Adjusting for Publication Bias Reveals That Evidence for and Size of Construal Level Theory Effects is Substantially Overestimated

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Abstract

Construal Level Theory (CLT) is one of the most foundational theories in social cognition. However, the few replication studies available indicate a mixed pattern regarding the evidence supporting this theory. This article assesses the credibility of CLT more widely by using publication bias correction techniques. First, we reanalyse the largest and most recent meta-analysis on CLT using a ‘RoBMA (robust Bayesian meta-analysis) multiverse’ approach with 12 different model specifications. We find strong evidence for publication bias across all 12 models, which has inflated previous effect size estimates. Moreover, the majority of these model specifications show evidence against CLT effects. Second, we conduct a quantitative assessment of evidence in more recent studies on CLT using z-curve, which indicates a strong mismatch between observed and expected discovery rates. These analyses call into question the evidence underlying CLT and highlight the pressing need for replications in the form of high-powered registered reports.

Keywords: z-curve, RoBMA, multiverse analysis, psychological distance

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Construal Level Theory (CLT) is one of the most prominent social psychology and decision-making theories. In recent years a large number of studies have been inspired by this theory (Figure 1), and the foundational review article (Trope & Liberman, 2010) has been cited >5500 times at the time of writing (Google Scholar, May 21st 2022).

Construal level theory proposes that thinking about temporal distance, spatial distance, another person’s perspective (interpersonal distance), and counterfactual alternatives all constitute different forms of transversing psychological distance and as such invoke the same underlying psychological mechanisms. Greater psychological distance leads to a higher construal level or more abstract representation of phenomena (Liberman & Trope, 1998, 2014; Trope & Liberman, 2003, 2010, 2011). For example, participants indicated a greater preference to describe the activity, ‘filling out a personality test’ as ‘revealing what you’re like’ (rather than ‘answering questions’) when thinking about their life further in the future (Sánchez et al., 2021). This straightforward theoretical framework has inspired a variety of applications of psychological science to real-world problems, including important issues such as the way people think about infectious diseases (e.g., Cai & Leung, 2020; Li et al., 2020; Van Lent et al., 2017) and climate change (e.g., Brügger, 2020; Brügger et al., 2016; Chu & Yang, 2019; Jones et al., 2017; Keller et al., 2021; Loy & Spence, 2020; Maiella et al., 2020; Reczek et al., 2018; Rickard et al., 2016; Schuldt et al., 2018; Spence et al., 2012; Wang et al., 2019), with some even stating that “the key problem [underlying climate change] is psychological distance” (Van Lange & Huckelba, 2021, p.49).

Motivation for the Current Research

In light of the theoretical and applied importance of CLT, it is crucial to rigorously assess the evidence underlying this theory. So far, only a few replication efforts have been directed towards studies supporting CLT and they show a mixed pattern.

Figure 1*Research on Construal Level Theory Has Been Increasing Rapidly*

Note. Increase of publications on Construal Level Theory (black) in comparison to the general increase in scientific output (red) based on Bornmann et al. (2021) and scaled to overlap in 2010.

First, some replication studies have directly examined the effect of psychological distance on construal level. Early small studies consistently found that increased psychological distance led to higher levels of abstraction (Liberman & Trope, 1998; Liviatan et al., 2008; Wakslak et al., 2006). In addition, a high powered replication showed a moderate effect of $d = 0.276$ (Sánchez et al., 2021) of temporal distance. However, another higher-powered replications did not observe a predicted effect of likelihood on abstraction (Calderon et al., 2020). A recent paper suggests that the effect of likelihood on abstraction might occur only when the study explicates the contrast between proximal and distal by including both high and low likelihood for reference (Grinfeld et al., 2021).

Two further replications on CLT studies have been conducted in the domain of moral psychology. Eyal et al. (2008) showed that virtuous acts are judged more positively, and moral transgressions more negatively, when psychological distance is large rather than

small. This effect is predicted by CLT on the basis that people tend to construe more distant acts more in terms of moral principles rather than considering situation specific factors (Eyal et al., 2008). However, Gong and Medin (2012) found opposing results. Žeželj and Jokić (2014) conducted a third replication that tried to reconcile these findings. They found yet a different pattern of results: no effect of temporal distance on moral judgement, but an effect of social distance in line with CLT predictions, and the opposite effect for a direct manipulation of construal level. The inconsistent and partially opposing findings across a set of three studies prompted a series of commentaries (Eyal et al., 2014; Gong et al., 2014) proposing different mechanisms to recast these findings, a detailed examination of which is beyond the scope of this short summary.

Third, in the domain of climate change, Yang et al. (2021) unsuccessfully attempted to replicate the effects of psychological distance mental construal using a cued distance manipulation originally devised by Schuldt et al. (2018). This unsuccessful replication further calls into question the relationship between spatial distance and higher-level construal. In addition, the overall findings of applications of CLT to climate change are mixed and contradictory, a pattern which is often diagnostic of underlying null effects (for a review, see Maiella et al., 2020).

Next to these direct replications, there is also evidence based on conceptual replications. Notably Snefjella and Kuperman (2015) find support for CLT predictions in a large study analyzing natural language use. However, while mostly in line with CLT predictions, their study finds a curvilinear relationship with a small increase of concreteness for large distances. In addition, Bhatia and Walasek (2016) find strong support for CLT predictions using large natural language corpora. While these studies are an important piece of evidence also due to their large sample size (all studies $Ns > 1\,000\,000$), they suffer from the drawbacks of being conceptual rather than direct replications. The most important such drawback is that if a conceptual replication fails, it is usually less informative as it is unclear whether this reflects failure to replicate the underlying theory

or problems with the idiosyncratic adaptations made in this replication. This asymmetric informativeness may cause favorable publishing of conceptual replications that are successful (Chambers, 2019; Pashler & Harris, 2012).

This section shows that evidence from high-powered replications is only available in a few domains and in these areas the patterns of evidence are often conflicting. In addition, the wide set of predictions and implications that can be derived from CLT make it difficult to assess credibility using large-scale replications alone. However, given the substantial influence of CLT on psychology, it is important to obtain a wider overview of its evidential standing. Therefore, this article takes a different approach. Here we ask whether the available literature on CLT shows any evidence of being tainted by publication bias. To the extent that publication bias exists in a field, with positive findings being selectively published relative to null or negative findings (the file-drawer effect, Rosenthal and Gaito, 1964), the published research will present an inflated estimate of the robustness of the key effects. Hence the present work adds to the existing evidence base by examining the evidence for underlying effects in CLT studies using cutting-edge publication bias detection methods.

Reanalysing a Prominent Meta-Analysis on Construal Level Theory

Soderberg et al. (2015, cited more than 200 times at the time of writing, May 21st 2022) represents the most recent and largest meta-analysis of the effects of psychological distance on abstraction (310 estimates) as well as the downstream consequences of abstraction (426 estimates). Consequently, Soderberg et al. (2015) provides an appropriate dataset for an initial exploration of the potential role of publication bias in CLT research, and its influence on the conclusions of the meta-analysis. From their meta-analysis, Soderberg et al. (2015, p. 525) reported “a reliable and medium-sized effect of psychological distance on both level of abstraction in mental representation and the downstream consequences of abstraction”. But is this conclusion robust to the possible

presence of publication bias?

Soderberg et al. (2015) explicitly address the potential for publication bias in their meta-analysis with methods available at the time. They provide a funnel plot (Figure 2), demonstrating a considerable skew in effect sizes, which is indicative of potential publication bias. Soderberg et al. (2015) subsequently quantified the influence of the suggested publication bias via the trim-and-fill method (Duval & Tweedie, 2000), finding that there were only 39 “missing” studies whose inclusion would reduce the meta-analytic effect size to $g = 0.30$. In addition, using fail-safe N (Rosenthal, 1979) they showed that 15,202 studies with a null effect size would have needed to be missing to reduce the aggregate effect to zero.

Figure 2

Funnel plots for Soderberg et al. (2015)

Note. Panel A shows the influence of (A) construal level on abstraction and (B) downstream consequences of abstraction. Five (A) and eight (B) estimates are not shown as they were outside the plotting range.

There is, however, a large and rich literature on bias correction methods which shows that both of these approaches, as well as visual inspection of funnel plots, are

insufficient to detect and correct for publication bias and often have high false positive rates (concluding that a true effect exists when it does not; e.g., Bartoš et al., 2020; Bartoš et al., 2021; Carter et al., 2019; Hong and Reed, n.d.; Hunter and Schmidt, 2004; Kvarven et al., 2020; Lau et al., 2006; Maier, Bartoš, and Wagenmakers, 2022; Mathur and VanderWeele, 2020).

Whilst a number of alternative approaches have been suggested in the literature (for a review, see Carter et al., 2019), a shared problem for these approaches is that they only perform well in some meta-analytic conditions (i.e., type of publication bias and heterogeneity). However, it is not possible to know the meta-analytic conditions without having adjusted for publication bias, which poses a conundrum to model selection. The recent robust Bayesian meta-analysis (RoBMA) approach (Bartoš & Maier, 2020; Bartoš et al., 2020; Bartoš et al., 2021; Maier, Bartoš, & Wagenmakers, 2022) is an advance on previous methods because it uses Bayesian model-averaging to apply multiple methods at the same time and it is therefore the approach employed here. RoBMA combines selection models (models that use a weighted likelihood to account for studies that are missing due to publication bias; Iyengar & Greenhouse, 1988; Maier et al., 2021; Vevea & Hedges, 1995) and PET-PEESE (a method that employs linear and quadratic regression to correct for publication bias based on the relationship between effect sizes and standard errors; Stanley, 2017; Stanley & Doucouliagos, 2014) under the Bayesian model-averaging umbrella (Hinne et al., 2020; Hoeting et al., 1999). Rather than selecting a single method, Bayesian model-averaging combines the estimates obtained from different methods based on their posterior probability. In other words, models contribute to the inference proportional to how likely they are to have generated the data. This allows the data to guide the inference to be based most strongly on those models that capture the publication process best. RoBMA allows us to draw inferences about the presence versus absence of the effect, heterogeneity, and publication bias.

RoBMA’s ability to efficiently correct for publication bias has been demonstrated

repeatedly in simulation studies as well as applied examples. First, in simulations, Bartoš et al. (2021) tested RoBMA by reproducing a large simulation by Hong and Reed (n.d.), which combined four previous simulation studies (Alinaghi & Reed, 2018; Bom & Rachinger, 2019; Carter et al., 2019; Stanley et al., 2017). This indicated that RoBMA performs well under a wide range of settings and outperforms other bias correction methods most of the time in terms of bias and root mean squared error. RoBMA’s performance was also evaluated on empirical data. Kvarven et al. (2020) compared the accuracy of meta-analytic estimates to the estimates of registered replication reports (RRR, Chambers, 2013; Chambers et al., 2015) – often considered the gold standard of empirical evidence. Bartoš et al. (2021) reproduced this analysis with RoBMA, showing that RoBMA’s meta-analytical estimate tracks the RRRs substantially better than naive random-effects meta-analysis and better than other bias correction techniques. Finally, Maier, Bartoš, and Wagenmakers (2022) tested the false positive rate of different bias correction techniques using studies from Many Labs 2 (Klein et al., 2018). Many Labs 2 is a collection of registered replication reports; therefore, we know that publication bias is absent. If a bias correction technique finds evidence for bias nevertheless, this is likely to be a false positive. This analysis showed that whereas many other methods detect publication bias where none is possible, RoBMA maintained a low false positive rate.¹

When RoBMA is applied to the Soderberg et al. (2015) dataset about the effects of psychological distance on abstraction, the results indicate: (1) very strong evidence for the

¹ RoBMA might be criticised for being too conservative. However, evidence from the comparison of RRRs to bias-corrected meta-analyses as well as the low false positive rate of the test for publication bias on RRRs shows that this is not the case. In addition, RoBMA often finds no bias in published meta-analyses. For example, Shipley and van Riper (2022) show that pride and guilt predict pro-environmental behavior; a reanalysis with RoBMA comes to the same conclusion for both experimental and correlational studies with no evidence of bias in either of the domains or study types.

presence of publication bias, $BF_{pb} > 10^{100}$ ²³; (2) moderate evidence for the absence of the effect, $BF_{01} = 7.75$; (3) a model-averaged effect size of $\delta = -0.002$, 95% $[-0.113, 0.056]$. In other words, the data are 7.75 times more likely under the null hypothesis of no effect than under the alternative prior of $\text{Normal}(0,1)$ ⁴.

When considering the downstream consequences of abstraction, RoBMA also revealed overwhelming evidence for publication bias, $BF_{pb} > 10^{100}$, and evidence in favor of the effect for downstream consequences of abstraction, $BF_{10} = 16.29$; however, the resulting effect size estimate was in the direction *opposite* than in the original meta-analysis, $\delta = -0.360$, 95% $[-0.657, 0.000]$. When a bias correction technique reverses the estimate of an effect size to the opposite direction, this should usually be considered as indicative of a null effect (e.g., Carter et al., 2019).⁵ Notably, both meta-analyses provided extreme evidence in favor of heterogeneity with the between study heterogeneity estimate $\tau = 0.367$, 95% $[0.314, 0.421]$, and $\tau = 0.712$ $[0.616, 0.804]$, for the effects on abstraction and downstream consequences, respectively.

Due to the inability of publication bias adjustment methods to perform 3-level meta-analysis that accounts for clustering of effect size estimates within individual studies

² The publication bias correction models were preferred beyond the numerical precision of R (around 10^{308}).

³ As a general rule of thumb, Bayes factors between 1 and 3 are regarded as anecdotal evidence, Bayes factors between 3 and 10 are regarded as moderate evidence and Bayes factors larger than 10 are regarded as strong evidence. When evidence for the null is considered the Bayes factor is inverted (i.e., $BF_{01} = 1/BF_{10}$) (e.g., Jeffreys (1939); Lee and Wagenmakers (2013, p. 105); Wasserman (2000)).

⁴ For more information about prior specification, see Bartoš et al. (2021) Appendix B

⁵ This correction in the opposite direction is likely due to the extreme bias observed based on p -value selection. The relative publication probability of studies estimating effects in the opposite direction is estimated to be extremely low; therefore, adjusting for this difference in publication probability flips the overall estimate to be in the opposite direction. As Table B2 indicates, the estimate is corrected to zero, when $|d| = 1.5$ are removed, indicating that the interpretation as a true zero effect is more appropriate than concluding an effect in the opposite direction.

(the previous results ignored the dependencies between individual effects), we also re-analysed the data in two additional ways: (1) selecting the most precise estimate from each study; (2) aggregating the estimates from each study. In addition, Soderberg’s meta-analysis contained several unrealistically large effects which might have a strong impact on the PET-PEESE regression line. Therefore, we also analyzed the data with all effects larger than $|d| = 1.5$ removed (19 estimates for the effect of psychological distance on abstraction and 21 estimates for downstream consequences of abstraction). Finally, because regression based methods are sometimes criticized for finding bias for reasons other than publication bias (Lau et al., 2006), we also reanalyzed the data by only using the selection models in RoBMA (excluding PET-PEESE) as in Bartoš et al. (2022) and Maier, Bartoš, Stanley, et al. (2022). Overall, this resulted in 12 different analyses (2x2x3). The results of this ‘RoBMA multiverse’ (Gelman & Loken, 2013; Orben & Przybylski, 2019; Wagenmakers et al., 2022) can be found in Appendix B. All of these permutations show very strong evidence for publication bias and smaller effect sizes than the original meta-analysis. Regarding the effect of psychological distance on abstraction, six analyses show evidence against an effect⁶, three are undecided and three show evidence in favor. For downstream consequences of abstraction, six show evidence against an effect, two are undecided, and four show evidence in favor of an effect. In other words, the CLT literature is certainly contaminated by publication bias which leads to strong inflation of effect sizes. However, while the default analysis and the majority of additional analyses show evidence of absence regarding a mean effect, this finding is not entirely robust to model specification and should be considered with caution (see Appendix B for details).

Figure 3 shows the footprints of publication bias in the Soderberg et al. (2015) dataset. Panel 1 visualises the RoBMA model-based estimate of the relationship between standard errors and effect sizes; the results show that studies with smaller sample sizes (and more noisy estimates) systematically lead to higher (biased) effect size estimates. In

⁶ Including two cases that show evidence for an effect in the opposite direction.

addition, the intercept of the PET-PEESE regression line shows that the hypothetical effect size for a study with infinite precision would be tiny for the effects on abstraction (A1). Panel 2 visualises the relationship between one-sided p -values and relative publication probabilities. This visualization suggests that studies with statistically non-significant effect size estimates are considerably less likely to be published than significant ones, especially in the studies on downstream consequences (B2). This pattern is especially severe for studies with effects in the opposite direction to what CLT predicts.

Figure 3*Footprints of Publication Bias in Soderberg and Colleagues' (2015) Dataset*

Note. Column 1 shows the relationship between standard errors (x -axis) and effect sizes (y -axis) based on the conditional RoBMA PET-PEESE models (five and eight estimates out of the plotting range not shown); Column 2 shows the relationship between one-sided p -values (x -axis) and relative publication probabilities (y -axis) based on the conditional RoBMA selection models. Row A shows the results for the effect of psychological distance on abstraction, and row B shows the results for the downstream consequences of abstraction.

Quantitative Assessment of More Recent Evidence

Our reanalysis of the dataset by Soderberg et al. (2015) has two limitations: (1) it only concerns studies published up to 2013, and (2) it focuses only on the effect of abstraction and the downstream consequences. Given the recent credibility revolution and the concomitant improvement of the quality of evidence in behavioural research (Nosek et al., 2018; Nosek et al., 2022; Vazire, 2018), it is crucial to examine more recent studies. In addition, to obtain a sense of the complete research field it is also important to examine a wider range of studies, with all interventions and dependent measures that could be associated to CLT.

In order to investigate more recent evidence and also to obtain a wider overview of the CLT literature, we further coded the 400 most highly-cited studies (in terms of citations/year) on CLT published since 2013 for a z-curve analysis. We choose to focus on the most highly-cited studies, rather than for example a random subset, as we thought it most important to check the robustness of those studies that researchers will read and cite in practice. Notably, the sample of studies z-curved comprises XXX% of the overall citations of the CLT studies identified based on our search criteria.

Z-curve estimates the mean statistical power of the published studies after selection for statistical significance (Bartoš & Schimmack, 2020a; Brunner & Schimmack, 2020). Unlike standard meta-analytic methods such as RoBMA, z-curve does not assume the studies originate from a single distribution of effects. Therefore, it is uniquely suited for assessing the credibility of literature – even if the different studies tested completely different effects. This is not the case of RoBMA which assumes that the individual studies are interested in a common underlying construct. Therefore, conducted the following wider assessment of CTL literature with z-curve.

To estimate the mean statistical power of a diverse set of studies and account for a possibly large degree of heterogeneity, z-curve models the distribution of test statistics. First, z-curve transforms the test-statistics into z-scores and then models them as a

mixture of truncated folded normal distributions. In other words, z-curve's goal is to approximate the overall distribution of all statistically significant studies regardless of the direction of the effect (under the assumption that all significant studies get published). This approximate distribution is then used to compute summaries about the published statistically significant studies, the expected replication rate, or to extrapolate and provide information about all conducted (and possibly unpublished) studies, the expected discovery rate.

Z-curve quantifies the degree of publication bias in terms of the expected replication rate and expected discovery rate. The expected replication rate is the proportion of statistically significant studies that are expected to produce a statistically significant result in an exact replication with the same sample size. In other words, it corresponds to the unconditional power of statistically significant studies (i.e., power not assuming $\mathcal{H}_1$ to be true). The expected discovery rate reflects the expected proportion of statistically significant studies amongst all conducted (and possibly unobserved) studies. A lower expected discovery rate than the observed discovery rate indicates that a proportion of non-significant studies is likely to be missing from the literature (Bartoš & Schimmack, 2020a).

The accuracy of the expected replication rate estimates under heterogeneity was corroborated in a simulation study by Brunner and Schimmack (2020). Later, Bartoš and Schimmack (2020a) extended these simulations and tested z-curve's performance in both the expected discovery rate as well as the expected replication rate in cases with studies generated from true null hypotheses. Furthermore, Bartoš and Schimmack (2020a) used two large data sets of hand-coded test statistics from social psychology journals and found that the expected replication rate estimated by z-curve overestimated the observed replication rate of social psychology studies from the Open Science Collaboration project, which suggests that, if anything, inferences from z-curve may yield conservative estimates of the degree of publication bias in a literature (Open Science Collaboration, 2015).

We obtained 306 test statistics of focal-hypotheses and conducted a z-curve analysis to estimate replicability of the selected studies from the published literature (see Bartoš and Schimmack, 2020b, Appendix A). To achieve a more powerful assessment of the estimated replicability across an extended period, we further combined the newly coded test statistics with data from the Soderberg et al. (2015) meta-analysis. Figure 4 compares histograms of the observed distribution of test-statistics (converted to z -scores) to the z-curve based estimate of the expected distribution of test-statistics (full blue line). Results of both z-curve analyses suggest considerable publication bias indicated by the mismatch between the expected discovery rate and the observed discovery rate; 18%, 95% CI [7%, 53%], vs. 80%, 95% CI [75%, 84%], based on the newly recoded studies (Panel A), and 37%, 95% CI [17%, 48%], vs. 65%, 95% CI [62%, 68%], based on the extended data set (Panel B). However, the estimated replication rate of 60%, 95% CI [49%, 70%] based on the newly recoded studies (Panel A), and 55%, 95% CI [49%, 60%], based on the combined data set (Panel B), shows that some studies on Construal Level Theory might hold up under replication.

Conclusion

We critically assessed the evidence underlying Construal Level Theory. Our major conclusion is that the evidential value of studies in this field is questionable at best. First, a RoBMA multiverse re-analysis of a large dataset – previously interpreted as providing strong evidence for CLT – found strong evidence of publication bias. In addition, most model specifications show evidence against both an effect of construal level on abstraction and on downstream consequences of construal level after publication bias is accounted for. For those model specifications where we did not find evidence of absence, the effect size was still considerably smaller than in the original meta-analysis. Second, we quantitatively assessed more recent evidence with a z-curve analysis. This analysis indicated a striking mismatch between the expected and observed discovery rates – a pattern that is indicative

Figure 4*Z-curve Analysis of Test-Statistics from Construal Level Theory Studies*

Note. Estimated distribution (and 95% CI) of test statistics based on statistically significant results (blue line) vs. observed distribution (histogram). The much lower number of observed than expected nonsignificant z-scores indicates the existence of studies missing due to publication bias. Panel A shows z-curve fitted to the 306 hand-coded test statistics of focal hypothesis tests from the 400 most highly-cited articles on Construal Level Theory published since 2013. Panel B shows z-curve fitted to an expanded data set including these 306 effects as well as test statistics from Soderberg and colleague's (2015) meta-analysis.

of publication bias.

Importantly, it is not clear which part of the research process causes the estimated low replicability and discovery rates. It is possible that CLT as an idea is fundamentally correct, but that poor measurement or weak manipulations cause low expected discovery rates in practice (Eronen & Bringmann, 2021; Flake & Fried, 2020; Lilienfeld & Strother, 2020). For example, measures of concreteness have been criticized for lack of validity (Yeomans, 2021). In addition, many CLT experiments employ between-subjects priming procedures (e.g., imagine oneself engaging in and describing activities either 'tomorrow' or

'next year' as the manipulation Liberman and Trope, 1998 or viewing a map that is displayed on a larger vs smaller screen Schuldt et al., 2018), and recent large scale replications show that between-subjects priming effects are often not replicable (Camerer et al., 2018). In addition, the extremely large heterogeneity in the Soderberg et al. (2015) meta-analysis questions whether we can draw strong conclusions about the individual studies based on the evidence regarding a mean effect. In other words, due to the large variability of true effects, some CLT methods are likely still effective (while others probably show backfire effects) even if the mean effect was zero.

Finally, bias correction techniques are no panacea. Importantly, the estimates from these methods cannot be seen as an alternative to high-powered replications (Carter et al., 2019; Kvarven et al., 2020). Therefore, it is important to carry out more empirical research to critically assess the evidence underlying CLT. This new research should ideally be conducted via high-powered registered reports to arrive at a more credible assessment of the theory (Chambers, 2013; Chambers et al., 2015). We especially welcome one such initiative (<https://climr.org/>), which examines the effect of the four different distance domains on abstraction and plans to finish data collection in late 2023. We hope that more replication efforts like this will also be started on the applied implications of the theory and downstream consequences of abstraction. Until these replication projects are conducted, researchers should treat CLT based interventions with caution, given the doubts raised here about the underlying evidence base.

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Appendix A

Coding procedure

We used search for the topic (*“construal level theory” or “construal level”*) or (*“psychological distance” AND “abstract* spatial/social/temporal/probability/power”*) on Web of Science. Then we sorted the resulting articles based on citations/year and read the abstracts in descending order. We identified 400 relevant articles based on their abstracts. We extracted the first test statistic from each article that related to construal level as dependent variable, independent variable, or moderator (306 out of the 400 articles). Often no test statistic could be extracted, mostly due to inadequate reporting (e.g., only $p < .05$).

Appendix B

Additional RoBMA Results

Table B1

Summary of RoBMA Estimates for Soderberg et al. (2015).

Abstraction	All	Most Precise	Aggregated
BF ₀₁	7.75	9.09	6.57
BF _{rf}	$> 10^{100}$	3.70×10^6	$> 10^{100}$
BF _{pb}	$> 10^{100}$	22826	4.19×10^6
δ (95% CI)	-0.002 [-0.113, 0.056]	0.004 [-0.036, 0.116]	0.003 [-0.093, 0.149]
τ (95% CI)	0.367 [0.314, 0.421]	0.217 [0.142, 0.347]	0.437 [0.251, 0.557]

Downstream Consequences	All	Most Precise	Aggregated
BF ₀₁	0.061	5.92	4.03
BF _{rf}	$> 10^{100}$	$> 10^{100}$	$> 10^{100}$
BF _{pb}	$> 10^{100}$	4.34×10^6	$> 10^{100}$
δ (95% CI)	-0.360 [-0.657, 0.000]	0.012 [-0.004, 0.167]	-0.021 [-0.054, 0.247]
τ (95% CI)	0.712 [0.616, 0.804]	0.311 [0.240, 0.387]	0.398 [0.316, 0.461]

δ and τ correspond to mean model-averaged estimates with 95% credible intervals.

BF₀₁ corresponds to the Bayes factor *against* an effect, BF_{rf} corresponds to the Bayes factor for heterogeneity, and BF_{pb} to the Bayes factor for publication bias. The Bayes factors equal to $> 10^{100}$ correspond to values beyond the numerical precision of R.

Table B2

Summary of RoBMA Estimates for Soderberg et al. (2015) with $|\delta| < 1.5$.

Abstraction	All	Most Precise	Aggregated
BF_{01}	0.240	5.29	14.71
BF_{rf}	$> 10^{100}$	35293	$> 10^{100}$
BF_{pb}	1.40×10^6	24946	$> 10^{100}$
δ (95% CI)	0.143 [0.000, 0.279]	0.020 [0.000, 0.237]	0.003 [0.000, 0.053]
τ (95% CI)	0.232 [0.166, 0.302]	0.155 [0.091, 0.265]	0.158 [0.116, 0.205]

Downstream Consequences	All	Most Precise	Aggregated
BF_{01}	1.17	0.312	0.032
BF_{rf}	$> 10^{100}$	740002	$> 10^{100}$
BF_{pb}	$> 10^{100}$	26900	$> 10^{100}$
δ (95% CI)	0.068 [0.000, 0.238]	0.153 [0.000, 0.312]	0.276 [0.000, 0.373]
τ (95% CI)	0.315 [0.251, 0.372]	0.212 [0.136, 0.304]	0.223 [0.171, 0.317]

δ and τ correspond to mean model-averaged estimates with 95% credible intervals.

BF_{01} corresponds to the Bayes factor *against* an effect, BF_{rf} corresponds to the Bayes factor for heterogeneity, and BF_{pb} to the Bayes factor for publication bias.

The Bayes factors larger 10^{100} correspond to values beyond the numerical precision of R.

Table B3

Summary of RoBMA Estimates for Soderberg et al. (2015) When Using Only Selection Models.

Abstraction	All	Most Precise	Aggregated
BF ₀₁	0.292	2.54	6.25
BF _{rf}	$> 10^{100}$	$> 10^{100}$	$> 10^{100}$
BF _{pb}	1099	1596	6816563
δ (95% CI)	0.175 [0.000, 0.365]	0.046 [0.000, 0.295]	0.002 [-0.118, 0.153]
τ (95% CI)	0.490 [0.409, 0.583]	0.373 [0.264, 0.474]	0.472 [0.393, 0.562]

Downstream Consequences	All	Most Precise	Aggregated
BF ₀₁	0.061	5.93	4.04
BF _{rf}	$> 10^{100}$	$> 10^{100}$	$> 10^{100}$
BF _{pb}	$> 10^{100}$	8678036	$> 10^{100}$
δ (95% CI)	-0.360 [-0.657, 0.000]	0.012 [-0.004, 0.167]	-0.021 [-0.054, 0.247]
τ (95% CI)	0.712 [0.616, 0.804]	0.311 [0.240, 0.385]	0.398 [0.316, 0.461]

δ and τ correspond to mean model-averaged estimates with 95% credible intervals.

BF₀₁ corresponds to the Bayes factor *against* an effect, BF_{rf} corresponds to the Bayes factor for heterogeneity, and BF_{pb} to the Bayes factor for publication bias.

The Bayes factors larger 10^{100} correspond to values beyond the numerical precision of R.

Table B4

Summary of RoBMA Estimates for Soderberg et al. (2015) When Using Only Selection

Models with $|\delta| < 1.5$.

Abstraction	All	Most Precise	Aggregated
BF ₀₁	0.161	0.754	1.40
BF _{rf}	$> 10^{100}$	27441367	$> 10^{100}$
BF _{pb}	2588362	7986	6581221
δ (95% CI)	0.154 [0.000, 0.279]	0.106 [0.000, 0.290]	0.072 [0.000, 0.282]
τ (95% CI)	0.233 [0.168, 0.304]	0.217 [0.129, 0.316]	0.284 [0.198, 0.369]

Downstream Consequences	All	Most Precise	Aggregated
BF ₀₁	1.17	0.288	0.032
BF _{rf}	$> 10^{100}$	$> 10^{100}$	$> 10^{100}$
BF _{pb}	$> 10^{100}$	52154	$> 10^{100}$
δ (95% CI)	0.068 [0.000, 0.238]	0.157 [0.000, 0.312]	0.276 [0.000, 0.373]
τ (95% CI)	0.315 [0.251, 0.372]	0.214 [0.142, 0.304]	0.223 [0.171, 0.317]

δ and τ correspond to mean model-averaged estimates with 95% credible intervals.

BF₀₁ corresponds to the Bayes factor *against* an effect, BF_{rf} corresponds to the Bayes factor for heterogeneity, and BF_{pb} to the Bayes factor for publication bias.

The Bayes factors larger 10^{100} correspond to values beyond the numerical precision of R.