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Intuitive Honesty Versus Dishonesty: Meta-Analytic Evidence

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Abstract

Is self-serving lying intuitive? Or does honesty come naturally? Many experiments have manipulated reliance on intuition in behavioral-dishonesty tasks, with mixed results. We present two meta-analyses (with evidential value) testing whether an intuitive mind-set affects the proportion of liars (k = 73; n = 12,711) and the magnitude of lying (k = 50; n = 6,473). The results indicate that when dishonesty harms *abstract others*, promoting intuition causes more people to lie, log odds ratio = 0.38, p = .0004, and people to lie more, Hedges's g = 0.26, p < .0001. However, when dishonesty inflicts harm on *concrete others*, promoting intuition has no significant effect on dishonesty (p > .63). We propose one potential explanation: The intuitive appeal of prosociality may cancel out the intuitive selfish appeal of dishonesty, suggesting that the social consequences of lying could be a promising key to the riddle of intuition's role in honesty. We discuss limitations such as the relatively unbalanced distribution of studies using concrete versus abstract victims and the overall large interstudy heterogeneity.

Keywords

unethical behavior, intuition, cheating, lying, ethical behavior, honesty, moral psychology, behavioral ethics

You pay for your $3 cappuccino with a $5 bill. The sleepy cashier mistakenly assumes you have paid with a $20 bill and gives you $17 in change. The person behind you is already eager to order, so time is of the essence. Deciding quickly, do you take the money? Or do you return the undue amount? Almost daily, people face similar temptations to bend ethical rules to serve their self-interest. For example, people may decide to free-ride on public transport or exaggerate the costs of a business trip. When making those decisions, people are often distracted, stressed, or under pressure and thus do not take time to deliberate. Faced with the temptation to lie for profit, what is people's basic inclination: honesty or dishonesty?

Dual-process models provide a useful framework for answering this question. These models postulate that human decision making results from the interplay of an intuitive System 1 that is fast and inflexible and a deliberate System 2 that is slow and flexible (Kahneman, 2011). In recent years, the dual-process perspective has gained popularity in the study of self-serving dishonesty—accruing benefits to the self while violating accepted standards or rules (Shu, Gino, & Bazerman, 2011, p. 330). Results about the extent to which honesty is intuitive are mixed. Whereas some find that people's intuitive response in tempting situations is to selfishly lie, others find honesty intuitive. This is the puzzle we seek to solve.

Intuitive Honesty?

People have the truth in mind, and to modify it they need to exert cognitive effort and craft a lie. This is the logic underlying the prominent cognitive theory that regards truth telling as the more automatic, dominant response and lying as a complex cognitive function that imposes greater demand on cognitive skills (Vrij, Fisher, Mann, & Leal, 2006). Indeed, people react faster when instructed to tell the truth compared with a lie (for meta-analyses, see Suchotzki, Verschuere, Van Bockstaele, Ben-Shakhar,

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& Crombez, 2017; Verschuere, Köbis, Bereby-Meyer, Rand, & Shalvi, 2018); and when instructed to lie, people exhibit heightened activity in the control regions of the brain (Spence et al., 2001). Lying, accordingly, requires cognitive capacity. Indeed, people tend to lie less for their own profit when distracted by a demanding memory task compared with a less demanding task (Van't Veer, Stel, & Van Beest, 2014). Furthermore, people are less likely to send deceptive messages to their counterparts when acting under time pressure compared with no time pressure (Capraro, 2017). These complementary lines of work advocate the following: Honesty is intuitive.

Intuitive Dishonesty?

When people are tired, under time pressure, or doing many things at once (compared with being energized and focused) they are more prone to cave to various temptations, even if those require lying. Being honest and resisting unethical temptations requires self-control (Gino, Schweitzer, Mead, & Ariely, 2011; Tabatabaeian, Dale, & Duran, 2015). This is the logic underlying various lines of recent work. For example, correlational studies have revealed that impulsivity—the tendency to decide intuitively—is positively associated with academic cheating (Anderman, Cupp, & Lane, 2009) and that when people are drained of the cognitive resources required for deliberation they are more likely to engage in workplace deviance (Christian & Ellis, 2011) and unethical behavior (Barnes, Schaubroeck, Huth, & Ghumman, 2011). Experimental work has similarly revealed that restraining participants' deliberate thinking through cognitive load (e.g., Welsh & Ordonez, 2014), time pressure (Shalvi, Eldar, & Bereby-Meyer, 2012), mental or physical depletion (e.g., Kouchaki & Smith, 2014), priming of intuition concepts (e.g., Zhong, 2011), or conducting experiments in a native language (vs. a foreign language; Bereby-Meyer et al., 2018) increases self-serving dishonesty. Together, these findings suggest the following: Dishonesty is intuitive.

Social Harm Moderates Intuitive Honesty and Dishonesty: Evidence From Two Meta-Analyses

Taken together, the question about people's intuitive inclinations in tempting situations in which one can profit from lying remains open. Although a large amount of data is available, the results are mixed. To provide an aggregated overview of existing evidence, we conducted meta-analytical tests on experiments on intuitive honesty and dishonesty. In addition to evaluating whether the aggregated evidence supports the intuitive-honesty-versus-dishonesty hypotheses, we further tested a potential moderation that may explain the expected heterogeneity in results.

Our core moderator of interest is whether negative externalities of dishonesty hurt a concrete other (e.g., another participant) or an abstract, vaguer entity (e.g., the experimental budget). Previous theories have stressed the importance of the social element of unethical behavior, outlining that abstract victims and cobeneficiaries of unethical behavior alleviate guilt (Köbis, van Prooijen, Righetti, & Van Lange, 2016). Empirical support stems from studies indicating that people tend to lie when lying benefits in-group members (Cohen, Gunia, Kim-Jun, & Murnighan, 2009; Weisel & Shalvi, 2015; Wiltermuth, 2011) yet are reluctant to do so when lying harms concrete others (Pitesa, Thau, & Pillutla, 2013 Yam & Reynolds, 2016). Furthermore, a substantial body of work on the social heuristics hypothesis (Bear & Rand, 2016; Rand, Greene, & Nowak, 2012) suggests that intuition favors cooperation over interpersonal selfishness in economic games (for a meta-analysis, see Rand, 2016). Applying this theoretical framework to dishonesty suggests that when lying harms a concrete victim, the intuitive urge to be prosocial may be invoked—which may in turn cancel out (or even overpower) the intuitive appeal of self-serving lies.

Directly testing the social-harm account of intuitive honesty and dishonesty in a series of experiments, Pitesa and colleagues (2013) found intuitive honesty when harm was inflicted on another participant but intuitive dishonesty when the research budget was hurt by people's lies. The moderating role of social harm in determining the direction in which intuition affected dishonesty also fits squarely with the social heuristics hypothesis, proposing an intuitive inclination to cooperate in many social dilemmas (for a meta-analysis, see Rand, 2016).

Method

Search for studies

First, we searched without any restrictions on the publication year Web of Science, PsycINFO, and Google Scholar using the following combinations of the keywords in the first and second brackets with the Boolean operator "OR": ["deprivation" OR "depletion" OR "cognitive load" OR "intuition" OR "priming" OR "time pressure"] and ["cheating" OR "lying" OR "deception" OR "dishonesty" OR "unethical behavior"].

Second, as for other mass-solicitation methods (see Balliet, Wu, & De Dreu, 2014), a call for published and unpublished work was disseminated via various

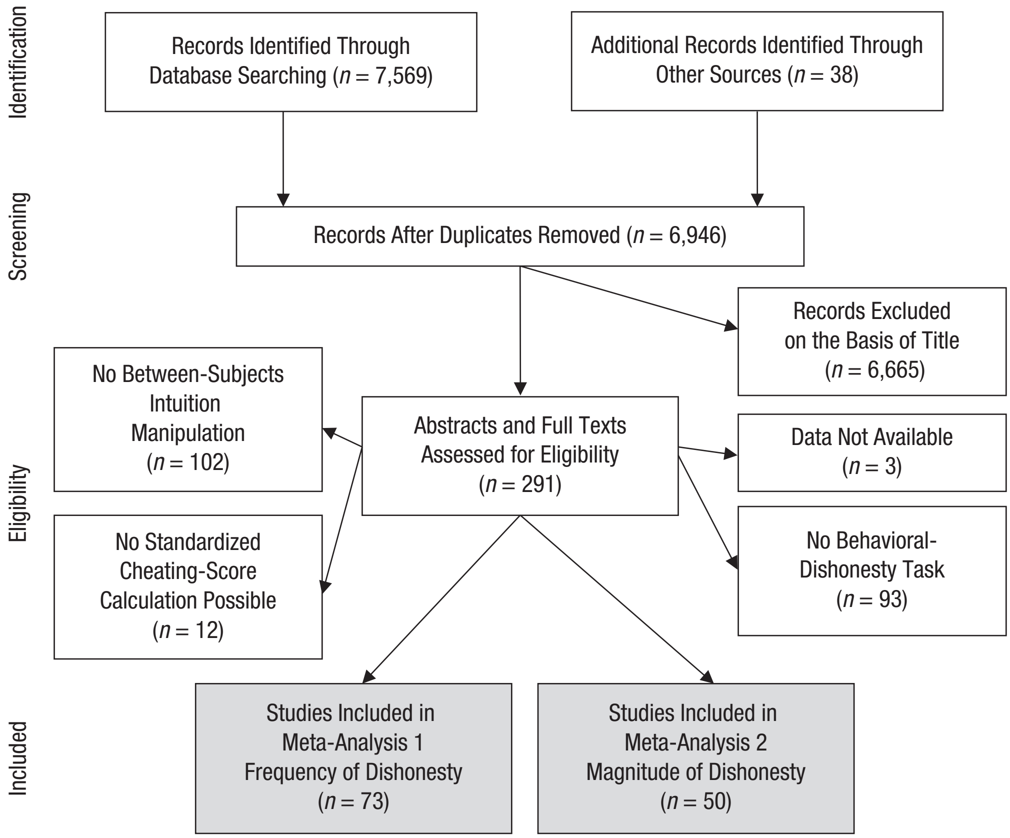

Fig. 1. Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) flow diagram illustrating the identification, screening-eligibility, and inclusion stages of the composition for both meta-analyses.

associations and mailing lists. The Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) reporting scheme (see Fig. 1) provides more details about the identification and selection procedure. After a first round of identifying relevant studies and asking authors to send us their work (resulting in the identification of 44 relevant studies), we conducted a second call for papers and a literature search (preregistered; see https://osf.io/8wtcy/), resulting in the identification of an additional 22 relevant studies. During the revision of the manuscript, we issued another call and online search (preregistered; see https://osf.io/bdvmx/), yielding an extra five studies.

Inclusion criteria

Studies were included if they fulfilled two criteria. First, to enable causal inferences about the link between intuition and honesty/dishonesty, we included only experimental setups, hence excluding studies that used a correlational design (Anderman et al., 2009). To achieve the highest possible comparability across study designs, we further excluded within-subject manipulations of intuition (e.g., Foerster, Pfister, Schmidts, Dignath, & Kunde, 2013), thus reducing potential learning effects. Second, the study used a behavioral task to assess dishonesty as the dependent variable in which the participant stood to gain from dishonesty (financially or otherwise).

Inducing an intuitive mind-set

We focused on experiments with a manipulation of intuition and compared those with a control condition—to increase comparability we did not include studies that compared, for example, a control condition with a deliberation condition (such as Wang, Zhong, & Murnighan, 2014). In line with previous researchers who have studied intuitive decision making (Rand, 2016; Verschuere et al., 2018), we classified existing methods of intuition manipulation into five categories:

- *Time-pressure* manipulations ($k = 14$) require participants to speed up their responses. A short deadline to respond is typically compared with a long deadline or none at all (Shalvi et al., 2012). Limiting the response-time window induces a reliance on intuition because it impairs the participants' ability to reflect (Rand, 2016; see also Evans, Dillon, & Rand, 2015).

- *Cognitive-load* manipulations (*k* = 7) require participants to engage in a cognitive task while they engage in a behavioral-dishonesty task (Van't Veer et al., 2014) that can either be easy (e.g., memorizing a three-digit number) or difficult (e.g., memorizing a seven-digit number). Previous research shows that engaging in the difficult memorization task limits the cognitive resources available to people and thus induces an intuitive mind-set (Gilbert, Pelham, & Krull, 1988; Greene, Morelli, Lowenberg, Nystrom, & Cohen, 2008).
- *Depletion* manipulations (*k* = 30) either require participants to fulfill a taxing task prior to the behavioral task or deplete their cognitive resources physically—for example, by depriving them of sleep or food (Christian & Ellis, 2011)—which impairs the self-control abilities needed for deliberate decision making (for recent meta-analyses on ego depletion, see Carter, Kofler, Forster, & McCullough, 2015; Hagger, Wood, Stiff, & Chatzisarantis, 2010).
- *Induction* manipulations (*k* = 11) prime participants to decide intuitively. This can either be an instruction to recall a past situation in which they relied on their intuition (Cappelen, Sørensen, & Tungodden, 2013) or can consist of priming emotions that are associated with intuitive decision making (Kouchaki & Desai, 2015).
- *Foreign-language* manipulations (*k* = 11) were included because recent evidence suggests that completing a study in one's native language (vs. in a foreign language) can induce an intuitive mind-set (Bereby-Meyer et al., 2018; Geipel, Hadjichristidis, & Surian, 2016).

Measuring dishonesty

We focused on behavioral measures of self-serving dishonesty as an outcome measure, thus excluding studies that used hypothetical scenarios or studies that relied on self-reported dishonesty. Instead, we included only studies in which participants faced the unethical temptation to pursue their self-interest by lying. Multiple methods have been developed to capture such dishonest behavior, and we clustered them into four categories:

- *Performance-enhancement tasks* (*k* = 33) allow participants to inflate their test score. For example, participants can claim to have solved more matrix puzzles than they actually did—and get paid according to the number they report (Mazar, Amir, & Ariely, 2008).
- *Stochastic tasks* (*k* = 18), participants privately observe the outcome of a random device, such as a die roll. They then get paid according to the number they report to have seen and can thus lie to increase their financial rewards (Fischbacher & Föllmi-Heusi, 2013).
- *Sender-receiver games* (*k* = 13), a first player can either send an honest or a deceptive message to a second player, who then decides whether to follow the recommendation influencing both parties' outcomes (e.g., Gneezy, 2005).
- *Several other tasks* (*k* = 9) were included, such as perceptual tasks that allow participants to make self-serving mistakes (Kouchaki & Smith, 2014) or tasks in which participants had the opportunity to report they were overpaid by the experimenter (Chiou, Wu, & Cheng, 2017).

More liars or more lying?

Intuition manipulations can affect dishonesty in two ways: changing how *many* people lie or how *much* people lie. To test both possible pathways, we standardized the behavior in the dishonesty tasks into two outcome measures. First, to test whether an intuitive mind-set leads more people to lie, Meta-Analysis 1 uses the percentage of liars in the intuition and control conditions as an outcome measure. If direct observation of whether one is dishonest was not possible, such as in the standard die-rolling task (Fischbacher & Föllmi-Heusi, 2013), we estimated the proportion of liars using the algorithms put forth by Garbarino, Slonim, and Villeval (2016). In brief, the algorithm compares the reported proportion of favorable outcomes with their expected frequency assuming honest success. In the Supplemental Material available online, we provide a full description of the Garbarino et al. (2016) algorithm as well as analyses using an alternative algorithm put forth by Moshagen and Hilbig (2017) that yields similar results.

Second, to test whether people lie more, Meta-Analysis 2 compares the magnitude of dishonesty (from fair play to maximal possible lying) in intuition and control conditions. We thus included only behavioral-dishonesty tasks that allowed the calculation of a standardized dishonesty score, which in turn allowed a comparison across dishonesty paradigms (see Abeler, Nosenzo, & Raymond, in press). The standardization of dishonesty scores uses a score of 1 to indicate that participants were dishonest in the most self-serving way possible, whereas a score of 0 indicates that participants were fully honest. For example, in the matrix paradigm that entails five unsolvable matrices (e.g., Yam, Chen, & Reynolds, 2014), claiming to have solved four correctly yields a standardized lying score of 0.8. Hence, Meta-Analysis 2 included only dishonesty tasks with a continuous outcome measure and a well-defined maximum performance score that could be obtained dishonestly. Together, these measures provide a comprehensive overview of the most current methods for studying intuitive honesty/dishonesty. Using this procedure, we identified 73 studies (30 of which were unpublished when we conducted the meta-analyses) with 12,711 participants for Meta-Analysis 1 and 50 studies (22 unpublished) with 6,473 participants for Meta-Analysis 2.

Coding procedure

The assessment of eligibility and the ensuing coding was performed by two independent coders. One author (N. C. Köbis) extracted and coded the data from all included studies, and a second blind coder independently coded the extracted data. Disagreements between coders were resolved by consensus after consulting with at least one of the other authors.

Moderators

In addition to standard demographic information such as the percentage of female participants, age, location, and type of sample (students or general population), we coded the characteristics of the intuition manipulations and dishonesty paradigms—details and results are reported in the Supplemental Material. We further coded the key proposed moderator social harm to indicate whether the victim of participants' dishonesty was abstract (e.g., the researcher's budget; $k = 57$) or concrete (e.g., another participant; $k = 16$). To conduct the latter analysis, we split two studies (Experiments 2 and 3 from Pitesa et al., 2013) into separate independent samples per their experimental manipulation of the victim of dishonesty.

Analysis

Using the *metafor* package (Viechtbauer, 2010) for the R software environment (Version 3.5.1; R Core Team, 2018), we estimated the overall effect of the first meta-analysis with a random-effects logistic regression model using a treatment-arm correction as a correction method for cells containing small numbers or zeros. We used the log-transformed odds ratio of dishonesty in the intuition condition compared with the control condition as a dependent variable. In line with common recommendations, the second meta-analysis used a random-effects model with the bias-corrected standardized mean difference (Hedges's g) of a standardized lying score between the intuition condition and the control condition as a dependent variable (for further details, see Supplemental Material). To test the expected moderation effect of social harm, we conducted mixed-effects meta-regression analyses as well as random-effects subgroup analyses. In both meta-analyses, we estimated the interstudy heterogeneity of variance (τ^2) with the restricted maximum-likelihood estimator.

Publication bias and questionable research practices

Given the large proportion of unpublished studies in the sample (41.1%), we tested for publication bias within our sample by evaluating whether the distribution of significant findings differs across published and unpublished studies. Furthermore, we conducted cumulative meta-analyses using the most accurate study as a starting point (Ioannidis & Lau, 2001) as well as p-curve analyses using p values of the main effects included in the meta-analyses (Simonsohn, Simmons, & Nelson, 2015) to assess evidential value. The large interstudy heterogeneity in our sample undermines the reliability of standard procedures to test for publication bias, such as funnel-plot asymmetry, trim and fill, precision-effect test (PET), precision effect estimate with standard error (PEESE), and Egger's regression (see Carter, Schönbrodt, Gervais, & Hilgard, 2019; Sterne et al., 2011). We report the results of these standard procedures in the Supplemental Material.

Meta-Analysis 1: Frequency of Dishonesty

Results

Intuitive dishonesty and social harm. Across all 73 studies, the overall estimate of a random-effects logistic regression model reveals a significant intuitive-dishonesty effect, log odds ratio (OR) = 0.28, 95% confidence interval (CI) = [0.093, 0.473], $Z = 2.92$, $p = .0035$. However, because the ratio of studies with and without a concrete victim was not evenly distributed, the overall odds ratio was not a useful summary estimate of the data. We therefore tested the social-harm moderation effect using a mixed-effects meta-regression model, which revealed a moderation effect of $Z = -1.94$, $p = 0.052$, 95% CI = [−0.854, 0.003], with the remaining heterogeneity being $\tau^2 = 0.39$ ($SE = 0.10$). A random-effects subgroup analysis revealed an intuitive-dishonesty effect for studies in which dishonesty affects an abstract victim, log OR = 0.38, 95% credibility interval (CrI) = [−0.86, 1.63], $Z = 3.64$, $p = .0004$. Thus, 95% of the observed effect sizes fall within that range. However, for studies in which dishonesty affects a concrete victim, no significant effect appears, log OR = −0.04, 95% CrI = [−1.32, 1.24], $Z = -0.21$, $p = .861$ (see Fig. 2). Taken together, the odds for dishonesty are 46% higher in the intuition condition a

Study	Experimental Events	Experimental Total	Control Events	Control Total	Log Odds Ratio Mean	95% CI
Abstract Victim						
Van't Veer et al. (2014)	1	87	10	86	−2.43	[−4.50, −0.35]
Gai & Puntoni (n.d.): Exp. 4	44	63	52	56	−1.73	[−2.88, −0.57]
Gai & Puntoni (n.d.): Exp. 1	52	66	83	87	−1.72	[−2.88, −0.56]
Van der Cruyssen et al. (2019): Pilot	0	9	1	8	−1.27	[−4.55, 2.02]
Gai & Puntoni (n.d.): Exp. 3	21	33	31	37	−1.08	[−2.21, 0.04]
Gross (n.d.)	4	259	11	280	−0.96	[−2.12, 0.20]
Khaw & Christian (n.d.)	23	41	33	47	−0.61	[−1.49, 0.27]
Lohse et al. (2018)	43	188	41	117	−0.60	[−1.11, −0.09]
Cappelen et al. (2013)	40	69	47	68	−0.48	[−1.19, 0.22]
Keith (2018)	15	34	16	31	−0.30	[−1.28, 0.68]
Yam (2018): Exp. 3	19	75	22	72	−0.26	[−0.98, 0.46]
Gunia et al. (2014): Exp. 2	6	69	7	73	−0.11	[−1.25, 1.04]
Yam et al. (2014a): Exp. 1	33	107	35	107	−0.09	[−0.66, 0.49]
Barcelo & Capraro (2018)	51	345	53	344	−0.05	[−0.47, 0.37]
Bereby-Meyer et al. (2018): Exp. 4	3	101	3	98	−0.03	[−1.66, 1.59]
Yam et al. (2014b): Exp. 3	19	45	18	43	0.01	[−0.83, 0.86]
Jones & Paulhus (2017): Exp. 3	63	305	43	216	0.05	[−0.39, 0.48]
Thomas (n.d.): Exp. 1	23	59	20	53	0.05	[−0.71, 0.82]
Van't Veer (2015): Exp. 3	66	87	67	90	0.08	[−0.61, 0.76]
Yam (2018): Exp. 4	33	76	32	77	0.08	[−0.56, 0.72]
Bereby-Meyer et al. (2018): Pilot	1	233	1	259	0.11	[−2.67, 2.88]
Van't Veer (2015): Exp. 4	110	129	104	125	0.16	[−0.52, 0.83]
Yam (2018): Exp. 2	13	45	9	36	0.20	[−0.79, 1.19]
Yam et al. (2014a): Exp. 3	18	65	16	67	0.20	[−0.58, 0.98]
Shalvi et al. (2012): Exp. 2	3	39	2	33	0.26	[−1.60, 2.11]
Gai & Puntoni (n.d.): Exp. 2	42	67	37	66	0.28	[−0.42, 0.97]
Sai (n.d.) Exp. 2	15	40	12	39	0.30	[−0.63, 1.23]
Kouchaki & Desai (2015): Exp. 3	28	30	30	33	0.34	[−1.53, 2.20]
Thomas (n.d.): Exp. 2	23	64	18	65	0.38	[−0.36, 1.13]
Wang et al. (2017): Exp. 3	24	64	19	67	0.42	[−0.32, 1.15]
Pitesa et al. (2013): Exp. 2a	22	24	21	24	0.45	[−1.43, 2.34]
Pitesa et al. (2013): Exp. 3a	20	26	17	26	0.57	[−0.65, 1.79]
Zhang (2013)	65	251	51	309	0.57	[0.16, 0.98]
Muraven et al. (2006)	35	46	35	56	0.65	[−0.22, 1.51]
Hausladen (n.d.)	8	53	4	52	0.76	[−0.51, 2.02]
Bereby-Meyer et al. (2018): Exp. 2	105	206	75	235	0.80	[0.41, 1.18]
Motro et al. (2016): Exp. 2	37	80	21	80	0.88	[0.22, 1.55]
Rua et al. (2017): Exp. 2	22	41	14	44	0.91	[0.03, 1.79]
Chiou et al. (2017): Exp. 4	41	70	25	70	0.93	[0.25, 1.62]
Bereby-Meyer et al. (2018): Exp. 1	4	49	2	61	0.96	[−0.78, 2.71]
Grover & Hui (2013)	4	24	2	29	0.99	[−0.80, 2.79]
Sai (n.d.) Exp. 1	18	37	9	37	1.08	[0.09, 2.07]
Gino et al. (2011): Exp. 3	18	32	10	33	1.08	[0.06, 2.10]
Chiou et al. (2017): Exp. 2	17	37	8	37	1.13	[0.11, 2.14]
Bereby-Meyer et al. (2018): Exp. 3	6	88	2	88	1.15	[−0.48, 2.77]
Gino et al. (2011): Exp. 2	20	48	9	49	1.16	[0.23, 2.08]
Gino et al. (2011): Exp. 1	17	50	7	51	1.17	[0.19, 2.16]
Welsh (n.d.)	72	99	44	99	1.20	[0.61, 1.80]
Motro et al. (2016): Exp. 1	28	37	19	40	1.24	[0.26, 2.21]
Chiou et al. (2017): Exp. 3	18	30	9	30	1.25	[0.18, 2.32]
Mead et al. (2009): Exp. 2	28	38	16	40	1.44	[0.48, 2.40]
Gotlib & Converse (2010)	15	29	5	26	1.50	[0.29, 2.72]
Welsh & Ordonez (2014)	28	34	14	29	1.61	[0.47, 2.75]
Kouchaki & Desai (2015): Exp. 2	23	26	15	25	1.63	[0.19, 3.08]
Shalvi et al. (2012): Exp. 1	5	34	1	38	1.85	[−0.35, 4.05]
Rua (2013)	28	41	10	41	1.90	[0.93, 2.87]
Kouchaki & Smith (2014): Exp. 4	20	21	19	27	2.13	[−0.04, 4.30]
Random Effects Model		4,445		4,456	0.38	[0.18, 0.59]

Heterogeneity: $I^2 = 64\%$, $\tau^2 = 0.3649$, $p < .01$

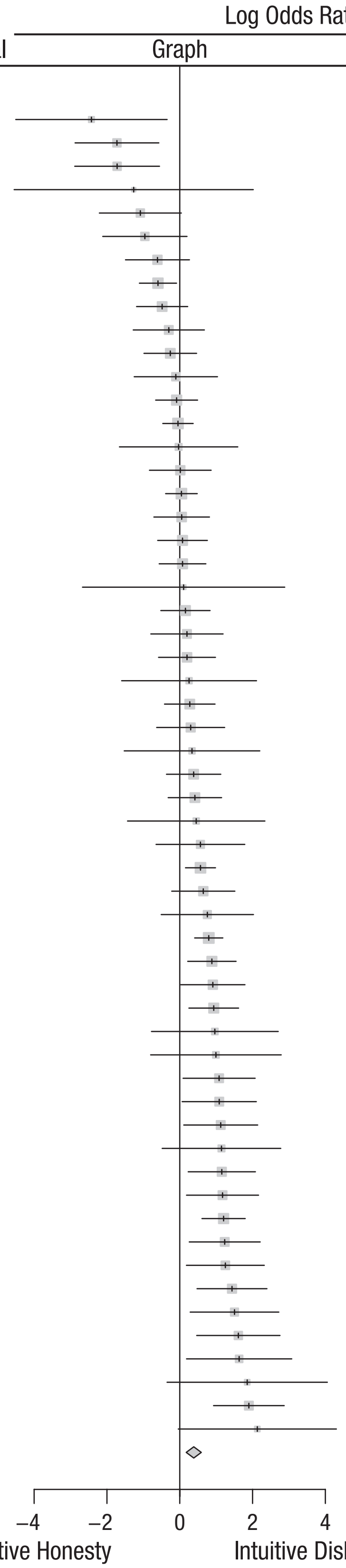

Fig. 2. *(continued on next page)*

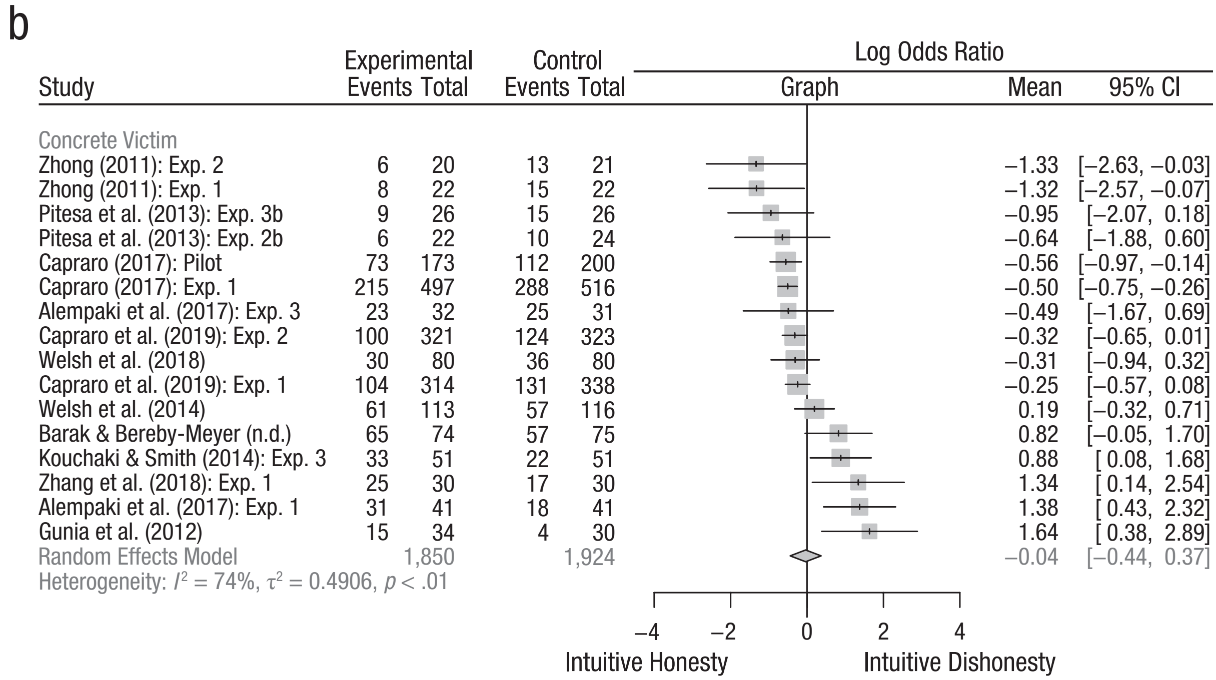

Fig. 2. Forest plot of the estimated effects of the first meta-analysis for the subgroups of (a) abstract victims and (b) concrete victims. In the graph column, the vertical line inside the gray box represents the mean value, the size of the gray box represents the study's weight in the meta-analysis, and the horizontal lines represent the 95% confidence interval. The diamond at the bottom represents the overall effect and its 95% confidence interval.

compared with the control condition when the victim of dishonesty is abstract but extremely similar when the victim of dishonesty is concrete.

Because of the uneven distribution of studies using concrete and abstract victims, we also conducted a Top10 analysis (Stanley, Jarrell, & Doucouliagos, 2010), which restricts the sample to the 10% of studies with the smallest standard error—a method that often provides a more accurate estimate of the overall effect than relying on the entire sample (see Nuijten, Van Assen, Veldkamp, & Wicherts, 2015). Running the meta-analysis selecting only the top decile of studies with the lowest standard error (k = 7, n = 4,372; 34% of the entire sample) reveals a nonsignificant main effect for intuition, log OR = −0.05, 95% CrI = [−1.08, 0.98], Z = −0.26, p = 0.78, and a full crossover moderation effect, Z = −4.02, p < .0001, confirming an intuitive dishonesty for abstract victims, log OR = 0.45, 95% CrI = [−0.06, 0.96], Z = 2.68, p = .007, compared with an intuitive-honesty effect for concrete victims, log OR = −0.41, 95% CrI = [−0.87, 0.07] Z = −3.09, p = 0.002; see also Figure 3.

Heterogeneity. Overall estimates of heterogeneity indicate that the effect sizes significantly differ across studies, $Q(72)$ = 258.38, p < .001. The absolute variance is estimated to be τ^2 = 0.42, and the ratio between true and overall heterogeneity is estimated to be I^2 = 72.1%, 95% CI = [64.8%, 77.9%]. These estimates suggest that approximately 70% of the observed variance in the effect sizes is due to real differences—which represents a medium-to-large degree of heterogeneity according to Higgins, Thompson, Deeks, and Altman (2003).

Additional analyses. We conducted several analyses using alternative meta-analytical techniques to account for interstudy heterogeneity such as the Hartung-Knapp adjustment, Peto odds ratios, and arsine meta-analyses. Moreover, we used different correction methods for small or zero cell sizes by following a standard approach to add an increment of 0.5 to 0 cell sizes as well as using different classification criteria of liars and different lying estimations altogether. These analyses provide qualitatively similar results and are reported in detail in the Supplemental Material. Additional analyses testing the other moderators outlined above (see Method section) are also described in the Supplemental Material.

Publication bias and questionable research practices. The large proportion of unpublished studies included in the meta-analysis (41.1%) allowed us to test whether significant findings are more likely to be published than nonsignificant findings. A Fisher's exact test comparing the distribution of significant and nonsignificant findings across unpublished and published studies revealed no significant

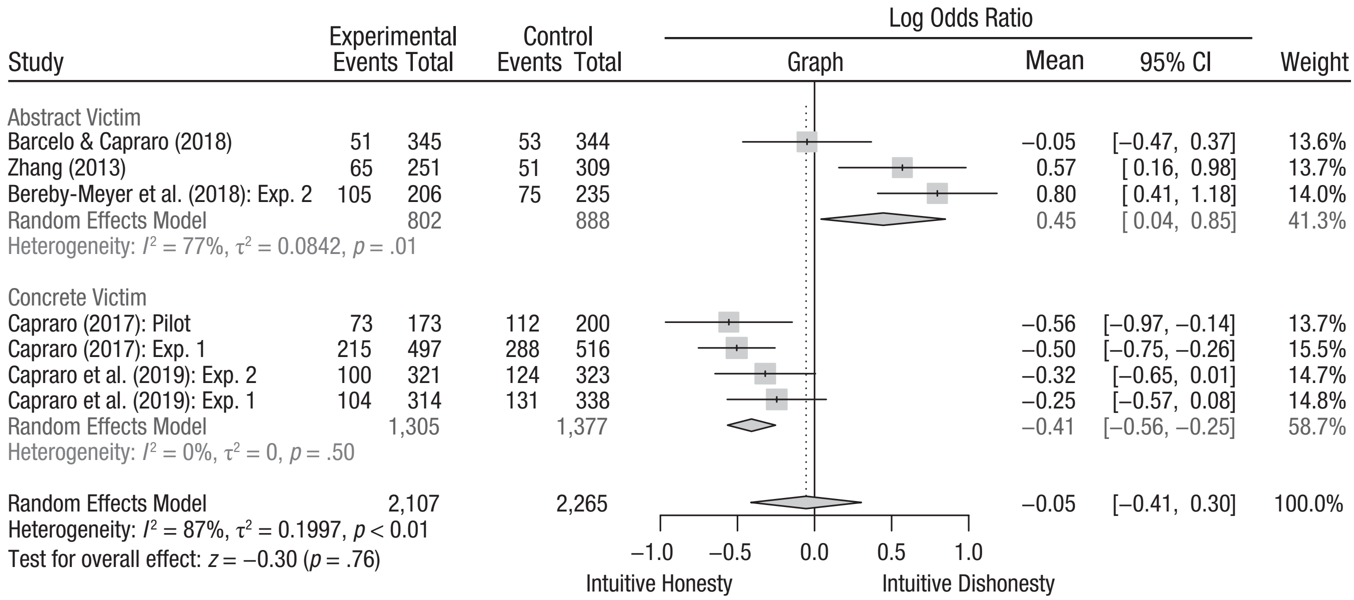

Fig. 3. Forest plot of the most precise 10% of estimated effects of the first meta-analysis for the subgroups of abstract victims and concrete victims. In the graph column, the vertical line inside the gray box represents the mean value, and the horizontal lines represent the 95% confidence interval. The dotted vertical line represents the overall estimated effect. The diamond at the bottom represents the overall effect and its 95% confidence interval.

differences (OR = 1.72, p = .33), indicating no evidence of bias among the included studies—however, these results must be interpreted with caution because it remains unknown whether both samples are represented in (similarly) unbiased ways (Kepes, Banks, McDaniel, & Whetzel, 2012).

Next, we conducted cumulative meta-analyses for both social-harm conditions. The cumulative meta-analysis technique first calculates the effect with the most precise study (i.e., smallest standard error) and then adds the remaining studies and recalculates the overall estimate for each study using a random-effects weighting scheme. An indication of publication bias is the suppression of small studies with small effect sizes, which becomes visible if the overall effect swiftly drifts toward a larger overall effect when smaller studies are added.

For studies using an abstract victim, the effect with the most precise estimate, log OR = 0.80, exceeds the overall estimate, log OR = 0.38. Contrary to the pattern indicative of publication bias, smaller studies reduce rather than inflate the overall effect. Moreover, the direction of the effect points toward intuitive dishonesty for all studies (see Fig. 4a). For studies with a concrete victim, the most precise study indicates an intuitive-honesty effect, log OR = −0.50. With the inclusion of smaller studies, the overall estimate moves toward a null effect, log OR = −0.04. The shift toward nonsignificance suggests that smaller, more imprecise studies sway the overall estimate toward a null effect (see Fig. 4b). Hence, the results of both cumulative meta-analyses contradict the pattern expected if small study effects indicate potential publication bias.

To assess whether the effect sizes included in the meta-analysis have evidential value, we conducted a p-curve analysis (Simonsohn et al., 2015). The distribution of p values stemming from all Z values of the 73 studies included in the first meta-analysis was significantly right-skewed (full curve: Z = −3.89, p < .0001; half curve: Z = −2.91, p = .002) and thus suggests evidential value (see also Fig. 5). Note that we imputed the Z values mostly stemming from the recalculations of the original data and did not use the test statistics provided in the original manuscript because only a small proportion of studies (32.8%) hypothesized an intuitive (dis)honesty main effect and tested it by comparing the percentage of liars in the intuition condition with that in the control condition (e.g., by χ^2 statistics). Thus, the analysis is useful for confirming the evidential value of the data but not for assessing questionable research practices. Other common procedures for assessing publication bias are undermined by the large interstudy heterogeneity; thus, we report and discuss those in the Supplemental Material.

Discussion

Drawing on 73 original studies that experimentally manipulated intuition and behaviorally assessed dishonesty, the results reveal an intuitive-dishonesty effect when harm was inflicted on abstract others—for these tasks, an intuitive mind-set heightened the chances of

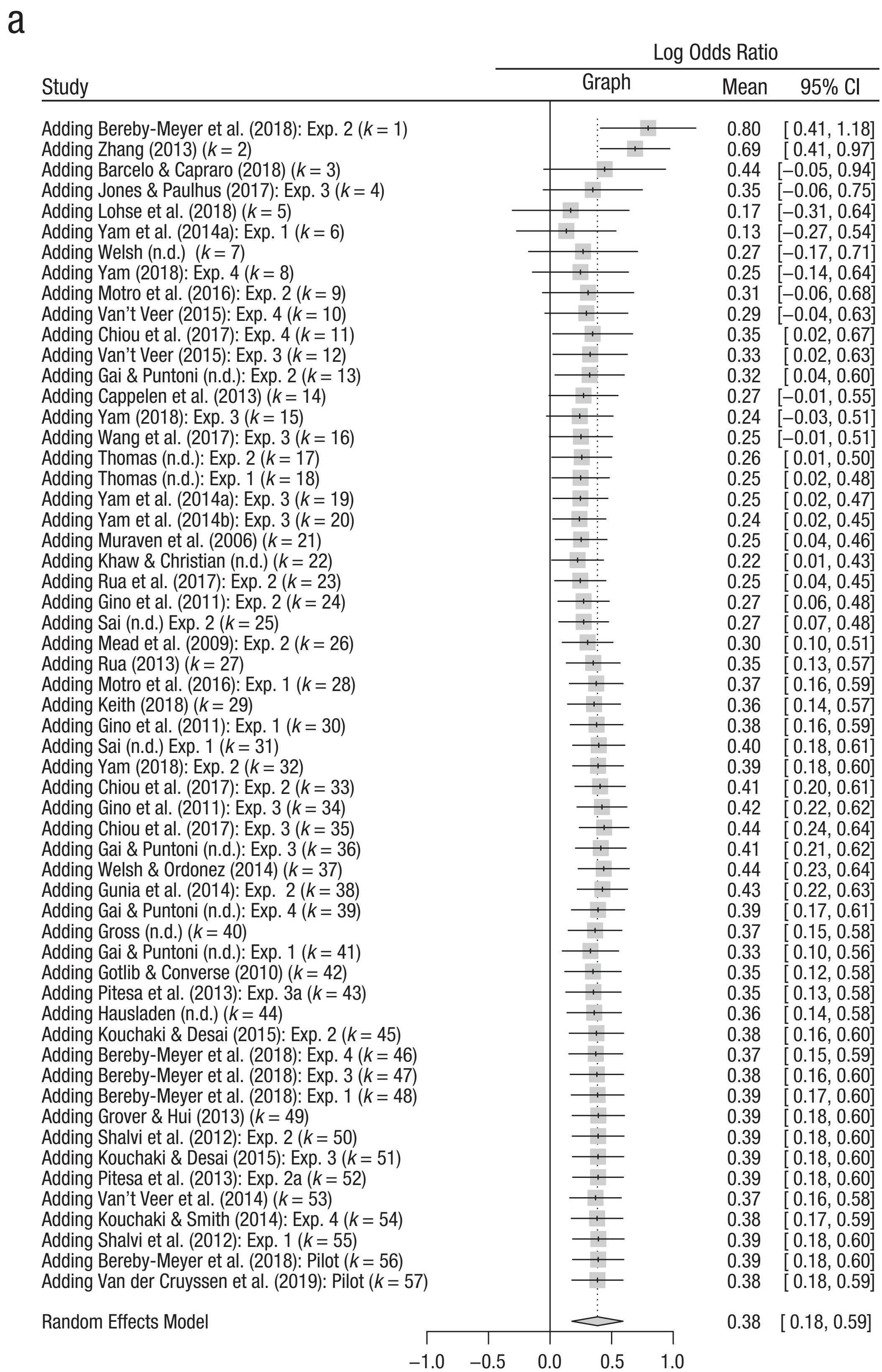

Fig. 4. *(continued on next page)*

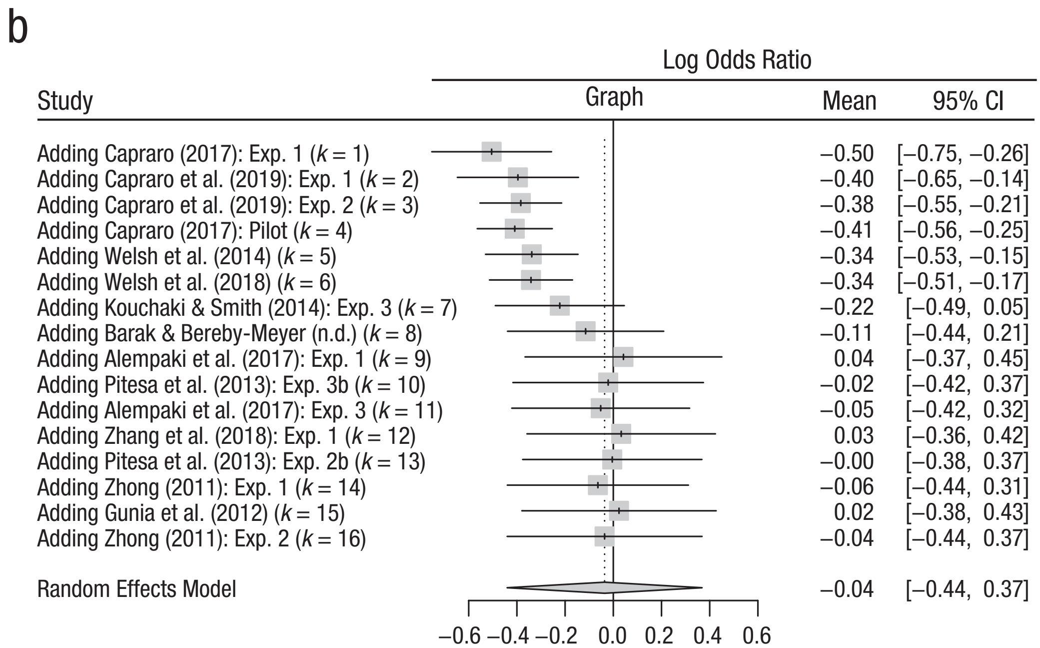

Fig. 4. Cumulative forest plots for studies using (a) abstract victims and (b) concrete victims. The most accurate effect was chosen as a first study. The outcome measure is the log-transformed odds ratio of liars in the intuition and control conditions. In the graph column, the vertical line inside the gray box represents the mean value, and the horizontal lines represent the 95% confidence interval. The dotted vertical line represents the overall estimated effect. The diamond at the bottom represents the overall effect and its 95% confidence interval.

dishonesty. Yet this intuitive-dishonesty effect was not present when lying caused harm on a concretely identifiable other person. With regard to potential publication bias, although it is generally safe to assume that nonsignificant findings were less likely to be published and thus do not enter the meta-analysis, there is little evidence that our results are artifacts of such biases. Contrary to the pattern expected for the existence of publication biases, cumulative meta-analyses suggest that small studies reduce the intuitive-dishonesty effect for abstract victims while potentially suppressing an intuitive-honesty effect for concrete victims. For studies with abstract victims of dishonesty, the overall effect remains significant with the inclusion of smaller studies, which underlines the validity of the intuitive-dishonesty effect. Possible invalidation of the findings due to publication bias is further reduced by the fact that a large proportion of the studies is unpublished and significant findings are evenly distributed across publication status. Finally, a *p*-curve analysis drawing on all 73 effect sizes confirms that the overall sample contains evidential value.

Meta-Analysis 2: Magnitude of Dishonesty

When assessing dishonesty, a high average level of dishonesty can result either from a few liars lying a lot or from many liars lying just a bit. The second meta-analysis aimed to test the extent to which people lie and whether intuition leads to larger lies compared with a control setting. Thus, we compared the magnitude of lying for a standardized lying score between the intuition and control conditions.

Results

Intuitive honesty/dishonesty and social harm. The aggregate result of 50 experiments included in the second meta-analysis confirms and extends the results of the first meta-analysis. The overall estimate suggests an intuitive-dishonesty effect, g = 0.23, 95% CrI = [0.137, 0.328], Z = 4.764, p < 0.0001, however, akin to the first meta-analysis, studies using abstract victims (k = 45) and concrete victims (k = 5) were unevenly distributed. Because the overall estimate might provide a potentially skewed estimation, we conducted mixed-effects meta-regression models that revealed a moderation effect of social harm, Z = −1.987, p = .047. The residual remaining heterogeneity was τ^2 = 0.073 (SE = 0.02).

The random-effects subgroup analysis shows an intuitive-dishonesty effect when an abstract victim is harmed by dishonesty, g = 0.261, 95% CrI = [−0.280, 0.802], Z = 5.186, p < .0001, but no such effect when a concrete victim is harmed by dishonesty, g = −0.076, 95% CrI = [−0.696, 0.544], Z = −0.470, p = .63. Overall,

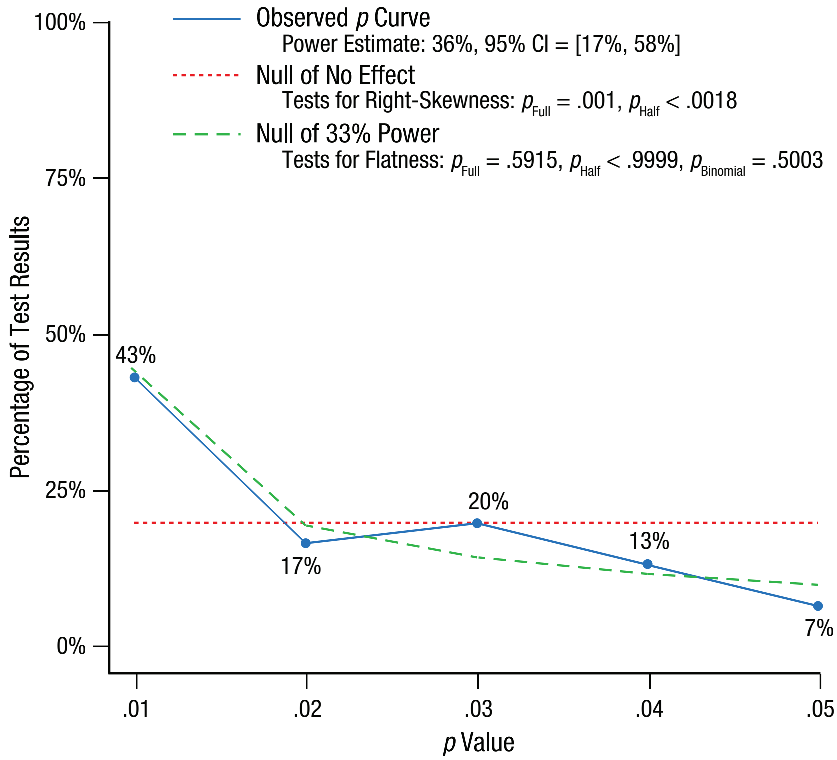

Fig. 5. Observed *p* curve for all studies in the first meta-analysis. The observed *p* curve includes 30 statistically significant ($p < .05$) results, of which 21 were $p < .025$. Forty-three additional results were entered but excluded from the *p* curve because they were $p > .05$. The blue line shows the observed *p* curve, the dashed red line shows the uniform distribution of the *p* values, and the green line plots the right-skewed distribution for a power level of 33%.

we therefore conclude that an intuitive mind-set increases the magnitude of lying for tasks with abstract victims, yet this intuitive-dishonesty effect disappears when a concrete victim is harmed by dishonesty (see also Fig. 6). For the second meta-analysis, the Top10 analysis cannot provide useful information about a moderation effect because no study using a concrete victim is among the most precise decile of estimates. We report the Top10 analysis for the second meta-analysis in the Supplemental Material.

Heterogeneity. Again, heterogeneity estimators reveal that there is substantial variation in the effect-size distribution, $Q(49) = 150.32$, $p < .0001$, whereas the overall variance is estimated to be $\tau^2 = 0.06$. In total, 67.4% of the variance in the effect sizes stems from true heterogeneity, $I^2 = 67.4$, 95% CI = [56.3, 75.7].

Publication bias and questionable research practices. A Fisher's exact test comparing the distribution of significant and nonsignificant results across published and unpublished studies reveals that significant findings are not evenly distributed in the sample ($OR = 5.30$, $p = .016$). The odds of a significant study being published are 5.3 higher than the odds of a nonsignificant study being published. This finding may stem from publication bias. We also conducted two separate cumulative meta-analyses, one for studies using an abstract victim, the other for studies using a concrete victim. For studies using an abstract victim, the initial estimated effect (Hedges's $g = -0.14$) differs substantially from the overall estimated effect (Hedges's $g = 0.26$). Although the effect of the most precise study points toward intuitive honesty, including smaller studies continually moves the overall effect toward intuitive dishonesty (see Fig. 7a). For studies using a concrete victim, the estimated effect for the initial study (Hedges's $g = -0.10$) as well as the overall estimate suggest a null effect (Hedges's $g = 0.08$; Fig. 7b). Taken together, the pattern for studies with an abstract victim suggests the existence of a small-study effect (i.e., the phenomenon that smaller studies sometimes show different, often larger, treatment effects than larger ones)—one potential reason is publication bias. For studies with concrete victims, there is little indication of small study effects.

Study	Experimental			Control			Standardized Mean Difference	
	Total	Mean	*SD*	Total	Mean	*SD*	Mean	95% CI
Abstract Victim								
Van't Veer et al. (2014)	87	0.02	0.2606	86	0.12	0.2476	−0.41	[−0.72, −0.11]
Gai & Puntoni (n.d.): Exp. 3	33	0.61	0.6667	37	0.84	0.7222	−0.33	[−0.80, 0.15]
Gross (n.d.)	259	0.03	0.2622	280	0.07	0.2701	−0.14	[−0.31, 0.03]
Welsh (n.d.)	99	0.16	0.2960	99	0.20	0.2900	−0.12	[−0.39, 0.16]
Keith (2018)	34	−0.17	0.0274	31	−0.17	0.0267	−0.11	[−0.59, 0.38]
Bereby-Meyer et al. (2018): Pilot	233	0.04	0.1270	259	0.05	0.1275	−0.08	[−0.25, 0.10]
Yam et al. (2014a): Exp. 1	107	0.18	0.2976	107	0.18	0.3370	−0.01	[−0.28, 0.26]
Gai & Puntoni (n.d.): Exp. 4	63	0.50	0.3889	56	0.51	0.2884	−0.01	[−0.37, 0.35]
Gai & Puntoni (n.d.): Exp. 1	66	0.58	0.3185	87	0.57	0.2768	0.01	[−0.31, 0.33]
Yam (2018): Exp. 3	75	0.14	0.0381	72	0.14	0.0400	0.03	[−0.29, 0.35]
Bereby-Meyer et al. (2018): Exp. 4	101	0.05	0.2701	98	0.03	0.2511	0.05	[−0.23, 0.33]
Gunia et al. (2014): Exp. 2	69	0.10	0.2515	73	0.09	0.2793	0.06	[−0.27, 0.39]
Van't Veer (2015): Exp. 4	129	0.21	0.2269	125	0.20	0.2183	0.07	[−0.18, 0.32]
Bereby-Meyer et al. (2018): Exp. 3	88	0.08	0.2682	88	0.05	0.2490	0.08	[−0.22, 0.38]
Yam (2018): Exp. 2	45	0.22	0.0688	36	0.21	0.0759	0.09	[−0.35, 0.52]
Bereby-Meyer et al. (2018): Exp. 2	206	0.26	0.2325	235	0.22	0.2208	0.14	[−0.05, 0.32]
Sai (n.d.): Exp. 2	40	0.18	0.1994	39	0.15	0.1625	0.14	[−0.30, 0.58]
Thomas (n.d.): Exp. 1	59	0.09	0.1760	53	0.06	0.1210	0.16	[−0.22, 0.53]
Van't Veer (2015): Exp. 3	87	0.16	0.1200	90	0.14	0.1000	0.18	[−0.11, 0.48]
Van der Cruyssen et al. (2019): Pilot	9	0.02	0.3200	8	−0.05	0.2867	0.21	[−0.74, 1.17]
Gai & Puntoni (n.d.): Exp. 2	67	0.40	0.2988	66	0.32	0.2968	0.26	[−0.09, 0.60]
Motro et al. (2016): Exp. 2	80	0.17	0.2936	80	0.10	0.2339	0.27	[−0.04, 0.59]
Gino et al. (2011): Exp. 1	50	0.12	0.2535	51	0.05	0.2060	0.29	[−0.10, 0.68]
Muraven et al. (2006)	46	0.53	0.3950	56	0.41	0.3970	0.30	[−0.09, 0.69]
Thomas (n.d.): Exp. 2	64	0.14	0.2545	65	0.07	0.1585	0.31	[−0.04, 0.65]
Khaw & Christian (n.d.)	41	0.15	0.1673	47	0.10	0.1533	0.31	[−0.11, 0.73]
Yam et al. (2014a): Exp. 3	65	0.14	0.2092	67	0.08	0.0775	0.38	[0.03, 0.72]
Bereby-Meyer et al. (2018): Exp. 1	49	0.11	0.2680	61	0.00	0.2951	0.38	[0.00, 0.76]
Wang et al. (2017): Exp. 1	50	0.49	0.1100	50	0.45	0.0850	0.39	[0.00, 0.79]
Gino et al. (2011): Exp. 2	48	0.12	0.1995	49	0.04	0.1685	0.44	[0.03, 0.84]
Shalvi et al. (2012): Exp. 1	34	0.18	0.2467	38	0.06	0.2683	0.44	[−0.03, 0.91]
Welsh & Ordonez (2014)	34	0.05	0.0910	29	0.02	0.0225	0.46	[−0.04, 0.96]
Sai (n.d.): Exp. 1	37	0.18	0.1672	37	0.11	0.1378	0.47	[0.00, 0.93]
Wang et al. (2017): Exp. 3	67	0.10	0.1613	64	0.04	0.0900	0.47	[0.12, 0.82]
Motro et al. (2016): Exp. 1	37	0.20	0.2190	40	0.10	0.2020	0.48	[0.03, 0.94]
Gino et al. (2011): Exp. 3	32	0.20	0.2155	33	0.09	0.2130	0.49	[0.00, 0.99]
Pitesa et al. (2013): Exp. 2a	24	0.41	0.0950	24	0.36	0.0585	0.55	[−0.03, 1.13]
Shalvi et al. (2012): Exp. 2	39	0.15	0.2464	33	−0.01	0.3067	0.58	[0.10, 1.05]
Chiou et al. (2017): Exp. 4	70	0.14	0.1370	70	0.06	0.1225	0.59	[0.25, 0.93]
Chiou et al. (2017): Exp. 3	30	0.16	0.1460	30	0.06	0.1375	0.66	[0.14, 1.18]
Kouchaki & Smith (2014): Exp. 4	21	0.45	0.2720	27	0.26	0.2710	0.67	[0.08, 1.26]
Pitesa et al. (2013): Exp. 3a	26	0.15	0.0780	26	0.10	0.0550	0.73	[0.17, 1.29]
Zhang et al. (2018): Exp. 1	32	0.55	0.1474	33	0.37	0.1475	1.17	[0.64, 1.70]
Kouchaki & Desai (2015): Exp. 3	30	0.42	0.1133	33	0.30	0.0821	1.22	[0.68, 1.76]
Kouchaki & Desai (2015): Exp. 2	26	0.29	0.0844	25	0.18	0.0468	1.49	[0.86, 2.11]
Random Effects Model	2,988			3,093			0.26	[0.16, 0.36]
Heterogeneity: $I^2 = 67\%$, $\tau^2 = 0.0709$, $p < .01$								
Concrete Victim								
Pitesa et al. (2013): Exp. 2b	22	0.29	0.0500	24	0.33	0.0925	−0.57	[−1.16, 0.02]
Pitesa et al. (2013): Exp. 3b	26	0.03	0.0460	26	0.04	0.0500	−0.31	[−0.85, 0.24]
Alempaki et al. (2017): Exp. 3	32	0.57	0.4254	31	0.62	0.4629	−0.11	[−0.61, 0.38]
Barak & Bereby-Meyer (n.d.)	74	0.13	0.1837	75	0.15	0.1883	−0.10	[−0.42, 0.22]
Alempaki et al. (2017): Exp. 1	41	0.41	0.3793	41	0.21	0.3298	0.55	[0.10, 0.99]
Random Effects Model	195			197			−0.08	[−0.43, 0.27]
Heterogeneity: $I^2 = 64\%$, $\tau^2 = 0.1030$, $p = .03$								

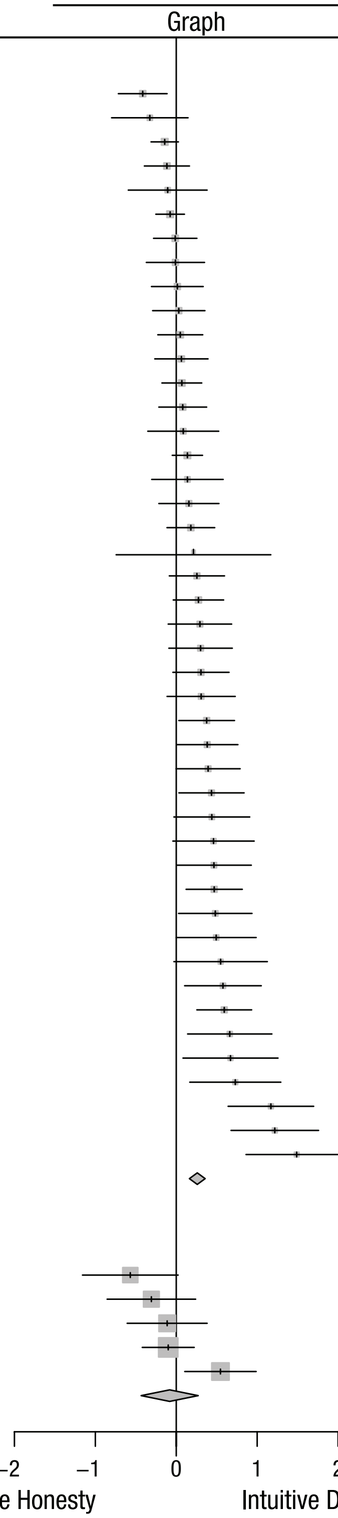

Fig. 6. Forest plot showing the overall estimated effect for the second meta-analysis using a random-effects model and the effects estimated for the subgroups using a mixed-effects model. The outcome variable is the bias-corrected standardized mean difference of lying between the intuition and control conditions. In the graph column, the vertical line inside the gray box represents the mean value, the size of the gray box represents the study's weight in the meta-analysis, and the horizontal lines represent the 95% confidence interval. The diamond at the bottom represents the overall effect and its 95% confidence interval.

To assess the evidential value of the effect sizes in the sample, we again conducted a *p*-curve analysis by imputing all 50 *Z* scores. The results reveal that both the full curve ($Z = -3.08$, $p = .001$) and the half curve ($Z = -4.00$, $p < .0001$) are significant, which suggests evidential value of the obtained effects (see Fig. 8). We again did not conduct a *p*-curve analysis of the test statistics reported in the original manuscripts because only 14% of the overall sample qualified. Taken together, the fact that most of the effects included in the meta-analysis (86.1%) are based on our original calculations and the significantly right-skewed *p* curve of the sample suggest evidential value of the findings.

Discussion

The results of the second meta-analysis corroborate those of the first meta-analysis: The overall estimate of

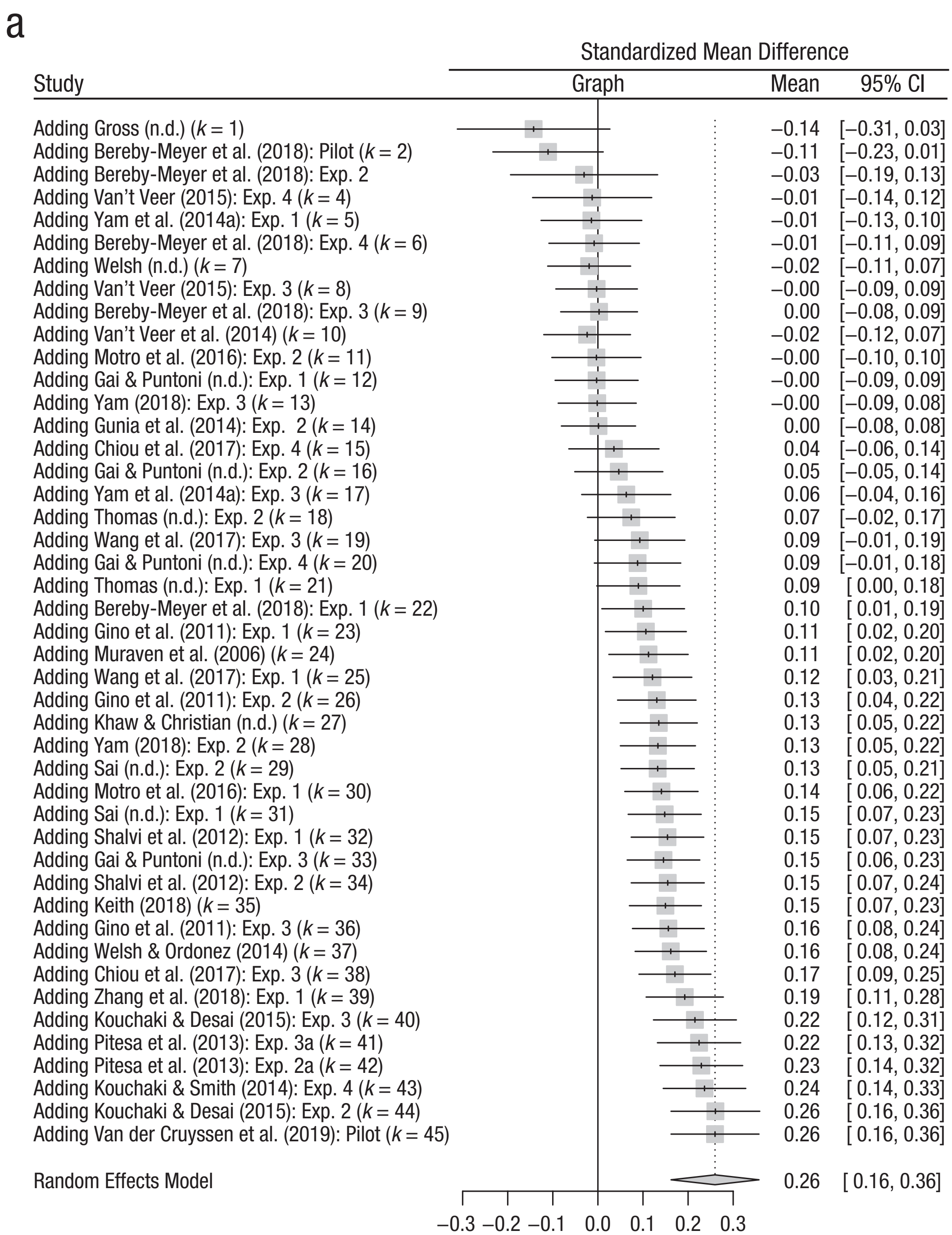

Fig. 7. *(continued on next page)*

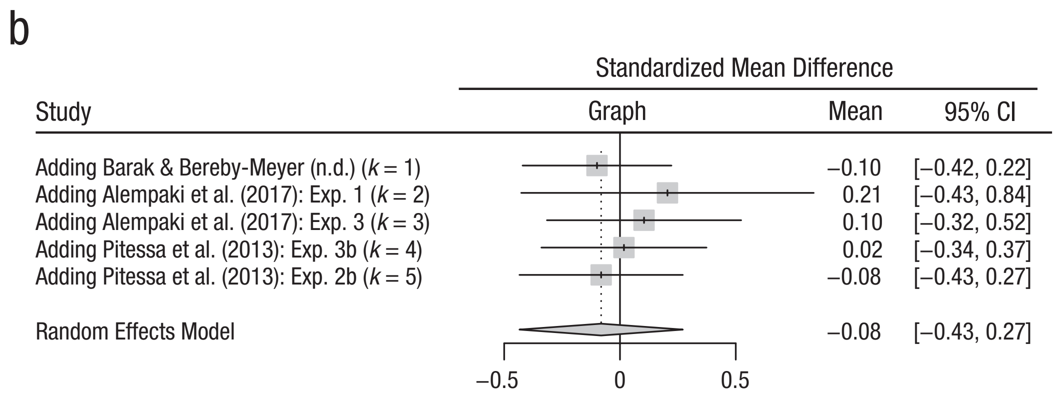

Fig. 7. Cumulative forest plot for the second meta-analysis using a random-effects model for studies using abstract victims (a) and studies using concrete victims (b). In the graph column, the vertical line inside the gray box represents the mean value, the size of the gray box represents the study's weight in the meta-analysis, and the horizontal lines represent the 95% confidence interval. The dotted vertical line represents the overall estimated effect. The diamond at the bottom represents the overall effect and its 95% confidence interval.

50 studies supports the social-harm moderation. That is, we found an intuitive-dishonesty effect when harm was inflicted on abstract others—compared with participants in a control condition, those who adopted an intuitive mind-set lied to a larger extent in these setups. People intuitively engage in more dishonesty when no concrete victim is harmed by it, yet such an effect is not observed when a concrete victim suffers. That said, the result of the moderation analysis has to be interpreted with caution because of the small number of

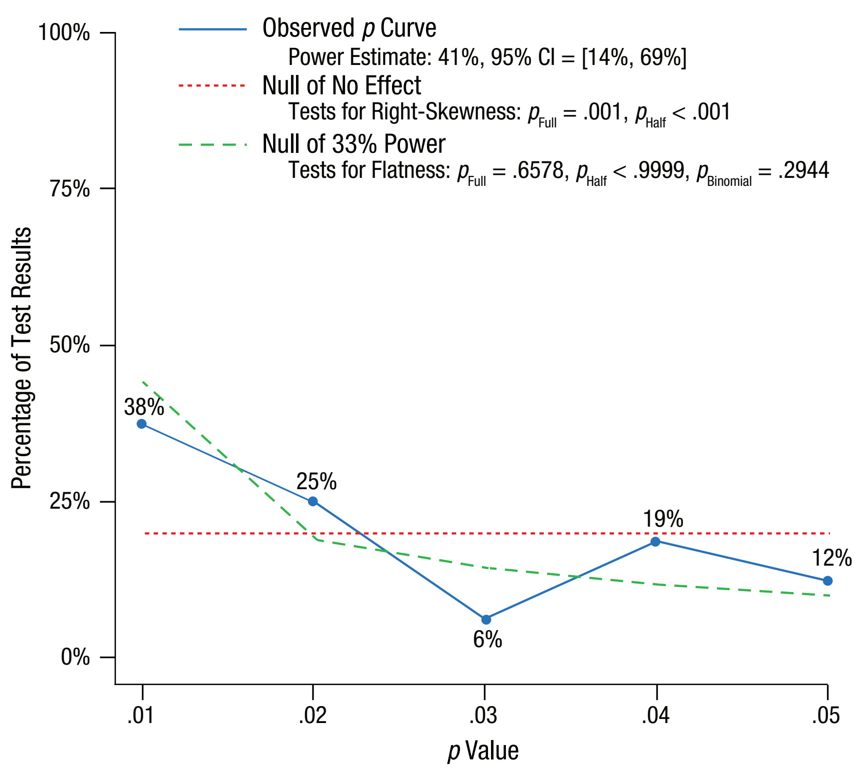

Fig. 8. Observed p curve for all studies included in the second meta-analysis. The observed p curve includes 16 statistically significant ($p < .05$) results, of which 10 are $p < .025$. Thirty-four additional results were entered but excluded from the p curve because they were $p > .05$. The blue line shows the observed p curve, the dashed red line shows the uniform distribution of the p values, and the green line plots the right-skewed distribution for a power level of 33%.

studies that used a concrete victim (see Kepes et al., 2012). We found a higher proportion of significant findings among the published studies, which indicates that nonsignificant effects are less likely to be published. Cumulative meta-analyses reveal that small studies with larger imprecision influence the studies with abstract victims more strongly than those using a concrete victim. However, the large proportion of unpublished studies in our sample reduces the danger that the file-drawer problem invalidates the obtained findings. Finally, a *p*-curve analysis again emphasizes the evidential value of the effect sizes in the sample.

General Discussion

The current research set out to solve a puzzle: Are people intuitively honest or dishonest? We conducted two meta-analyses to gain a more definite answer than can be gained from a single experiment (Lakens, Hilgard, & Staaks, 2016). Confirming previous theorizing (Bereby-Meyer & Shalvi, 2015; Verschuere & Shalvi, 2014), we found an intuitive-dishonesty effect in anonymous settings in which punishment is not a threat and dishonesty harms an abstract victim. In these settings, self-interest leads to more people lying (Meta-Analysis 1) and people lying more (Meta-Analysis 2). In addition to self-interest, a second force influences whether people intuitively resist or succumb to lie self-servingly: the social heuristic to do no harm (Baron, 1996). In settings in which dishonesty harms concrete others, we did not observe such an intuitive-dishonesty effect.

When facing ethical dilemmas between dishonestly maximizing self-interest and following normative rules of conduct, people often seek to maintain a positive (self- and public) image and restrain their self-interest to a level that allows them to both feel and appear to be honest (Abeler et al., in press). Adding to this literature, we present the first meta-analyses on the interplay of dual-process models and behavioral ethics. Our results suggest that “thinking fast” amplifies the force of self-interest leading to ethical rule violations, as long as those violations do not directly harm others.

Providing first insights into the contextual factors of the intuitive-dishonesty effect, our moderation analyses provide suggestive evidence that the relationship between intuition and dishonesty is shaped by social harm. In accordance with previous theorizing, particularly the social heuristics hypothesis (Bear & Rand, 2016; Rand, 2016) our data are in line with the idea that salient consequences for others have a substantial impact on people’s intuitive decisions. In particular, prior work has shown that cooperating with others is an intuitive inclination in many social-dilemma-type situations (e.g., Halali, Bereby-Meyer, & Meiran, 2014; Rand, 2016). Our results contribute to this stream of literature, suggesting that the automatic tendency to cooperate might cancel out the selfish urges of dishonesty when knowing that lying comes at a price for a concrete other.

The meta-analyses draw on laboratory research that raises the question of what these results can tell us about intuitive dishonesty outside the lab. For one, recent empirical evidence underlines the external validity of lying in economic games as a proxy for real-life dishonesty. Lying in a controlled laboratory context correlates with a variety of ethical rule breaking outside the lab, ranging from academic fraud (Cohn & Maréchal, 2017) and fare dodging (Dai, Galeotti, & Villeval, 2017) to deceptive market practices (Kröll & Rustagi, 2016). People frequently encounter such situations in daily life, often deciding quickly without much thought. Like in the experiments included in the current meta-analyses, these temptations mostly entail relatively small (financial) incentives. Although each individual act might seem mundane and merely harm vague entities such as “the bus company” when fare dodging or “society as a whole” when fudging a tax payment, the aggregated costs are immense (Gino, 2015). Our results provide the first aggregated evidence that deciding intuitively might lead to more self-favoring dishonesty when those suffering from dishonesty are vague and difficult to identify with.

Another line of work to which our results relate is the identified-victim effect (Jenni & Loewenstein, 1997; Kogut & Ritov, 2005), which suggests that people act more prosocially toward identified rather than unidentified others. In our meta-analyses, we found evidence that the type of victim, concrete or vague, moderates the effect of intuition on self-serving dishonesty. This finding opens up various avenues for future work to explore. In many tasks included in the meta-analyses, lying that hurts a concrete victim marks a strategic choice. The few studies in the meta-analyses that disentangled the social consequences from strategic deception indicate a full moderation of intuition and social harm (Experiments 2 and 3 from Pitesa et al., 2013). To provide additional support for the moderating role of social harm on the link between intuition, we particularly encourage preregistered studies that experimentally manipulate concrete victims compared with abstract victims. In this way, future research can contribute to overcoming the uneven distribution of studies in the current meta-analyses.

Moreover, social factors such as the relationship between the person benefiting and the person suffering from lying likely matter. Previous research has shown that people willingly lie to favor their own ingroup (Shalvi & De Dreu, 2014). Does highlighting social-identity features of the liar and the victim lead people to engage in intuitive dishonesty when doing so harms an out-group member but not an in-group member? Conversely, might a concrete representation of the victim of seemingly “victimless crimes” such as corruption curb the intuitive tendency to break (ethical) rules (Köbis et al., 2016)?

Limitations worth noting are the low number of preregistered studies included in both meta-analyses, the unbalanced sample distribution across the key moderator of social harm, and the large methodological heterogeneity, both in terms of intuition manipulation and dishonesty tasks. These limitations undermine the power of moderation and publication-bias analyses. It is generally contested whether a statistical method can detect publication bias and, if so, which one (Carter, et al., 2019). It thus remains unknown whether the strategic nonpublication of empirical results undermines the accuracy of the obtained aggregated estimates. Although we found mixed evidence for small study effects based on cumulative meta-analyses, the fact that large proportions of both meta-analyses draw on unpublished studies (> 40%) reduces the concern about publication bias to some extent. In addition, a large proportion of effect sizes stems from the recalculation of original data (> 65%), and two *p* curves of the obtained effect sizes in both meta-analyses suggest evidential value.

Conclusion

Understanding whether honesty is intuitive requires a closer look at the cognitive, motivational, and situational factors in which decisions are made. In this current age of distraction, people frequently decide without much thought (Williams, 2018). Not surprisingly, a large collection of behavioral studies has used experimental manipulations to trigger an intuitive mind-set and subsequently give people the chance to pursue their self-interest through dishonest means. Results from two meta-analyses provide the first aggregated evidence of this literature and suggest that people’s intuitive response is to selfishly lie, but only when no concrete other is harmed.

Action Editor

Laura King served as action editor for this article.

Author Contributions

S. Shalvi, Y. Bereby-Meyer, B. Verschuere, and D. Rand developed the study concept. N. C. Köbis collected and analyzed the data and drafted the manuscript. All of authors contributed to the study design, provided critical revisions, and approved the final version of the manuscript for submission.

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Declaration of Conflicting Interests

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Supplemental Material

Additional supporting information can be found at http://journals.sagepub.com/doi/suppl/10.1177/1745691619851778

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Supplemental Material

Overview

This Supplementary Material provides background information on the methodological procedures (see Method) and the additional analyses (see Analysis) conducted on both meta-analyses reported in the main manuscript. We also report the different components of the introduction, method, analysis and results according to the Preferred Reporting Items for Systematic reviews and Meta-Analyses (PRISMA overview in Table S8, see Liberati et al., 2009). It contains detailed information on the methodological and analytical steps that were conducted. More detailed information on where to find a specific item can be obtained from the Content Table on the ensuing pages. Data sets and R code for the analyses of reported here and in the main manuscript will be available on the Open Science Framework (OSF) upon publication via https://osf.io/ghtbu/. To facilitate continuously cumulating meta-analysis, we encourage researchers to use the tag *intuitive (dis)honesty* on OSF when pre-registering or uploading research on the subject.

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Method

Mass solicitation method

We spread the call for papers across the following associations and mailing lists: Economic Science Association (ESA), Society of Judgement and Decision-Making (SJDM), European Association for Decision Making (EADM), Academy of Management – Organizational Behavior (AOM OB), Society for Personality and Social Psychology (SPSP) and European Association for Social Psychology (EASP). We also contacted over 100 researchers from the International Conference on Social Dilemmas 2017 for further data-sets. In the process of the meta-analysis, we issued a total of three calls, two of which were pre-registered (see, https://osf.io/vdyxq; and https://osf.io/hxwny).

Coding of additional variables

Demographics. We coded the following (demographic) information about the sample of the study: study location, percentage of female participants, mean and standard deviation of age, the sample composition (student vs. general population), as well as the experimental setting of the study (lab vs. online).

Intuition Manipulation. The sample contained overall ten different methods to induce an intuitive mindset, belonging to five clusters. That is, besides the classification of the standard four categories (see for example, Rand, 2016), of (ego-)depletion ($k = 30$), time pressure ($k = 14$), induction ($k = 11$) and cognitive load ($k = 7$), we also coded foreign language ($k = 11$) as a separate category of intuition (see for a similar methodology, Verschuere, Köbis, Bereby-Meyer, Rand, & Shalvi, 2018).

Dishonesty Paradigms. The included studies employed overall ten unique behavioral paradigms to measure dishonesty. These were clustered into the following four categories: a) *deception*, consisting of sender-receiver games containing deceptive messages (k = 13), b) *performance enhancement*, tasks that allowed participants to inflate their score on a test such as the matrix paradigm (k = 33), c) *stochastic* tasks in which participants can over-report the outcome of a random device like a die or coin (k = 18), d) *other*, which includes other tasks such as the dot-paradigm that allows participants to make self-favoring mistakes over several rounds (Kouchaki & Smith, 2014), as well as tasks in which dishonesty was operationalized by assessing whether participants reported an undue overpayment by the experimenter (see for example, Chiou, Wu, & Cheng, 2017, k = 9).

We furthermore coded the following characteristics of the dishonesty tasks: First, whether the proportion of liars was directly observable (k = 58), based on estimations (k = 14) or subjective ratings (k = 1). Second, we coded whether financial incentives for lying existed (k = 67), or not e.g. food vouchers (k = 5), one study manipulated the existence of incentives (k = 1). Third, we standardized the size of the incentives into a purchasing power parity score in US $. Finally, we coded whether the study entailed experimental deception – that is whether the experimenter(s) deceived the participants about the actual purpose of the study (see for similar approaches, Gerlach, Teodorescu, & Hertwig, 2019).

Meta-analysis 1 - Estimation of the percentage of liars

When dishonesty was not directly observable we relied on different algorithms to estimate the percentage of liars. Below we outline the calculations for each:

Dot Paradigm

To explain how we categorized individuals as liars, let us briefly outline the dot paradigm (see for example, Gino, Norton, & Ariely, 2010; Kouchaki & Desai, 2015). In the dot paradigm, participants are instructed to count and report the number of dots on two sides of a screen. Participants see a square that is divided into two equal halves. They then have to indicate which of the two sides of contains more dots. Reporting more dots in the right side is incentivized with 0,05$ while indicating more dots on the left side is merely incentivized with 0,005$.

There are three types of trials:

1. clear left trials (*n* = 34)

2. clear right trials (*n* = 16)

3. ambiguous trials (*n* = 50)

Thus, participants have an incentive to break the rules to report honestly and instead indicate that more dots are on the right side independent on whether this is indeed the case.

Estimation of liars. In order to get an estimate of the number of liars we therefore calculated:

$$\left(\frac{R_{sfm\,(lt)}}{n(lt)} - \frac{R_{shm\,(rt)}}{n(rt)} > \alpha\right) \rightarrow E^{+}$$

With number of self-favoring mistakes in clear left trials $R_{sfm\,(lt)}$ divided by the overall number of clear left trials $n(lt)$.

We subtract from this term the number of self-hurting mistakes in clear right trials $R_{shm\,(rt)}$ in relation to the overall number of right trials $n(rt)$

As a criterion to be classified as dishonest E^{+} we employ a significance level of $\alpha < .05$

Spot-the-difference paradigm

Gai and Putoni (n.d.) use the spot-the-difference task in which participants observe a pair of pictures and have to indicate whether they found three differences between them. Different trials of the task contain different pairs of pictures that have zero (A0), one (A1), two (A2) or three (A3) differences between them. Hence, claiming to have found two or more difference is only possible in A3 trials. Participants who claim to have found in the other three trials (A0-2) are lying. Given that the task is played repeatedly and perceptual mistakes especially in the A2 trials might inflate the number of liars, we classify liars by analyzing the clear trials in which there are in fact only zero or one difference (A1, A0). Here, we code all participants who claimed to have identified 3 mistakes as lying.

Stochastic tasks

To estimate the percentage of liars for studies that employed stochastic tasks such as coin flipping and die rolling paradigm, we used an algorithm proposed by Garbarino, Slonim & Vileval (2016). Their algorithm is:

$$\mathrm{E}_{N,R,p} = \sum_{T=0}^{R} \left(P_{T,R,N,p} * L_{T,R,N}\right)$$

where $\mathrm{E}_{N,R,p}$ refers to the expected number of liars out of a total sample (*N*), given the number of participants who reported a higher outcome (*R*) and the probability the worse outcome (*p*). $P_{T,R,N,p}$ refers to the probability of each possible realization of participants who actually observed the high outcome (*T*), these are the honest winners.

In order to get an estimate of the honest winners, Garbarino et al. (2016) make the assumption that participants are not dishonest to worsen their outcome, i.e. they exclude downward lying. Given this assumption, the number of participants that observed the high outcome (*T*) cannot exceed the number of participants who report the higher outcome (*R*) because the number of participants who observed the low outcome is at least as large as the difference between the overall sample and the number of participants who reported the higher outcome (*N*-*R*).

Therefore, the probability of participants *observing* the high outcome (T) given the number of participants who *report* the high outcome (*R*) is:

$$\mathrm{P}_{T,R,N,p} = \frac{Q_{T,N,p}}{\sum_{k=0}^{R} Q_{k,N,p}}$$

with T = 0 to R,

$Q_{T,N,p}$ refers to the probability of the realization of T based on the binominal distribution with N observations, R successes and the probability of *p.*

$\sum_{k=0}^{R} Q_{k,N,p}$ refers to the adjustment to the probabilities to the cumulative likelihood that 0-R participants observe the high outcome. More specifically this means that the Probability Distribution Function (PDF) as well as the Cumulative Distribution Function (CDF) are taken into account. The PDF for a given number of participants being dishonest ($y = L_{T,R,N}$) given the number of participants who reported high outcomes (R) is:

$$\Pr\left(y = L_{\mathrm{T,R,N}}\right) = \mathrm{P}_{\mathrm{T,N,R},p}$$

The CDF is then derived by summing the number of participants who were dishonest over the PDF:

$$\Pr\left(y \geq L_{\mathrm{T,R,N}}\right) = \sum_{k=0}^{T}\left(\mathrm{P}_{\mathrm{k,R,N},p}\right)$$

Taken together, the algorithm estimates the proportion of liars based on the observed number of reported favorable outcomes. It takes the number of people who report the positive outcome, subtracts from it the probability of the negative outcome. This term is then divided by the difference score between the overall sample minus the probability of the negative outcome. This term is then multiplied by the probability function (PDF) and Cumulative Distribution Function (CDF) of the negative outcome.

Best outcomes

For the analysis reported in the main manuscript, we estimated the proportion of liars by defining the *best* outcome as favorable. That means for the die-rolling paradigm (Fischbacher & Föllmi-Heusi, 2013), only those who reported the highest outcome were classified as obtaining a favorable outcome. For example, when die rolls are paid according to the number that is reported (e.g. 1=1€,2=2€ etc.), only a ‘6’ is coded as a ‘favorable outcome’.

Another stochastic task is the coin-tossing task that let participants flip a coin repeatedly (e.g. 20 times) and pay them for reported heads. Here, the probability of the best outcome (20 heads) is:

$$\Pr(best\ outcome) = \binom{20}{20} 0{,}5^{(20)}(1-0{,}5)^{20-(20)} = 9{,}53674 * 10^{-7}$$

Positive outcomes

Since previous research shows that participants frequently do not only lie to the maximum possible extend but frequently lie to a small extend (Fischbacher & Föllmi-Heusi, 2013; Shalvi, Eldar, & Bereby-Meyer, 2012; Verschuere, Meijer, et al., 2018), we also estimated the proportion of liars based on “better outcomes”. Thus, we coded all outcomes higher than the mean as positive. To exemplify this logic, for the die-rolling paradigm this criterion considers outcomes of ‘4’, ‘5’ and ‘6’ as favorable.

With the one study that used 20-coin flips as a lying measure the probability calculations are more complex. In order to calculate the proportion of liars, the probability of flipping the coin 20 times and having 11-20 heads is needed first.

$$\Pr(x = s) = \binom{n}{s} p^s(1-p)^{n-s}$$

With *s* being the number of successes and *n* the number of trials and *p* being the probability of success

Based on $\binom{n}{s} = \frac{n!}{(s!n-s)!}$ we first calculated the probability of a positive outcome:

$$\Pr(positive\ outcome) = \binom{20}{11,12,13 \dots 20} 0{,}5^{(11,12,13 \dots 20)}(1-0{,}5)^{20-(11,12,13 \dots 20)} = 0{,}4119$$

Alternative estimations

Moshagen and Hilbig (2017) propose a similar, yet slightly different algorithm to estimate the number of liars in a sample. This algorithm compares the observed frequency of favorable outcomes with the expected frequency to estimate how many participants likely lied. In more detail, the algorithm extends the lying estimations for the six-sided die rolling paradigm originally put forth by Fischbacher and Fölmi-Heusi (2013):

$$\mathrm{E}_{N,R,p} = (R - p(x_2)) * \frac{6}{5}$$

Where R is the number of participants who reported the high outcome (x_2) and p(x_2) refers to the likelihood of winning. The multiplication by $\frac{6}{5}$ corrects for the honest winners as in the die rolling

$$\frac{1}{(1 - p(x_2))} = \frac{6}{5}$$

Moshagen & Hilbig (2017) generalize this formula to all sorts of stochastic dishonesty tasks, by outlining the algorithm for estimations of liars:

$$\mathrm{E}_{N,R,p} = (R - p(x_2)) * \frac{1}{(1 - p(x_2))}$$

Where $\mathrm{E}_{N,R,p}$ refers to the expected number of liars out of a total sample (*N*), given the number of participants who reported a higher outcome (R) and the probability of the worse outcome (p).

Also for this algorithm we used twofold lying estimations according to the definition of a favorable outcome. That means, we once coded the highest outcome as a favorable outcome and once the positive outcome as a favorable outcome. Results for all three alternative estimations as well as for an aggregate estimation that combines all four estimations are reported in the Results section below.

Analysis

Statistical analysis software

For the main analysis, we used the R packages meta and metafor (Viechtbauer, 2010). The data sets and the R scripts for all analyses and figures reported here are freely available on the OSF (see, https://osf.io/8scw2/). To conduct the p-curve analyses (Simonsohn, Nelson, & Simmons, 2013), we calculated the Z-value for each study and imputed them into the *p*-curve app 4.052 (at http://www.p-curve.com/app4/).

Results – Additional analyses – Meta-analysis 1

Fixed effects analysis

Although in the majority of cases of meta-analyses in behavioral research random effects models are an adequate choice because they account for *within* and *between* study heterogeneity, fixed effects models can provide useful insights about the sample of studies included in a given meta-analysis (Hedges & Vevea, 1998; Schwarzer, Carpenter, & Rücker, 2015; Viechtbauer, 2010). We therefore here report the fixed effect meta-regression testing whether a difference in the effect of intuition on (dis)honesty exists in the studies included in the sample.

The meta-regression analysis using a fixed effects model for the first meta-analysis reveals a significant social harm moderation effect (Z = -6.82; *p* <.0001). Fixed effect subgroup analysis show an intuitive dishonesty effect for studies entailing an abstract victim (log *OR* = 0.37, 95%CrI = [0.24; 0.45], *Z* = 6.27, *p* <.0001), the odds of dishonesty being 46.9% higher in the intuition condition compared to the control condition. For studies in which dishonesty inflicted harm on a concrete victim, conversely, fixed effects analysis reveals an intuitive *honesty* effect (log *OR* = -0.25, 95%CrI = [-0.38; -0.12], *Z*= -3.68, *p* = .0002). That is, the odds for *honesty* are 19.7% higher in the intuition condition. This effect stems from the fact that in fixed effects models more weight is assigned to more precise studies as it rests on the underlying assumption that all studies share on fixed true effect (Borenstein, Hedges, & Rothstein, 2007).

Robustness tests

Small cell size corrections. We use three different ways to correct for small cell sizes that can potentially inflate odds ratios and thus distort the analysis. As a first alternative to treatment's arm correction reported in the main manuscript, we employed a continuity correction. Gart and Zweifel (1967) show that adding a constant increment to all cells (most commonly 0.5) reduces the bias of the overall estimate. Running the meta-regression analysis using this continuity correction replicates the moderation of social harm (*Z* = -1.95; *p* = .0512). Random effects subgroup analyses reveal a significant intuitive dishonesty effect for abstract victims (log *OR* = 0.39; 95%CrI = [-0.86; 1.64]) and no effect for concrete victims (log *OR* = -0.04; 95%CrI = [-1.32; 1.24]).

A second alternative to deal with small or zero-cell sizes is to exclude all studies for which the cell size is zero from the analysis (Schwarzer et al., 2015). In the current meta-analysis, one study has an estimated number of liars that is zero (Van der Cruyssen, D´hondt, Meijer, & Verschuere, 2019). Running the meta-regression analysis without that study largely replicates the moderation effect reported in the main manuscript (Z = -1.95; p = 0.051). Random subgroup analyses replicate the significant intuitive dishonesty effect for abstract victims (log OR = 0.386 ; 95%CrI = [-0.86; 1.63]) and no intuitive dishonesty effect for concrete victims (log OR = -0.041; 95%CrI = [-1.32; 1.24]). For both analysis the results are very close to the results reported and plotted in the main manuscript. We therefore do not present forest plots here (see for R code to produce these plots, OSF).

Third, given the criticism of different forms of continuity correction, we also conducted analysis using the Peto odds ratios because this method does not require a continuity correction altogether (Yusuf, Peto, Lewis, Collins, & Sleight, 1985). This method has been shown to perform worse when the distribution of observations across the experimental control condition is severely unbalanced (Greenland & Salvan, 1990). Given that the current meta-analysis draws on experimental set-ups with random assignment to control and experimental condition, the overall distribution is evenly balanced ($N_{intuition}$ = 6,295; 49.7% vs. $N_{control}$ = 6,380; 50.3%). In addition to that, Peto odds ratio meta-analysis provides a fixed effects analysis reliable results for effect sizes that are relatively small, which is the case in this meta-analysis.

Running the analysis with Peto log odds ratios confirms a significant intuitive dishonesty effect for abstract victims (log Peto OR = 0.34, 95%CrI = [0.23; 0.44], Z =

6.33, $p < .0001$) and a significant intuitive honesty effect for studies with a concrete victim (*log Peto OR* = -0.24, 95%CrI = [-0.37; -0.11], Z = -3.56, p = .0004).

Inter-study heterogeneity

Besides Peto odds ratios, we also employed a second method that has proven robust to large inter-study heterogeneity, namely arcsine meta-analysis (Rücker, Schwarzer, & Carpenter, 2008). Instead of odds ratios, this method uses the difference of the arcsine-transformed event probabilities as an outcome measure, hence:

$$\Delta_k = arcsin\sqrt{p_{ek}} - arcsin\sqrt{p_{ck}},$$ which range from –π/2 to π/2.

Using the inverse sine transformed difference as the outcome measure and the restricted maximum likelihood method as an estimator for the inter-study heterogeneity, the mixed effects meta-regression analysis reveal a social harm moderation of (Z = -1.84, p = 0.066). Subgroup analysis reveal an intuitive dishonesty effect for abstract victims (*ASD* = 0.08, 95%CrI = [-0.31; 0.29], Z =3.51, $p < 0.004$) and no effect for concrete victims (*ASD* = -0.009, 95%CrI = [-0.21; 0.367], Z = -0.22 , p = .825), see also Figure S1.

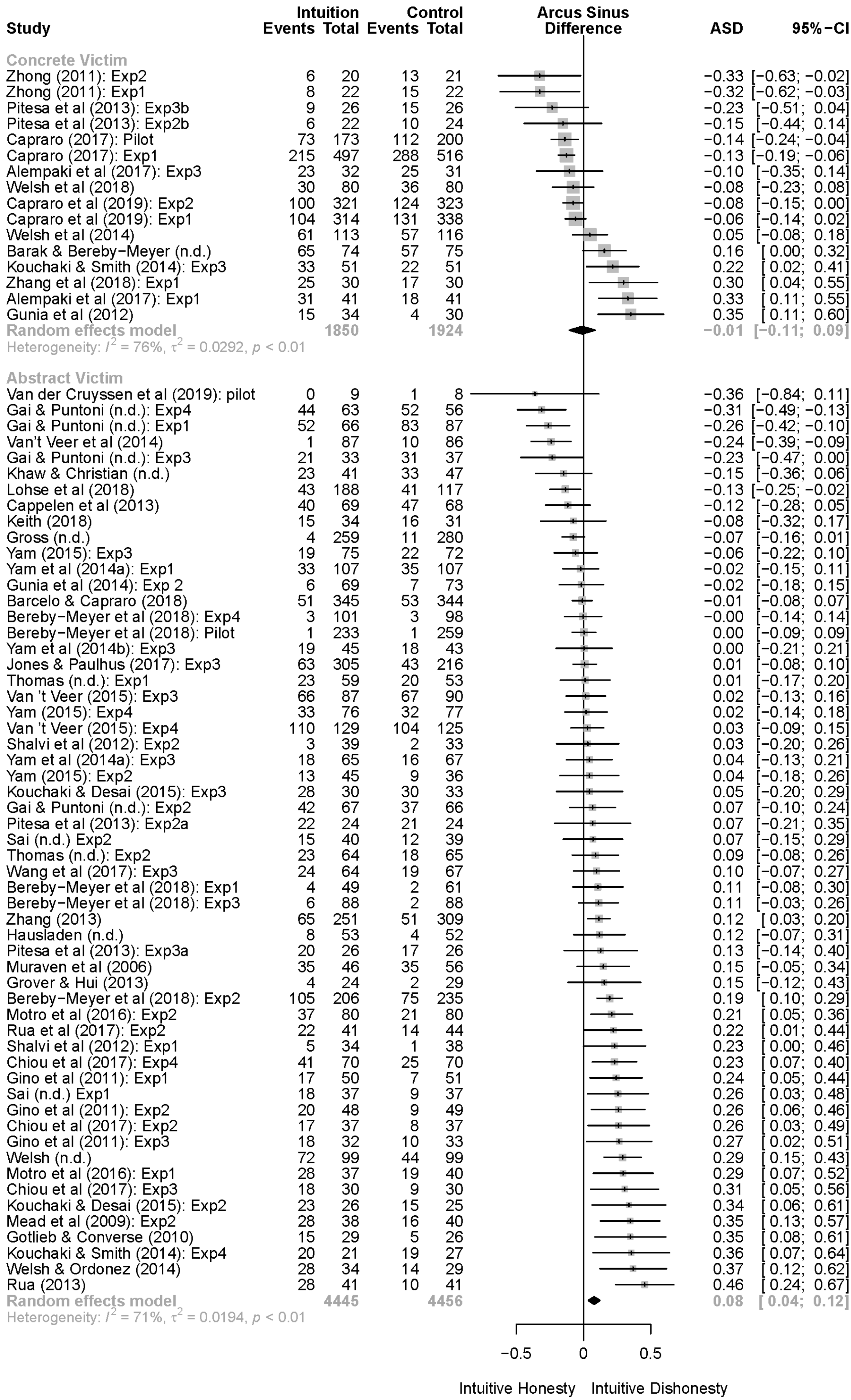

Study
Intuition Events Total
Control Events Total
Arcus Sinus Difference
ASD
95%−CI
Concrete Victim
Zhong (2011): Exp2 6 20 13 21 −0.33 [−0.63; −0.02]
Zhong (2011): Exp1 8 22 15 22 −0.32 [−0.62; −0.03]
Pitesa et al (2013): Exp3b 9 26 15 26 −0.23 [−0.51; 0.04]
Pitesa et al (2013): Exp2b 6 22 10 24 −0.15 [−0.44; 0.14]
Capraro (2017): Pilot 73 173 112 200 −0.14 [−0.24; −0.04]
Capraro (2017): Exp1 215 497 288 516 −0.13 [−0.19; −0.06]
Alempaki et al (2017): Exp3 23 32 25 31 −0.10 [−0.35; 0.14]
Welsh et al (2018) 30 80 36 80 −0.08 [−0.23; 0.08]
Capraro et al (2019): Exp2 100 321 124 323 −0.08 [−0.15; 0.00]
Capraro et al (2019): Exp1 104 314 131 338 −0.06 [−0.14; 0.02]
Welsh et al (2014) 61 113 57 116 0.05 [−0.08; 0.18]
Barak & Bereby−Meyer (n.d.) 65 74 57 75 0.16 [0.00; 0.32]
Kouchaki & Smith (2014): Exp3 33 51 22 51 0.22 [0.02; 0.41]
Zhang et al (2018): Exp1 25 30 17 30 0.30 [0.04; 0.55]
Alempaki et al (2017): Exp1 31 41 18 41 0.33 [0.11; 0.55]
Gunia et al (2012) 15 34 4 30 0.35 [0.11; 0.60]
Random effects model 1850 1924 −0.01 [−0.11; 0.09]
Heterogeneity: I^2 = 76%, τ^2 = 0.0292, p < 0.01
Abstract Victim
Van der Cruyssen et al (2019): pilot 0 9 1 8 −0.36 [−0.84; 0.11]
Gai & Puntoni (n.d.): Exp4 44 63 52 56 −0.31 [−0.49; −0.13]
Gai & Puntoni (n.d.): Exp1 52 66 83 87 −0.26 [−0.42; −0.10]
Van't Veer et al (2014) 1 87 10 86 −0.24 [−0.39; −0.09]
Gai & Puntoni (n.d.): Exp3 21 33 31 37 −0.23 [−0.47; 0.00]
Khaw & Christian (n.d.) 23 41 33 47 −0.15 [−0.36; 0.06]
Lohse et al (2018) 43 188 41 117 −0.13 [−0.25; −0.02]
Cappelen et al (2013) 40 69 47 68 −0.12 [−0.28; 0.05]
Keith (2018) 15 34 16 31 −0.08 [−0.32; 0.17]
Gross (n.d.) 4 259 11 280 −0.07 [−0.16; 0.01]
Yam (2015): Exp3 19 75 22 72 −0.06 [−0.22; 0.10]
Yam et al (2014a): Exp1 33 107 35 107 −0.02 [−0.15; 0.11]
Gunia et al (2014): Exp 2 6 69 7 73 −0.02 [−0.18; 0.15]
Barcelo & Capraro (2018) 51 345 53 344 −0.01 [−0.08; 0.07]
Bereby−Meyer et al (2018): Exp4 3 101 3 98 −0.00 [−0.14; 0.14]
Bereby−Meyer et al (2018): Pilot 1 233 1 259 0.00 [−0.09; 0.09]
Yam et al (2014b): Exp3 19 45 18 43 0.00 [−0.21; 0.21]
Jones & Paulhus (2017): Exp3 63 305 43 216 0.01 [−0.08; 0.10]
Thomas (n.d.): Exp1 23 59 20 53 0.01 [−0.17; 0.20]
Van 't Veer (2015): Exp3 66 87 67 90 0.02 [−0.13; 0.16]
Yam (2015): Exp4 33 76 32 77 0.02 [−0.14; 0.18]
Van 't Veer (2015): Exp4 110 129 104 125 0.03 [−0.09; 0.15]
Shalvi et al (2012): Exp2 3 39 2 33 0.03 [−0.20; 0.26]
Yam et al (2014a): Exp3 18 65 16 67 0.04 [−0.13; 0.21]
Yam (2015): Exp2 13 45 9 36 0.04 [−0.18; 0.26]
Kouchaki & Desai (2015): Exp3 28 30 30 33 0.05 [−0.20; 0.29]
Gai & Puntoni (n.d.): Exp2 42 67 37 66 0.07 [−0.10; 0.24]
Pitesa et al (2013): Exp2a 22 24 21 24 0.07 [−0.21; 0.35]
Sai (n.d.) Exp2 15 40 12 39 0.07 [−0.15; 0.29]
Thomas (n.d.): Exp2 23 64 18 65 0.09 [−0.08; 0.26]
Wang et al (2017): Exp3 24 64 19 67 0.10 [−0.07; 0.27]
Bereby−Meyer et al (2018): Exp1 4 49 2 61 0.11 [−0.08; 0.30]
Bereby−Meyer et al (2018): Exp3 6 88 2 88 0.11 [−0.03; 0.26]
Zhang (2013) 65 251 51 309 0.12 [0.03; 0.20]
Hausladen (n.d.) 8 53 4 52 0.12 [−0.07; 0.31]
Pitesa et al (2013): Exp3a 20 26 17 26 0.13 [−0.14; 0.40]
Muraven et al (2006) 35 46 35 56 0.15 [−0.05; 0.34]
Grover & Hui (2013) 4 24 2 29 0.15 [−0.12; 0.43]
Bereby−Meyer et al (2018): Exp2 105 206 75 235 0.19 [0.10; 0.29]
Motro et al (2016): Exp2 37 80 21 80 0.21 [0.05; 0.36]
Rua et al (2017): Exp2 22 41 14 44 0.22 [0.01; 0.44]
Shalvi et al (2012): Exp1 5 34 1 38 0.23 [0.00; 0.46]
Chiou et al (2017): Exp4 41 70 25 70 0.23 [0.07; 0.40]
Gino et al (2011): Exp1 17 50 7 51 0.24 [0.05; 0.44]
Sai (n.d.) Exp1 18 37 9 37 0.26 [0.03; 0.48]
Gino et al (2011): Exp2 20 48 9 49 0.26 [0.06; 0.46]
Chiou et al (2017): Exp2 17 37 8 37 0.26 [0.03; 0.49]
Gino et al (2011): Exp3 18 32 10 33 0.27 [0.02; 0.51]
Welsh (n.d.) 72 99 44 99 0.29 [0.15; 0.43]
Motro et al (2016): Exp1 28 37 19 40 0.29 [0.07; 0.52]
Chiou et al (2017): Exp3 18 30 9 30 0.31 [0.05; 0.56]
Kouchaki & Desai (2015): Exp2 23 26 15 25 0.34 [0.06; 0.61]
Mead et al (2009): Exp2 28 38 16 40 0.35 [0.13; 0.57]
Gotlieb & Converse (2010) 15 29 5 26 0.35 [0.08; 0.61]
Kouchaki & Smith (2014): Exp4 20 21 19 27 0.36 [0.07; 0.64]
Welsh & Ordonez (2014) 28 34 14 29 0.37 [0.12; 0.62]
Rua (2013) 28 41 10 41 0.46 [0.24; 0.67]
Random effects model 4445 4456 0.08 [0.04; 0.12]
Heterogeneity: I^2 = 71%, τ^2 = 0.0194, p < 0.01
−0.5
0
0.5
Intuitive Honesty
Intuitive Dishonesty

Figure S1. Mixed effects analysis for the social harm subgroups using Arcus Sinus Difference as a dependent variable.

Hartung-Knapp Adjustment

Recent simulations suggest an alternative method to estimate the overall variance that yields more accurate estimations, particularly when the samples are small (IntHout, Ioannidis, & Borm, 2014). Running the meta-regression analysis using this so-called Hartung and Knapp (Hartung & Knapp, 2001) adjustment reveals a social harm moderation effect of $F(1,72) = -1.857$, $p = .067$. Subgroup analysis again confirms that an intuitive mindset leads to more dishonesty when an abstract victim is hurt (log *OR* = 0.385, 95%CrI = [-0.89; 1.66]), yet no such effect when a concrete victim gets hurt by dishonesty (*log OR* = -0.041, 95%CrI = [-1.35; 1.27]), see also Figure S2.

Study	Intuition Events	Intuition Total	Control Events	Control Total	logOR	95%-CI
Concrete Victim						
Zhong (2011): Exp2	6	20	13	21	−1.33	[−2.63; −0.03]
Zhong (2011): Exp1	8	22	15	22	−1.32	[−2.57; −0.07]
Pitesa et al (2013): Exp3b	9	26	15	26	−0.95	[−2.07; 0.18]
Pitesa et al (2013): Exp2b	6	22	10	24	−0.64	[−1.88; 0.60]
Capraro (2017): Pilot	73	173	112	200	−0.56	[−0.97; −0.14]
Capraro (2017): Exp1	215	497	288	516	−0.50	[−0.75; −0.26]
Alempaki et al (2017): Exp3	23	32	25	31	−0.49	[−1.67; 0.69]
Capraro et al (2019): Exp2	100	321	124	323	−0.32	[−0.65; 0.01]
Welsh et al (2018)	30	80	36	80	−0.31	[−0.94; 0.32]
Capraro et al (2019): Exp1	104	314	131	338	−0.25	[−0.57; 0.08]
Welsh et al (2014)	61	113	57	116	0.19	[−0.32; 0.71]
Barak & Bereby−Meyer (n.d.)	65	74	57	75	0.82	[−0.05; 1.70]
Kouchaki & Smith (2014): Exp3	33	51	22	51	0.88	[0.08; 1.68]
Zhang et al (2018): Exp1	25	30	17	30	1.34	[0.14; 2.54]
Alempaki et al (2017): Exp1	31	41	18	41	1.38	[0.43; 2.32]
Gunia et al (2012)	15	34	4	30	1.64	[0.38; 2.89]
Random effects model		**1850**		**1924**	**−0.04**	**[−0.51; 0.43]**
Heterogeneity: $I^2 = 74\%$, $\tau^2 = 0.4906$, $p < 0.01$						
Abstract Victim						
Van't Veer et al (2014)	1	87	10	86	−2.43	[−4.50; −0.35]
Gai & Puntoni (n.d.): Exp4	44	63	52	56	−1.73	[−2.88; −0.57]
Gai & Puntoni (n.d.): Exp1	52	66	83	87	−1.72	[−2.88; −0.56]
Van der Cruyssen et al (2019): pilot	0	9	1	8	−1.27	[−4.55; 2.02]
Gai & Puntoni (n.d.): Exp3	21	33	31	37	−1.08	[−2.21; 0.04]
Gross (n.d.)	4	259	11	280	−0.96	[−2.12; 0.20]
Khaw & Christian (n.d.)	23	41	33	47	−0.61	[−1.49; 0.27]
Lohse et al (2018)	43	188	41	117	−0.60	[−1.11; −0.09]
Cappelen et al (2013)	40	69	47	68	−0.48	[−1.19; 0.22]
Keith (2018)	15	34	16	31	−0.30	[−1.28; 0.68]
Yam (2015): Exp3	19	75	22	72	−0.26	[−0.98; 0.46]
Gunia et al (2014): Exp 2	6	69	7	73	−0.11	[−1.25; 1.04]
Yam et al (2014a): Exp1	33	107	35	107	−0.09	[−0.66; 0.49]
Barcelo & Capraro (2018)	51	345	53	344	−0.05	[−0.47; 0.37]
Bereby−Meyer et al (2018): Exp4	3	101	3	98	−0.03	[−1.66; 1.59]
Yam et al (2014b): Exp3	19	45	18	43	0.01	[−0.83; 0.86]
Jones & Paulhus (2017): Exp3	63	305	43	216	0.05	[−0.39; 0.48]
Thomas (n.d.): Exp1	23	59	20	53	0.05	[−0.71; 0.82]
Van't Veer (2015): Exp3	66	87	67	90	0.08	[−0.61; 0.76]
Yam (2015): Exp4	33	76	32	77	0.08	[−0.56; 0.72]
Bereby−Meyer et al (2018): Pilot	1	233	1	259	0.11	[−2.67; 2.88]
Van't Veer (2015): Exp4	110	129	104	125	0.16	[−0.52; 0.83]
Yam (2015): Exp2	13	45	9	36	0.20	[−0.79; 1.19]
Yam et al (2014a): Exp3	18	65	16	67	0.20	[−0.58; 0.98]
Shalvi et al (2012): Exp2	3	39	2	33	0.26	[−1.60; 2.11]
Gai & Puntoni (n.d.): Exp2	42	67	37	66	0.28	[−0.42; 0.97]
Sai (n.d.) Exp2	15	40	12	39	0.30	[−0.63; 1.23]
Kouchaki & Desai (2015): Exp3	28	30	30	33	0.34	[−1.53; 2.20]
Thomas (n.d.): Exp2	23	64	18	65	0.38	[−0.36; 1.13]
Wang et al (2017): Exp3	24	64	19	67	0.42	[−0.32; 1.15]
Pitesa et al (2013): Exp2a	22	24	21	24	0.45	[−1.43; 2.34]
Pitesa et al (2013): Exp3a	20	26	17	26	0.57	[−0.65; 1.79]
Zhang (2013)	65	251	51	309	0.57	[0.16; 0.98]
Muraven et al (2006)	35	46	35	56	0.65	[−0.22; 1.51]
Hausladen (n.d.)	8	53	4	52	0.76	[−0.51; 2.02]
Bereby−Meyer et al (2018): Exp2	105	206	75	235	0.80	[0.41; 1.18]
Motro et al (2016): Exp2	37	80	21	80	0.88	[0.22; 1.55]
Rua et al (2017): Exp2	22	41	14	44	0.91	[0.03; 1.79]
Chiou et al (2017): Exp4	41	70	25	70	0.93	[0.25; 1.62]
Bereby−Meyer et al (2018): Exp1	4	49	2	61	0.96	[−0.78; 2.71]
Grover & Hui (2013)	4	24	2	29	0.99	[−0.80; 2.79]
Sai (n.d.) Exp1	18	37	9	37	1.08	[0.09; 2.07]
Gino et al (2011): Exp3	18	32	10	33	1.08	[0.06; 2.10]
Chiou et al (2017): Exp2	17	37	8	37	1.13	[0.11; 2.14]
Bereby−Meyer et al (2018): Exp3	6	88	2	88	1.15	[−0.48; 2.77]
Gino et al (2011): Exp2	20	48	9	49	1.16	[0.23; 2.08]
Gino et al (2011): Exp1	17	50	7	51	1.17	[0.19; 2.16]
Welsh (n.d.)	72	99	44	99	1.20	[0.61; 1.80]
Motro et al (2016): Exp1	28	37	19	40	1.24	[0.26; 2.21]
Chiou et al (2017): Exp3	18	30	9	30	1.25	[0.18; 2.32]
Mead et al (2009): Exp2	28	38	16	40	1.44	[0.48; 2.40]
Gotlieb & Converse (2010)	15	29	5	26	1.50	[0.29; 2.72]
Welsh & Ordonez (2014)	28	34	14	29	1.61	[0.47; 2.75]
Kouchaki & Desai (2015): Exp2	23	26	15	25	1.63	[0.19; 3.08]
Shalvi et al (2012): Exp1	5	34	1	38	1.85	[−0.35; 4.05]
Rua (2013)	28	41	10	41	1.90	[0.93; 2.87]
Kouchaki & Smith (2014): Exp4	20	21	19	27	2.13	[−0.04; 4.30]
Random effects model		**4445**		**4456**	**0.38**	**[0.16; 0.60]**
Heterogeneity: $I^2 = 64\%$, $\tau^2 = 0.3649$, $p < 0.01$						

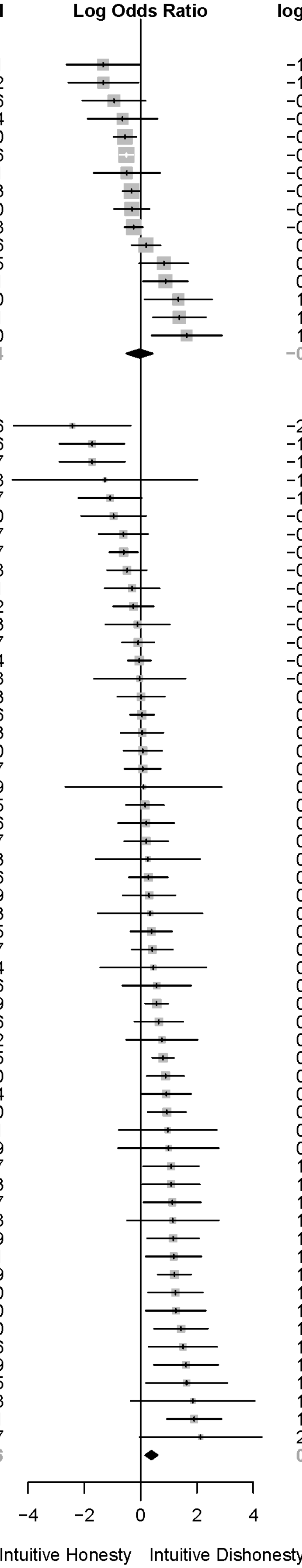


Figure S2. Mixed effects analysis for the social harm subgroups using Hartung Knapp adjustments.

Different estimations for number of liars. We also tested whether the results are robust to the different ways in which the percentage of liars can be estimated. We conducted four separate analysis using both algorithms by Moshagen & Hilbig (2017) (2017) and Garbarino, Slonim and Vileval (2016) and used for each estimation technique two different criteria of favorable outcomes, namely (a)highest possible outcome and (b) higher possible outcomes (see for more details, Method section of the SOM). The results presented in Table S1 show that the subgroup estimates all indicate a significant intuitive dishonesty abstract victims get harmed (all *ps* $< .001$), yet no effect for tasks in which concrete victims get harmed (all *ps* $>.8$). The moderation effect differs across estimations (see Table S1).

Table S1. Overview of overall estimates using different estimations to determine the proportion of liars for abstract and concrete victim subgroups.

Estimation technique	Social Harm moderation	Victim	*Log(OR)*	95%CredInt	*Z*	*p*
GSV6	**Z= -1.94 *p* = .051**	**Abstract**	**0.384**	**[-0.86; 1.63]**	**3.55**	**<.001**
		Concrete	**-0.040**	**[-1.32; 1.24]**	**-0.21**	**.831**
GSV456	*Z* = -1.76; *p* = .078	Abstract	0.389	[-1.05; 1.83]	3.388	<.001
		Concrete	-0.035	[-1.52; 1.45]	-0.165	.869
MH456	Z = -1.27 *p* = .20	Abstract	0.315	[-1.17; 1.80]	2.6063	.0092
		Concrete	0.008	[-1.52; 1.53]	0.0379	.9698
MH6	*Z* = -2.03; *p* = .042	Abstract	0.403	[-0.84; 1.64]	3.6907	<.001
		Concrete	-0.041	[-1.32; 1.23]	-0.2157	.829
AGGREGATE	Z = -1.75; *p* = .079	Abstract	0.359	[-0.96; 1.68]	3.2942	.0010
		Concrete	-0.038	[-1.40; 1.32]	-0.1921	.8476

Note. GSV refers to the algorithm introduced by Garbarino, Slonim & Vileval (2016) and MH refers to the algorithm put forth by Moshagen and Hilbig (2017). The ending '456' refers to the classification of better than average outcomes as favorable, e.g. die rolls of 4,5,6 in die rolling task, while the ending '6' refers to the classification of the best outcome as favorable, e.g. a '6' in the die rolling task. The bolded row GSV6 refers to the data presented in the main text.

Outlier and influential cases. At times the overall estimate of a meta-analysis can be driven by single outliers. To detect such influential outliers, we conducted influence analysis which analyzes each study according to (1) externally standardized residuals, (2) DFFITS values, (3) Cook's distances, (4) covariance ratios, (5) leave-one-out estimates of the amount of heterogeneity, (6) leave-one-out values of the test statistics for heterogeneity, (7) hat values, and (8) weights (see for more information, Viechtbauer & Cheung, 2010). Following the guidelines proposed by Viechtbauer and Cheung (2010), a study is commonly defined as influential if one of the following conditions is met:

1. $DFFITS > 3\sqrt{(\frac{p}{(k-p)})}$, with p referring to number of model coefficients and k the number of studies.
2. lower tail area of a χ^2 distribution with p degrees of freedom cut off by the Cook's distance is larger than 50%.
3. Hat value > 3 $\frac{p}{k}$.
4. DFBETAS values > 1.

Using these criteria, this analysis suggest that no influential studies exist in the first meta-analysis (see Figure S3).

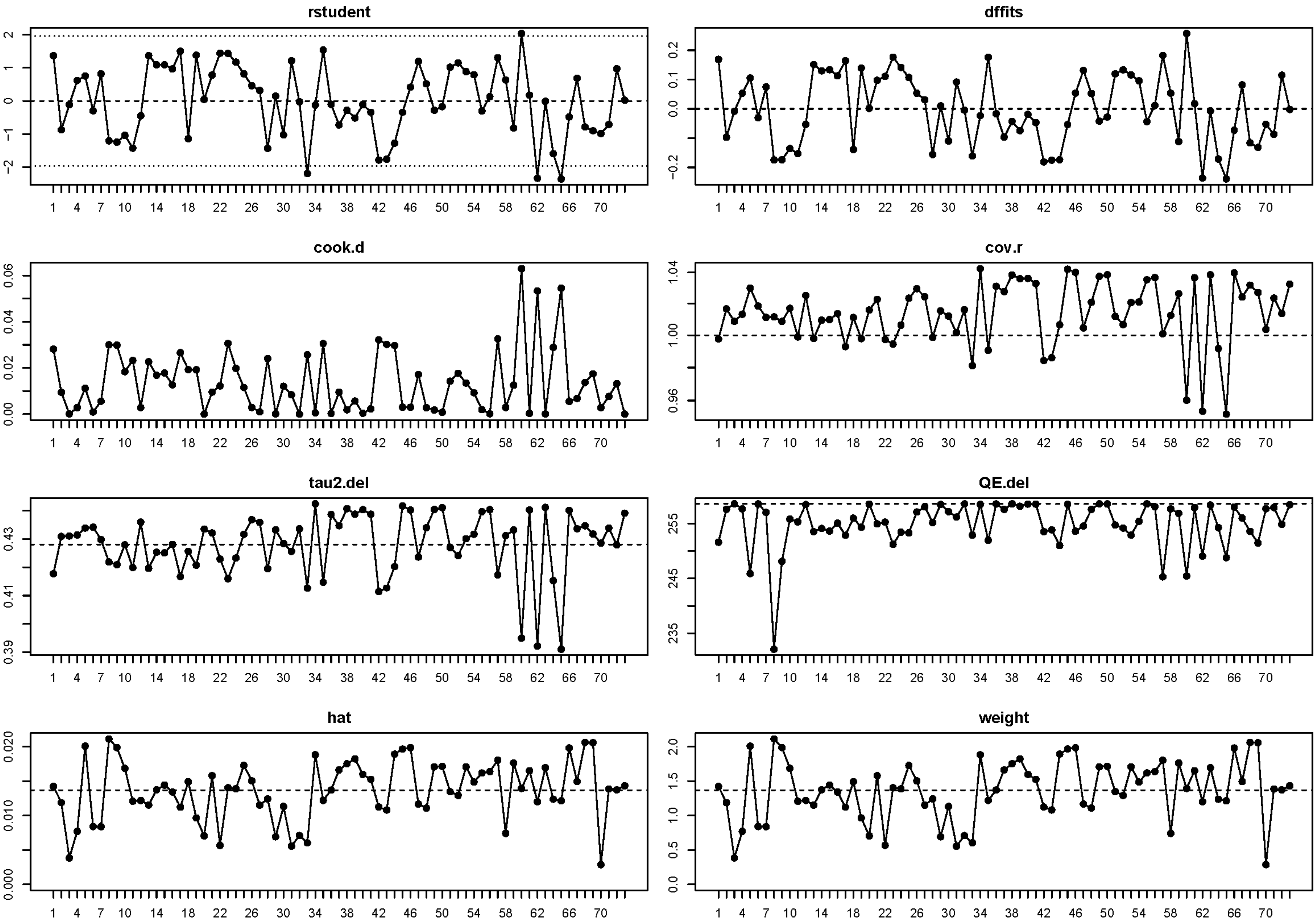
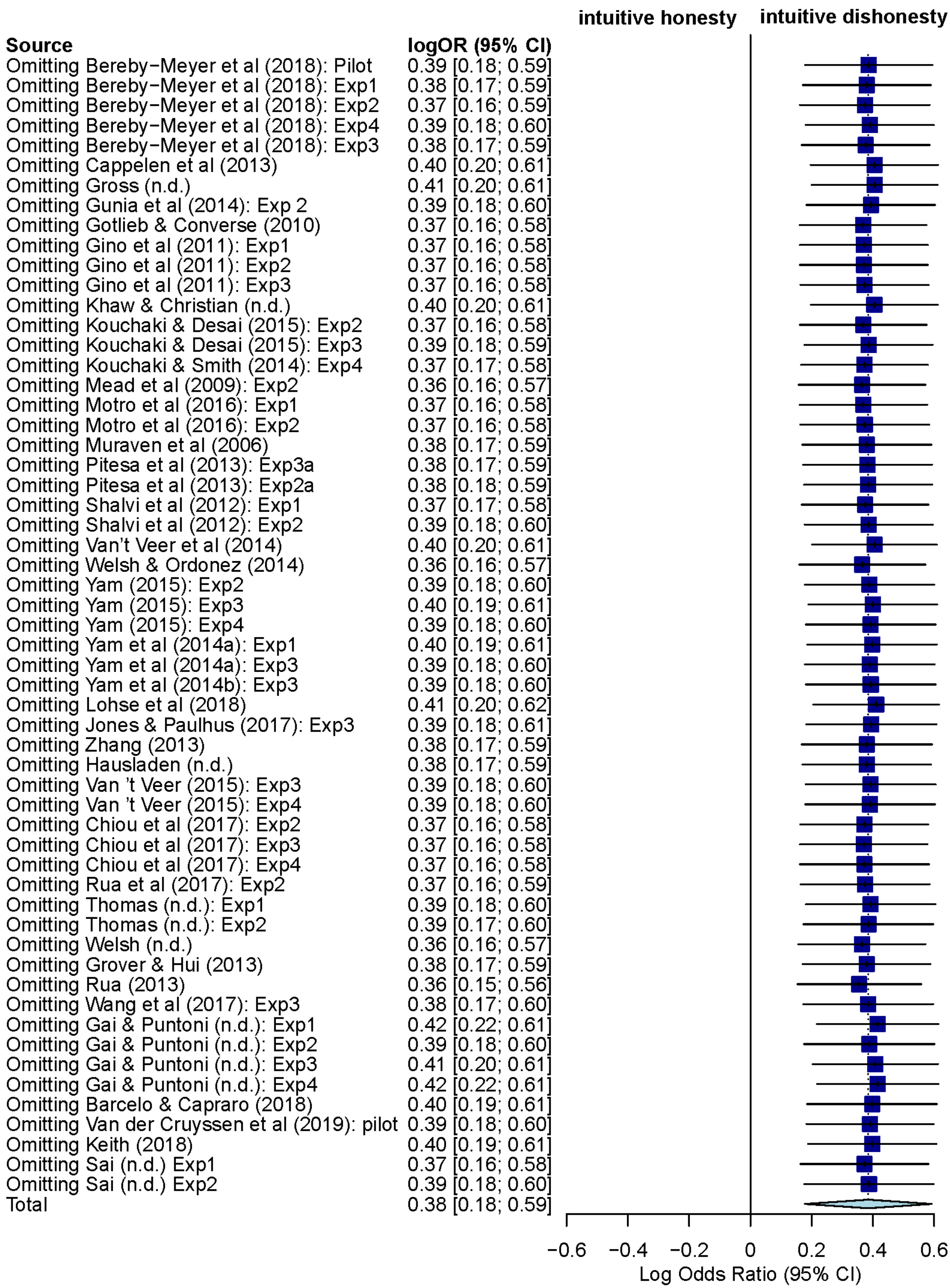

rstudient
dffits
cook.d
cov.r
tau2.del
QE.del
hat
weight
-2 -1 0 1 2
-0.2 0.0 0.1 0.2
0.00 0.02 0.04 0.06
0.96 1.00 1.04
0.39 0.41 0.43
235 245 255
0.000 0.010 0.020
0.0 0.5 1.0 1.5 2.0
1 4 7 10 14 18 22 26 30 34 38 42 46 50 54 58 62 66 70

Figure S3. Plots of the eight different algorithm to identify influential study in the sample of the first meta-analysis.

Although the algorithm does not detect single influential outliers, we nonetheless tested whether the overall estimate is robust to the exclusion of each individual study. As can be seen in Figures S4a, the intuitive dishonesty effect for the abstract victim subgroup is robust to the exclusion of each individual study. Displayed in Figure 4b, also for the concrete victim subgroup the effect remains insignificant for the exclusion of each individual study – although relatively more fluctuation exist compared to the abstract victim subgroup.


intuitive honesty
intuitive dishonesty
Source
logOR (95% CI)
Omitting Bereby−Meyer et al (2018): Pilot 0.39 [0.18; 0.59]
Omitting Bereby−Meyer et al (2018): Exp1 0.38 [0.17; 0.59]
Omitting Bereby−Meyer et al (2018): Exp2 0.37 [0.16; 0.59]
Omitting Bereby−Meyer et al (2018): Exp4 0.39 [0.18; 0.60]
Omitting Bereby−Meyer et al (2018): Exp3 0.38 [0.17; 0.59]
Omitting Cappelen et al (2013) 0.40 [0.20; 0.61]
Omitting Gross (n.d.) 0.41 [0.20; 0.61]
Omitting Gunia et al (2014): Exp 2 0.39 [0.18; 0.60]
Omitting Gotlieb & Converse (2010) 0.37 [0.16; 0.58]
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Omitting Gino et al (2011): Exp2 0.37 [0.16; 0.58]
Omitting Gino et al (2011): Exp3 0.37 [0.16; 0.58]
Omitting Khaw & Christian (n.d.) 0.40 [0.20; 0.61]
Omitting Kouchaki & Desai (2015): Exp2 0.37 [0.16; 0.58]
Omitting Kouchaki & Desai (2015): Exp3 0.39 [0.18; 0.59]
Omitting Kouchaki & Smith (2014): Exp4 0.37 [0.17; 0.58]
Omitting Mead et al (2009): Exp2 0.36 [0.16; 0.57]
Omitting Motro et al (2016): Exp1 0.37 [0.16; 0.58]
Omitting Motro et al (2016): Exp2 0.37 [0.16; 0.58]
Omitting Muraven et al (2006) 0.38 [0.17; 0.59]
Omitting Pitesa et al (2013): Exp3a 0.38 [0.17; 0.59]
Omitting Pitesa et al (2013): Exp2a 0.38 [0.18; 0.59]
Omitting Shalvi et al (2012): Exp1 0.37 [0.17; 0.58]
Omitting Shalvi et al (2012): Exp2 0.39 [0.18; 0.60]
Omitting Van't Veer et al (2014) 0.40 [0.20; 0.61]
Omitting Welsh & Ordonez (2014) 0.36 [0.16; 0.57]
Omitting Yam (2015): Exp2 0.39 [0.18; 0.60]
Omitting Yam (2015): Exp3 0.40 [0.19; 0.61]
Omitting Yam (2015): Exp4 0.39 [0.18; 0.60]
Omitting Yam et al (2014a): Exp1 0.40 [0.19; 0.61]
Omitting Yam et al (2014a): Exp3 0.39 [0.18; 0.60]
Omitting Yam et al (2014b): Exp3 0.39 [0.18; 0.60]
Omitting Lohse et al (2018) 0.41 [0.20; 0.62]
Omitting Jones & Paulhus (2017): Exp3 0.39 [0.18; 0.61]
Omitting Zhang (2013) 0.38 [0.17; 0.59]
Omitting Hausladen (n.d.) 0.38 [0.17; 0.59]
Omitting Van 't Veer (2015): Exp3 0.39 [0.18; 0.60]
Omitting Van 't Veer (2015): Exp4 0.39 [0.18; 0.60]
Omitting Chiou et al (2017): Exp2 0.37 [0.16; 0.58]
Omitting Chiou et al (2017): Exp3 0.37 [0.16; 0.58]
Omitting Chiou et al (2017): Exp4 0.37 [0.16; 0.58]
Omitting Rua et al (2017): Exp2 0.37 [0.16; 0.59]
Omitting Thomas (n.d.): Exp1 0.39 [0.18; 0.60]
Omitting Thomas (n.d.): Exp2 0.39 [0.17; 0.60]
Omitting Welsh (n.d.) 0.36 [0.16; 0.57]
Omitting Grover & Hui (2013) 0.38 [0.17; 0.59]
Omitting Rua (2013) 0.36 [0.15; 0.56]
Omitting Wang et al (2017): Exp3 0.38 [0.17; 0.60]
Omitting Gai & Puntoni (n.d.): Exp1 0.42 [0.22; 0.61]
Omitting Gai & Puntoni (n.d.): Exp2 0.39 [0.18; 0.60]
Omitting Gai & Puntoni (n.d.): Exp3 0.41 [0.20; 0.61]
Omitting Gai & Puntoni (n.d.): Exp4 0.42 [0.22; 0.61]
Omitting Barcelo & Capraro (2018) 0.40 [0.19; 0.61]
Omitting Van der Cruyssen et al (2019): pilot 0.39 [0.18; 0.60]
Omitting Keith (2018) 0.40 [0.19; 0.61]
Omitting Sai (n.d.) Exp1 0.37 [0.16; 0.58]
Omitting Sai (n.d.) Exp2 0.39 [0.18; 0.60]
Total 0.38 [0.18; 0.59]
−0.6
−0.4
−0.2
0
0.2
0.4
0.6
Log Odds Ratio (95% CI)

Figure S4a. Plot of the estimated effect omitting each individual study separately for the abstract harm subgroup in the first meta-analysis.

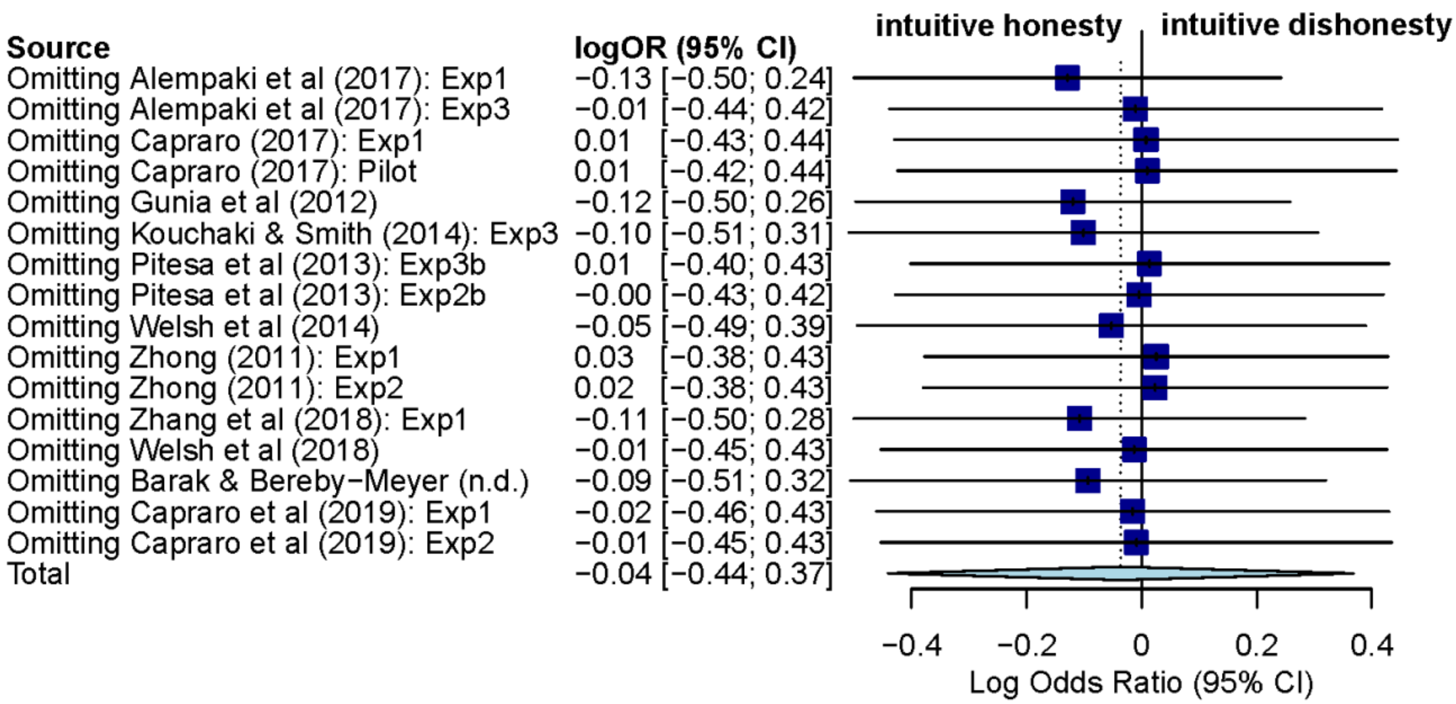

Figure S4b. Plot of the estimated effect omitting each individual study separately for the concrete harm subgroup in the first meta-analysis.

Additional Moderation Analysis

Demographics

Age and Gender. For exploratory purposes, we examined whether the average age of the sample had an effect on the intuitive dishonesty effect. Mixed effects meta regression with the average age of the sample as a predictor shows no significant effect (b = 0.03, SE = .02, Z = -1.85, p = .06). The trend indicates that samples with older average age show a less pronounced intuitive dishonesty effect. Additional analysis reveals no indication for an interaction between social harm and age (Z = 0.52, p = .60).

Next, we tested whether the percentage of females in the sample impacted the results by conducting a mixed meta-regression using the percentage of females in the sample as a predictor. The results indicate a significant negative effect (b = -1.37, SE = 0.61, Z = -2.23, p = .025), suggesting that higher the proportions of females in the sample yields a less pronounced intuitive dishonesty (see also Figure S5). This is in line with other meta-analyses on the subject that show that men on average lie more (Abeler, Nosenzo, & Raymond, 2019; Gerlach et al., 2019). Given that the proportion of females is used as an aggregate measure for each study the danger of ecological bias exists (Berlin, Santanna, Schmid, Szczech, & Feldman, 2002). Thus, this finding should be interpreted with caution. Furthermore, we find no interaction between social harm and the gender composition of the sample (Z = 1.08, p = .27).

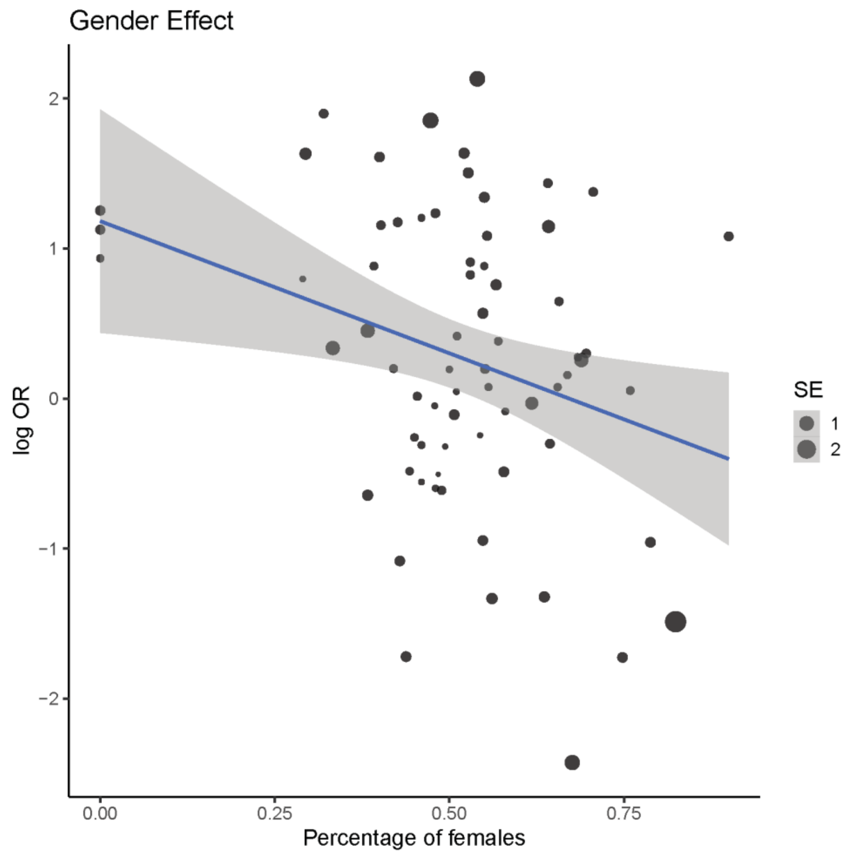

Figure S5. Slope of the meta regression of the percentage of females on the log odds ratios for the first meta-analysis.

Location. We ran subgroup analyses to assess differences across countries. The results of a mixed effect regression analysis reveals significant differences between the different locations of the study ($Q(11) = 38.69$, $p < .0001$). Moreover, we find tentative evidence for an interaction between location and social harm ($Q(17) = 41.725$, $p = 0.001$). That is, in different countries the moderation of social harm might differ in strength (see full results in Table S2). However, the small number of studies in most countries does not reach the minimum threshold of $k = 3$. When following the recommendations by Kepes and colleagues (2012) who argue that results stemming from cells with $k < 10$, should be interpreted with caution, in this analysis only one study site (USA) qualifies for substantiated analysis. Hence, the small k-count prevents us to draw meaningful conclusions about inter-country differences of the intuitive dishonesty effect and its moderation by social harm.

Table S2. Overview of differences in the overall estimate according to the country in which the study was conducted, separated for both social harm conditions.

Country	**Social Harm**	***k***	***Log(OR)***	***Z***	***p***
Canada	Concrete	2	-1.327	-2.493	.013
China	Abstract	6	-0.103	-0.352	.725
	Concrete	2	1.362	2.916	.004
France	Abstract	2	-0.565	-0.882	.378
	Concrete	1	-0.644	-0.873	.383
Germany	Abstract	3	-0.192	-0.472	.637
	Concrete	1	-0.489	-0.688	.492
Israel	Abstract	3	0.954	1.482	.138
	Concrete	1	0.825	1.406	.160
Korea	Abstract	1	0.797	1.390	.165
NA	Abstract	1	0.993	0.936	.349
Netherlands	Abstract	5	-0.418	-1.143	.253
Norway	Abstract	1	-0.484	-0.749	.454
Russia	Abstract	1	0.106	0.070	.944
Spain	Abstract	1	1.146	1.158	.247
Taiwan	Abstract	3	1.083	2.639	0.008
USA	Abstract	30	0.574	4.427	<.0001
	Concrete	9	-0.152	-0.950	0.342

Population

We tested whether differences across different samples exist by coding samples as either student ($k = 49$) or general population ($k = 24$), similar to previous meta-analysis in dishonesty by Abeler and colleagues (2019). Mixed effect meta-regression reveals that the two groups significantly differ ($Z = 2.54$, $p = 0.01$), indicating that the intuitive dishonesty effect is stronger for student samples than for participants sampled from the general population. The interaction between population and harm is not significant ($Z = -0.22$; $p = .823$).

Intuition Manipulation

To test whether different ways to induce an intuitive mindset impacted the effect. Mixed effects regression models indicate no significant differences across the various intuition manipulations ($Q(4) = 4.886$, $p = .29$), nor an interaction effect with social harm ($Q(8) = 13.09$, $p = .11$). The interaction analysis is undermined by small cell counts with 9 out of 10 cells being $k < 10$. Results for the subgroup analysis separated for tasks using concrete and abstract victims are presented in Table S3.

Table S3. Overview of results for sub-group analysis of the different intuition manipulations using a random effects model, separated between both social harm subgroups.

Intuition manipulation	**Social Harm**	***k***	***Log(OR)***	***Z***	***p***
(Ego) Depletion	abstract	25	0.497	3.332	.001
	concrete	5	-0.107	-0.264	.792
Cognitive Load	abstract	7	0.382	1.290	.197
Foreign Language	abstract	9	-0.156	-0.551	.582
	concrete	2	0.510	0.753	.451
Induction	abstract	8	0.821	3.007	.003
	concrete	3	-0.410	-0.700	.484
Time Pressure	abstract	8	0.022	0.074	.941
	concrete	6	0.012	0.034	.973

Successful manipulation check.

We coded whether the studies reported a successful manipulation check (k = 45) or not (k = 28). A mixed effects meta regression model reveals that significant differences exist between studies that report a successful manipulation check and those that do not (Z = 2.38, p = .01). Subgroup analysis reveal that the intuitive dishonesty effect is significant for studies with successful manipulation checks (log OR = 0.44, Z = 3.82, p = .001) while being non-significant for those that do not (log OR = -0.02, Z = -0.14, p = .88). Mixed effects meta-regression models testing an interaction between the reporting of a successful manipulation check and social harm is not significant (Z = -0.44; p = .44). We additionally tested whether systematic differences in the reporting of successful manipulation checks exist across tasks using a concrete vs. abstract victim. A Fisher's exact test reveals no such evidence (OR = 0.54, p = .38).

Dishonesty paradigms

Mixed effects regression analysis testing whether significant differences across the four clusters of dishonesty paradigms exist reveal no overall differences ($Q(3)$ = 5.75, p = 0.12). Interaction analysis between dishonesty paradigms and social harm also reveal no overall interactions ($Q(5)$ = 7.82, p = 0.16). Like the interaction analysis for different intuition manipulations, low cell counts (only 3/8 cells being k >10) undermine the statistical power of the analysis. We report the results for each dishonesty paradigm subgroup separately for both social harm conditions in Table S4.

Table S4. Overview of results for sub-group analysis of the different dishonesty paradigms using random effects model, separated for both social harm conditions.

Dishonesty Paradigm	**Social Harm**	***k***	***Log(OR)***	***Z***	***p***
Other	abstract	9	0.557	2.177	.030
Performance Enhancement	abstract	29	0.497	3.489	.001
	concrete	4	-0.116	-0.248	.804
Sender-Receiver Games	abstract	1	-0.484	-0.698	.486
	concrete	12	-0.012	-0.050	.960
Stochastic Paradigms	abstract	18	0.124	0.615	.539

Experimental Setting. We coded whether the study was conducted in the lab (k = 50), online (k = 21) or in both locations without further specification (k = 1). To test whether the experimental setting influenced the overall estimate, we conducted mixed effect meta-regressions analyses which reveal no significant overall differences ($Q(2)$ = 2.94, p = .22), and no indication for an interaction between experimental setting and social harm ($Q(4)$ = 6.87, p = .14).

Experimental Deception. We coded whether the study used experimental deception – hence, whether the experimenter provided information to the participant that is untruthful. Based on the information provided in the original manuscript and through direct correspondence with the original authors we coded 28 studies as not using deception (k = 28), while 38 studies did employ experimental deception (k = 38) and for the remaining seven studies it remained unclear (k = 7). Mixed effect meta-regression analyses reveal overall no differences across these three categories ($Q(2)$ = 0.58, p =

.750) nor a significant interaction between experimental deception and social harm ($Q(4) = 6.03$, $p = .196$).

Incentives. We coded whether the study entailed financial incentives ($k = 67$) or not ($k = 6$). For those tasks using financial incentives, we transferred the expected incentives for maximum lying into purchasing power parity corrected US$. Using this continuous variable as a predictor in a mixed effect meta-regression reveals no influence on the overall estimate ($b = 0.01$, $SE = 0.02$, $Z = 0.50$, $p = .614$). We also find no indication for an interaction between social harm and expected incentives ($Q(3) = 3.95$, $p = .267$).

Publication Bias

Publication status. We conducted mixed effect and fixed effect meta-regression to test whether a difference between published and unpublished studies exists. The analysis reveal no significant difference ($Z = 1.55$, $p = .12$). We furthermore tested whether an interaction between publication status and social harm exists. The results provide no statistical evidence for such an interaction effect ($Q(3) = 6.13$, $p = 0.10$). It is however important to note that analyses comparing published and unpublished studies assumes that both sets of studies are represented in (similarly) unbiased ways (Kepes et al., 2012). Although this meta-analysis contains a relatively high proportion of these unpublished work, it is reasonable to assume that other (non-significant) findings are missing from the analysis. Below, we report some of the most commonly used statistical analyses to estimate the size of these studies in the file-drawer.

Funnel Plot Asymmetry. Ioannidis & Trikalinos (2007) specify 4 preconditions in order to draw meaningful conclusions from the distribution of effect sizes and standard errors. These are:

#1.The meta-analysis needs to include more than 10 studies

#2. The meta-analysis needs to include studies with significant results

#3. Heterogeneity cannot be large ($I^2 > 50\%$)

#4. There primary studies need to vary in precision

The first meta-analysis fulfils the first, second and fourth preconditions. However, the included studies show a large degree heterogeneity ($I^2 = 67.4\%$). Hence, the results of funnel plot analysis need to be interpreted with extreme caution. The funnel plot of all studies is displayed in Figure S6. Visual inspection suggest slight asymmetry.

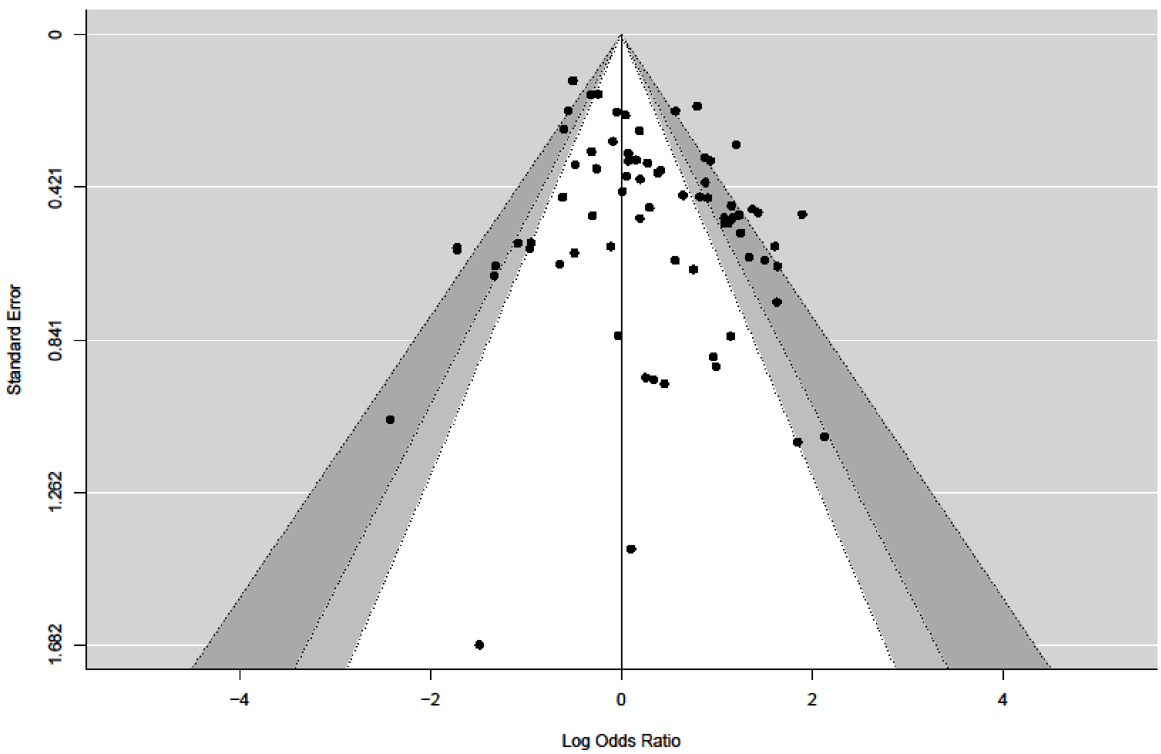

Figure S6. Funnel plot for all studies included in the first meta-analysis. The white area shows non-significant studies ($p > .1$), the dark grey area shows studies close to significance level $\{p \mid .05 < p < .1\}$, the medium grey area shows significant p-levels $\{p \mid .01 < p < .05\}$, the light grey area shows p-levels of smaller than $p < .01$.

Trim-and-Fill. We also conducted the trim-and-fill method that gained popularity in recent decades with the promise to correct for missing studies (Duval & Tweedie, 2000). Based on the distribution of studies in the funnel plot, the trim and fill method seeks to add studies to the funnel plot until it becomes symmetric. It then recalculates the overall effect with this new sample. For the first meta-analysis, the trim-and-fill analysis estimates that five studies are missing from the meta-analysis which are depicted as white dots in the funnel plot in Figure S7. Imputing these five studies and re-running the analysis confirms the significant overall intuitive dishonesty effect (log *OR* = 0.21, Z = 2.10; p = .036).

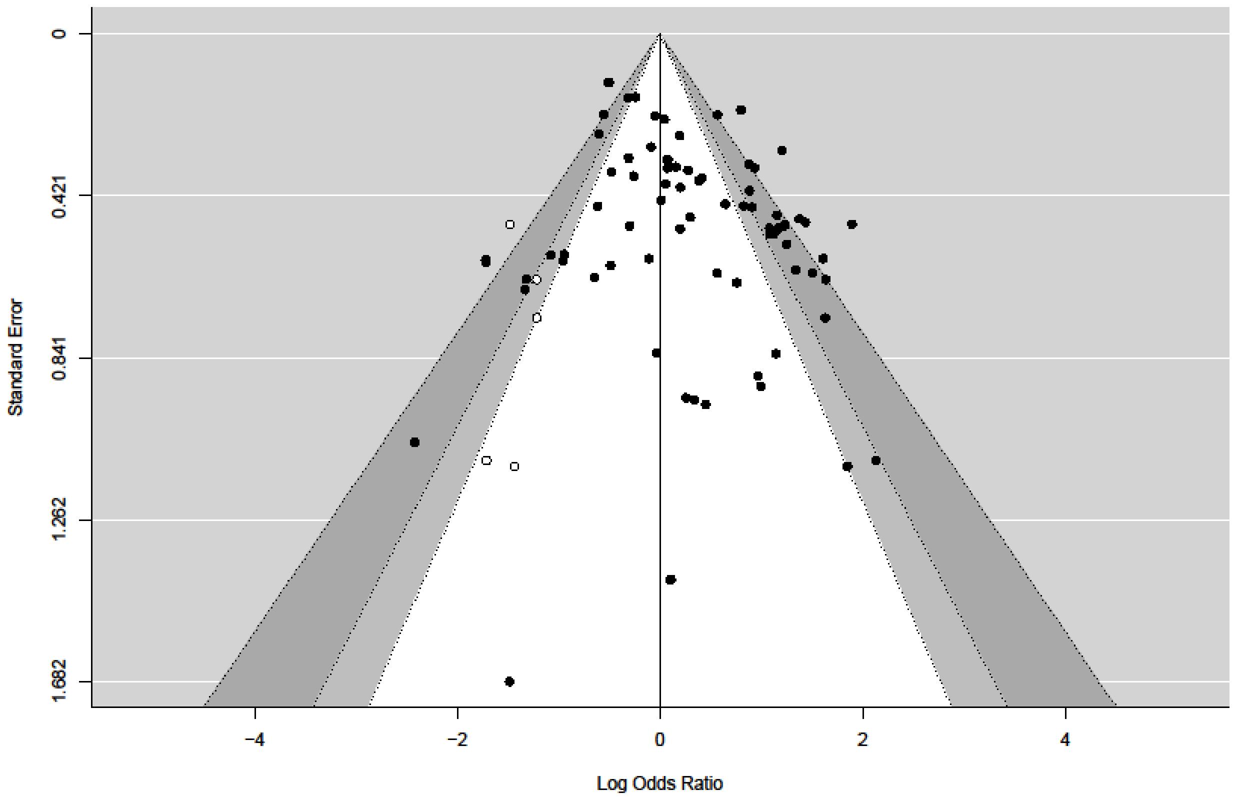

Figure S7. Contour funnel plot for the percentage of lying meta-analysis. Dashed line indicating the random effect, the white area shows non-significant studies ($p > .1$), the dark grey area shows studies close to significance level $\{p \mid .05 < p < .1\}$, the medium grey area shows significant p-levels $\{p \mid .01 < p < .05\}$, the light grey area shows p-levels of smaller than $p < .01$. White dots indicate studies estimated to be missing from the sample using Trim and Fill method.

Harbord Regression Test. As a statistical tests of asymmetry in the funnel plot, we conducted weighted linear regression between the treatment effect size and the inverse standard error adjusted for binary outcomes corrections suggested by Harbord and colleagues (2006). The results suggest that significant asymmetry exists (b = -0.27, t(71) = 2.61, p = .01, see Figure S8).

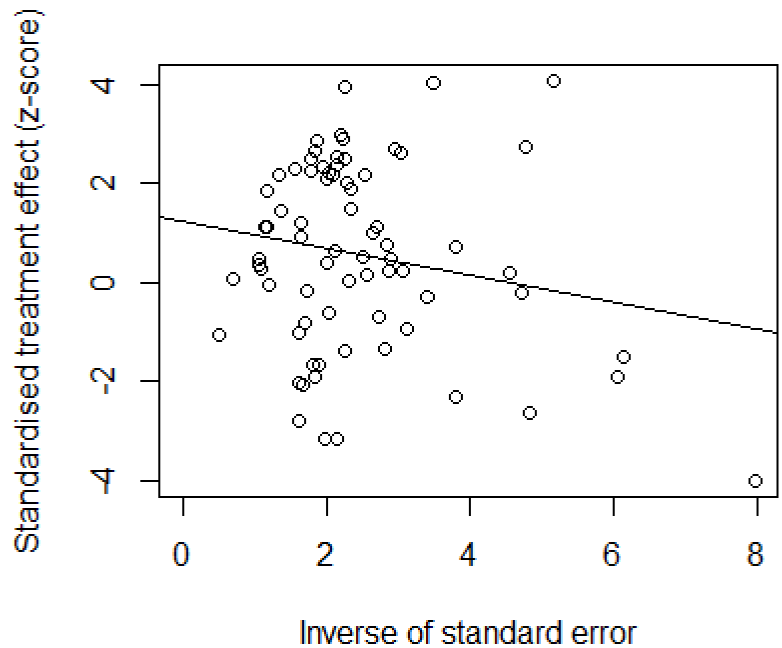

Figure S8. Weighted linear regression of the standardized treatment effect (y-axis) and inverse standard error (x-axis) using Harbord's correction.

Funnel Plot Asymmetry for Social Harm Subgroups. We also examined the funnel plot asymmetry, separately for both social harm subgroups. For the concrete victim subgroup, trim and fill analysis estimates that no studies are missing from the sample (see Figure S9 left pane) and Harbord regression analysis suggests no significant asymmetry (b = -0.567; t(14) = 1.56, p = 0.141). For the abstract victim subgroup, trim and fill analysis estimates that five studies are missing (see Figure S9 right pane). Rerunning the analysis with these imputed studies confirms the intuitive dishonesty effect (log *OR* = 0.29; *Z* = 2.67; *p* = .007).

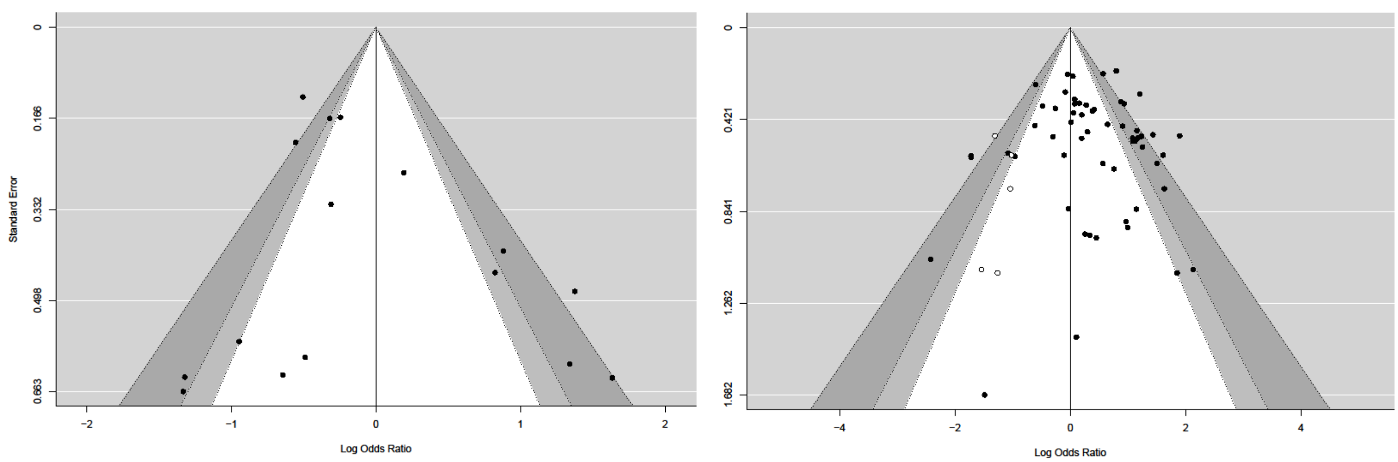

Figure S9a & b. Contour enhanced funnel plots for concrete victims (left pane) and abstract victims (right pane). White dots indicate studies that are estimated to be missing from the sample based on the trim and fill method.

PET & PEESE. As another statistical estimation of publication bias we also report precision-effect test (PET) and precision effect estimate with standard error (PEESE). These recently proposed methods are forms of conditional regression-based meta-analytic tools aim to assess the presence or absence of publication bias, and akin to the trim and fill method provide an overall estimate that corrects for publication bias. Based on the effect sizes (PET), or the standard errors of the effect sizes (PEESE) the effect size is predicted, using a weighted least squares approach. The weight is the inverse of the squared standard error.

Analysis reveals that the PET estimate of the true underlying effect correcting for small-study effects is not significant (b_0= -0.299, SE = 0.16, t = -1.86, p = .06). This result suggests that the significance level of the overall estimate for a hypothetical study for which the standard error is zero is p = .06. Moreover, the slope is significant (b_1 = 1.28, SE = 0.44, t = 2.91, p = .004), which suggests that significant asymmetry in the funnel plot exists.

Discussion Publication Bias

It is important to note that the predictive value of the funnel plot significantly drops when substantial inter-study heterogeneity exists (Carter, Hilgard, Schönbrodt, & Gervais, 2019; Peters, Sutton, Jones, Abrams, & Rushton, 2007). Simulations show that the results can be also misleading when other reasons for asymmetry in the funnel plot exists, like the aforementioned strategic choice to conduct larger studies to detect smaller effect sizes (Simmons, Nelson, & Simonsohn, 2011). Hence, it is generally contested whether funnel plot asymmetry is a reliable sign of publication bias (Lau, Ioannidis, Terrin, Schmid, & Olkin, 2006).

Also, recent simulations testing the validity of PET and PEESE for various settings suggest that the method is not particularly accurate in predicting whether publication bias exists (Carter et al., 2019). For example, results of simulation studies suggest that the method provides unsuitable results when inter-study heterogeneity is large (Moreno et al., 2009). Especially, if researchers adjust their sample sizes to the effect size they expect to find, e.g. through a priori power analysis the method provides false evidence for publication bias.

In sum, given the large cross-study heterogeneity and the possibility for alternative explanation for the relationship between effect size and standard error, the funnel plot analysis does not allow to detect the degree of publication bias in the sample. Similarly, with high degree of cross-study heterogeneity also PET and PEESE have been shown to perform poorly in detecting publication bias.

Results Additional Analysis – Meta-analysis 2

Fixed effects analysis

We report fixed effect meta regression analysis in addition to the random effects model reported in the main manuscript. Fixed effects meta-regression analysis indicates a significant difference in the magnitude of lying between the included studies using an abstract vs. a concrete victim (Z = -2.04; p = .042). Fixed effects subgroup analysis confirms the previously obtained significant intuitive dishonesty effect for abstract victims (g = 0.16, 95%CrI = [0.11; 0.21], Z = 6.31, p <.0001), while showing no effect for concrete victims (g = -0.05, 95%CrI = [-0.25; 0.15], Z = -0.50, p =.619).

Robustness tests

Outliers and influential studies. We performed outlier analysis to detect individual studies that might disproportionally influence the overall estimate of the random effects model. This time, the algorithm proposed by Viechtbauer and Cheung (2010) identified one study as influential (Kouchaki & Desai, 2015: Exp2), see also Figure S10.

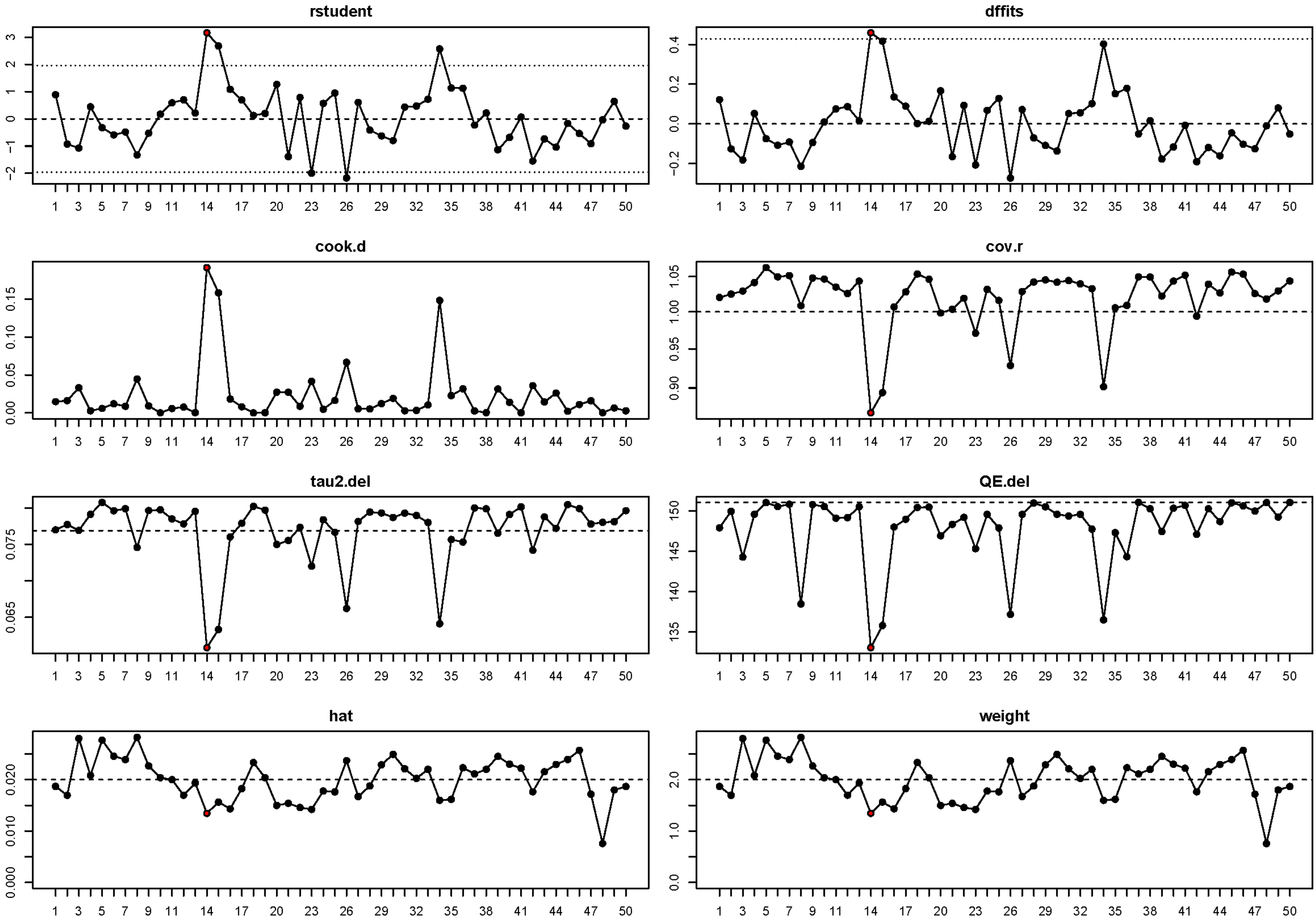
rstudent
dffits
cook.d
cov.r
tau2.del
QE.del
hat
weight

Figure S10. Plots of eight different algorithm to identify influential study in the sample of the second meta-analysis. Red dots indicate studies identified as influential.

Rerunning the overall analysis after the exclusion of the influential study reveals a significant intuitive dishonesty effect (g = 0.21, Z = 4.64, p <.0001). The social harm moderation effect becomes Z = -1.95, p = 0.051, when excluding the positive outlier. Subgroup analysis confirm a significant intuitive dishonesty effect when no concrete victim gets harmed (g = 0.24; Z = 5.04; p < .001), while no such effect appears when a concrete other suffers the consequences of lying (g = -0.07; Z = -0.48; p = 0.632). Figure S11 displays the results of this random effects subgroup analysis.

Study	Experimental Total	Experimental Mean	Experimental SD	Control Total	Control Mean	Control SD	SMD	95%−CI
Concrete Victim								
Pitesa et al (2013): Exp2b	22	0.29	0.0500	24	0.33	0.0925	−0.57	[−1.16; 0.02]
Pitesa et al (2013): Exp3b	26	0.03	0.0460	26	0.04	0.0500	−0.31	[−0.85; 0.24]
Alempaki et al (2017): Exp3	32	0.57	0.4254	31	0.62	0.4629	−0.11	[−0.61; 0.38]
Bereby−Meyer (n.d.)	74	0.13	0.1837	75	0.15	0.1883	−0.10	[−0.42; 0.22]
Alempaki et al (2017): Exp1	41	0.41	0.3793	41	0.21	0.3298	0.55	[0.10; 0.99]
Random effects model	**195**			**197**			**−0.08**	**[−0.43; 0.27]**
Heterogeneity: $I^2 = 64\%$, $\tau^2 = 0.1030$, $p = 0.03$								
Abstract Victim								
Van't Veer et al (2014)	87	0.02	0.2606	86	0.12	0.2476	−0.41	[−0.72; −0.11]
Gai & Puntoni (n.d.): Exp3	33	0.61	0.6667	37	0.84	0.7222	−0.33	[−0.80; 0.15]
Gross (n.d.)	259	0.03	0.2622	280	0.07	0.2701	−0.14	[−0.31; 0.03]
Welsh (n.d.)	99	0.16	0.2960	99	0.20	0.2900	−0.12	[−0.39; 0.16]
Keith (2018)	34	−0.17	0.0274	31	−0.17	0.0267	−0.11	[−0.59; 0.38]
Bereby−Meyer et al (2018): Exp1	233	0.04	0.1270	259	0.05	0.1275	−0.08	[−0.25; 0.10]
Yam et al (2014a): Exp1	107	0.18	0.2976	107	0.18	0.3370	−0.01	[−0.28; 0.26]
Gai & Puntoni (n.d.): Exp4	63	0.50	0.3889	56	0.51	0.2884	−0.01	[−0.37; 0.35]
Gai & Puntoni (n.d.): Exp1	66	0.58	0.3185	87	0.57	0.2768	0.01	[−0.31; 0.33]
Yam (2015): Exp3	75	0.14	0.0381	72	0.14	0.0400	0.03	[−0.29; 0.35]
Bereby−Meyer et al (2018): Exp4	101	0.05	0.2701	98	0.03	0.2511	0.05	[−0.23; 0.33]
Gunia et al (2014): Exp 2	69	0.10	0.2515	73	0.09	0.2793	0.06	[−0.27; 0.39]
Van't Veer (2015): Exp4	129	0.21	0.2269	125	0.20	0.2183	0.07	[−0.18; 0.32]
Bereby−Meyer et al (2018): Exp5	88	0.08	0.2682	88	0.05	0.2490	0.08	[−0.22; 0.38]
Yam (2015): Exp2	45	0.22	0.0688	36	0.21	0.0759	0.09	[−0.35; 0.52]
Bereby−Meyer et al (2018): Exp3	206	0.26	0.2325	235	0.22	0.2208	0.14	[−0.05; 0.32]
Sai (n.d.) Exp2	40	0.18	0.1994	39	0.15	0.1625	0.14	[−0.30; 0.58]
Thomas (n.d.): Exp1	59	0.09	0.1760	53	0.06	0.1210	0.16	[−0.22; 0.53]
Van't Veer (2015): Exp3	87	0.16	0.1200	90	0.14	0.1000	0.18	[−0.11; 0.48]
Van der Cruyssen et al (2019): pilot	9	0.02	0.3200	8	−0.05	0.2867	0.21	[−0.74; 1.17]
Gai & Puntoni (n.d.): Exp2	67	0.40	0.2988	66	0.32	0.2968	0.26	[−0.09; 0.60]
Motro et al (2016): Exp2	80	0.17	0.2936	80	0.10	0.2339	0.27	[−0.04; 0.59]
Gino et al (2011): Exp1	50	0.12	0.2535	51	0.05	0.2060	0.29	[−0.10; 0.68]
Muraven et al (2006)	46	0.53	0.3950	56	0.41	0.3970	0.30	[−0.09; 0.69]
Thomas (n.d.): Exp2	64	0.14	0.2545	65	0.07	0.1585	0.31	[−0.04; 0.65]
Khaw & Christian (n.d.)	41	0.15	0.1673	47	0.10	0.1533	0.31	[−0.11; 0.73]
Yam et al (2014a): Exp3	65	0.14	0.2092	67	0.08	0.0775	0.38	[0.03; 0.72]
Bereby−Meyer et al (2018): Exp2	49	0.11	0.2680	61	0.00	0.2951	0.38	[0.00; 0.76]
Wang et al (2017): Exp1	50	0.49	0.1100	50	0.45	0.0850	0.39	[0.00; 0.79]
Gino et al (2011): Exp2	48	0.12	0.1995	49	0.04	0.1685	0.44	[0.03; 0.84]
Shalvi et al (2012): Exp1	34	0.18	0.2467	38	0.06	0.2683	0.44	[−0.03; 0.91]
Welsh & Ordonez (2014)	34	0.05	0.0910	29	0.02	0.0225	0.46	[−0.04; 0.96]
Sai (n.d.) Exp1	37	0.18	0.1672	37	0.11	0.1378	0.47	[0.00; 0.93]
Wang et al (2017): Exp3	67	0.10	0.1613	64	0.04	0.0900	0.47	[0.12; 0.82]
Motro et al (2016): Exp1	37	0.20	0.2190	40	0.10	0.2020	0.48	[0.03; 0.94]
Gino et al (2011): Exp3	32	0.20	0.2155	33	0.09	0.2130	0.49	[0.00; 0.99]
Pitesa et al (2013): Exp2a	24	0.41	0.0950	24	0.36	0.0585	0.55	[−0.03; 1.13]
Shalvi et al (2012): Exp2	39	0.15	0.2464	33	−0.01	0.3067	0.58	[0.10; 1.05]
Chiou et al (2017): Exp4	70	0.14	0.1370	70	0.06	0.1225	0.59	[0.25; 0.93]
Chiou et al (2017): Exp3	30	0.16	0.1460	30	0.06	0.1375	0.66	[0.14; 1.18]
Kouchaki & Smith (2014): Exp4	21	0.45	0.2720	27	0.26	0.2710	0.67	[0.08; 1.26]
Pitesa et al (2013): Exp3a	26	0.15	0.0780	26	0.10	0.0550	0.73	[0.17; 1.29]
Zhang et al (2018): Exp1	32	0.55	0.1474	33	0.37	0.1475	1.17	[0.64; 1.70]
Kouchaki & Desai (2015): Exp3	30	0.42	0.1133	33	0.30	0.0821	1.22	[0.68; 1.76]
Random effects model	**2962**			**3068**			**0.24**	**[0.15; 0.33]**
Heterogeneity: $I^2 = 64\%$, $\tau^2 = 0.0556$, $p < 0.01$								

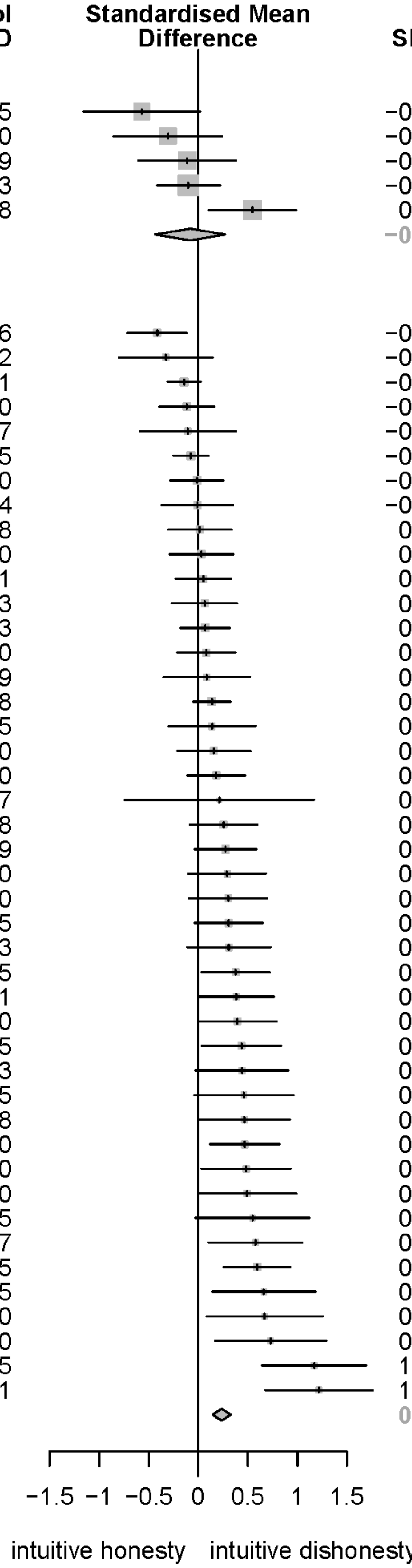

Figure S11. Social harm moderation analysis excluding one outlier identified through a pre-specified outlier detection algorithm.

Excluding any study individually does not change the intuitive dishonesty effect for abstract victims (see Figure S12a). For the subgroup of studies with a concrete victim the aggregate effect is more influenced by omitting individual studies (see Figure S12b). One plausible reason lies in the small number of studies that use a concrete victim.

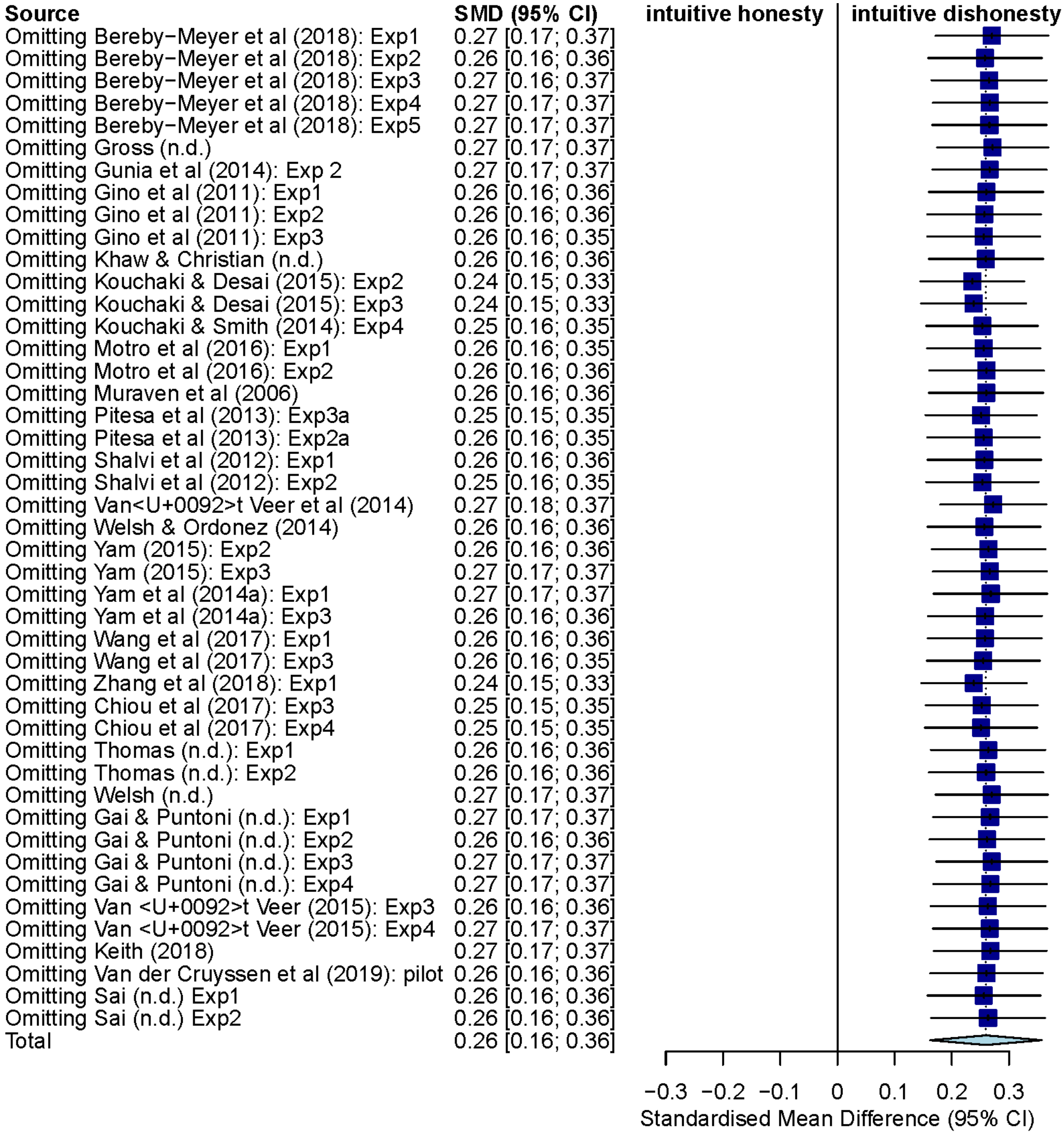

Figure S12a. Plot of the estimated effect omitting each individual study separately for the subgroup of studies using an abstract victim in the second meta-analysis.

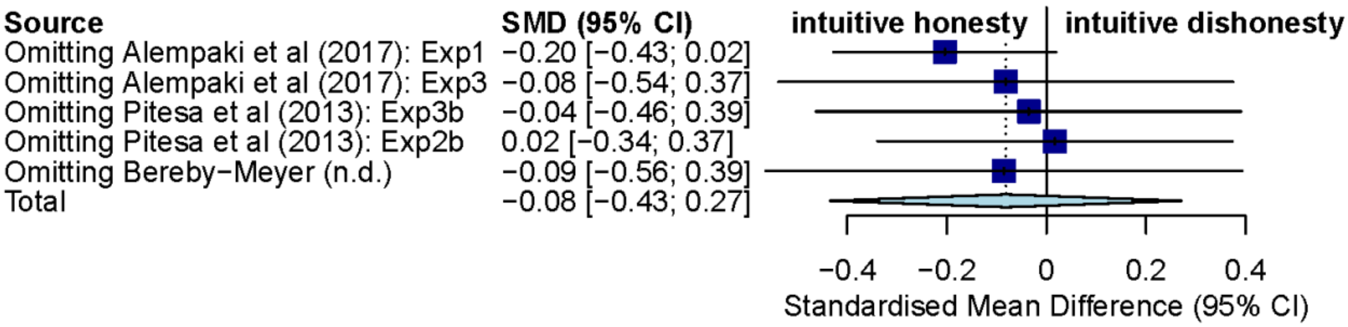

Figure S12b. Plot of the estimated effect omitting each individual study separately for the subgroup of studies using a concrete victim in the second meta-analysis.

Top 10. As outlined in the manuscript, for the second meta-analysis the Top10 analysis cannot provide additional evidence for the social harm moderation effect as no study using a concrete victim is among top decile of studies. For completeness, we report Top 10 analysis here. Running the meta-analysis on the reduced sample (k = 5), the results for the magnitude of lying reveal no significant difference (g = -0.01, 95%CrI = [-0.20; 0.17], Z = -0.25; p = 0.80). This analysis draws on 29.9 percent of the total sample (n = 1,940).

Additional Moderation Analysis

We again conducted moderation analyses to test whether the overall estimate differs across a range of moderating variables. For continuous predictors, we additionally conduct interaction analysis between our key moderator of social harm and other moderators. However, the small number of studies using a concrete victim (k = 5) renders the statistical power of interaction analyses between social harm and other *categorical* moderators unreliable (see also, Kepes et al., 2012). We therefore do not report them here, but provide the R code to run these interaction analyses for future meta-analyses that contain more studies.

Demographics

Age and Gender. Meta-regression using the average age of the sample as a predictor reveals no significant overall effect (b = -0.01, p = .510). Using the interaction term between age and social harm reveals no significant interaction effect ($Q(3) = 6.36$, p = .093). Secondly, using meta-regression analysis to test for a gender effect by using the percentage of females as a predictor, reveals a significant gender effect on the overall estimate (b = -0.65; Z = -2.33; p = .019). Testing the interaction between gender and social harm reveals a significant interaction effect ($Q(3) = 18.24$, $p < 0.001$).

As Figure S13 displays, for the abstract victim subgroup a higher female percentage of females in the sample leads to a weaker intuitive dishonesty effect (b = -0.75; Z = -2.81, p = 0.005). However, for the concrete victim subgroup, more females in the sample corresponds to a stronger intuitive dishonesty effect (b = 3.47; Z = 3.13; p = 0.002). We reiterate that the interpretation of this interaction effect warrants caution due to the small k-count in the concrete victim subgroup and because gender in our analysis consists of an aggregate measure for each study (Berlin et al., 2002).

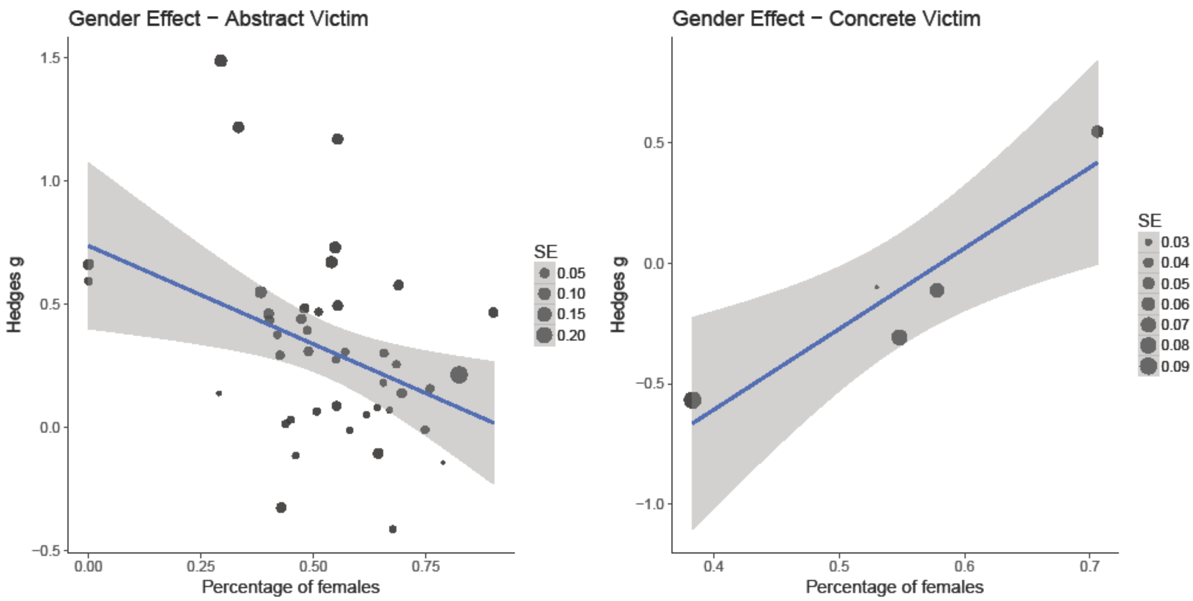

Figure S13a & b. Slopes of the meta regression analyses of the percentage of females on the standardized mean difference score (Hedges' *g*) for both social harm subgroups.

Study Location. Mixed effects regression analysis suggests that the overall differences across the sampled countries are $Q(9) = 16.02$, $p = .066$. The small number of studies from several countries undermines the interpretability of these differences see Table S5. Hence, only tentative conclusions can be drawn.

Table S5. Estimates of the biased corrected standardized mean difference for each country

Country	***k***	***g***	***95%CredInt***	***Z***	***p***
China	9	0.35	[-0.183; 0.886]	3.257	.001
France	3	-0.13	[-0.775 0.521]	-0.588	.556
Germany	2	-0.11	[-0.803; 0.585]	-0.438	.663
Israel	4	0.28	[-0.290; 0.881]	1.815	.069
Korea	1	0.14	[-0.582; 0.857]	0.513	.607
Netherlands	5	-0.06	[-0.618; 0.501]	-0.427	.669
Russia	1	-0.08	[-0.792; 0.641]	-0.283	.776
Spain	1	0.08	[-0.675; 0.835]	0.274	.784
Taiwan	2	0.62	[-0.050; 1.292]	2.658	.007
USA	22	0.30	[-0.207; 0.812]	4.343	<.0001

Population

Again, we tested whether the intuitive dishonesty effect and the social harm moderation differs between student samples (k = 37) and samples stemming from the general population (k = 13). Mixed effect meta-regression reveals no significant differences (Z = 1.70, p = .089). This indicates that the intuitive dishonesty effect on the magnitude of lying does not differ across student and non-student samples.

Intuition Manipulation

Mixed effects meta-regression analyses across the different intuition manipulations reveal differences in the overall effect ($Q(4) = 29.47$, $p < .001$). For studies using ego depletion and induction mechanisms to manipulate intuition, the intuitive dishonesty is the strongest (see Table S6).

Table S6. Estimate of the overall biased corrected standardized mean difference between intuition and control conditions across different clusters of intuition manipulations

Intuition Manipulation	*k*	*g*	*95%CredInt*	*Z*	*p*
(Ego) Depletion	19	0.245	[-0.155; 0.644]	3.725	<.001
Cognitive Load	6	-0.012	[-0.444; 0.419]	-0.116	.908
Foreign Language	11	0.085	[-0.32; 0.492]	1.094	.274
Induction	7	0.745	[0.306; 1.185]	6.521	<.001
Time Pressure	7	0.157	[-0.276; 0.591]	1.447	.148

Successful manipulation check. Mixed effects models provide evidence that the effect does not differ between studies that reported a successful manipulation check and those that do not ($Q(1) = 4.58$; $p = .10$).

Dishonesty Paradigm

A mixed effects regression model indicates differences across dishonesty paradigms for the overall estimate ($Q(3) = 9.86$, $p = .019$). The intuitive dishonesty effect is the most pronounced for performance enhancement tasks and for those classified as others (see Table S7).

Table S7. Overview of estimate of the bias corrected standardized difference in means across different dishonesty paradigms.

Dishonesty Paradigm	***k***	***g***	***95%CI***	***Z***	***p***
Performance Enhancement	27	0.25	[0.13; 0.36]	4.30	<.0001
Stochastic Tasks	10	0.07	[-0.10; 0.23]	0.80	0.42
Sender-Receiver Games	1	-0.10	[-0.63; 0.44]	-0.35	0.72
Other	6	0.56	[0.32; 0.79]	4.55	<.0001

Experimental Setting.

Using mixed effects regression analyses we gain tested whether the overall estimate differs for studies conducted in the lab (k = 39) and those conducted online (k = 11). The results reveal significant overall differences (Z = -2.04, p = .041). Subgroup analyses indicate that the intuitive dishonesty effect is significant for lab studies (g = 0.28; Z = 5.27, p <.0001) and not significant for studies conducted online (g = 0.05, Z = 0.54, p = 0.587). This result provides a first indication that the intuitive dishonesty effect might primarily appear in the controlled environment of the lab.

Experimental Deception. We compared the overall estimate between studies that entailed experimental deception (k = 28) vs. those that did not (k = 19) vs. for those where it remained unclear ($k = 3$). Mixed effect meta-regression analyses reveal overall no differences across these three categories ($Q(2) = 4.78$, $p = .092$).

Incentives. Like in the first meta-analysis, we ran meta-regression analysis for tasks that entailed financial incentives ($k = 48$) using the expected incentives in purchasing power parity corrected US$. The mixed effect meta-regression reveals no influence on the overall estimate ($b = -0.00$, $SE = 0.001$, $Z = -0.25$, $p = .80$), nor an indication for an interaction between social harm and expected incentives ($Q(3) = 5.17$, $p = .159$).

Publication Bias

Publication Status. We again tested whether the strength of the effect differs between published and unpublished studies and find evidence that it does ($Z = 2.81$, $p = .004$). The intuitive effect is significant for published studies ($g = 0.35$; 95%CrI = [-0.15; 0.85], $Z = 5.57$; $p < .0001$), yet not for unpublished studies ($g = 0.09$; 95%CrI = [-0.41; 0.60], $Z = 1.47$; $p =.141$). In line with the results of the χ^2 test reported in the main manuscript, it suggests a significant relationship between publication status and strength of the effect, one possible explanation being that significant results are more likely to be published.

Funnel Plot Asymmetry. Examination of the funnel plot for the second meta-analysis is equally undermined by the large degree of inter-study heterogeneity ($I^2 = 67.4\%$). We note that examination of the funnel plot (Figure S14) and a significant weighted linear regression analysis of the treatment effect on its standard error ($t(48) = 4.44$, $p = <.0001$; see also Figure S15) suggest funnel plot asymmetry.

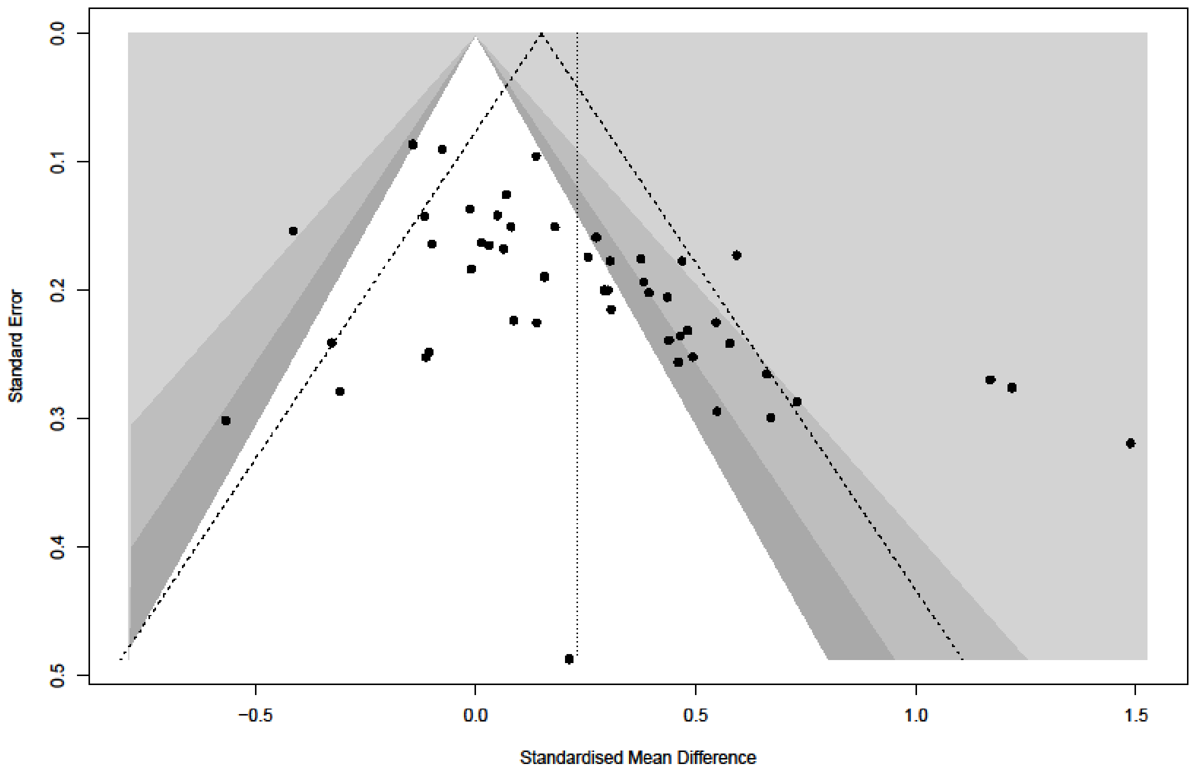

Figure S14. Contour funnel plot for the magnitude data. Dashed lines indicating the random effect, the white area shows non-significant studies ($p > .1$), the dark grey area shows studies close to significance level $\{p \mid .05 < p < .1\}$, the medium grey area shows significant p-levels $\{p \mid .01 < p < .05\}$, the light grey area shows p-levels of smaller than $p < .01$.

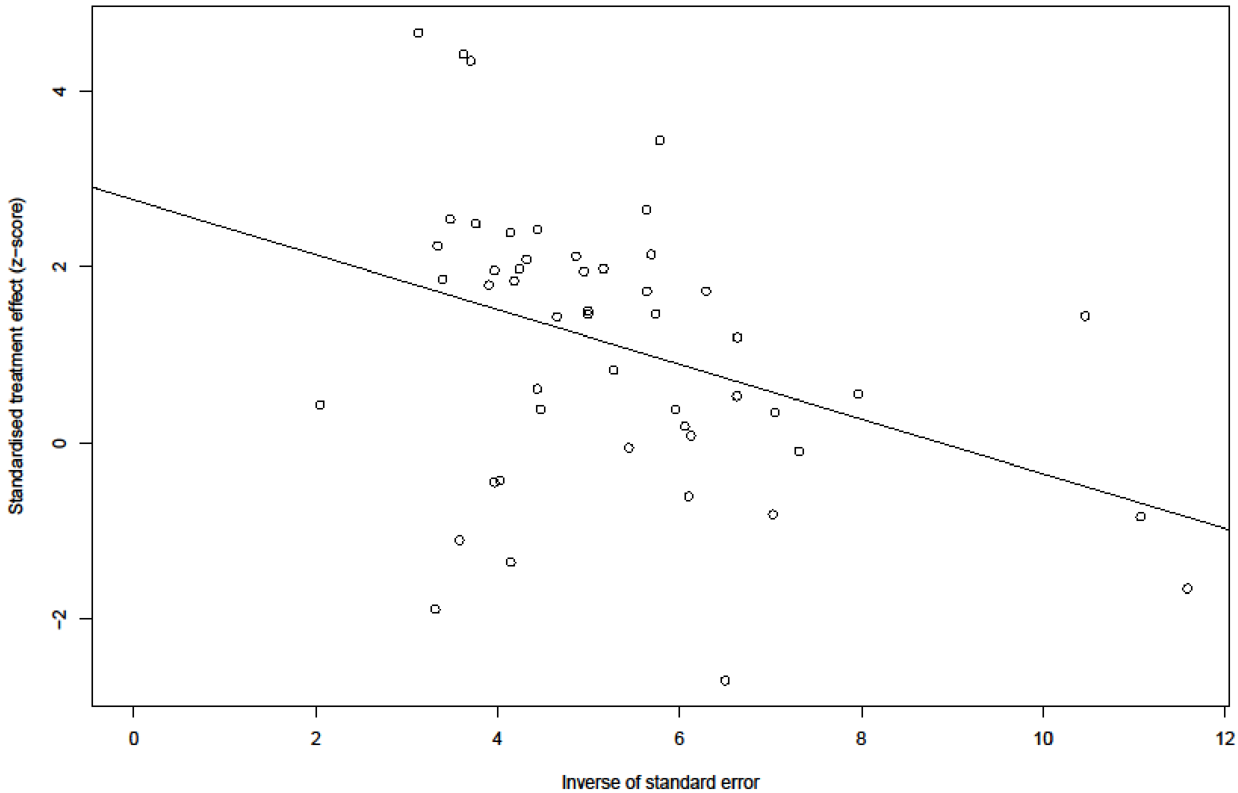

Figure S15. Weighted linear regression of the standardized mean difference score effect (y-axis) and the inverse standard error (x-axis).

Trim and Fill. To adjust for funnel plot asymmetry, estimations using trim-and-fill method suggests that 10 studies need to be imputed. After imputing these studies the overall effect remains significant (g = 0.12, Z = 2.08, p = .03, see also Figure S16).

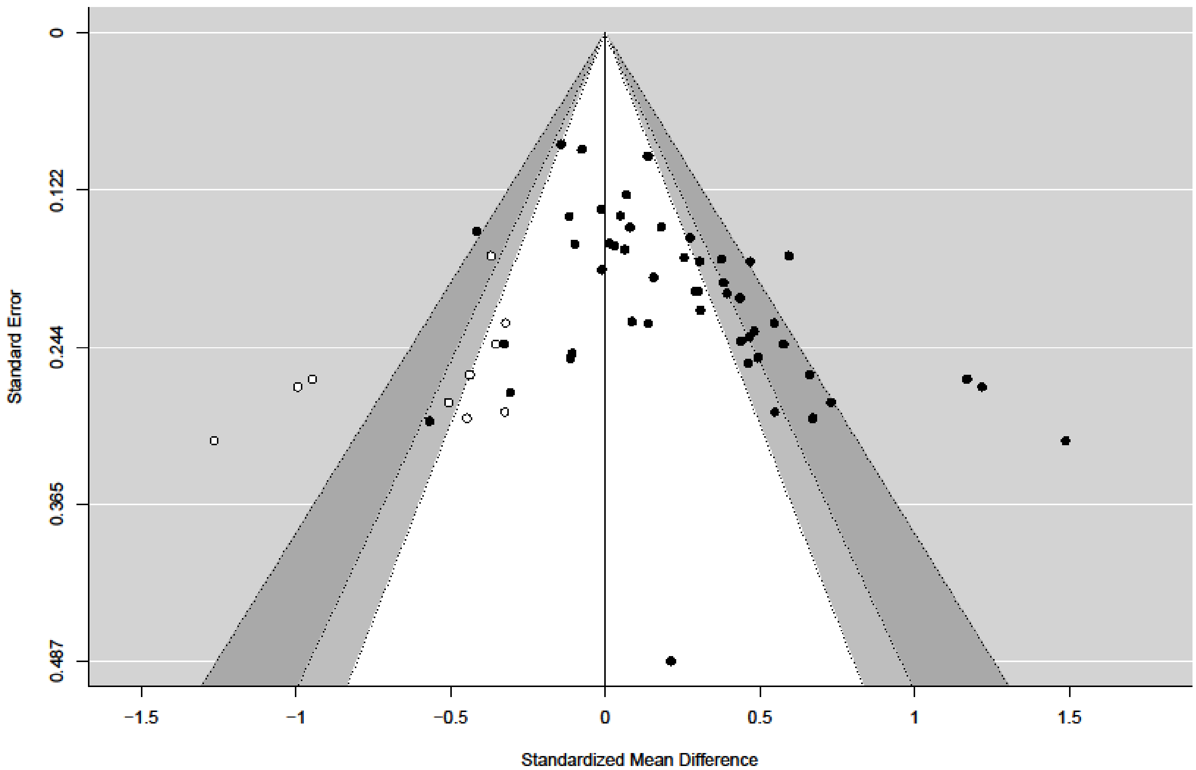

Figure S16. Contour funnel plot with white dots displaying simulated studies using the trim and fill technique for the second meta-analysis.

Funnel Plot Asymmetry for Social Harm Subgroups. Standard tests of funnel plot asymmetry for social harm subgroups are not possible as the concrete victim subgroup does not reach the required minimum of k = 10.

PET & PEESE. We again also conducted precision-effect test (PET) and precision effect estimate with standard error (PEESE). The PET intercept of the regression analysis is significant (b_0 =-0.31, SE = 0.11, t = -2.82, p = .006; b_1=2.76, SE = 0.62, t = 4.44, p < . 0001). Hence, we refer to the estimate produced by the PEESE meta-regression method which is not significant (b_0 = 0.05; SE = 0.06, t(48) = -0.89, p = 0.37;

b_1=6.58, *SE* = 1.63, *t* = 4.03, *p* < .001). The zero-intercept is proposed as the best estimate of the true effect, which suggests no overall effect. As in the first meta-analysis, these methods suggests that the effect size distribution is highly asymmetric, which can be an indication for publication bias. As outlined above, these statistical methods do not perform well when large inter-study heterogeneity exists. The large heterogeneity in the present meta-analysis thus undermines the explanatory power of these statistical methods.

Table S8. PRISMA Checklist for meta-analysis

Section/topic	*#*	*Checklist item*	*Reported on page #*
TITLE			
Title	1	Identify the report as a systematic review, meta-analysis, or both.	Title identifies the paper as a meta-analysis on page 1.
ABSTRACT			
Structured summary	2	Provide a structured summary including, as applicable: background; objectives; data sources; study eligibility criteria, participants, and interventions; study appraisal and synthesis methods; results; limitations; conclusions and implications of key findings; systematic review registration number.	The abstract outlines the key points on page 2.
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of what is already known.	The paper embeds the current meta-analysis in the existing literature on pages 3-5, and integrates the current meta-analysis in the light of existing aggregate knowledge about dishonesty on page 28.
Objectives	4	Provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS).	The concrete purpose of the meta-analysis is described on page 3.
METHODS			
Protocol and registration	5	Indicate if a review protocol exists, if and where it can be accessed (e.g., Web address), and, if available, provide registration information including registration number.	After a first round of identifying relevant studies and asking authors to send us their work (resulting in us identifying 44 relevant studies), we conducted two more pre-registered calls for papers (see call 1 https://osf.io/vdyxq & call 2 https://osf.io/hxwny). We refer to these pre-registrations on page 6.
Eligibility criteria	6	Specify study characteristics (e.g., PICOS, length of follow-up) and report characteristics (e.g., years considered, language, publication status) used as criteria for eligibility, giving rationale.	Eligibility criteria are outlined in the pre-registration, mentioned on page 6 and in the method section of the main text on page 7.
Information sources	7	Describe all information sources (e.g., databases with dates of coverage, contact with study authors to identify additional studies) in the search and date last searched.	Search for studies is outlined in detail in the pre-registration form and in the method section of the main text on page 6. We describe additional details in the SOM on page 4.

Search	8	Present full electronic search strategy for at least one database, including any limits used, such that it could be repeated.	The full online search criteria are outlined in the pre-registration form and in the method section of the main text on pages 5-7.
Study selection	9	State the process for selecting studies (i.e., screening, eligibility, included in systematic review, and, if applicable, included in the meta-analysis).	The coding procedure is described in a separate section in the method section of the main text on pages 10-11.
Data collection process	10	Describe method of data extraction from reports (e.g., piloted forms, independently, in duplicate) and any processes for obtaining and confirming data from investigators.	The procedure of having one of the authors extracting the data and a second blind coder collection procedure is outlined in the method section of the main text on pages 10-11.
Data items	11	List and define all variables for which data were sought (e.g., PICOS, funding sources) and any assumptions and simplifications made.	The paper outlines that the team of authors sought to extract a) percentages of liars for both conditions; and b) in a second step, we also included degree of lying for both conditions, this is outlined in detail in the Measuring Dishonesty section of the main text starting on page 9.
Risk of bias in individual studies	12	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.	The meta analyses do not employ any methods to assess the risk of bias in the individual studies. However, as a quality control, for both set of studies we coded whether the study reported a successful manipulation check. Results are reported in the SOM on page 28 (meta-analysis 1) and on page 48 (meta-analysis 2).
Summary measures	13	State the principal summary measures (e.g., risk ratio, difference in means).	The fact that the first analysis used log odds ratios as the dependent measure and the second analysis used bias corrected standardized mean differences (Hedges's *g*) is described in detail in the Analysis section of the main text on page 11.

Synthesis of results	14	Describe the methods of handling data and combining results of studies, if done, including measures of consistency (e.g., I^2) for each meta-analysis.	For each meta-analysis, we used three indicators of heterogeneity I^2, τ^2 and Q. More details are outlined in the Analysis section of the main text on page 10 For the first meta-analysis, we run several additional analysis with different corrections to assess the robustness of the overall estimate to inter-study heterogeneity. The results are reported in the SOM on pages 12-18.
Risk of bias across studies	15	Specify any assessment of risk of bias that may affect the cumulative evidence (e.g., publication bias, selective reporting within studies).	We conducted several analyses to assess the risk of publication bias and questionable research practices (QPR). Namely, a) a χ^2 test to compare the distribution of significant results across published and unpublished studies in both samples b) cumulative meta-analysis adding individual studies to the most precise estimate in the sample c) *p*-curve analysis, plotting the calculated p-values to assess evidential value d) assessment of Funnel Plot asymmetry using weighted regression e) estimating missing studies using Trim and Fill method f) PET PEESE corrected regression analysis We refer to these on page 12 of the main manuscript.
Additional analyses	16	Describe methods of additional analyses (e.g., sensitivity or subgroup analyses, meta-regression), if done, indicating which were pre-specified.	We report the result of the subgroup analysis social harm in the main manuscript for the first meta-analysis on pages 12-14 and for the second meta-analysis on pages 21-23. We refer to these extra analyses in the manuscript (page 12). The data sets as well as the R code for all these analyses is available online via https://osf.io/8scw2/.
RESULTS			
Study	17	Give numbers of studies screened, assessed for eligibility, and included in the review, with reasons for	The number of studies in the identification, screening, eligibility and

selection		exclusions at each stage, ideally with a flow diagram.	inclusion are outlined in the Flow diagram in Figure 1 in the main text on page 7.
Study characteristics	18	For each study, present characteristics for which data were extracted (e.g., study size, PICOS, follow-up period) and provide the citations.	The study characteristics are displayed in the openly available data sets https://osf.io/ghtbu/.
Risk of bias within studies	19	Present data on risk of bias of each study and, if available, any outcome-level assessment (see Item 12).	We report an analysis restricted to studies with successful manipulation check in the SOM pages 28 and page 48
Results of individual studies	20	For all outcomes considered (benefits or harms), present, for each study: (a) simple summary data for each intervention group and (b) effect estimates and confidence intervals, ideally with a forest plot.	The results for each study in both meta-analyses are displayed in Figures in the manuscripts. For the first meta-analysis see forest plot in Figure 2 on page 14 of the main text; for the second meta-analysis see Forest plot in Figure 6 on page 23 of the main text.
Synthesis of results	21	Present results of each meta-analysis done, including confidence intervals and measures of consistency.	The results including credibility intervals for both meta-analyses are displayed in mentioned in the manuscripts. We include customary 95% Confidence Intervals. For the first meta-analysis see forest plot in Figure 2 on page 14 of the main text; for the second meta-analysis see Forest plot in Figure 6 on page 23 of the main text.
Risk of bias across studies	22	Present results of any assessment of risk of bias across studies (see Item 15).	In the main text we report threefold publication bias analysis for each meta-analysis: a comparison of the distribution of significant results across published and unpublished studies, cumulative meta-analysis and *p*-curve. The details are reported in the Method section on pages 16-20 for meta-analysis 1 and on pages 24-27. In the SOM we report and discuss additional analyses, namely Funnel Plot asymmetry, Trim and Fill, and PET PEESE corrected regression analysis. For the first meta-analysis we report these on pages 29-39. For the second meta-analysis on pages 48-54.
Additional analysis	23	Give results of additional analyses, if done (e.g., sensitivity or subgroup analyses, meta-regression [see Item 16]).	All additional subgroup analyses and meta-regressions are reported in the SOM. For meta-analysis 1, see pages 24-

			29 and for meta-analysis 2, see pages 41-46.
DISCUSSION			
Summary of evidence	24	Summarize the main findings including the strength of evidence for each main outcome; consider their relevance to key groups (e.g., health care providers, users, and policy makers).	The results are summarized in separate discussion sections in the main text, for meta-analysis 1 on page 20-21 and for meta-analysis 2 on pages 27-28).
Limitations	25	Discuss limitations at study and outcome level (e.g., risk of bias), and at review level (e.g., incomplete retrieval of identified research, reporting bias).	Potential limitations of the study are discussed in a separate paragraph on page 30.
Conclusions	26	Provide a general interpretation of the results in the context of other evidence, and implications for future research.	The research finding is embedded in the general literature in the general discussion on pages 28-31.
FUNDING			
Funding	27	Describe sources of funding for the systematic review and other support (e.g., supply of data); role of funders for the systematic review.	Funding for this study comes from European Research Council (ERC) under the European Union's Horizon 2020 research and innovation program (grant agreement ERC-StG-637915); the funding organization had no influence on the study.

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