with “+” and unpublished studies with “-”.

Figure 7*Forest Plot of the Effect of the Manipulation on Punishment*

Note. Published studies are annotated with “+” and unpublished studies with “-”.

Sensitivity Analyses

To test the robustness of our main results, we ran the same 3 sensitivity analyses as for the first research question. As before, none of these three analytical variations had a significant influence on the results (see OSF; <https://bit.ly/2L69prl>).

Discussion

Past research has shown that reducing individuals' belief in free will affects societally relevant behaviors such as cheating (Vohs & Schooler, 2008) and other anti-social behaviors (Baumeister et al., 2009; Zhao et al., 2014). These and similar results have been used by some scholars as an argument that anti-free will and deterministic viewpoints should be kept away from society, because they may change the way people interact with each other. However, a number of studies reported difficulties in replicating some of the key results in the field (Genschow, Hawickhorst, et al., 2020; Giner-Sorolla et al., 2016; Monroe et al., 2017; Open Science Collaboration, 2015; Schooler et al., 2014; Shariff & Vohs, 2014). While these failed replications call into question the societal relevance of belief in free will, it is not yet clear what caused them. In this article, we tested three possible explanations that may account for the failed replications. First, it could be that the failed replications are false negatives. That is, they were not able to detect an effect that actually is real. Second, it could be that the manipulations commonly used in the literature do not alter individuals' belief in free will and thus have no impact on other behaviors. Third, it could be that free will belief manipulations do reduce belief in free will, but that this does not have any downstream consequences as the belief in free will does not cause other behaviors. Our meta-analysis supports the third explanation. That is, we found that beliefs related to free will can effectively be manipulated by commonly used experimental manipulations, although the effects were rather small, with $g = -0.29$ for free will beliefs and $g = 0.17$ for determinism beliefs. At the same time, we found little evidence that manipulating free will beliefs had any downstream consequences on attitudes, behavior, or cognition. In the remainder of this article, we discuss the implications of the meta-analytical findings for the processes underlying free will belief manipulations, potential reasons for why these manipulations did not lead to downstream consequences, societal implications, possible steps for future research, and the limitations of the present meta-analysis.

Processes Underlying Free Will Belief Manipulations

The meta-analysis on the effectiveness of free will belief manipulations (i.e., Research Question 1) indicated that beliefs related to free will can be influenced by experimental manipulations. Although we found evidence for publication bias, we also found that this cannot explain the effect. To investigate the conditions under which these manipulations were effective, we conducted several moderator analyses. The results of these analyses further the understanding of free will belief manipulations manifold.

First, previous research indicated that most people believe that they have free will (Baumeister et al., 2009; Nahmias et al., 2005). Based on this finding, it has been assumed that free will beliefs can only be decreased by exposing individuals to anti-free will (deterministic) viewpoints (for an overview, see Ewusi-Boisvert & Racine, 2018). While this assumption has never been systematically investigated, the present meta-analysis allowed us to test whether experimental manipulations can also increase the belief in free will. The results demonstrate that belief in free will can be reduced as well as increased by experimental means.

Second, from a methodological point of view, it is important to know which manipulations are most effective to reliably alter the belief in free will. By comparing all the different manipulations applied in previous research, our analysis suggests a combination of letting participants read a text and reproducing statements produces the largest effects.

Third, an interesting and open question was how deeply participants have to process the manipulation for it to succeed. To answer this question, we investigated the degree to which the effectiveness of the manipulation depends on whether participants have to engage with the task (e.g., by summarizing or rewriting presented messages). Manipulations worked best when participants had to summarize the content of the manipulation directly after the manipulation as compared with when they merely attended to the content. In other words, actively processing the provided information (e.g., by summarizing it) increased the effects of

the manipulation. This indicates that participants' involvement in the task strengthens the effect of free will belief manipulations—a finding in line with previous research on the self-generation effect (Slamecka & Graf, 1978) where self-generated information increases memory performance of that information.

Fourth, past research left open how long the effects of free will belief manipulations last. Measuring beliefs at the end of the experiment assumes the manipulation lasts through the duration of the experiment; this is not necessarily the case. For instance, it could also be that free will belief manipulations do not fully change people's beliefs but rather activate an anti-free will mindset for a brief moment. To test how long-lasting the effects of free will belief manipulations are within the span of an experiment, we investigated if the effect of the manipulation differed depending on whether beliefs were measured immediately after the manipulation or at the end of the experiment (i.e., after completing another task). At least for the belief in free will, the effect reduced over time. While the same was not true for belief in determinism, it is important to note that deterministic beliefs were generally less influenced by moderators. This could mean that determinism beliefs are more robust than beliefs in free will, but there are also several alternative explanations. For example, the meta-analysis included less studies measuring belief in determinism than belief in free will and the determinism moderator analyses may therefore have had less statistical power to detect such effects (Hempel et al., 2013). Alternatively, it could be that given that belief in determinism was less influenced by the manipulation as such, there may have been a floor effect, making it difficult to further reduce the effect of the manipulation.

Fifth, the manipulations used in the literature do not solely alter free will or determinism beliefs but also other (related) beliefs. For example, we found that the manipulations also affected belief in dualism and that the effect size for dualism was even larger than the effect size for belief in determinism. Similarly, we found that free will belief manipulations influence not only the belief in scientific determinism but also the belief in

fatalistic determinism. There are different possible explanations for these unspecific effects. Some of the beliefs are correlated with each other and are thus rather unspecific themselves. For example, belief in free will positively correlates with belief in dualism (Nadelhoffer et al., 2014; Wisniewski et al., 2019). In addition, some manipulations, such as the Crick text, are unspecific in the sense that they do not only argue against free will but also against dualism. Finally, it is likely that the manipulations affect not only beliefs related to free will, but also other psychological and cognitive factors. For, instance, it is conceivable that challenging the fundamental belief that free will exists leads to a general feeling of confusion and uncertainty about the world. An important question for future work will be if the unspecific nature of the manipulations can help explain why we found no influence of free will belief manipulations on attitudes, behavior, and cognition.

Little Evidence for Downstream Consequences and its Potential Reasons

While we found clear evidence that free will related beliefs can be experimentally manipulated, there was little evidence these free will manipulations have downstream consequences. That is, *p*-curve analyses across all dependent variables and traditional meta-analyses on internally coherent sets of dependent variables found either no evidence for downstream consequences or weak evidence that disappeared when correcting for extreme values or small-sample effects. This remained true even if only studies with significant manipulation checks (i.e., significant effects on belief in free will and/or determinism) were used in the analyses. This indicates that there is currently little to no evidence for downstream consequences.

However, it is important to note that absence of evidence does not equal evidence of absence. Indeed, the *p*-curve analysis lumped together a variety of different variables and the meta-analyses focused only on a limited subsample of outcomes for which there were enough data points. Thus, we cannot rule out that there are other specific single variables that are influenced by free will belief manipulations. For example, we did not have enough data to

investigate the influences of free will belief manipulations on neurocognitive processes (Rigoni et al., 2012; Rigoni et al., 2011; Rigoni et al., 2015), feelings of alienation (Seto & Hicks, 2016), attributions of other people's actions (Genschow et al., 2017a), perceived meaningfulness of life (Crescioni et al., 2016; Moynihan et al., 2019), perceived gratitude (MacKenzie et al., 2014), counterfactual thinking (Alquist et al., 2015), or risk taking behavior (Schrage et al., 2016). For these and potential other variables that have not been studied yet, it remains open to what degree they are influenced by free will belief manipulations, although one of the effects on neurocognitive processes recently failed to replicate (Eben et al., 2020).

Another explanation for the difficulty to find downstream effects might be that existing manipulations are not suited for this purpose. In other words, it might well be that experimentally reducing beliefs in free will has downstream consequences, but that established free will belief manipulations are not able to produce them. There are several reasons why this might be the case. As mentioned above, the manipulations are rather unspecific and do not only affect belief in free will and determinism, but also other beliefs. This is problematic for at least two reasons. On the one hand, an unspecific manipulation may be detrimental in finding downstream consequences, because the different factors influenced by the manipulation may counteract the influence of free will beliefs on the dependent variable. On the other hand, an unspecific manipulation opens the question to which degree downstream consequences (if any) are actually driven by free will beliefs or rather by other beliefs and psychological variables. In addition, existing manipulations of free will beliefs have only weak effects on free will ($g = -0.29$) and determinism ($g = 0.17$) beliefs. Given that any effect of anti-free will manipulations on attitudes and behavior is likely smaller than their effect on the beliefs they purport to change, this makes finding evidence for downstream consequences particularly challenging. Indeed, research on the relationship between behavioral intentions and actual behavioral change shows that the influence of behavioral

interventions on behavior is roughly twice as small as their influence on behavioral intentions (Webb & Sheeran, 2006). The same problem is likely true for belief in free will and its downstream consequences and may additionally be exacerbated by the fact that most variables assumed to be influenced by belief in free will are complex and multifaceted behaviors that are likely determined by more than just one factor.

Societal Implications

Whether free will exists is part of a longstanding philosophical debate (e.g., Dennett, 2015; Van Inwagen, 1983). However, since cognitive neuroscientists and psychologists started claiming that humans' perception of free will is nothing more than an illusion (e.g., Crick, 1994; Harris, 2012; Wegner, 2002), anti-free will viewpoints have become in vogue not only in academia (e.g., Greene & Cohen, 2004), but also in popular media (e.g., Chivers 2010; Griffin, 2016; Wolfe, 1997). When psychological research found that presenting individuals with such anti-free will viewpoints influences fundamental behavior, cognition, and attitudes, the question arose about the ethics of publishing such anti-free will viewpoints. While some philosophers argue that undermining people's belief in free will would have catastrophic consequences, as free will forms the basis for moral behavior (e.g., Smilansky, 2000, 2002), other philosophers argue that disbelieving in free will might have positive effects, because it could lead to abandoning retribution-based morality and illusory beliefs in a just world (Caruso, 2014; Greene & Cohen, 2004; Nadelhoffer, 2011). The present research adds to this debate by suggesting that confronting individuals with anti-free will viewpoints might not have as strong consequences as has been previously assumed.

Although these manipulations affect people's beliefs in free will, there is no apparent influence on behavior, cognition, or attitudes. This is in line with recent findings indicating that although professional judges are influenced by reading anti-free will texts, these viewpoints do not influence their judgments (Genschow et al., 2020). However, it is important to keep in mind that all these findings only speak to the effect of presenting individuals with a

single anti-free will viewpoint. While such a short exposure may not have downstream consequences, it remains an open question whether more concentrated and repeated presentations of anti-free will messages, as may happen in real-life, could nevertheless have important consequences. Indeed, preliminary support for this possibility comes from the results of the present meta-analysis, demonstrating that a combined manipulation of presenting participants with a text and statements has the strongest impact on individuals' belief in free will. It would hence be interesting to test to what extent repeated exposure to anti-free will messages may have stronger effects on individuals' belief in free will and thereby also leads to downstream consequences and social impact on the public.

In the same vein, although we did not find support for the idea that conventional belief in free will manipulations have downstream consequences, it is important to note that we do not necessarily argue that belief in free will itself has no impact. That is, on an interindividual level, belief in free will may well contribute to societal relevant behaviors, including retributive punishment as well as anti- and pro-social behavior—to name just a few examples. Indeed, several studies could robustly replicate the link between belief in free will and retributive punishment on a correlational level (Genschow et al., 2017a; Martin et al., 2017). Similar robust and replicable correlations were also found between belief in free will and job satisfaction (Feldman et al., 2018) and between free will beliefs and just world beliefs, religious worldviews, and a conservative world view (Carey & Paulhus, 2013; Genschow & Vehlow, 2021). This suggests that on a correlational level, belief in free will may well be connected to societal relevant behaviors.

Taken together, there is a debate whether researchers should publicly debate whether free will exists or not, because this may have harmful consequences for society. Our findings suggest that the influence on society may be weaker than previously assumed. In this respect, we would like to argue that discussions of free will should distinguish between scientific facts and philosophical speculations: “Rather than using science as a pulpit for indoctrination to our

own personal metaphysics, let's simply be frank with the public about what we know as scientists and what we believe as individuals, and then encourage people decide what they think for themselves.” (Schooler, 2010; p. 213).

How to Move Forward? Possible Steps and Recommendations for Future Research

Our results offer several promising routes for future research. First, future research should continue investigating the exact underlying mechanisms of free will belief manipulations to further increase the understanding of these manipulations. For example, recent research suggests that it is important to consider not just beliefs but also attitudes towards free will (Cracco et al., 2020). From this perspective, a potential avenue for future research could be to investigate whether attitudes towards free will alter the effect of anti-free will manipulations.

Second, to better test the downstream consequences of free will belief manipulations, researchers should aim at developing manipulations that (1) more specifically manipulate belief in free will and determinism, but not other factors and (2) lead to larger effect sizes, for example by using dual-approach manipulations where participants read and repeat the presented messages. Implementing these changes may allow answering the question whether belief in free will manipulations have a meaningful societal relevance.

Third, future research may also investigate the somewhat surprising results obtained in this meta-analysis. For example, an interesting finding is that anti-free will manipulations appeared to have stronger effects in the US than in Europe. In line with a recent call for cross-cultural replications in social psychology (Genschow, Westfal, et al., 2020), future research may explore in more detail the influence of the manipulation on different continents and cultures.

Fourth, although we did not find support for the hypothesis that free will belief manipulations have downstream consequences, it might still be that on a correlational level, free will beliefs relate to individuals' behavior, cognition, and attitudes. Future research

could, thus, (1) test to which degree previous findings reported in the literature can be replicated on a correlational level and (2) investigate whether other relevant societal factors are modulated by individuals' belief in free will.

Limitations of the Present Meta-Analysis

There are a few limitations to this meta-analysis that call for a careful discussion. First, all publication bias correction methods have downsides (Carter et al., 2019; Stanley, 2017) and should hence be interpreted with care. We used PET and PEESE because they are easily incorporated within the RVE framework (Rodgers & Pustejovsky, 2020) and have been shown to retain reasonable false positive rates across a wide range of scenarios (Carter et al., 2019). However, PET-PEESE can suffer from low power when sample sizes are small, heterogeneity high, or when there is either very little or very heavy use of questionable research practices (Carter et al., 2019). Therefore, in addition to using bias correction methods, we also compared published with unpublished studies and tested if the effects remained significant if only unpublished studies were considered. While no single method is perfect by itself, we believe that by combining these different methods, we were able to get a clearer overview of the underlying true effects and the degree to which they were inflated by publication bias.

Second, as is the case in almost all meta-analyses (Lipsey & Wilson, 2001), some moderators were confounded with other moderators. Whereas most effects remained even after controlling for these confounds, truly controlling for moderator overlap is difficult and these findings should hence be tested more directly in future empirical work.

Finally, meta-analytical moderator analyses are known to often be underpowered (Hempel et al., 2013). It is therefore possible that some of the non-significant moderator effects found here could be explained by a lack of power. However, it is worth noting that at least for the meta-analysis on beliefs in free will and determinism, most analyses included a relatively large number of studies (see Tables 3 and 4). While we are not aware of research

that has systematically assessed the statistical power of RVE meta-analyses under different conditions, previous simulation work using regular meta-analytical approaches suggests that given the parameters of the current meta-analysis, many of the moderator analyses were, in fact, well powered to detect even fairly small effect sizes (Hempel et al., 2013).

Summary

The present meta-analysis finds support for the idea that beliefs related to free will can be experimentally manipulated. These effects are stronger when participants were presented with a combination of texts and statements, when they had to report on the content of the manipulation, and when belief in free will was measured directly after the manipulation. Moreover, beliefs related to free will can be experimentally increased as well as decreased. However, the used manipulations are rather unspecific in the sense that beliefs and concepts other than belief in free will are affected also. Although we find support for the idea that individuals' belief in free will can be experimentally manipulated, the meta-analysis found little evidence for the idea that these manipulations have meaningful downstream consequences. These findings bring into question prior claims of a causal relationship between the belief in free will and behaviors, thoughts, and attitudes. Further research is warranted to determine whether an impact of free will beliefs may still be implicated by correlational evidence, particular measures and/or stronger interventions.

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