Action and inaction in moral judgments and decisions:

Meta-analysis of Omission-Bias omission-commission asymmetries

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Pre-testing		V	V
Pre-registration		V	V
Data curation	V	V	V
Formal analysis	V	V	V
Funding acquisition			
Investigation	V	V	V
Pre-registration peer review/verification		V	
Literature search	V	V	V
Datafile study/effect coding	V	V	V
Reproducible code (e.g., RMarkdown)	V	V	V
Contacting authors		V	V
Data analysis peer review/verification	V		
Methodology	V	V	V
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Abstract

Omission bias is people's tendency to evaluate harm done through omission as less morally wrong and less blameworthy than commission when there is harm. However, findings are inconsistent. We conducted a pre-registered meta-analysis, with 21 samples (13 articles, 49 effects) on omission-commission asymmetries in judgments and decisions. We found an overall effect of g=0.45[0.14,0.77], with stronger effects for morality and blame than for decisions. Publication bias tests produced mixed results with some indication for publication bias, though effects persisted even after most publication-bias adjustments. The small sample of studies included limited our ability to draw definite conclusions regarding moderators, with inconclusive findings when applying different models. After compensating for low-power we found indication for moderation by role responsibility, perspective (self-versus-other), outcome-type, and study-design. We hope this meta-analysis will inspire research on this phenomenon and applications to real-life, especially given the raging pandemic. Materials, data, and code are available on https://osf.io/9fcqm/?view_only=f502cf30d3894fbea86726f4f4def936

Keywords: omission bias; commission; morality; judgment and decision making; pre-registered meta-analysis

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Introduction

A large body of social and cognitive psychology research claimed empirical support for an omission bias, commonly defined as evaluating commission as more morally wrong and blameworthy compared to omission, in cases of potential harm. Omission commonly refers to no deviation from a reference point, either doing nothing, a deliberate choice of not taking action, or making no change (Feldman et al., 2021). Commission commonly refers to a deviation from a reference point, making an active decision, doing something, or making a change (Feldman et al., 2021). For example, omission bias has been demonstrated in the preference not to vaccinate (omission) rather than to vaccinate (commission) (Asch et al., 1994; Ritov & Baron, 1990), or in preference for doing nothing (omission) over doing something (commission) to prevent harm that can be easily stopped (Spranca et al., 1991).

Omission bias studies demonstrated that when faced with a dilemma between taking action and doing nothing, with both likely resulting in similar negative outcomes, people tend to prefer omission over commission. Omission bias holds implications for several domains, such as moral and/or legal judgements (Spranca et al., 1991) and in medical/health decision making regarding vaccinations (Connolly & Reb, 2003). These seem especially relevant given the raging COVID-19 pandemic during the time of writing (the year 2021). The omission bias has been linked to the “action principle of harm” which describes a phenomenon in which harm through action tends to be judged as morally worse than harmful omission (Cushman et al., 2006).

However, several scholars raised doubts regarding the reliability, generalizability, and boundary conditions of the omission bias effect. The methods for studying the effect were criticized, such as the choice of the moral scenarios used in the early demonstrations, raising concerns about oversimplicity and relevance for the complexity of real-life situations (e.g., Connolly & Reb, 2003). Further criticism was raised regarding methodology, such as the use of numerical risk-balancing[1] procedures in some vaccination scenarios (Connolly & Reb, 2003, 2012), and the lack of control for equivalence of norms violated by commission and omission (Willemsen & Reuter, 2016). Moreover, some observed weaker effects when using between-subject designs compared to within-subject designs (e.g., Jamison et al., 2020). It has also been suggested that omission bias is sensitive to moderating factors, such as familiarity with the target, social role responsibility for the target (Haidt & Baron, 1996), outcome (Spranca et al., 1991), and the target (self vs others) (Ubel et al., 2011).

The importance and relevance of the phenomenon combined with the mixed findings raise the need for a meta-analytic review of the existing evidence. A meta-analysis allows for a systematic integration and comparison of different results to attempt more accurate estimations of the true effect. We aimed to determine the overall weighted effect size of omission-commission (action-inaction) asymmetries on judgments of morality, blame, and decisions. We also aimed to examine possible moderators of the effect, and explore potential directions that have yet to receive attention.

We first review the literature and discuss the theories and hypotheses. Then, we discuss possible moderators. After that, we report the meta-analysis we conducted and discuss its findings. Finally, we discuss uncertainties regarding the phenomenon and possible future research directions.

[1] Risk balancing procedure refers to the procedure in which the experimenter only provides the probability of omission leading to harm and asks the participants the probability of commission (e.g. vaccination) causing harm, which would make both options indifferent. Check Connolly and Reb (2003) Appendix for examples. Our meta-analysis focuses on morality, judgments, and decision as DVs but not risk-premium.

Scope

In this meta-analysis we focused on omission bias as capturing omission-commission asymmetries regarding morality and blame judgments and decisions. We make the distinction between omission bias and other related biases, and attempted to clearly define the scope of the investigation in our methods to differentiate omission bias from other phenomena (see Feldman et al., 2021 for a review). We do not address omission-commission asymmetries related to emotions (action-effect), or normal-abnormal asymmetries (normality, exceptionality effect, status quo bias, default bias, etc.). We also attempted to disentangle omission-bias from trolley like moral dilemmas, by focusing on studies in which the outcomes are the same or very similar, and therefore do not confound omission bias with utilitarianism versus deontology. We also note, that in the omission bias literature, the vast majority of studies adopted a narrow meaning of commission as being about "doing something" and of omission as being about "doing nothing/not doing something" (rather than about "change" or "no change" from some reference point).

Theories, Mechanisms, Explanations, and Hypotheses

Spranca et al. (1991) were among the first to discuss possible explanations for the omission bias. The causal relevance hypothesis focuses on the different causal attributions in omission and commission (Willemsen & Reuter, 2016). Spranca et al. (1991) suggested that people's actions were judged as having stronger causal links to the outcome compared to inactions. Follow-up studies supported this idea (Henne et al., 2019; Kordes-de Vaal, 1996; Willemsen & Reuter, 2016) with action being perceived as more intentional than inaction (Hayashi, 2015; Jamison et al., 2020; Kordes-de Vaal, 1996). A related explanation is about the role of information. Lacking information in an uncertain situation makes people more sensitive to action cues (Frisch & Baron, 1988; Spranca et al., 1991). Ritov and Baron (1990) found more statistical information resulted in weaker omission bias, supporting this notion.

With limited information, people's judgments may be more biased by action cues, which is associated with higher causality and higher perceived intentionality mentioned above.

Omission bias is related to the classic action effect (Kahneman & Tversky, 1982) the first demonstration of action-inaction asymmetries in the domain of evaluations and emotions (Anderson, 2003; Connolly et al., 1997; Ritov & Baron, 1995). Though action-effect and omission bias are related, and omission bias may partly be explained by action-effect (discussed below), there are key differences. Omission bias focuses on omission-commission asymmetries regarding morality, blame, and decisions. Specifically, it is often mentioned in contexts of moral judgments, especially regarding harm and blame (DeScioli et al., 2012). Action-effect focuses on the emotional reactions to outcomes, most notably the demonstration of stronger regret over negative outcomes for action than inaction. Action-effect seems broader and extends beyond moral contexts, such as the classic financial investment context used in Kahneman and Tversky (1982). See Feldman et al. (2020) for a detailed discussion and comparison.

The link between omission bias and action-effect lies in the attempt to avoid stronger negative feelings associated with harmful outcomes resulting from having taken action. People may aim to prevent future regret over harm inflicted by not acting. However, action-effect likely cannot fully account for the omission bias (DeScioli et al., 2011). For example, in a recent replication and extension of the classic omission bias study by Spranca et al. (1991), Jamison et al. (2020) measured attributions of both morality and regret in moral dilemmas and found regret was more strongly associated with inaction rather than action, opposite to findings of Kahneman and Tversky (1982) non-moral investor scenario. In this case, it is likely that this was because those scenarios involved intended harm, so regret was associated with having not inflicted any harm. Therefore, the link between regret and

morality seems to do more with reference points, goals, and intent rather than strictly with morality.

Sense of responsibility, which is associated with but distinct from regret (for details, see Zeelenberg et al., 1998), is another key element in the moral judgements of omission and commission. Sense of responsibility is closely associated with causal attributions and intentionality mentioned above. Kordes-de Vaal (1996) and Zeelenberg et al. (2000) both found that commissions were perceived as more causal than omissions.

Commissions also seem to be more salient and clearer to define than omissions (Kordes-de Vaal, 1996). Inaction could be a deliberate decision not to act, but it could simply reflect not having made any choice, or choosing to defer the decision. In choosing between harm through action and harm through inaction, people may strategically aim to minimize accountability, responsibility, possible blame, and possible punishment through the more ambiguous inaction rather than the clearer more observable action (termed "omission strategy"; DeScioli et al., 2011a).

Omission bias: Main-effect

Our first aim was to examine the effect of the omission bias. We expected the evidence to be in support of the omission bias as meaningfully different from the null (null not included in confidence intervals). The below hypotheses are based on the assumption that the consequences of action and inaction are the same.

> H1a: Harm through action is perceived as less moral than harm through inaction. Harm through action is perceived as more morally accountable and blameworthy than harm through inaction.
>
> H1b: When facing a dilemma between inflicting undesired harm through action or inaction, harm through inaction is preferred to harm through action.

Moderators

The demonstration of omission bias was followed by studies that examined potential boundary conditions and factors that impact the strength of omission bias. We discuss each of the theorized moderators below, yet we note that after completing the coding procedure, we realized that for one of the pre-registered moderators - availability of statistical information regarding chances of harm through omission and commission - there were too few studies for a meaningful analysis. We therefore removed this moderator and do not discuss it in this section, and we detail this deviation in the Supplementary (subsection "Preregistration Versus Final Report Deviations".

Familiarity with target and Social Role Responsibility for the Target

We classified familiarity with target into two categories: 1) decision maker knows the target personally (e.g. friends, family members), and 2) decision maker does not know the target personally (e.g. stranger).

We classified social role responsibility over the target into two types: 1) having clear responsibility over target (e.g. as a parent, as a doctor, under the social role to prevent harm or directly responsible for the negative outcome), and 2) unclear responsibility over target (e.g. as a friend, not under the direct social role to prevent harm).

The above two factors have been previously suggested as moderators of omission bias. The two are likely related, as having more responsibility over a target is likely to be associated with higher familiarity with the target. Role responsibilities over familiar targets, such as between family members, involve more concern regarding possible harm than between strangers that the person is unfamiliar with (Haidt & Baron, 1996).

Haidt and Baron (1996) found that omission bias was substantially weakened for decision-makers with responsibility over the target aiming to minimize harm similarly through both commission and omission. This is due in part to social role-based expectations for caregivers to do what they can to prevent harm to their care dependents (Baron & Ritov,

2004; Haidt & Baron, 1996). However, we also note that there appears to be evidence supporting a meaningful and strong omission bias for familiar targets. For example, in their classic omission bias article, Ritov and Baron (1990) demonstrated the effect for cases of vaccination for children by their parents, with the explanations provided by parents who refuse to vaccinate, thereby exhibiting omission bias, were related to their responsibility for their children's life or death. However, in a follow-up heated debate in the literature, Connolly and Reb (2003) offered contradictory findings and despite several back-and-forth debates between the two groups regarding methodology claiming to have resolved the issue, the mixed findings suggest complex moderators.

However, there are situations in which the decision-maker does not have a close relationship with the target but is responsible for the target, such as medical doctors who are responsible for their patients' health (e.g., Baron & Ritov, 2004; Spranca et al., 1991). These are especially relevant in pandemic situations in which caregivers, key health policymakers, and doctors have to decide whether they vaccinate, provide medical treatments, or make key public health decisions (act or do not act).

We set forth the following hypotheses:

H2a: Omission bias is stronger for more familiar targets[2].

H2b: Omission bias is stronger for less familiar targets[3].

H3a: Omission bias is stronger with higher perceived responsibility for the target.

H3b: Omission bias is stronger with lower perceived responsibility for the target.

[2] H2a and H3a: We pre-registered this hypothesis, as a result of an initial misunderstanding regarding Haidt & Baron, 1996, check supplementary Preregistration Versus Final Report Deviations p. 20 to p. 21 for details.
[3] H2b and H3b: Based on a revised understanding of Haidt and Baron (1996), they were added as counter to H2a and h3a after pre-registration. H2a/b and H3a/b test the same moderation, though framed differently. Both H2a/H3a and H2b/H3b are included to reflect the process that we went through between pre-registration and the final write-up.

Presence/Absence of Negative Outcome

People often evaluate the quality of a decision based on the outcome (Baron & Hershey, 1988). Spranca et al. (1991) proposed that the valence of an outcome may moderate evaluations of morality and responsibility. They found that some participants exhibited outcome bias in omission bias, with stronger omission bias when outcomes were negative and involved actual harm to the target. In some omission bias studies, there is no outcome information specified (e.g. DeScioli et al., 2012), or it is uncertain if the outcome will be negative (e.g. Connolly & Reb, 2003). In a recent replication of this work by Jamison et al. (2020) the researchers found mixed support depending on the scenario used.

We hypothesized that:

> H4*:* There would be an omission bias effect regardless of outcome (null not included in confidence interval).
>
> H5*:* Omission bias is stronger when outcomes are negative compared to when outcomes are not negative (neutral/positive/unknown/uncertain).

Experimental design

There has been some debate in the literature on action and inaction regarding study designs, on differences in effect size comparing within-subject to between-subject designs. N'gbala and Branscombe (1997) first suggested that action-effect in within-subject designs may not replicate well to between-subject designs, though since then there have been many follow-up studies that have convincingly demonstrated the action-effect also using between-subject designs, though possibly with weaker effects (e.g., Feldman & Albarracín, 2017). There has been a similar debate in the omission bias literature between the Baron and Ritov team and the Connolly and Reb team contrasting different study designs. Since this debate, there have been successful extensions of omission bias initially demonstrated using within-subject design (e.g., Spranca et al., 1991) to between-subject designs (Jamison et al., 2020).

Given the mixed findings and the ensuing debates regarding both omission bias and action-effect, we aimed to examine study design as a moderator and compare the omission bias effect size using these two designs. We therefore outlined the following competing hypotheses:

> H6a: Omission bias is stronger for studies using a within-subject design compared to between-subject designs.
>
> H6b: Study design has no impact on the omission bias (overlapping confidence intervals).

Target: self vs others

We did not pre-register this moderator, yet we identified this as a moderator while coding, noticing that some studies use self as the target. In medical treatments such as vaccination, psychiatric treatment, surgery, and cancer treatments, studies found that when deciding for others people (laypeople or medical doctors) are more likely to prefer the active decision compared to when deciding for self, in which people are more likely to exhibit omission bias (Janssen et al., 2015; Mendel et al., 2010; Ubel et al., 2011; Zikmund-Fisher et al., 2006). This may be because when deciding for others, people are in the role of being responsible for others by taking action to prevent harm. Another explanation is that when deciding for others, people tend to focus on a single dimension of the decision, which is the risk-benefit tradeoff, an easily defensible justification whereas when deciding for self, people are influenced by more factors such as emotions and biases (Ubel et al., 2011). Therefore, as an exploratory hypothesis, we hypothesized the following:

> Exploratory hypothesis: The omission bias effect is stronger when the target is self, compared to when the target is others.

Method

Pre-registration and open-science

We pre-registered the meta-analysis on the Open Science Framework before search and data coding (https://osf.io/hyebx/?view_only=fd97dfb852f7479a8820472477c9a533; https://osf.io/jw62m/?view_only=15348c2437384eb9b9036a3eec0da1f0; these are the same registrations, only the latter was adjusted for the "pre-registration challenge" format conducted by the Center of Open Science, more details on: https://osf.io/x5w7h/). Pre-registration, coding sheet, and all additional materials used in the meta-analysis were made available on the Open Science Framework (https://osf.io/9fcqm/?view_only=f502cf30d3894fbea86726f4f4def936).

We included a coding sheet with a codebook detailing all fields, elaborated, and coded, which is used directly by the accompanied Rmarkdown code and exported outputs detailing all our analyses and results (see "Coding and analyses" directory on the OSF). We report all search, coding, measures, and exclusions in this meta-analysis.

Literature search

To find articles relevant to our topic, we used Google Scholar (for suitability for meta-analyses see Gehanno et al., 2013; Walters, 2007) and identified a sample of studies based on various steps illustrated in Figure 1. In the first initial online search, we decided to identify articles including variations of keywords such as *omission bias, omission effect, action principle, commission, and omission,* aimed to identify relevant literature, related topics, and the scholars in this field. As a result of the first search round, we were able to identify more specific search terms on the omission bias. Consequently, our search patterns were: *("omission bias" OR "omission effect" OR "omission strategy" OR "action effect" OR "action principle" OR (Baron AND Ritov) OR (Connolly AND Reb) OR (action AND inaction) OR (actions AND inactions) OR (omission AND commission) OR (omissions AND*

commissions) OR (direct AND indirect) OR (doing AND allowing) OR (active AND passive)) AND (morality OR intent OR harm OR vaccinations OR moral OR blame OR responsibility) – "Trolley". This process was followed by a scan of the reference sections of found articles. This enabled us to find other articles that were not detected through the search procedure. Additionally, we searched for "related articles" and "cited by" Google Scholar options based on the found articles.

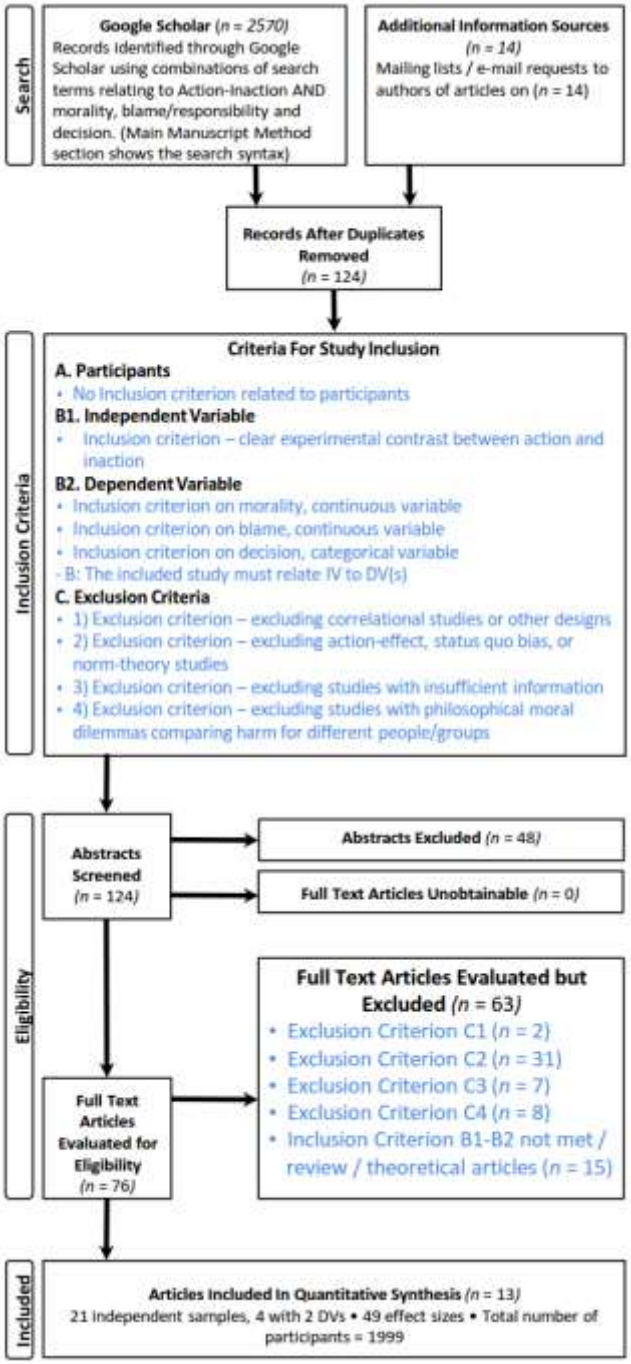
As the last step of this search round, we contacted published authors on the topic and announced our search on ResearchGate, Twitter, and mailing lists asking for published and unpublished to maximize coverage and access to unpublished data and/or manuscripts (Feltz & May, 2017). We conducted this procedure twice, once in 2017 and again in 2020. We received 14 articles previously not found in our search.

As a second step, we scanned all abstracts, tables, and method sections to identify the relevance of the sources. If the articles indicated relevance for our analysis, we read more of the articles to determine whether inclusion criteria were met or whether articles had to be excluded based on our search criteria (see next paragraph). A second scan round enabled us to exclude 48 articles and reduced our sample of studies to 13 articles, including 21 samples. We note that we were surprised by this low number of studies, given how prominent and well-cited the omission bias is in the literature, which is partly the reason for us repeating the search and call procedures. This suggests the need for more studies in this domain. We return to this point in the general discussion.

We listed the included articles in Table 1.

Figure 1

Meta-analysis flow diagram



Note. We included studies that measured any of the dependent variables. For example, if a study focuses on a decision but does not measure morality/blame, we include the study. We adapted the diagram based on Moreau and Gamble (2020).

Table 1

Studies included in the Meta-analysis

	Study	Study/Sample	N	Country	Sample population	Design	Publication status	DV type
1.	Baron and Miller (2000) (a)	1/1	50	United States	Students	Within	Published	Decision
2.	Baron and Miller (2000) (b)	1/2	70	United States	Students	Within	Published	Decision
3.	Baron and Ritov (2004)	1	112	United States	General population	Within	Published	Decision, Blame
4.	Bergstrand (2014)	2	115	United States	Students	Between	Published	Morality, Blame
5.	Blahunka (2014)	1	50	United States	Students	Within	Not published	Morality
6.	Connolly and Reb (2003)	1	293	United States	Other	Between	Published	Decision
7.	Cushman and Young (2011)	1	20	United States	Students	Within	Published	Morality
8.	DeScioli et al. (2011) (a)	1/1	41	United States	General population	Within	Published	Morality
9.	DeScioli et al. (2011) (b)	1/2	95	United States	General population	Within	Published	Morality
10.	DeScioli et al. (2011) (c)	2	77	United States	General population	Within	Published	Morality
11.	DeScioli et al. (2012)	1	151	United States	Students	Within	Published	Morality
12.	Hayashi (2015) (a)	1/1	80	Japan	Other	Within	Published	Morality
13.	Hayashi (2015) (b)	1/2	76	Japan	Other	Within	Published	Morality
14.	Hayashi (2015) (c)	1/3	60	Japan	Other	Within	Published	Morality
15.	Kordes-de Vaal (1996) (a)	1	74	The Netherlands	Students	Between	Published	Morality, Blame
16.	Kordes-de Vaal (1996) (b)	3	80	The Netherlands	Students	Between	Published	Blame
17.	Spranca et al. (1991) (a)	1	38	United States	Students	Within	Published	Morality
18.	Spranca et al. (1991) (b)	4	48	United States	Students	Within	Published	Morality
19.	Willemsen and Reuter (2016) (a)	2	119	United States	General population	Between	Published	Blame
20.	Willemsen and Reuter (2016) (b)	3	116	United States	General population	Between	Published	Blame
21.	Jamison et al. (2020)	1	313	United States	General population	Between	Published	Morality, Blame

Inclusion and exclusion criteria

We established strict inclusion-exclusion criteria to ensure we are capturing omission bias and not any of the other action-inaction effects (see review by Feldman et al., 2020). Excluded articles were noted in the search sheet with exclusion reasons.

We focused on articles referring to omission bias. We excluded articles that were about the action-effect (e.g., Zeelenberg et al., 2000), status quo bias (e.g., Baron & Ritov, 1994), or norm-theory (normality/exceptionality biases, etc. e.g., Prentice & Koehler, 2002).

Second, we focused on experiments that had the same consequences for both action and inaction, and we only included articles in which the independent variable (IV) had a clear contrast between omission and commission and the dependent variables (DV) were related to morality (e.g. DeScioli et al., 2011), blame (e.g. Willemsen & Reuter, 2016), or a decision (e.g. Connolly & Reb, 2003) between possible harm through action and possible harm through inaction. This included evaluations of morality wrongness, immorality, moral acceptability, responsibility, blame, and/or moral decisions. We excluded correlational (e.g. Dibonaventura & Chapman, 2008) and one-sample comparison studies (e.g. Zikmund-Fisher et al., 2006).

Third, the studies had to include adequate statistical information for computing the effect size for a contrast effect between omission and commission. In cases of missing statistical data (e.g., t-statistics / mean or standard deviation for calculating Hedge's *g*, counts, and proportions for omission and commission), we attempted to retrieve the information from authors, and if we failed then we excluded those articles, even if all other search criteria were met. For example, we excluded Haidt and Baron (1996) as they did not report standard deviation, t-statistics, and cohen's d for our effect of interest. Another example is, we excluded Ritov and Baron (1990) because they did not report information relevant for computation of overall omission bias effect size. Their Experiment 1 only reported risk tolerance of different risk levels. Experiment 2 focused on personal decisions vs support for law given different levels of risk. Experiment 4 reported differences between risk cases.

Fourth, we focused on comparisons of morality, blame, and/or decision for a single target and not comparisons of harm inflicted to a different number of people, which confounds moral dilemmas examining utilitarianism. Therefore, trolley dilemmas studies or similar philosophical moral dilemmas were excluded (e.g. Hauser et al., 2007) .

Studies collected through the database searches and through contacting authors were assessed for their eligibility based on their titles, abstracts, and contents. One author determined the adequacy of the study for the meta-analysis and coded the studies, and two other authors verified and adjusted the coding when necessary. All decisions for exclusion were documented and explained.

Coding

We developed a data coding sheet and codebook (see link above). Before we began with the coding process, we pilot-tested six randomly-selected studies in two stages and refined it accordingly in every stage. Once the pre-test was completed, we pre-registered our plan and proceeded to the full search and coding. One author coded all studies and two other authors verified and adjusted coding when needed. We reported and justified the decisions clearly.

Analyses

We used RStudio 1.3.1093 Version (RStudio Team, 2020) and metafor package 2.4-0 Version (Viechtbauer, 2010) for the statistical analyses. Given the range of different types of studies and experimental designs, we expected heterogeneity in the sample to be relatively high. Therefore, we used random effect models.

We converted all effect sizes into Hedge's *g* to allow for a comparison. We also collapsed split conditions due to moderators in the original studies to allow for a comparison of omission-commission (IVs).

Whenever available, we collected standardized effect sizes directly from authors of original papers. We checked for the accuracy of these analyses based on the provided information and details. If unavailable, we used either descriptive statistics or inferential statistics to re-compute standardized effect sizes. We documented all conversions and coding decisions. We also included the original text in the coding sheet to facilitate reproducibility.

We first conducted a meta-analysis to examine the overall main-effect, for each dependent variable. We conducted two-level models, multivariate three-level models, and MetaForest to examine the impact of the described moderators.

Statistical heterogeneity was determined using the Tau-squared test and quantified using I^2, which represents the percentage of the total variation in a set of studies that is actually due to heterogeneity (Higgins et al., 2003). This yielded a point estimate, confidence intervals, and p-value, along with statistics for heterogeneity, assessed using the Q-statistics, and the I^2 statistic. If there was indeed significant heterogeneity, we explored potential moderators.

We conducted several publication bias analyses, examining publication status as a moderator, testing for funnel plot asymmetry (trim and fill, rank test, Egger's unweighted regression symmetry test) (Begg & Mazumdar, 1994; Duval & Tweedie, 2000; Sterne & Egger, 2005), and conducted bias assessment and adjustment techniques of p-uniform (van Assen et al., 2015), p-curve (Simonsohn et al., 2014), PET, and PEESE (Stanley & Doucouliagos, 2014).

Data analysis strategy

When the project was initiated and pre-registered in 2017, we planned to report two-level model results, yet following the completion of the search and coding and recommendations received by open peer-review, we decided to adopt multivariate three-level models and moved the two-level model results to the Supplementary. We note that the results were very similar.

We used the following strategy for aggregation. If one study with a sample reported two effect sizes of two different dependent variables, we collapsed those for the main effect analyses and then analyzed them as separate effect sizes for the separate DV meta-analyses.

If one sample reported two effect sizes of the same dependent variable type (e.g., Jamison et al., 2020), we collapsed those into a single effect size. See provided code for more details.

The results for analyses collapsing two effect sizes of two dependent variables within the sample as one effect size, or analyzing and treating two or more effect sizes from two or more scenarios within the sample as separate effect sizes, are to a large extent similar. We detail these and discuss the differences in the Supplementary (under "additional analyses").

Results

Omission bias: Overall main effect

We conducted the analysis on 21 samples (4 with two dependent variables, the second level in the model), 49 effect sizes, 13 articles (the third level in the model), and found an overall effect size of g = 0.45 [0.14, 0.77], with very high heterogeneity. The posthoc statistical power was above 99.99% (Tiebel, 2018). The median power across studies was 66.8% (see Figure 3 metaviz plot for the power distribution of studies; Kossmeier et al., 2020).

We note that we conducted the multivariate-three-level model, to account for the dependence (correlations) between effect sizes of the same articles (Cheung, 2019). We did not pre-register the use of multivariate three-level analysis, yet adopted the recommendations from an open peer review that the multivariate multilevel model provides a more accurate estimate of the true effect size. The results for the pre-registered analysis and the multi-level analysis were very similar, and the analysis for the pre-registered analysis was moved to the supplementary.

Omission bias: Main effect for each dependent variable

We proceeded to conduct separate analyses for each of the three dependent variable types. We provide a summary of the results in Table 2. We visualized the results in Figure 2 (forest plots) and Figure 3 (funnel plots).

Studies measuring morality as the dependent variable (k = 14) had an overall effect size of g = 0.71 [0.47, 0.94]. Studies with blame (k = 7) as the dependent variable had an overall effect size of g = 0.32 [0.01, 0.64]. Finally, studies examining decision as the dependent variable (k = 4) had an effect of g = 0.30 [-0.62, 1.21], with confidence intervals overlapping with the null, suggesting weak to no support for omission bias.

Following up with an unregistered exploratory analysis, we excluded Connolly and Reb (2003), which was flagged as an outlier, as an article showing a general tendency towards commission. When excluding this article, we found a slightly larger overall effect size of g = 0.56 [0.32, 0.80], and a more consistent and larger effect size for decision as a dependent variable g = 0.71 [0.16, 1.25].

Table 2

Meta-analytic results for dependent variables of the omission effect

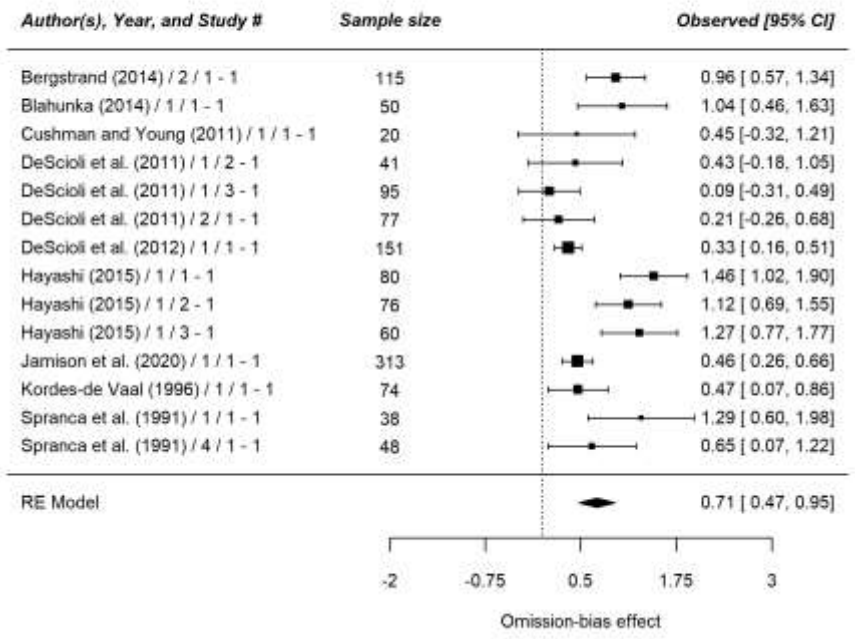
Measure	*k*	*Q*	df	I^2[%]
Morality	14	56.56	13	79.72
Decision	4	66.39	3	94.69
Blame	7	26.51	6	82.31

Note. *k* = number of samples, *Q* = Cochran's Q test for heterogeneity, I^2 = I square.

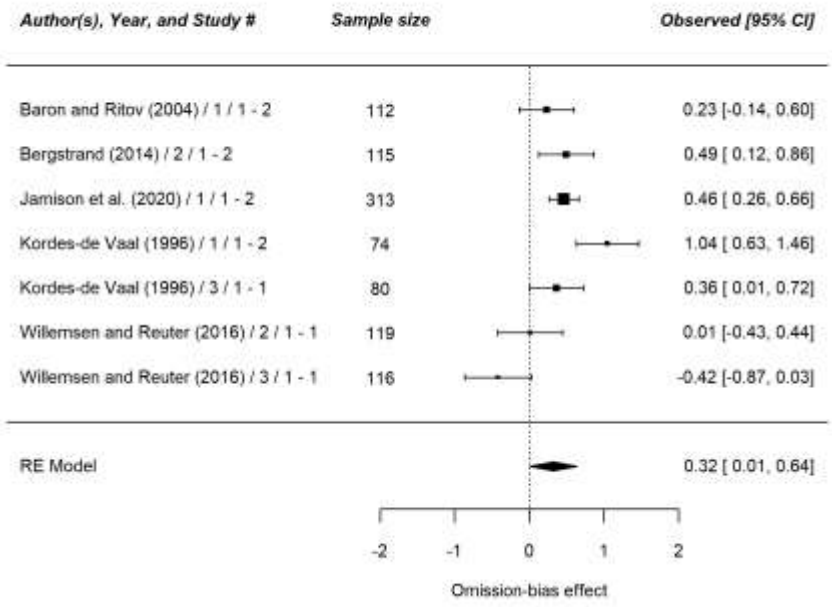
Figure 2

Meta-analysis forest plots - Morality, Blame, and Decision

Morality



Blame


Author(s), Year, and Study #
Sample size
Observed [95% CI]
Baron and Ritov (2004) / 1 / 1 - 2
112
0.23 [-0.14, 0.60]
Bergstrand (2014) / 2 / 1 - 2
115
0.49 [0.12, 0.86]
Jamison et al. (2020) / 1 / 1 - 2
313
0.46 [0.26, 0.66]
Kordes-de Vaal (1996) / 1 / 1 - 2
74
1.04 [0.63, 1.46]
Kordes-de Vaal (1996) / 3 / 1 - 1
80
0.36 [0.01, 0.72]
Willemsen and Reuter (2016) / 2 / 1 - 1
119
0.01 [-0.43, 0.44]
Willemsen and Reuter (2016) / 3 / 1 - 1
116
-0.42 [-0.87, 0.03]
RE Model
0.32 [0.01, 0.64]
-2
-1
0
1
2
Omission-bias effect

Decision

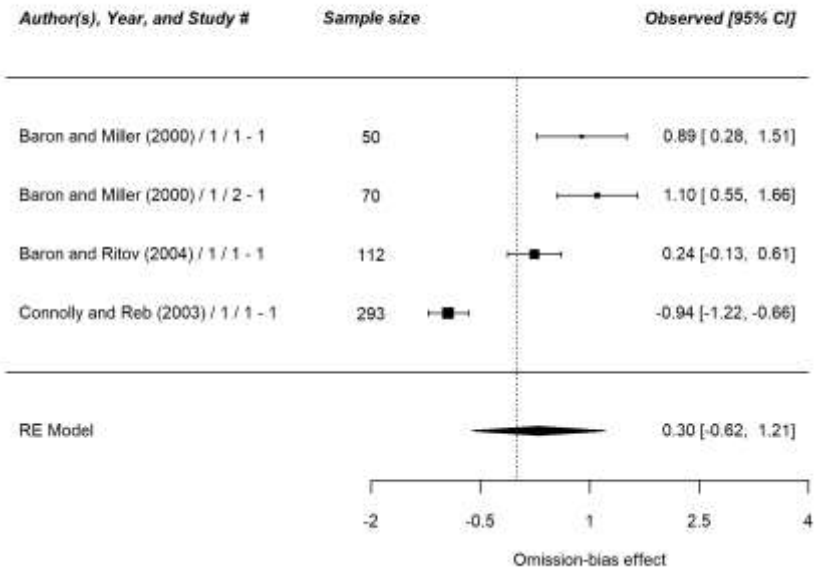
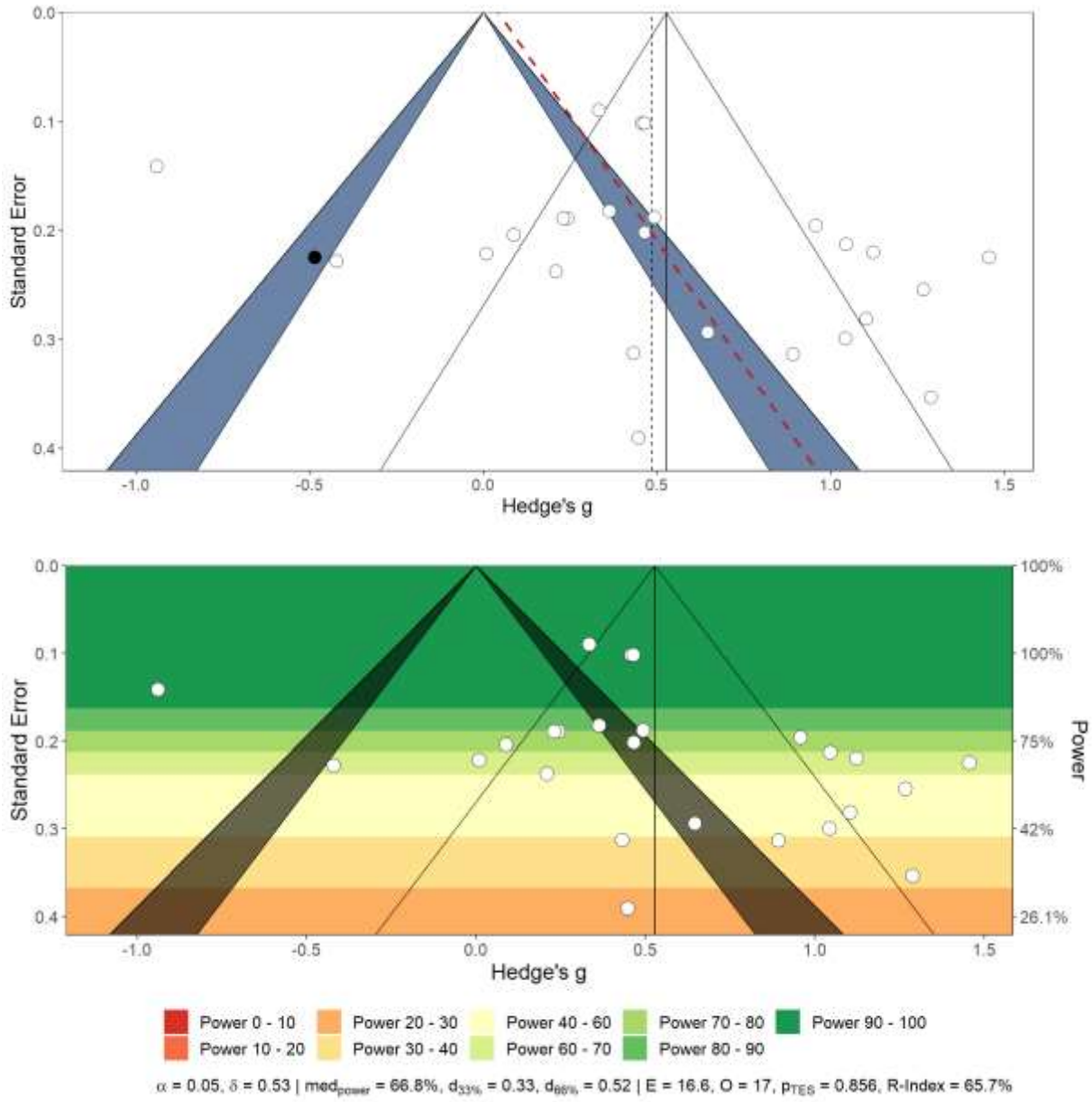

Author(s), Year, and Study #
Sample size
Observed [95% CI]
Baron and Miller (2000) / 1 / 1 - 1
50
0.89 [0.28, 1.51]
Baron and Miller (2000) / 1 / 2 - 1
70
1.10 [0.55, 1.66]
Baron and Ritov (2004) / 1 / 1 - 1
112
0.24 [-0.13, 0.61]
Connolly and Reb (2003) / 1 / 1 - 1
293
-0.94 [-1.22, -0.66]
RE Model
0.30 [-0.62, 1.21]
-2
-0.5
1
2.5
4
Omission-bias effect

Figure 3

Meta-analysis funnel and sunset plots



Note. Created using with metafor (Viechtbauer, 2010) and metaviz (Kossmeier et al., 2020) R packages. The blank dots of the trim-and-fill funnel plot refer to the actual observed effects of different studies whereas the black dot refers to the effect of the missing imputed additional study to ensure the plot is symmetric. The different colours in the sunset plot refer to different levels of statistical power.

Heterogeneity

Using a multivariate three-level model, we conducted a Cochran's Q test to check whether variations in the observed effect size were likely to be attributable solely to sampling

error. The variation in effect size was greater than what would be expected from sampling error alone $Q(48) = 271.22$, $p < .001$, indicating a variation of true effect across studies.

Moreover, we computed I^2 and found high heterogeneity between the studies (I^2 = 93.68%, 75% is regarded as high variability; Huedo-Medina et al., 2006). We reported the heterogeneity statistics of all three DVs in Table 2.

Excluding Connolly and Reb (2003), the heterogeneity was lower compared to that with Connolly and Reb (2003) but still substantial, $Q(47) = 180.04$, $p < .001$, $I^2 = 84.24\%$.

Publication bias

We used several statistical approaches to examine publication bias. The omission bias using adjustment methods of P-uniform, P-Curve, and Henmi and Copas (2010) showed effects meaningfully different from the null. However, the adjusted effect using the three-parameter selection model (Iyengar & Greenhouse, 1988) was $g = 0.34$ [-0.03, 0.72]), with the effect overlapping with the null, with weak to no omission bias effects using PET and PEESE techniques.

We provide funnel plot and asymmetry tests, including Egger's regression test and Rank correlation test, in Table 3, Table 4, and Figure 3, showing several p-values close to .05, slightly below or above, which with the publication bias assessment are indicative of possible publication bias. We discuss publication bias results in more detail in the discussion section.

Table 3

Publication bias adjustment

	Hedge's *g*	CI
Trim and Fill (Duval & Tweedie, 2000)	0.49	[0.26, 0.72]
Three-parameter selection (Iyengar & Greenhouse, 1988)	0.34	[-0.03, 0.72]
Henmi and Copas (2010)	0.41	[0.12, 0.70]
P-uniform (van Assen et al., 2015)	0.74	[0.48, 1.00]

Note. Analyses conducted on multivariate three-level model: $k = 49$, $g = 0.45$ [0.14, 0.77], before correction.

Table 4

Other publication bias analyses

Publication bias analysis method	Results
PET (Stanley & Doucouliagos, 2014)	$b = 0.04$ [-0.49, 0.57]
PEESE (Stanley & Doucouliagos, 2014)	$b = 0.21$ [-0.11, 0.53]
Rank correlation test (Begg & Mazumdar, 1994)	Kendall's tau = .20, $p = .170$
Sterne and Egger Regression test (2005)	$z = 1.97$, $p = .048$
P-curve (Simonsohn et al., 2014)	Evidential value is present, adjusted effect: $d = 0.70$

Note. Values in parentheses indicate 95% confidence intervals [lower bound, upper bound]

We also pre-registered and conducted a moderator analysis comparing the effects for published vs unpublished studies. However, we were only able to identify one unpublished study, and so the analysis is reported in the supplementary (section "published vs unpublished moderator analysis").

Moderators Analysis

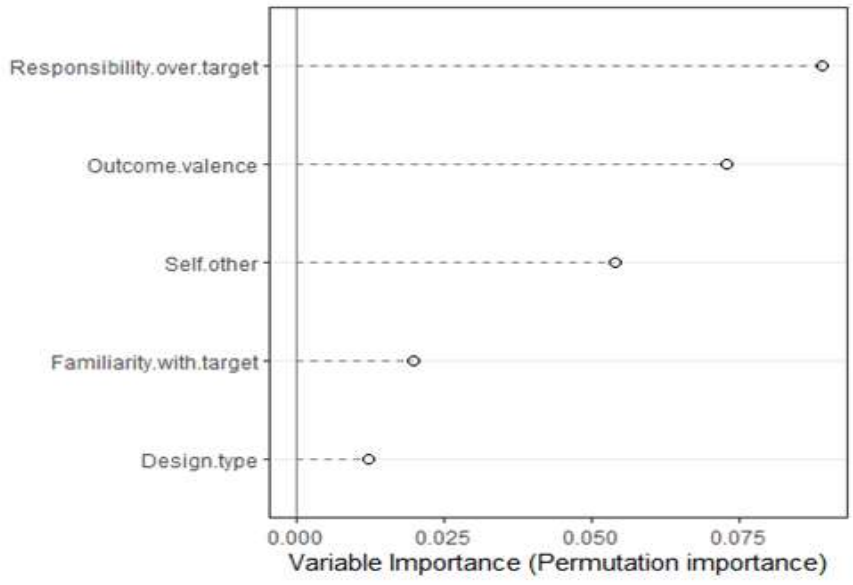
We initially adopted fixed effects two-level models to contrast effects of moderators' categories and conducted multivariate three-level models to account for confounding relationships and dependence of effects within the same article. However, due to the small

number of studies included we realized we have insufficient power to draw conclusions from these moderator analyses. Therefore, following peer review advice we supplemented our analyses with MetaForest moderator analyses (Curry et al., 2018; van Lissa, 2017), which applies bootstrapping, to address the issue of limited statistical power as each moderator category has limited studies. We report the underpowered two and three levels moderator analyses in the supplementary (sections "two-level and multivariate three-level moderators analyses").

Firstly, the key indicator of the MetaForest model – R-squared (R-OOB) was 0.29, meaning that the moderators predicted variance in the effect (I^2 = 84.56%; Q: 267.61, p < .001). Furthermore, we found that all five moderators had positive variable importance, with responsibility for target being the most important variable (stronger effects for no clear responsibility for target studies compared to clear responsibility for target studies), followed by presence/absence of negative outcome information, self vs other, familiarity with target, and design type. See Figure 4.

Figure 4

MetaForest Variable Importance Plot



Note. See the rest of MetaForest analyses in the supplementary. The higher the variable importance, the stronger the moderating effects.

Familiarity with the target

Twelve studies for familiarity with the target had an effect size of g = 0.58 [0.24, 0.91]. Six studies in which the decision-maker was not familiar with the target showed an effect of g = 0.28 [0.14, 0.41]. We failed to find support for the hypothesized difference between more familiar with target and less familiar with target studies with fixed-effects model and multivariate three-level model. Using MetaForest, familiarity with target was the second least important variable, with positive variable importance. We conclude weak support for this moderator, though not in the expected direction.

Role Responsibilities for targets.

Seven studies that employed scenarios with clear role responsibilities for targets had an effect size of g = -0.05 [-0.42, 0.32]. Seventeen studies with studies that had no clear responsibility over the target showed an effect size of g = 0.60 [0.44, 0.76]. Using MetaForest, responsibility over target was the most important variable. The results indicate support for the hypothesis that there is stronger omission bias when people have no clear social role responsibility, compared to when they are in a social role with a clear responsibility.

Self vs Others.

Four studies with self as the target had a mean effect of g = 1.42 [0.60, 2.24] whereas the 25 studies with others as targets had a much weaker effect of g = 0.46 [0.26, 0.65]. With MetaForest, self vs others was the third most important variable in predicting the effect.

Outcome.

Thirteen studies with negative outcomes had a mean effect of g = 0.71 [0.42, 1.01]. Thirteen studies with no clear negative outcome or neutral outcome also had a weaker effect size of g = 0.30 [0.02, 0.58]. With MetaForest, the presence/absence of clear negative outcomes was the second most important variable. This indicates that it is likely there would be a stronger effect for studies with negative outcomes compared to studies with no clear negative outcome.

Study design.

We analyzed nine between-subject studies and found an effect size of g = 0.43 [0.17, 0.70]. Sixteen within-subject studies had a mean effect size of g = 0.59 [0.28, 0.91]. Using MetaForest, even though study design was the least important variable, it has positive variable importance. We conclude weak support for this moderator, though in the expected direction.

Discussion

Omission bias: Summary of findings

We provided a summary of our meta-analysis findings in Table 5. The meta-analysis findings were in support of an omission bias with a mean effect of g = 0.45 [0.14, 0.77], though we note there were indications of a publication bias and the number of studies we were able to identify was surprisingly low.

This suggests one of several insights. First, despite three decades of research on omission bias and being a well-cited and reputable phenomenon, we required more research to allow for stronger conclusive evidence for omission bias, and to address the possible issues of publication bias, using solutions such as Registered Reports, crowdsourced replications, and/or adversarial collaborations.

Second, our moderator analyses suggest that omission bias is affected by several moderators, raising the need for a follow-up systematic investigation of the promising moderators we identified and tested in this meta-analysis. We discuss those in more detail below.

Furthermore, we found that the omission effect was meaningfully different from null for morality and blame as dependent variables. The effect confidence intervals for decision overlap with null.

Table 5

Summarized results of the meta-analysis

Measure/ Moderator	Hypothesis	Result
Morality, Blame, and Decision as DVs	H1a for morality: People judge harm through action as less moral, compared to harm through inaction.	H1a for morality: Supported. g = 0.71 [0.47, 0.94]
	H1a for blame: People judge harm through action as more morally responsible/blameworthy, compared to harm through inaction.	H1a for blame: Supported. g = 0.32 [0.01, 0.64]
	H1b: People prefer inactive decision over active decision given a possibility of harm	H1b: Not supported. g = 0.30 [-0.62, 1.21]
Familiarity with target	H2a: Omission bias is stronger for more familiar targets	H2: No evidence for H2b, weak evidence for H2a.
	H2b: The omission bias effect is stronger for less familiar targets (Based on Haidt & Baron, 1996). (Not pre-registered)	
Responsibility for target	H3a: The omission bias effect is stronger the higher the perceived responsibility for the target.	H3b: Supported.
	H3b: The omission bias effect is stronger given unclear or lower social role responsibility for the target	
Self-Other	Exploratory not pre-registered: The omission bias effect is stronger when the target is self, compared to when the target is others.	Support for moderation.
Negative outcome information	H4: There is an omission bias effect even when outcomes are not negative (null not included in confidence interval).	H4: Supported.
	H5: The omission bias effect is stronger when outcome is negative compared to when it is not negative (neutral or no information).	H5: Mixed support, likely moderation. Supported in MetaForest and the two-level model, not supported in three-level.
Study design	Competing H6a: The omission bias will be stronger for studies using a within-subject design.	H6a: Mixed support, likely moderation. Supported with MetaForest, not supported in the two-level or the three-level models.
	Competing H6b: Study design will have no impact on the omission bias.	

Note. "likely moderation" concluded based on MetaForest, which uses bootstrapping to compensate for low power.

Omission Bias: Effects of Different Dependent Variables

We found support for omission effect in third-person morality ratings and blame of others' behavior yet failed to find support for a reliable effect for decisions (confidence intervals overlapped with the null). The lacking support for decisions may be attributed to the following: 1) low power with only four samples, 2) findings in the opposite direction in the context of vaccination (Connolly & Reb, 2003).

We therefore note caution regarding drawing any strong conclusions regarding the effect of decision based on the limited set of studies included. It appears that the effect of omission bias in first-person decision making is less reliable and less consistent, as our decisions are related but not simply influenced by moral judgments. We are unable to draw strong inferences regarding the causes or mechanisms regarding decisions based on our small sample of included studies (causal chain being explicit, Baron & Ritov, 2004; vaccination context, Connolly & Reb, 2003; differences between decisions and judgments). More research is needed to address this debate, and we see special value in conducting a large-scale investigation of this research question, preferably involving an adversarial collaboration in the form of a Registered Report.

Moderators

Familiarity with target.

Previous studies (e.g., Baron & Ritov, 2004; Haidt & Baron, 1996) demonstrated that familiarity with the target affects omission bias. Specifically, omission effect was weakened when actors had high solidarity with the target (Haidt & Baron, 1996). We refrain from drawing strong conclusions based on such uncertain null findings, yet identified differences in the opposite direction may be due to some unknown confounding variables or artifacts of some experiments. Another possible explanation for the confusing findings is that in Haidt and Baron (1996) study Experiment 1 and Experiment 2, the affected targets are friends but

not family members. We included studies in which affected targets are friends but also studies in which affected targets are family members (e.g., children or spouse). It is possible that the weakening of omission bias is only constrained to situations in which the affected targets are friends, but not family members. It is also possible that the role of familiarity in omission bias is minimal, or in the opposite direction to Haidt and Baron (1996) findings. We believe there is value in conducting more well-powered replications of the classic findings in omission bias (notably Haidt & Baron, 1996) with extensions testing different levels of familiarity.

Responsibility over target.

The findings support the hypothesis, based on Haidt and Baron (1996), that omission bias is weakened given clear social role responsibility, in contrast to situations in which the decision-maker has no clear social role responsibility. This may be because when people have no clear responsibility for the target, they are not expected or required to act, thereby exhibiting omission bias, which means they are more likely to choose inaction or judge inaction less harshly than action.

Target: Self vs Others.

We found support for omission bias regardless of whether the affected target was not the self, different from the findings of Zikmund-Fisher et al. (2006). They found that most participants prefer active treatments when the affected target is others. Moreover, our findings that omission bias is stronger when affected target is self is consistent with studies in medical decision making (Janssen et al., 2015; Mendel et al., 2010; Ubel et al., 2011), which do not specifically contrast action versus inaction. However, we note that the number of studies where self is the target was very limited, with only four samples from two articles in our meta-analysis. We call for more studies, perhaps through direct and conceptual replications and more follow-up studies with self as target and others as target to compare.

For example, in the context of a pandemic, parents need to decide whether they would vaccinate themselves, and decide for their children as well. Furthermore, more work is needed to investigate the purposed mechanisms and factors (anticipated emotions, social role responsibility, cost-benefit analysis) if such effect is meaningful.

Presence/Absence of Negative Outcome.

Our findings indicated support for omission bias regardless of outcome valence, which is consistent with a recent replication (Jamison et al., 2020) of Spranca et al. (1991), and supported the notion that omission bias is stronger when the outcome is negative. In real life, as outsider observers, we are more likely to be informed about the decision if the outcome is negative compared to when the outcome is neutral. This may imply outcome bias in omission bias occur frequently in real-life settings, from daily life judgments to judgments in legal settings. Our findings are in line with findings of outcome bias in ethical judgments (Gino et al., 2009).

Before making a decision, there is no way of knowing for sure if the outcome will be negative, neutral, or positive. This may partly explain why omission bias findings are inconsistent and may be weakened or even reversed into general tendencies towards action, compared to outsider judgments of omission vs commission.

More studies using different moral scenarios with different measures, including binary decisions and third-person judgments, under different kinds of outcome information (positive outcome, no outcome information, neutral outcome, negative outcome) are needed to verify such notions and compare the effects more systematically in a well-controlled manner.

Study design.

In the domain of action-effect research, some (e.g., Connolly & Reb, 2001; N'gbala & Branscombe, 1997) have found that between-subject studies did not replicate the within-

subject findings in Kahneman and Tversky (1982) investor scenario successfully. We found meaningful effects even using between-subject design, though we note mixed support for differences in effect size between designs. We call for more research to adopt both within design and between design (e.g. Anvari et al., 2021; Ziano et al., 2021a, 2021b), compare effects in different settings, and comparing effects of judgment, emotions, and decisions of action-inaction-related effects using joint evaluation versus separate evaluation.

Both within and between designs have their merits. Jamison et al. (2020) adjusted to between-subject design as it can test the generalizability of omission bias to everyday life situations, with incomplete information regarding behavior and outcomes, as most times people do not witness all possibilities of a situation. Spranca et al. (1991) major rationale behind using a within-subject design was to hold intentionality constant, as in a between-subject design, people may infer differently about intentionality of commission and omission, as pointed out by Jonathan Baron in his peer review of Jamison et al. (2020). See Table 6 for a comparison of within-subject studies versus between-subject studies.

Table 6

Comparison Between Within-Subject Studies and Between-Subject Studies

	Within-Subject Design	Between-Subject Design
Usage	All participants compare or evaluate two options or decisions displayed together.	Participants are randomized into two or more conditions. One group evaluates one option or one decision whereas another group evaluates a different option or decision.
Example in Omission Bias	Evaluating morality ratings of both omission and commission (Spranca et al., 1991 Study 1). Omission refers to not saying anything to prevent despite knowing the tennis match opponent is allergic to the salad. Commission refers to recommending the tennis match opponent to eat the salad which he would be allergic to. g = 1.29 [0.60, 1.98], larger effect.	Evaluating morality ratings of either omission or commission (Jamison et al., 2020 Scenario 1; replication of Spranca et al., 1991 Study 1). g = 0.46 [0.26, 0.66], smaller effect.
Merits	1) Control for contextual factors or other confounding variables, such as intention in omission bias 2) Higher statistical power	1) More generalizable to real-life situations where people only evaluate one of the options or one of the decisions. We may not be exposed to all possibilities. 2) Reduces risks of demand effects
Key Issues or Debates	1) What effects demonstrated in within-subject design are replicable in a between-subject design? More replications with methodological adjustments of action-inaction literature are needed. 2) What phenomena show weaker effects with between-subject design compared to within-subject design? Future omission bias articles, and other Judgment and Decision Making research, can include both within-subject design and between-subject design. 3) Are there / how large are demand effects using within-subject designs? Charness et al. (2012) argued that demand effects are more likely using within-subject designs. However, some studies (e.g. Lambdin & Shaffer, 2009; Mummolo & Peterson, 2019) found that participants are very unlikely to	

guess the hypotheses of within-subject studies correctly or very unlikely to adjust their behavior to fit the researchers' expectations. Future omission bias studies should ask participants if they can guess the purposes or hypotheses and exclude participants who guess correctly or measure demand effects (De Quidt et al., 2018).

Reversal to Commission "Bias" / Effect[4] in Some Studies? Contexts, Expectations, Justifiability, Normality, and Individual Differences

Even though our study found an overall effect of omission bias, omission bias is not universally applicable to all contexts and all individuals. As mentioned above, omission bias was weakened by several moderators.

Furthermore, preferences may shift towards commission under some circumstances, a point that both Baron and Ritov (2004) and Connolly and Reb (2003) seemed to agree on. Connolly and Reb (2003) found that the majority of participants preferred vaccination vs non-vaccination, using choice measures. This is consistent with Zikmund-Fisher et al. (2006), which also used choice measures, yet contradicts most omission bias findings in vaccination that used risk-balancing procedures (e.g., Asch et al., 1994; Ritov & Baron, 1990), and also choice measures (e.g., Baron & Ritov, 2004). Moreover, Connolly and Reb (2003) argued that general tendencies for action versus inaction have limited generalizability to decisions regarding vaccinations. The method (binary choice vs risk balancing procedures) and context (vaccination) may influence whether omission bias or general tendencies towards commission is found. There may also be temporal and cross-cultural constraints of generality (Simons et al., 2017) regarding vaccination (e.g., Seo & Lim, 2018; Walsh et al., 2016). Unfortunately, in the recent decade, there are very limited studies tying vaccination decisions

[4] We note that the preference for commission instead of omission may be beneficial in some contexts (in those contexts, the term "effect" may be more appropriate), but may have disadvantages in some contexts (in those contexts, that may be a "bias"). However, this is not a major objective of our meta-analysis.

with omission bias, which is a great loss given how important this phenomenon is in the ongoing pandemic.

The context may affect whether it is actions or inactions that are more justified and socially expected. If an action is justified (Connolly & Reb, 2003) and expected (Bar-Eli et al., 2007; Baron & Ritov, 2004; Ritov & Baron, 1994), then there may be stronger preferences towards taking action than inaction. There is much promise in further studies on action versus inaction decision in moral scenarios looking at morality measures, and examining moderators regarding the role of justifiability, expectations, and normality.

Whether an action or an inaction is justified is associated with whether the decision-maker expects to feel regret for an action or an inaction (Connolly & Zeelenberg, 2002). Individual differences studies in omission bias/action-bias sometimes have so far yielded inconsistent results (e.g., Chung et al., 2014), and sometimes even contradictory findings (Ritov & Baron, 1999 versus Tanner & Medin, 2004). We believe that there is much need of revisiting these findings, attempting to resolve such discrepancies, and investigating the role of individual differences in the omission bias.

Implications, limitations, and directions for future research

Our meta-analysis is not without limitations. Below we discuss some of the issues encountered while conducting the meta-analysis.

First, during the meta-analysis coding process, we experienced difficulties in analyzing statistical information to calculate effect sizes. We had to exclude several studies due to insufficient statistical information. In addition, we excluded studies that had no clear contrast between action and inaction. As a result, the number of included studies in the meta-analysis is relatively low. Despite the tremendous implications of omission bias in real-life decision making, especially in health-medical settings, there are unfortunately very limited

studies testing binary decisions with clear action-inaction contrasts. We call for more studies testing binary decisions with the action-inaction framework.

Second, the number of studies included in the meta-analysis was too small to reliably test moderators using traditional moderator analyses, likely leading to less accurate effect size estimates.

Our findings using the different moderator methods were mixed for some of the moderators. Given the low power, we consider MetaForest as the most appropriate. We refrain from drawing strong conclusions regarding some analyses (especially regarding decision as dependent variable and study design moderating effect). We hope to see more work done in this domain, hopefully employing large samples and data sharing, conducting replications to revisit classic omission bias findings, and using pre-registrations and registered reports format. Furthermore, given the debates over this effect, we see great value in adversarial collaborations (see examples: Bateman et al., 2005; Mellers et al., 2001). These may help address some of the inconsistencies and contradicting findings in the omission bias literature, such as the vaccination decision debates between Connolly and Reb and Baron and Ritov. Such inconsistencies in findings, sometimes with findings in opposite direction, may be due to differences in methods (binary choice versus risk balancing methods). Furthermore, regarding the debates about study design (within-subject design versus between-subject design), adversarial collaborations may be helpful in resolving disagreements and gaining a better understanding of the differing perspectives.

Finally, the vast majority of included studies were from Western, Educated, Industrialized, Rich and Democratic (WEIRD) countries (Henrich et al., 2010), especially United States (Cheon et al., 2020), except Hayashi (2015) in Japan as well as Baron and Miller (2000) in India. There have been some cross-cultural studies in action-inaction attitudes and goals (Ireland et al., 2015; Zell et al., 2013) and action-effect (Chen et al., 2006;

Gilovich et al., 2003), but less have been done cross-culturally on the related omission bias focusing on action and inaction in domains of morality and harm. We call for more cross-cultural multi-lab collaborations to test the generalizability of omission bias (such as Psychological Science Accelerator, see Moshontz et al., 2018).

In summary, we call for conducting more studies on omission bias. Further studies are needed to better understand action-inaction decisions in situation involving possible harm, especially examining important real-life implications such as vaccination decisions. Future research may follow with a systematic investigation of moderators such as expectations, justifiability, normality, length of casual chain, individual and cultural differences, and examining robustness and generalizability using different methods, measures, and contexts.

Conclusion

We conducted a meta-analysis and found support for an omission bias yet note possible publication bias. Our moderator analyses showed strong support for responsibility over target, and mixed/some support for outcome, self vs other, and design type as moderating variables of the omission bias. The number of studies we found was small and so we hope that this meta would drive further research in this domain, and call for more pre-registered replications and extensions and follow-up work. We also call for adversarial collaborations using Registered Reports to help resolve mixed findings in the literature.

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Meta-analysis of the Omission-Bias
Supplementary Materials

Open Science Disclosures

Procedure and Data Disclosures

Search Procedure

Search patterns using Google Scholar: ("omission bias" OR "omission effect" OR "omission strategy" OR "action effect" OR "action principle" OR (Baron AND Ritov) OR (Connolly AND Reb) OR (action AND inaction) OR (actions AND inactions) OR (omission AND commission) OR (omissions AND commissions) OR (direct AND indirect) OR (doing AND allowing) OR (active AND passive)) AND (morality OR intent OR harm OR vaccinations OR moral OR blame OR responsibility) – "Trolley".

2570 articles were found through the primary database search with Google Scholar. Then 14 articles were found through searching through contacting authors in the field of omission effect, mailing lists, and calling for unpublished findings in ResearchGate. The last time searched with Google Scholar was 2017, and we contacted authors in both 2017 and 2020 June. After deduplicating, we are left with 124 articles. See the main manuscript and the flow diagram for details. We excluded 48 articles after Abstract screening.

Study Exclusions

Inclusion Criterion:

We only included articles in which the independent variable (IV) had a clear contrast between omission (inaction) and commission (action) and the dependent variable(s) (DV) was/were harm/morality related or a decision between possible harm through action and possible harm through inaction. This included evaluations of morality wrongness, immorality, moral acceptability, responsibility, blame, and/or decisions.

Exclusion Criteria:

1) Only articles focusing on the omission/action bias were included. This implies that articles that focus on the action-effect, status quo bias, norm-theory, or related but distinct phenomenon were excluded.
2) We focused on experimental studies. This means that we excluded correlational studies and other designs.
3) We only included studies with adequate statistical information for computing the effect size for a *contrast effect between omission and commission*. In cases of missing statistical data (e.g. mean or standard deviation for calculating Hedge's g, counts/proportions for omission and commission), the articles were excluded, even if all other search criteria were met.
4) We focused on the comparison of morality, responsibility, and/or decision for a single target instead of the comparison of harm to different people or groups of people. This

means that trolley dilemmas studies or similar philosophical moral dilemmas were excluded.

A total of 120 studies (63 articles) were excluded based on the above criteria, in which 5 studies (2 articles) did not meet criterion 1, 93 studies (31 articles) did not meet criterion 2, 15 studies (7 articles) did not meet criterion 3, 20 studies (8 articles) did not meet criterion 4. Some studies do not meet more than one criterion. 15 articles do not meet the inclusion criterion or are review/theoretical articles.

We documented reasons behind decisions for excluded studies in Omission-Bias-Coding-Sheet-Meta-v5-W.xlsx – tab: Coding decisions. We included 13 articles, which includes 21 samples, 4 with two DVs, and 49 scenarios/subgroups. Reporting

There are no other unreported/unlinked pre-registrations for this meta-analysis project.

Quality Control and Assurance

How do you ensure the quality and reproducibility potential of all conducted in this meta-analysis?

Studies were coded by one author, determining the adequacy of the study for the meta-analysis and coded the studies, and two other authors verified and adjusted the coding.

Conflicts of Interest

There is no conflict of interest to report.

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Preregistration

Note: There were minor English errors in the pre-registration in 2017 so we made a few very minor changes below. We also changed the font to Times News Roman to unify with texts throughout the supplementary, double-spaced the texts, and made minor style changes. However, we made no change in the contents below.

Background

Early data from the 90s in the USA showed that when faced with a decision between not vaccinating a child against the flu and risking death from flu and vaccinating a child and risking death as a result of drug side-effects parents often chose not to vaccinate. This phenomenon was termed the omission bias, that when faced with a dilemma between taking action and doing nothing that may result in negative outcomes, people often prefer harm that is caused through inaction to harm that is caused through action and therefore choose not to act. This was linked with an "action principle" that harm through action is often judged as morally worse than harm through inaction (Baron & Ritov, 1994; Kordes-de Vaal, 1996). Although there is much evidence to suggest the omission bias is a real phenomenon (Ritov & Baron, 1990), there have also been studies with contradictory findings that found no conclusive evidence for the omission-bias (e.g., Connolly & Reb, 2003). The mixed findings raise the need for a meta-analytic review of the existing literature and evidence.

Goals and research questions

Goal statement

In this meta-analysis, we examine the impact of action/inaction (commission/omission) on perceptions of responsibility or morality. We aim to determine the overall weighted effect-size (and whether confidence intervals do not include the null) and examine possible factors that moderate the effect.

Research questions

1. Is there evidence for the omission bias? Meaning, is there a (positive or negative) effect of inaction/action asymmetries on attributions of morality and harm (null not within confidence intervals for the effect)?
2. How strong is the effect? What is the overall effect size for the omission bias?
3. What factors affect the omission bias?

Hypotheses

Main hypothesis

The main hypothesis for the meta-analysis main effect:

Hypothesis: The omission bias effect: People attribute higher moral responsibility and blame to harm done through action than to harm done through inaction, all other things being equal (null not included in confidence intervals).

Moderators

Familiarity with target

The greater the responsibility, the more likely people are to be sensitive to harming others. Haidt and Baron (1996) showed that with close others (e.g. friends or family members) there is an even stronger preference for omission over commission.

Familiarity coding: not familiar with the target; familiar with the target but not close (family/friends); familiar and closely related to target (family/friends); rating self; familiarity not specified.

Hypothesis: The omission bias effect will be stronger the more familiar the person is with the target.

Responsibility for target

Related to familiarity, but focuses on responsibility for the target. The stronger the responsibility a person feels she/he has for the target person, the more likely they are to be sensitive to harm, and we therefore expect that they will demonstrate even stronger omission bias.

Coding: No responsibility over target; responsibility over target; rating self; 99 = undetermined.

Hypothesis: The omission bias effect will be stronger the more the person feels responsible for the target.

Outcome valence

Spranca et al. (1991) suggested that harm outcome moderates evaluations of harm and responsibility, that people evaluate outcomes, not only decisions so that there would only be an omission bias when decision indeed led to a bad outcome. In our replication, we also found an effect when there was no harm done but that the effect was weaker, suggesting that (1) there is an omission bias in decisions that may lead to harm even if there was eventually no harm done, (2) outcome harm moderates the omission bias.

Coding: -1 = negative outcome; 0 = no negative or positive outcome; 1 = positive outcome; 99 = undetermined.

Hypothesis: There is an omission bias effect when the outcome is not negative (null not included in confidence intervals).

Hypothesis: The omission bias effect will be stronger when the outcome is negative.

Study design

Connolly and Reb (2003) criticized earlier results for the omission bias. They claimed that between-subject studies do not replicate within-subject studies (e.g., Connolly & Reb, 2003; N'gbala & Branscombe, 1997). Baron and Ritov (2004) tried to address this criticism, and the debate is ongoing. Therefore, we test competing hypotheses regarding the impact of study design (within vs. between-subject design) over the omission bias.

Coding: 0 = between-subject design; 1 = within-subject design; 99 = undetermined.

Competing hypothesis 1: The omission-bias will be stronger for studies using a within-subject design.

Competing hypothesis 2: Study design will have no impact on the omission-bias (criteria 1: z-test comparisons not significant; criteria 2: 95% confidence intervals overlapping)

Outcome information availability

Frisch and Baron (1988) hypothesized that missing information about the decision in a current situation motivates people for omission. In one experiment, Ritov and Baron (1990) showed that greater statistical information about a situation led to weaker omission bias in the participants.

Coding: 0 = no statistical information about precise statistical odds provided; 1 = statistical information about precise statistical odds is provided; 99 = undetermined.

Hypothesis: The omission bias effect will be stronger when the decision-maker does not have precise information about statistical odds.

Harm specified for action-inaction

Some studies specify outcome odds (see "Outcome information availability"), and of those, some specify the odds so that chances for harm through omission are greater than, equal to, or smaller than chances for harm through commission.

Coding: Chances for harm through omission – chances for harm through commission.

Hypothesis: There is an omission bias effect when there are no differences in odds (null not included in confidence intervals).

Hypothesis: The omission bias effect will be stronger the greater the chances for harm through commission.

Additional 2nd priority moderators

These demographic moderators are the secondary priority, depending on project time constraints.

Age

Ritov and Baron (1999) found that older people demonstrate stronger omission bias. We will code the population age, code high and low based on above 40 and below 40 as default, but will also explore age distributions of study sample age means to determine the optimal cut off point

Hypothesis: The omission bias effect will be stronger for older sample populations compared to younger sample populations.

Cultural differences

There is some evidence to suggest that the omission-bias is culture-sensitive (e.g., Haidt & Baron, 1996). For example, Miller et al. (1990) showed that US students were more affected by familiarity compared to Indian students, meaning that Indians exhibited omission-bias regardless of familiarity since Indians tend to be more inclusive and perceive even strangers as close as familiar.

We will code samples by country of origin and will conduct cross-cultural analyses based on the Hofstede 5 culture dimensions and tightness-looseness.

We hypothesize the following based on the idea that omission-bias will be stronger in more inclusive cultures.

Hypothesis: The omission bias effect will be stronger the higher the country is on the collectivistic culture dimension.

Hypothesis: The omission bias effect will be stronger the lower the country is on the power-distance culture dimension.

Hypothesis: The omission bias effect will be stronger the lower the country is on the muscularity culture dimension.

Methods

Design

1. Independent variable:
    a. Omission (inaction, do nothing) or commission (take action)
2. Dependent variables:
    a. Choice/decision for action versus inaction
    b. Evaluations of the action versus inaction
    c. Morality/harm/blame/responsibility/intent rating of a negative outcome through action or inaction behavior
3. Moderators:
    a. See the hypotheses section above for explanation and coding.

Search Strategy

- Database: Google Scholar (for suitability for meta-analyses see Gehanno et al., 2013; Walters, 2007).
- Search patterns:
    - ("omission bias" OR "omission effect" OR "omission strategy" OR "action effect" OR "action principle" OR (Baron AND Ritov) OR (Connolly AND Reb) OR (action AND inaction) OR (actions AND inactions) OR (omission AND commission) OR (omissions AND commissions) OR (direct AND indirect) OR (doing AND allowing) OR (active AND passive)) AND (morality OR intent OR harm OR vaccinations OR moral OR blame OR responsibility) – "Trolley"
- A scan of reference sections of found articles
- Search for "related articles" and "cited by" Google Scholar options of the identified articles
- Contacting authors of identified articles to ensure full coverage and maximize access to unpublished data and/or manuscripts
- Abstracts, tables, and methods sections will be scanned to identify the relevance of a source.

Inclusion criteria

1. Articles that refer to the omission bias and action principle
    a. Context: Issues of morality of potential harm to self/others.
    b. IV: Clear contrast/comparison between action and inaction

    c. DV: Evaluations of morality/harm related DV
    d. DV: or a decision between (possible) harm through action and (possible) harm through inaction.
2. Empirical studies, experimental designs only
3. Harm/morality/decision is compared for a single or the same target(s), not between different people or groups of people (e.g., trolley)
4. Studies that match IV DV specifications and search criteria above

As we code, we will also make a decision regarding the following criteria:

1. Odds of action/inaction are not fully determined, involves a degree of uncertainty

Exclusion criteria

1. Action-effect/status-quo/norm-theory studies
    a. IV: Studies that are only about exception-routine or status-quo contrasts or the IV does not involve possible harm or a moral dilemma.
    b. DV: Studies that are only about regret/counterfactuals or DVs that do not relate to moral issues involving harm or moral dilemma.
2. Trolley dilemmas and similar:
    a. Harm/morality/decision is compared between different targets, not between different people or groups of people
3. Missing statistics are not reported:
    a. Studies that do not report crucial measures such as mean or standard needed for the calculation of the effect size deviation will be excluded from the sample.
4. Correlational designs

As we code, we will also make a decision regarding the following criteria:

5. Odds of action/inaction are fully determined, no uncertainty

Procedure for studies selection

Studies collected through the database searches will be assessed for their eligibility based on their titles, abstracts, and contents. One researcher will determine the adequacy of the study for the meta-analysis and a second researcher will do the verification of the results. All the decisions to exclude a study will be documented with reasons.

All decisions on inclusion and exclusion will be documented in any case.

Data extraction (coding)

- The coding sheet and codebook are attached

- The coding process for the pretests will be completed by two coders to ensure a high inter-rater-reliability. Gaps identified will be documented and decisions will be reported in detail.
- Once the pre-test is completed, one coder will code all studies, then 1 will verify coding.

Analysis plan

Specific

Priority

We're dealing with two types of studies:

1. Decisions between possible harm through action and possible harm through inaction: which would they choose
2. Ratings: Rating action and rating inaction on various proxies of a decision (morality, blame, responsibility, etc.)

We will focus on and prioritize decisions (#1).

General

We will use R and the metafor package for statistical analyses. Given the range of different types of studies and experimental designs, we expect heterogeneity in the sample to be relatively high. Therefore, a random-effects model will be used.

All effect sizes will be converted to Cohen's *d* and standardized to allow for a comparison. Split conditions due to moderators in the original studies will be collapsed to allow for a comparison of the main IV.

Whenever available, we will collect standardized effect sizes directly from authors of original papers. We will check for the accuracy of these analyses based on the provided information and details. If unavailable we will use either descriptive statistics or inferential statistics to re-compute standardized effect sizes.

All conversions and coding decisions will be documented and the original text will be included in the coding sheet to allow for reproducibility.

Forest plots presenting the effect size of each study will be produced. A meta-analysis will examine the overall main-effect, a meta-regression will be conducted to examine the impact of the described moderators.

Statistical heterogeneity will be determined using the Tau^2 test and quantified using I^2, which represents the percentage of the total variation in a set of studies that is actually due to heterogeneity (Higgins et al., 2003). This global meta-analysis will yield a point estimate, confidence interval, and p-value, along with statistics for heterogeneity, assessed using the Q-statistics, and the I^2 statistic. If there is indeed significant heterogeneity, we will explore potential moderators

We will report analyses for the presence of publication bias, including funnel plots and statistical tests for publication bias (minimum: publication status as a moderator, compare effects for only published findings) and asymmetry (minimum: trim and fill, rank test, Egger's unweighted regression symmetry test).

We will also conduct a p-curve (Simmons & Simonsohn, 2017; Simonsohn et al., 2014) and a p-uniform test (van Aert & van Assen, 2017).

We aim to share all coding and R code with reviewers and the academic community using the Open Science Framework.

Confirmatory analyses

We will test for the hypotheses detailed in section A "Hypotheses" using a random-effects meta-model.

We plan a-priori to also conduct meta-analyses on subsets of the data, in particular, we will split the data by study design and IV/DV types.

Exploratory analyses

The coding sheet includes many other collected variables. We expect that will conduct additional exploratory on some of these variables, but those will be considered exploratory.

We also expect that additional hypotheses and possible coding moderators will be identified as we examine the papers and collected studies.

In both cases, we will explicitly declare these analyses as exploratory.

Final questions:

Has data collection begun for this project?

- No, data collection has not begun
- **Yes**, data collection is underway or complete

If data collection has begun, have you looked at the data?

- Yes
- No

The (estimated) start and end dates for this project are (optional):

Any additional comments before I pre-register this project (optional):

Conflicts of Interest

There are no conflicts of interest to report.

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Preregistration Versus Final Report Deviations

Table 1

Preregistration Planning and Deviation Documentation

Components in your preregistration (e.g., hypotheses, exclusion rules)	Location of 1) preregistered decision/plan and 2) description for decision/plan [Location / link]	Were there deviations? What type? [no / minor / major]*	If yes - describe details of deviation(s) [brief description / location / link]	Rationale for deviation [brief description / location / link]	How might the results be different if you had/had not deviated [brief description / location / link]	Date/time of decision for deviation + stage	Any additional notes
Title	Pre-registered title: Omission bias: A meta-analysis - see https://osf.io/fj2mx/ pre-registration p. 1 We planned to conduct a meta-analysis of Omission bias.	Minor	Changed to Omission-Commission Asymmetries in Judgments and Decisions: Meta-analysis of the Omission-Bias Location: p. 1 of the main manuscript	Added "Omission-commission" to show that we focus on studies with clear omission-commission contrast; Added "Moral judgments and decisions" to clarify that we focus on judgments and decisions dependent variables.	It does not influence the results. We just clarified our objectives.	08/10/2020 Stage: Manuscript revision and writing	N/A
Registration	Pre-registration link: https://osf.io/fj2mx/, or p. 8 to p. 23 of this supplementary materials document We pre-registered our study in Open Science Framework. Our pre-registration includes hypotheses, method, and analysis plan.	No	NA	NA	NA	NA	NA
Support	N/A	N/A	N/A	N/A	N/A	N/A	N/A

Components in your preregistration (e.g., hypotheses, exclusion rules)	Location of 1) preregistered decision/plan and 2) description for decision/plan [Location / link]	Were there deviations? What type? [no / minor / major]*	If yes - describe details of deviation(s) [brief description / location / link]	Rationale for deviation [brief description / location / link]	How might the results be different if you had/had not deviated [brief description / location / link]	Date/time of decision for deviation + stage	Any additional notes
Rationale and Motivations	Main rationale: Mixed findings in omission-bias literature and explore possible moderators Check p. 8 of this document for details.	No	N/A	N/A	N/A	N/A	N/A

Research Questions and Hypotheses	We hypothesized a significant effect of omission bias. There are 8 other main priority moderator hypotheses and 2 second-priority moderator hypotheses. Please check p. 9 to p. 14 of this document.	Minor	Please check the main manuscript p. 9 for the hypotheses of familiarity with target and responsibility for target, which we added after pre-registration. We did not conduct analyses for the 2 second-priority moderators since there were insufficient non-US studies, and most studies did not provide age information. We initially hypothesized that omission bias would be stronger with lower availability of statistical information about outcomes, and omission bias would be stronger when harm through commission is more likely than harm through omission, but did not conduct analyses for statistical odds information and harm for action-inaction moderators due to insufficient studies. Additionally, we conducted moderator analyses for self vs others.	In the pre-registration, we misunderstood Haidt and Baron (1996) finding. We thought they found a stronger omission bias effect for familiar targets and under higher responsibility, but later we realized that they found the opposite – weaker omission effect for more familiar targets and given higher responsibility. In the words of Haidt and Baron (1996), "For actors in high solidarity or authority roles, the moral distinction between acting and omitting was at its minimum." (p. 201, Abstract); "The act-omission difference was also greater in the low-responsibility roles." (p. 201, Abstract)	It does not affect the results.	15/3/2020 Stage: Manuscript revision and writing	We have competing hypotheses for familiarity and responsibility moderator analyses – pre-registered hypothesis of stronger omission bias when familiar and responsible for the target, vs weaker omission effect when familiar and responsible for the target.
Eligibility criteria	Check p. 16 to p. 17 of this document for exclusion and inclusion criteria. In short, we focus on omission effect experiments with clear contrast between omission and commission.	No	N/A	N/A	N/A	N/A	N/A

Components in your preregistration (e.g., hypotheses, exclusion rules)	Location of 1) preregistered decision/plan and 2) description for decision/plan [Location / link]	Were there deviations? What type? [no / minor / major]*	If yes - describe details of deviation(s) [brief description / location / link]	Rationale for deviation [brief description / location / link]	How might the results be different if you had/had not deviated [brief description / location / link]	Date/time of decision for deviation + stage	Any additional notes
Search Strategy	Pattern: (“omission bias” OR “omission effect” OR “omission strategy” OR “action effect” OR “action principle” OR (Baron AND Ritov) OR (Connolly AND Reb) OR (action AND inaction) OR (actions AND inactions) OR (omission AND commission) OR (omissions AND commissions) OR (direct AND indirect) OR (doing AND allowing) OR (active AND passive)) AND (morality OR intent OR harm OR vaccinations OR moral OR blame OR responsibility) – “Trolley” using Google Scholar. For details, check P.15 of this document.	No	N/A	N/A	N/A	N/A	N/A

Components in your preregistration (e.g., hypotheses, exclusion rules)	Location of 1) preregistered decision/plan and 2) description for decision/plan [Location / link]	Were there deviations? What type? [no / minor / major]*	If yes - describe details of deviation(s) [brief description / location / link]	Rationale for deviation [brief description / location / link]	How might the results be different if you had/had not deviated [brief description / location / link]	Date/time of decision for deviation + stage	Any additional notes
Study Records	Plan: "The coding process for the pretests will be completed by two coders to ensure a high inter-rater-reliability. Gaps identified will be documented and decisions will be reported in detail. Once the pre-test is completed, one coder will code all studies, then 1 will verify coding. ", p. 18 of this document	Major	Second author verified and adjusted the coding. Third author then verified the changes.	Verification of study records is important to detect errors in initial coding.	As mentioned above, "major difference in the main effect size (g = 0.92 -> g = 0.45), due to coding errors, e.g. within-subject effect sizes, and some key changes in moderator analyses results (check p. 74 to p. 77 for initial manuscript moderator analyses, and p. 18 to p. 28 of the manuscript)", p. 25 of this document	28/10/2019 to 10/2020 Stage: Verification of study records	N/A
Data Items	Check https://osf.io/bt83p/ (pre-registered coding sheet) for data items. Key data items include but not limited to post-attrition sample size, mean and standard deviation, t-statistics, F-statistics, reported Cohen's d (for studies using scales), proportion and count of omission and commission	No	N/A	N/A	N/A	N/A	N/A
Risk of bias in individual studies	N/A	N/A	N/A	N/A	N/A	N/A	N/A

Data Synthesis	"All effect sizes will be converted to **Cohen's d** and standardized to allow for a comparison.", check p. 19 of this document for details. We did not explicitly mention in the pre-registration but we planned to conduct two-level analyses. We also planned to conduct a meta-regression for moderators.	Major	First, effect sizes were finally converted to Hedges' g. Second, apart from two-level analyses, we also conducted multivariate multilevel analyses. Third, since there are moderators with missing information, we did not conduct a meta-regression.	We converted to Hedges' g because Hedges' g is more accurate, especially if the sample sizes are small. We included multi-level analyses as it takes account into the dependence of effect sizes of the same article/ authors. We also conducted MetaForest analysis to address limited statistical power of moderator analyses. See the main manuscript p. 20 to p. 22 for details.	The difference between Hedges' g and Cohen's d estimate is minimal. There are some discrepancies in results between multivariate multilevel p-values and two-level p-values, and MetaForest results. MetaForest found all 5 moderators have positive variable importance values. Check the main manuscript p. 19 and p. 20 as well as p. 26 to p. 28 for three-level main effect results and MetaForest moderator results. Also check p. 30 to p. 34 of this document for two-level main effect results and two-level plus three-level moderator analyses results.	Conversion to Hedges' g: late 2017 after pre-registration Stage: Multi-level analyses added in RMD code: 15/03/2020 Stage: Analysis Methods Revision	N/A
Meta-bias(es)	"We will report analyses for the presence of publication bias, including funnel plots and statistical tests for publication bias (minimum: publication status as a	Minor	We added PET and PEESE.	PET and PEESE are commonly used in meta-analysis. The main rationale behind these tests is that in	We reported all publication bias analyses that are pre-registered. We do not	13/03/2020 Stage:	N/A

Components in your preregistration (e.g., hypotheses, exclusion rules)	Location of 1) preregistered decision/plan and 2) description for decision/plan [Location / link]	Were there deviations? What type? [no / minor / major]*	If yes - describe details of deviation(s) [brief description / location / link]	Rationale for deviation [brief description / location / link]	How might the results be different if you had/had not deviated [brief description / location / link]	Date/time of decision for deviation + stage	Any additional notes
	moderator, compare effects for only published findings) and asymmetry (minimum: trim and fill, rank test, Egger's unweighted regression symmetry test). We will also conduct a p-curve (Simmons & Simonsohn, 2017; Simonsohn et al., 2014) and a p-uniform test (van Aert & van Assen, 2017). ", from p. 19 of this document			general, publication bias is stronger with a larger standard error.	particularly prefer PET and PEESE, but it provides additional information. PET and PEESE seem to indicate that, taking account of publication bias, there is no meaningful omission bias. However, as mentioned in the main manuscript, PET and PEESE estimates are not accurate under high heterogeneity Check the main manuscript p. 26 for the results and p. 29 for the discussion.	Analysis Methods Revision	
Confidence in the cumulative estimate	N/A	N/A	N/A	N/A	N/A	N/A	N/A

Notes. Locations should include page number (section), paragraph, line number, or direct quotes (as specific as possible). Where possible, please embed in-document hyperlinks to make browsing easier. *Categories for deviations: Minor - Change probably did not affect results or interpretations; Major - Change likely affected results or interpretations.

The above template is adapted and integrated (for meta-analysis of experimental studies) based on Moreau and Gamble (2020) Meta-analysis templates and materials: Template 9 Protocol Deviations (osf.io/q8stz), as well as van ’t Veer et al. (2019) Preregistration Planning and Deviation Documentation (PPDD) (osf.io/ywrqe).

Two-Level Main Effect Analysis

We initially reported two-level main effects analysis in a draft. However, later we decided that reporting multivariate three-level model results is more appropriate and provides a more accurate estimation of the effect, as a two-level model does not account for the dependence of effect sizes within the same article. Nonetheless, we report the results of the two-level model below. The results are very similar to the three-level model with article as the third level. We found a medium effect with both models.

Random-Effects Two-Level Model Main Effect and Sub Effects

The analysis was based on 21 samples, four of them with two dependent variables, that evaluated the impact of omission (inaction) versus commission (action) over morality judgments and decisions. The analysis of all included studies on the omission bias demonstrated a medium mean effect size (two-level: g = 0.53 [0.31, 0.75]), meaningfully different from zero (the null). The statistical power, based on effect size, average sample size, number of effect size, and heterogeneity is >99.99% (Tiebel, 2018).

Random-Effects Two-Level Model Heterogeneity

Due to differences in methods and sampling characteristics within our included sampling studies, we expected heterogeneity to be very high. In the next step, with the two-level model, we did a Cochran's Q test to examine whether variations in the observed effect size were likely to be attributable solely to sampling error. The variation in effect-size was greater than would be expected from sampling error alone ($Q(24) = 194.18$, $p < .001$), indicating that the true effect varied between studies.

Furthermore, I^2 was computed to determine the proportion of variance in the observed effect attributable to the sampling error, independent of sampling size (Higgins et al., 2003). There was high heterogeneity between the studies (I^2 = 88.66%; 75% is regarded as high variability; Huedo-Medina et al., 2006).

Two-Level and Multivariate Three-Level Moderators Analyses

Table 2

Results of two-level and multivariate three-level moderator analyses

Moderator	k	Q	df	g	CI	Difference	2L p	MV 3L *p*
Familiarity with target								
Familiar with target	12	121.27	11	0.58	0.24, 0.91			
Not familiar with target	6	24.85	5	0.28	0.14, 0.41	-0.30	.106	.082
Responsibility for target								
Role responsibility for target	7	59.58	6	-0.05	-0.42, 0.32			
No clear role responsibility for target	17	36.74	16	0.60	0.44, 0.76	0.65	<.001	.065
Self vs Other								
Self	4	63.44	3	1.42	0.60, 2.24			
Other	25	154.67	24	0.46	0.26, 0.65	0.96	.026	.951
Presence/Absence of Negative Outcome								
Clear negative outcome	13	65.94	12	0.71	0.42, 1.01			
No clear negative outcome	13	256.19	12	0.38	0.02, 0.74	-0.33	.157	.878
Study design								
Between subject design	9	33.36	8	0.43	0.17, 0.70			
Within subject design	16	178.28	15	0.66	0.30, 1.01	0.22	.327	.830

Note. k = number of samples; N = total number of individuals in k; mean g = average Hedge's g effect size, CI = lower and upper limits of 95% confidence interval, * $p < .05$, ** $p < .01$, *** $p < .001$, (all two-tailed**);** 2L p stands for two-level analysis p-values; MV 3L p stands for multivariate three-level moderator analysis p-values. MV3L takes into account possible dependence of effect sizes within the same article/authors.

In summary, with both two-level models and multivariate three-level models, we failed to find support for hypothesized moderating effects of familiarity over target, outcome, and

study design. For self vs other, we found support with the two-level model but not the three-level model, which may be because out of the 4 studies for self as affected target, 3 of them came from the same article with similar sample size and highly dependent effect sizes (Hayashi, 2015), failing to find support for a significant difference. For role responsibility for the target, we found support with the two-level model, but the p-value was very close to but didn't fall below .05 with the multivariate three-level model. Please see the below table for the comparison in results with two-level models, multivariate three-level models, and MetaForest.

Table 3

Comparison in Moderator Analyses Results with Different Models

	Two-Level	Multivariate Three-Level	MetaForest
Familiarity with Target	Failed to Support	Not significant	Second lowest in Variable Importance, positive
Responsibility for Target	Supported	Not significant	Highest in Variable Importance, positive
Self Vs Others	Supported	Failed to Support	Third highest in variable importance, positive
Negative Outcome information	Supported	Failed to Support	Second highest in Variable Importance, positive
Study Design	Failed to Support	Failed to Support	Lowest in variable importance, positive

Publication Bias Tests Results and MetaForest Moderator Analyses

We summarized publication bias results and MetaForest analyses in the main manuscript. For further information, please check the knitted document (filename: omission-bias-syntax-markdown-v8a-with-loops.docx) of RMarkdown p. 10 to p. 19 for publication bias results. For detail of MetaForest moderator analyses, please check Please see omission-bias-syntax-markdown-v8a-with-loops.docx P.19-26 for RMarkdown outputs using MetaForest package (van Lissa, 2017).

Published vs Unpublished Moderator Analysis

Additionally, we conducted a moderator analysis comparing the effects for published studies and an unpublished study. It is important to recognize that we only included one unpublished study (Blahunka, 2014) so the results are not useful. See the table below.

Table 4

Publication status moderator analysis

Moderator	k	Q	df	g	CI	Difference	p
Published	24	189.60	23	0.51	0.28, 0.73		
Unpublished	1	Not Applicable	0	1.04	0.46, 1.63	0.54	.095 with 2-level; .268 with 3-level

Note. k = number of samples*; Q* = test for heterogeneity*; g* = average Hedge's g effect size, *CI* = lower and upper limits of 95% confidence interval

Additional Analyses

In the analyses reported in the main manuscript for the main effect and supplementary Two-Level and Multivariate Three-Level Moderators Analyses section, if one study has two or more dependent variables, they are analyzed as separate effect sizes, as shown in the forest plot. We also conducted analyses, in which 1) if one study with the same sample has two or more dependent variables, we combined/collapsed them as one effect size, 2) if one study with the same sample has different scenarios with the respective effect sizes, we treated them as separate effect sizes.

With method 1), the main effect analysis with two-level model showed that g = 0.53 [0.27, 0.79] whereas with multivariate three-level model, g = 0.45 [0.13, 0.76]. With method 2), the main effect analysis with two-level model showed that g = 0.48 [0.34, 0.62] whereas with multivariate three-level model, g = 0.45 [0.14, 0.76]. These effect sizes are similar to those reported in the main manuscript. See Tables 5 to 8 for publication bias results with method 1) and method 2), as well as Tables 9 and 10 for moderator analyses results with method 1) and method 2).

Table 5

Statistical approaches to correct overestimated effect sizes possibly due to publication bias, using Method 1

	Hedge's *g*	95% CI
Trim and Fill (Duval & Tweedie, 2000)	0.36	[0.09, 0.63]
Three-parameter selection (Iyengar & Greenhouse, 1988)	0.36	[-0.07, 0.78]
Henmi and Copas (2010)	0.39	[0.03, 0.75]
P-uniform (van Assen et al., 2015)	0.81	[0.55, 1.05]

Table 6

Other publication bias analyses, using Method 1

Publication bias analysis method	Results
PET (Stanley & Doucouliagos, 2014)	b = 0.02 [-0.59, 0.62]
PEESE (Stanley & Doucouliagos, 2014)	b = 0.18 [-0.18, 0.55]
Rank correlation test (Begg & Mazumdar, 1994)	Kendall's tau = 0.21, p = .197
Sterne and Egger (2005) Regression test	z = 1.85, p = .064
P-curve (Simonsohn et al., 2014)	Evidential value is present, adjusted effect: d = 0.82

Note. Values in parentheses indicate 95% confidence intervals [lower bound, upper bound]

Table 7

Statistical approaches to correct overestimated effect sizes possibly due to publication bias, using Method 2

	Hedge's *g*	95% CI
Trim and Fill (Duval & Tweedie, 2000)	0.48	[0.34, 0.62]
Three-parameter selection (Iyengar & Greenhouse, 1988)	0.30	[0.06, 0.54]
Henmi and Copas (2010)	0.39	[0.25, 0.53]
P-uniform (van Assen et al., 2015)	0.50	[0.41, 0.61]

Table 8

Other publication bias analyses, using Method 2

Publication bias analysis method	Results
PET (Stanley & Doucouliagos, 2014)	b = 0.09 [-0.21, 0.40]
PEESE (Stanley & Doucouliagos, 2014)	b = 0.26 [0.09, 0.42]
Rank correlation test (Begg & Mazumdar, 1994)	Kendall's tau = 0.19, p = .059
Sterne and Egger (2005) regression test	z = 2.52, p = .012
P-curve (Simonsohn et al., 2014)	Evidential value is present, adjusted effect: d = 0.58

Note. Values in parentheses indicate 95% confidence intervals [lower bound, upper bound]

Table 9

Results of two-level and multivariate three-level moderator analyses, with Method 1

Moderator	k	Q	df	g	CI	Difference	2L p	MV 3L *p*
Familiarity with target								
Familiar with target	10	114.23	9	0.58	0.18, 0.98			
Not familiar with target	6	3.60	5	0.28	0.14, 0.41	-0.30	.161	.082
Responsibility for target								
Role responsibility for target	6	57.99	5	-0.10	-0.51, 0.32			
No clear role responsibility for target	14	26.89	13	0.59	0.42, 0.75	0.68	.003	.065
Self vs Other								
Self	4	63.44	3	1.42	0.60, 2.24			
Other	21	141.89	20	0.44	0.21, 0.67	0.97	.025	.951
Presence/Absence of Negative Outcome								
Clear negative outcome	11	59.12	10	0.71	0.37, 1.05			
No clear negative outcome	11	93.79	10	0.30	-0.04, 0.63	-0.41	.090	.878
Study design								
Between subject design	6	23.80	5	0.34	-0.01, 0.68			
Within subject design	15	158.96	14	0.62	0.28, 0.95	0.28	.246	.830

Table 10

Results of two-level and multivariate three-level moderator analyses, with method 2

Moderator	k	Q	df	g	CI	Difference	2L *p*	MV 3L *p*
Familiarity with target								
Familiar with target	17	128.47	16	0.55	0.31, 0.80			
Not familiar with target	11	9.68	10	0.29	0.17, 0.41	-0.27	.055	.082
Responsibility for target								
Role responsibility for target	9	67.87	8	-0.04	-0.36, 0.27			
No clear role responsibility for target	19	41.46	18	0.57	0.42, 0.72	0.62	<.001	.065
Self vs Other								
Self	6	73.64	5	1.04	0.37, 1.72			
Other	43	187.17	42	0.40	0.27, 0.53	0.64	.067	.951
Presence/Absence of Negative Outcome								
Clear negative outcome	21	119.00	20	0.71	0.43, 0.98			
No clear negative outcome	28	117.94	27	0.32	0.19, 0.44	-0.39	.011	.900
Study design								
Between subject design	17	51.03	16	0.40	0.22, 0.59			
Within subject design	32	218.38	31	0.53	0.33, 0.73	0.13	.359	.829

Differences in Results using Method 1), Method 2) and method reported in the main manuscript

First, the main effect results and most moderator analyses results are very similar, and the ranking of moderator variable importance using three methods is the same. However, for the two-level model of self-other moderator, the difference reached significance with Method 1 and the method in Table 2 but just failed to reach significance with Method 2. For the two-level model of outcome moderator analysis, the difference reached significance with Method 2 but failed to reach significance with Method 1 and the method in Table 2. Also, for presence/absence of outcome information, the difference reached significance with the two-level model of Method 2 but did not reach significance with Method 1 or the method in Table 2. With Method 2, the CIs of three-parameter selection adjusted effect size did not overlap the null but the CIs with Method 1 and method in the main manuscript overlap with the null. With Method 2, the CIs of PEESE do not overlap with null whereas the CIs of PEESE with the method in the main manuscript and Method 1 overlap with null. However, PEESE is not reliable when heterogeneity is high. However, despite these differences, in general, the results and their interpretations are very similar.

Publication Bias Results and Moderator Results excluding Connolly and Reb (2003)

Table 11

Statistical approaches to correct overestimated effect sizes possibly due to publication bias, excluding Connolly and Reb (2003)

	Hedge's *g*	95% CI
Trim and Fill (Duval & Tweedie, 2000)	0.53	[0.33, 0.72]
Three-parameter selection (Iyengar & Greenhouse, 1988)	0.51	[0.21, 0.80]
Henmi and Copas (2010)	0.50	[0.28, 0.72]
P-uniform (van Assen et al., 2015)	0.74	[0.48, 1.00]

Table 12

Other publication bias analyses, excluding Connolly and Reb (2003)

Publication bias analysis method	Results
PET (Stanley & Doucouliagos, 2014)	b = 0.21 [-0.17, 0.60]
PEESE (Stanley & Doucouliagos, 2014)	b = 0.35 [0.11, 0.59]
Rank correlation test (Begg & Mazumdar, 1994)	Kendall's tau = 0.23, p = .119
Sterne and Egger (2005) Regression test	z = 1.59, p = .111
P-curve (Simonsohn et al., 2014)	Evidential value is present, adjusted effect: d = 0.70

Note. Values in parentheses indicate 95% confidence intervals [lower bound, upper bound]

Table 13

Results of two-level and multivariate three-level moderator analyses, excluding Connolly and Reb (2003)

Moderator	k	Q	df	g	CI	Difference	2L *p*	MV 3L *p*
Familiarity with target								
Familiar with target	11	24.17	10	0.66	0.48, 0.85			
Not familiar with target	6	3.60	5	0.28	0.14, 0.41	-0.39	<.001	.082
Responsibility for target								
Role responsibility for target	6	9.45	5	0.14	-0.07, 0.34			
No clear role responsibility for target	17	41.46	16	0.60	0.42, 0.72	0.46	<.001	.092
Self vs Other								
Self	4	63.44	3	1.42	0.60, 2.24			
Other	24	63.83	23	0.51	0.36, 0.66	0.91	.033	.982
Outcome information								
Clear negative outcome	13	65.94	12	0.71	0.42, 1.01			
No clear negative outcome	12	18.04	11	0.39	0.30, 0.48	-0.33	.038	.878
Study design								
Between subject design	9	33.36	8	0.43	0.17, 0.70			
Within subject design	15	78.31	14	0.70	0.45, 0.94	0.26	.155	.830

Excluding Connolly and Reb (2003), with MetaForest, presence vs absence of negative outcome information has the highest variable importance (the second highest including Connolly & Reb, 2003), followed by self vs other (the second highest including Connolly & Reb, 2003), responsibility over target (the highest including Connolly & Reb, 2003), design type, and familiarity with target. All 5 moderators have positive variable importance values. Also, excluding Connolly and Reb (2003), the difference between familiar with target studies and unfamiliar with target studies reached significance with fixed effects two-level, which is not significant including Connolly and Reb (2003). The results are mostly similar including or excluding Connolly and Reb (2003).

Adapted PRISMA Checklist for Reporting in Meta-Analysis

Table 14

Checklist for Reporting in Meta-Analysis

Section/topic	#	Checklist item	Brief Description and Reported Location (E.g. The page number of the main manuscript/ the supplementary, the tab name, the file name, with the link if applicable)
TITLE, AUTHOR INFORMATION, and TIME			
Title	1	Identify the report as a meta-analysis for experimental studies in a psychology phenomenon. Specify if the meta-analysis is an updated meta-analysis.	Main manuscript, p. 1
Authors Contact Information	2	Provide name(s), affiliation(s) and email address(es) of the meta-analysis author(s). Ideally, provide the physical mailing address of the corresponding author.	Main manuscript, p. 2
Authors' Qualifications/ Training, Roles, and Responsibilities	3	Provide information regarding authors' research qualifications or training, especially in terms of meta-analyses. Describe specifically and transparently the roles of responsibilities of each author, perhaps with CRediT (Contributor Roles Taxonomy) - https://www.casrai.org/credit.html	Main manuscript, p. 2 and p. 3
Start Date and End Date	4	State the start date and the planned/actual end date of the meta-analysis.	Supplementary, p. 4 - pre-registration date: 5/8/2017, end date: to be provided
ABSTRACT			
Structured summary	5	Provide a structured summary including, as applicable: background; objectives; study synthesis methods; results including main analysis and moderator analysis; conclusion(s)	Main manuscript, p. 4
INTRODUCTION			
Rationale and Motivations	6	Describe the rationale for the meta-analysis in the context of what is already known, controversies, and what is unclear about the phenomenon. Specify if there	Main manuscript, p. 5 to p. 11

		was any prior published meta-analysis on this topic. If there was, justify why a new meta-analysis is needed	
Research Questions, Hypotheses, and/or Exploratory Directions /Analyses	7	Provide explicit statements of questions and hypotheses being addressed, including the hypothesis for the main effect, in terms of the independent variable and dependent variable(s) and hypotheses for moderators analyses. If applicable, discuss the exploratory directions/analyses. Clearly distinguish between exploratory and confirmatory analyses.	Main manuscript, p. 7 to p. 11
METHODS			
Registration	8	Provide pre-registration / registered report information, including links (e.g. Open Science Framework).	Supplementary, p. 4, and p. 8 to p. 23
Eligibility criteria	9	Specify study characteristics (e.g. experimental studies with clear independent variable, dependent variables) and report characteristics (e.g. language, publication status) used as criteria for eligibility, giving rationale.	Main manuscript, p. 15 and p. 16
Information sources	10	Describe all non-database information sources (e.g., contacting authors to identify additional studies, specific journals, calls for unpublished data/papers on forums, social media, mailing lists, and grey literature search) in the search and date last searched.	Main manuscript, p. 12 and p. 13
Database search strategy	11	Present full electronic search strategy for at least one database, including any limits used, such that it could be repeated. If an interface/interfaces will be/was/were used to search the database(s), describe and explain.	Main manuscript, p. 12
Database search validation	12	Ideally, explain the procedure of database search validation. You may use several notable articles of the phenomenon, and check if your search strategy/pattern allows you to find those articles.	Not available
Search expiration and repetition	13	Specify the search expiration date of the meta-analysis, and if applicable, specify	Main manuscript, p. 12

		the number of times in repetition of search.	
Study selection	14	State the process for selecting studies (i.e., eligibility, inclusion/exclusion criteria, screening reliability check, resolution of disagreements, reasons for decisions). Describe qualifications/training and responsibilities of researchers involved in study selection. *(Optional)* Describe and justify the automation tools used	Main manuscript, p. 15 and p. 16
Data collection process	15	Describe the method of data extraction from reports (e.g., training, piloted forms, instructions to extractors, whether the extraction is conducted independently or in duplicate), and any processes for obtaining and confirming data from investigators.	Main manuscript, p. 15
Data items	16	List and define all variables for which data were sought and any assumptions and simplifications made.	Main manuscript, p. 16 and p. 17
Missing information and data	17	Explain procedures in dealing with missing information and data (e.g. effect size or other essential statistics for the transformation of obtaining missing data), and whether the reviewers attempt to contact the original authors.	Main manuscript, p. 15
Data sharing and management	18	Describe methods of managing the data (e.g. Dropbox, Google Drive, Github), list the files and file formats, and state whether the data would be openly available in the repository (e.g. in OSF).	Main manuscript, p. 16; Supplementary, p. 4
Risk of bias in individual studies	19	Describe methods used for assessing risk of bias of individual studies (including specification of whether this was done at the study or outcome level), and how this information is to be used in any data synthesis.	Not applicable
Summary measures	20	State the principle summary measures (e.g., Hedges' *g*, Cohen's *d*).	Main manuscript, p. 17
Synthesis of results	21	Describe the methods of handling data and combining results of studies, if done,	Main manuscript, p. 18

		including measures of heterogeneity (e.g., I^2) for each meta-analysis.	
Risk of bias across studies	22	Specify any assessment of risk of bias that may affect the cumulative evidence (e.g., publication bias). Elaborate and justify the usage of assessment methods.	Main manuscript, p. 18
Priori Power Analysis	23	Conduct a priori power estimation, which is based on the expected effect size, the expected number of studies included, the expected per study sample size, and the expected heterogeneity. Check the A Priori Power Analysis section in the manuscript template. You may use dmetar 0.0.9000 package power analysis function (Harrer et al., 2019, https://dmetar.protectlab.org/reference/power.analysis.html).	Not conducted
Additional analyses/ Planned exploratory analyses	24	If applicable, describe methods of additional or exploratory analyses (e.g., sensitivity or subgroup analyses, meta-regression) that the researchers did.	Main manuscript, p. 25 to p. 27 - MetaForest Supplementary, p. 37 to p. 45 - different methods of collapsing/separating effect sizes, excluding an outlier study
RESULTS			
Study selection	25	Give numbers of studies screened, assessed for eligibility, and included in the review, with reasons for exclusions at each stage, ideally with a flow diagram.	Main manuscript, p. 14 Supplementary, p. 5 and p. 6
Study characteristics	26	For each study, present characteristics (e.g., sample size, country, as well as DV type, moderators categories, etc) and provide the citations, and ideally quotations and explanations.	Omission-Bias-Coding-Sheet-Meta-v5-W.xlsx - Study effects coding tab
Risk of bias within studies	27	If applicable, present data on risk of bias of each study and, if available, any outcome-level assessment (see Item 19).	Not applicable
Results of individual studies	28	For all dependent variables considered, present, for each study: (a) simple summary data for each group (if provided by the authors) and (b) effect estimates and confidence intervals, ideally with a forest plot.	Main manuscript, p. 21 and p. 22

Synthesis of results	29	Present results of the meta-analysis clearly, including effect size, confidence intervals, and measures of heterogeneity, for both main effect and moderator analyses. Please include clear tables, with effect sizes and confidence intervals of each moderator category, ideally with relevant plots.	Main manuscript, p. 18 to p. 23
Statistical power	30	Calculate the statistical power of the main effect analysis.	Main manuscript, p. 19
Risk of bias across studies	30	Present results of any assessment of risk of bias across studies (see Item 22).	Main manuscript, p. 23 to p. 24
Additional/ Exploratory analysis	31	If applicable, provide results of additional or exploratory analyses, if done (e.g., sensitivity or subgroup analyses, meta-regression) (see Item 16). Label clearly whether the exploratory analysis is registered in Stage 1 or added later.	Main manuscript, p. 19 to p. 20, p. 25 to p. 27, three-level model and MetaForest not pre-registered. Supplementary, p. 37 to p. 42
DISCUSSION			
Summary of evidence	32	Summarize the main findings including the strength and certainty of evidence for each main research question and hypothesis; consider the implications.	Main manuscript, p. 27 to p. 35
Limitations	33	Discuss limitations at study level (e.g., risk of bias, insufficient data reported), and at review level (e.g., incomplete retrieval of identified research, publication bias). Also, consider the possible alternative explanations for the results, e.g. confounding variables, limited statistical power.	Main manuscript, p. 35 to p. 37
Future Research Directions	34	Discuss uncertainties, unknowns, and unexplored issues of the phenomenon, and discuss how researchers should tackle those problems, perhaps in terms of methodology, theoretical developments, and practical implications.	Main manuscript, p. 35 to p. 37
Conclusions	35	Provide a general interpretation of the results in the context of other evidence and implications for future research.	Main manuscript, p. 37
FUNDING OR SUPPORT			

Funding	36	Describe sources of funding for the meta-analysis and other support (e.g., supply of data); role of funders for the meta-analysis.	Main manuscript, p. 37
Non-Financial Support / Potential Conflict of Interest	37	Describe any kind of non-financial support and possible conflicts of interest for the meta-analysis, e.g. peer review, if not confidential, and if applicable only. This may include prestige and opportunities.	Not applicable
TRANSPARENCY			
Final Report Deviations From Registered Report	38	Describe deviations of Final Report from pre-registration transparently with justifications.	Supplementary, p. 24 to p. 29
Names, version numbers, and citations of software and packages	39	State the names and version numbers of all software and any packages, with citations, for example, RStudio Version 3.6 (RStudio Team, 2020), Metafor (Viechtbauer, 2010).	Main manuscript, p. 17
Open and Reproducible Code	40	Upload the code/scripts on online repositories, such as OSF and Github, with a persistent identifier such as Digital Object Identifier (DOI). The codes/scripts should be well-annotated and explained clearly so that researchers can reproduce conveniently.	RMarkdown code: omission-bias-syntax-markdown-v8a-with-loops.Rmd and RMarkdown knitted file: omission-bias-syntax-markdown-v8a-with-loops.docx, both will be uploaded to OSF
Record keeping	41	Record clearly and specifically your decision processes during the different stages of the meta-analysis, and then upload the records of decisions as open data, coding sheet, and supplementary materials, in OSF for example.	Supplementary p. 24 to p. 29, Omission-Bias-Coding-Sheet-Meta-v5-W.xlsx coding decisions tab
Inclusion of Studies by the Meta-Analysis Author(s)	42	Declare clearly if any studies done by any author of the meta-analysis will likely be/was/were included in the meta-analysis. If applicable and ideally, address how you would address such possible bias or conflict of interest.	Main manuscript, p. 2

We adapted Yeung et al. (2020) Registered Report template for the purpose of a completed pre-registered meta-analysis. Below information regarding the references is from Yeung et al. (2020) Supplementary template - “The above template is adapted (for meta-analysis of

experimental studies in psychology) from Moher et al. (2009) Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement (https://www.bmj.com/content/339/bmj.b2535/related), which was developed for the field of Medicine, as well as Pickering et al. (2020) Non-Interventional, Reproducible, and Open (NIRO) Systematic Review guidelines v0.1.0 (https://osf.io/f3brw/), which is developed for systematic reviews of non-interventional studies across different fields. We also refer to updated PRISMA by Page et al. (2020) Updated PRISMA, Appelbaum et al. (2018) APA standards, Wong et al. (2013) RAMESES publication standards, as well as Moher et al. (2015) PRISMA-P, van den Akker et al. (2020) Systematic Review Registration Form" (p. 28).

Email for Contacting Authors for Published and Unpublished Data

Dear Dr. [Last name],

We are conducting a meta-analysis on action-inaction asymmetries regarding harm, morality, and affect, including the action-effect (Kahneman & Tversky, 1982), the omission bias (Ritov & Baron, 1990), the action-harm principle (Cushman, Young, & Hauser, 2006), and the omission strategy (DeScioli, Christner, & Kurzban, 2011).

We have completed an initial exhaustive search of the literature and have identified you as an author who has published work on the topic, and so we are contacting you to ask for your unpublished manuscripts and data to be included in the meta-analysis.

We would appreciate references to your published data to make sure we have included it in our meta. More importantly, we are especially interested in any relevant unpublished manuscripts or data that cannot be found using regular literature search.

If you have unfinished or unpublished manuscripts, we would appreciate a copy.

Alternatively, for unpublished manuscripts and/or data, the information we require for inclusion is:

- A description of the manipulation and general description of the experimental conditions.
- For each experimental condition:
- 1- Brief description of the condition
- 2- Sample size
- 3 - Mean and standard deviation for each of the dependent variables
    - Brief description of the measures/scales were used for the dependent variables, and internal reliabilities if available/relevant.
- Sample characteristics, such as: overall sample size, country, sample type (students, MTurk, general population, etc.), mean age.
- Reference to be used when citing this data.

If you only have raw data that has not yet been analyzed, then we would be happy to help analyze it for inclusion. In such a case, please send us the dataset and a description of the key variables described above relevant for the analysis.

Please send all relevant information and/or data to =MASKED=

If you are interested in more information, we will be happy to answer any further questions. Additionally, you can follow updates and read further information on our ResearchGate project: =MASKED=

Best regards,

=MASKED=

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The below reference page includes references for sections apart from the pre-registration. Please see p. 22 for references for the pre-registration in 2017.

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