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# 'Fear of the Light'? Transparency does not reduce the effectiveness of nudges. A data-driven review.

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# ‘Fear of the Light’? Transparency does not reduce the effectiveness of nudges. A data-driven review.[1]

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**Abstract**

Does transparency reduce the effectiveness of nudges? The question is central in recent research about nudges, since it leads to ethical and practical implications regarding responsibility, agency, the design of nudges and policy-making. We meta-analysed results from 19 studies comparing transparent to opaque nudges and found no difference in the effectiveness of the nudge. We then tested several moderators such as the type of experiment (Online, Laboratory, Field), Category (structure, information, assistance) and domain (environment, food, health, pro-social and other) and found no meaningful moderator. We note that only two studies were conducted in the field and that there is an over-representation of default nudges in the studies included. We call for an improvement of research conducted on transparent nudges and the inclusion of more types of nudges, preferably in a field setting. It is also important to define what form of transparency societies require for respecting their citizen’s autonomy.

Keywords: nudge, transparency, meta-analysis, review

JEL Classification Codes: C9, D91

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[1] Corresponding author: Zacharias Maniadis (maniadis.zacharias@ucy.ac.cy). We would like to thank the contacted authors of the articles included in this review for the information they provided. Furthermore, we thank Patrik Michaelsen for his helpful comments and suggestions. Zacharias Maniadis is supported by the project *SInnoPSis*, funded by Horizon 2020 under grant agreement ID: 857636. The authors have no conflicts of interest to declare in this study. All materials, data, and code are available at https://osf.io/2u4ae/.

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| Role | Adrien Fillon | Hendrik Bruns | Zacharias Maniadis | Yavor Paunov |
|---|---|---|---|---|
| Conceptualization | | X | | X |
| Pre-registration | | | | |
| Data curation | | X | | X |
| Formal analysis | X | | | |
| Funding acquisition | | | | |
| Investigation | X | | X | |
| peer review / verification | | X | | X |
| Data analysis peer review/verification | | X | X | |
| Methodology | X | X | X | |
| Project administration | | X | | |
| Resources | | | | |
| Software | | | | |
| Supervision | | | X | X |
| Validation | | X | X | |
| Visualization | X | | | |
| Writing-original draft | X | | X | |
| Writing-review and editing | | X | | X |

# 1 Introduction

Policymakers around the world increasingly rely on behavioural insights to address a wide array of policy issues (Whitehead, 2019). Behavioural insights put human behaviour at the core of evidence-based policymaking. The great hope has been that such focus can improve existing policies and reveal alternative approaches to policy problems. One such approach consists of interventions called nudges. Nudges seek to systematically change behaviour by affecting the choice architecture of decision-makers, without using financial incentives or significant restrictions (Thaler and Sunstein, 2008). The application of nudges has been one of the most impressive developments in public policy in the last 15 years (Hallsworth, 2023). Nudges have the advantage of being effective, low-cost, and respectful towards people's freedom of choice. (Thaler and Sunstein, 2003).

Not everyone agrees with this assessment. In a critical review, Bovens (2009) delivered a detailed critique of nudges from an ethical standpoint. Bovens (2009) argue that nudges are more ethically permissible if they target behaviours that the individuals themselves would rate as desirable, if they do not disturb individuals' feelings of internal consistency, and if they allow people to maintain control over their behaviour. Fleshing out the last point, covert nudges may limit people's ability to make autonomous choices and can be perceived as external attempts to drive people's behaviour without their consent, typically manipulating automatic and non-deliberative processes. This is important, because the designers of a nudge have the option to enhance transparency over what the nudge is attempting to do (and via which means), and hence presumably protect individuals' agency.

Accordingly, there have been calls for a minimum level of transparency when these interventions are used (House of Lords, 2011). The key question is whether transparency is also likely to undermine the effects of nudges, and it has been theorized that nudges can become increasingly ineffective as transparency is introduced (Bovens, 2009). From a theoretical point of view, since people strive for self-determination (Deci and Ryan, 1985), they can perceive nudging as a limitation to their freedom of choice (Brehm, 1966). Hence, they could deliberately ignore or resist a nudge to reinstall that freedom (Paunov et al., 2019). On the other hand, the conditioning literature in psychology (Gorn, Jacobs, and Mana, 1987) suggests that people may respond more favourably when they are aware of the nudging intervention, while

the 'third person effect' (Perloff, 1993) suggests that participants may not worry too much about themselves being prone to manipulation, potentially moderating the possible effect of reactance.

Experiments and surveys in the recent literature have considered the effects of transparency on the efficacy of nudges. Michaelsen and Sunstein (2023), DeRidder, Kroese and van Gestel (2022) as well as Marchiori, Adriaanse and de Ridder (2016) review the literature and conclude that transparency does not seem to reduce nudge effects, while Sunstein (2016) provides evidence that the public also does not believe that transparency reduces the potential effectiveness of nudges. Very recently, Michaelsen and Sunstein (2023) reviewed the evidence on default nudges and made a relatively strong claim: "The dominant finding in the empirical literature is simple: *when a disclosure is presented along with a default nudge, the effect on behavior does not diminish*. To that extent, Boven's conjecture has been falsified." (p. 37).

While Michaelsen (2023) complements the rich non-quantitative survey literature in drawing conclusions about the limitations of the current literature, such informal reviews are prone to risks of incomplete, selective, and subjective coverage of the literature. What is missing is a rigorous quantitative analysis, summarizing our state of knowledge about the limitations in generalizing and drawing inferences from the current literature. Such an analysis of limitations can also guide future research and suggest best intervention practices. It is worth noting that a solid majority of European and American citizens support nudges (Sunstein 2016, Reisch and Sunstein, 2016), and public support for overt nudging is also greater than support for covert nudging. Accordingly, it is important to take stock in a systematic and rigorous manner of whether coupling nudges with a transparency message – thus alleviating most ethical concerns – would undermine their efficacy.

The main objective of this paper is to conduct a quantitative review of the effects of transparency in nudging interventions,[2] while providing useful insights to inform the general debate about the mechanisms behind the effect of nudges. We perform the latter by pointing at the most critical gaps in the literature and lessons for future developments as informed by psychological theories, and by conducting a rigorous summary of existing studies in the area.

[2] The challenges of conducting this review in the face of some recent meta-findings in the nudge literature (Mertens et al., 2022, Maier et al., 2022, also see discussion section) are worth emphasizing.

Our results indicate that the empirical literature does not support the notion that transparency modifies the efficacy of nudges, positively or negatively. With that said, we do not dismiss possible effects of transparency, as our study also reveals major limitations in the literature. While the evidence indicates a low probability that publication bias might have driven the results, the number of included studies is limited and dominated by a few research teams. The literature also did not provide us with meaningful meta-analytic moderators that could explain in which settings transparency can affect the efficacy of nudges. Most studies focused on default nudges in an online setting with weak incentives, and only two were conducted in a field setting with an actual meaningful nudge. Moreover, a great majority of studies used a default choice as a nudge, limiting generalizability.[3] Default nudges do not have to be transparent but most of them are. Michaelsen and Sunstein (2023) reviewed some studies on transparency and defaults indicating that non-transparent default nudges are less approved, found less acceptable, and their choices are rated as less authentic. Overall, the existing literature supports the view that nudges need not operate 'in the dark'. A systematic agenda for examining the robustness and generality of these limited results is needed.

## 2 Methods

Our analysis includes studies with a transparency condition[4] and an explicit comparison with a control condition, measuring the effect of a nudge on a variety of different outcome variables. To identify articles that are potentially relevant to our topic of investigation, we conducted searches using Scopus and the Web of Science Core Collection. For transparency, we used the following keywords: "transparen*" and "disclos*". For the nudge component, we used the keywords "choice architect*", "behavioural intervention*", "behavioral intervention*", "nudg*", "default", "social norm*". Related to the design, we used the terms "experiment*", "evidence", "empirical", "randomized control* trial", "survey*". During the search, keywords related to transparent nudges were linked with the Boolean logic operators "OR" and "AND". The terms used were ("transparen*" OR "disclos*") AND ("choice

[3] The last few limitations of the literature corroborate quantitatively – and independently – the suggestions made by Michaelsen (2023).
[4] Generally speaking, 'transparency' may be about the existence of a nudge, about what it is expected to achieve, about the behavioural mechanism it is expected to activate, or even the source of the nudge.

architect*" OR "behavioural intervention*" OR "behavioral intervention*" OR "nudg*" OR "default" OR "social norm*") AND ("experiment*" OR "evidence" OR "empirical" OR "randomized control* trial" OR "survey*")).

After excluding duplicates based on the Digital Object Identifier (DOI), we screened titles and abstracts using ASReview (https://asreview.nl/), which applies active learning algorithms to assist systematic reviews.[5] We also screened all articles manually and independently, leaving no record that was screened solely by the naïve Bayes classifier, which uses the term frequency-inverse document frequency of included abstracts. After the initial screening of titles and abstracts, full texts were reviewed. We also looked at other articles that were published by identified authors in the field, to check whether there are relevant papers that we may have missed. For all the articles, titles, abstracts, tables, and methods sections were scanned to identify the relevance of a given source.

We only included studies that reported a difference between a transparent and non-transparent nudge on an outcome. Particularly, the included studies measured both choices and behaviour, as well as a diverse set of self-reported outcome variables. We excluded studies that failed to report the crucial statistics necessary for a meta-analysis or for which we could not create these statistics after consulting supplementary materials and contacting the authors, if necessary. We reached out to authors after coding information regarding the studies in cases where things were unclear, to verify that the information was correct. We also excluded studies written in languages other than English, studies that did not focus on transparency understood as a message accompanying a nudge, that had no empirical (experimental, data-driven) method involved (e.g., literature reviews, perspectives), or that had no appropriate experimental setup (including an appropriate control group). Finally, we included articles published until 4 September 2022. Studies that met our criteria were included in the dataset.[6] The full inclusion process can be found in Figure 1, and a full list of included articles can be found in Table 1.

[5] For replication in ASReview, the following parameters were used: (1) Classifier: Naïve Bayes (default); (2) Feature extraction: TF-IDF (Default); (3) Query strategy: Certainty-based sampling (default); (4) Balance strategy: Dynamic resampling (double = default)

[6] The code and data used for the analysis can be found via https://osf.io/2u4ae/?view_only=6d47a26726884b48bc6436c4395ea51c.

When available, the descriptives (means, standard deviations, or counts) were recorded and transformed into Cohen's d to standardize the effect sizes. During the coding process, if the nudge was tested across several different outcome variables, or using different samples, the results were listed as separate rows. Their dependence was accounted for in the three-level meta-analysis. Within all papers, we identified 114 distinct tests of transparency effects. We did not label a test as distinct when one outcome variable was the derivative of another. For instance, regarding Bruns et al. (2018) – Reference 1 in Table 1 – we did not include both transparency effects on the donation amount, and the percentage of participants donating.

When it comes to moderator analysis, our approach follows Mertens et al. (2022), in using intervention categories (information, structure, and assistance), and domains (Health, Food, Environment, Finance, Pro-social, Other). We also added one moderator: we examined the impact of the type of experiment, namely whether the experiment took place in a laboratory, in the field, or online, which may give us a flavour of the generalizability of potential findings. Finally, we applied several publication bias tests (for instance, examining funnel plot symmetry or receiving estimates with correction for bias).

## 3 The Main Analysis

Our meta-analysis of 114 effect sizes from 19 publications (total number of subjects is $n$ = 20,988) did not support an effect of transparency on nudge efficacy (Hedges' $g$ = 0.04, 95% CI [0.00, 0.08]) (Figure 2). A three-level meta-analysis, considering variance within studies, led to a similar result (Hedges' $g$ = 0.07 [-0.03, 0.18]). Heterogeneity was high in the meta-analysis, with $Q(113)= 692.99$, $p < .001$, $I^2 = 92.5\%$. The removal of influential outliers reduced dramatically the within-study variance from 31.7% to 19.3%, without modifying the overall size of the effect (Hedges' $g$ = 0.02 [-0.01, 0.05]. Between-study heterogeneity is still high under this condition ($Q(110) = 544.64$, $p <.001$, $I^2 = 80{,}6\%$). The considerable variability in effect size between studies indicates that, in some cases, transparency can lead to greater effects of nudges, while, in other cases, it can do the opposite. It can also reflect some underlying process leading to differences between studies, due to moderators, publication biases, or other factors.

We performed publication bias analysis that indicated a small possibility that bias has influenced overall results (Figure 3). Visual inspection of the relationship between effect sizes

and their corresponding standard errors revealed a symmetry around an effect of 0, confirmed by Egger's test, $b$ = -0.06 [-0.18, 0.07] and Kendall's $\tau$ = 0.03, $p$ = 0.60. Having symmetry in the funnel plot is important because it implies that the more precise the effect is, the closer it is to the effect reported, and that there is no sign of a bias led by a "small studies effect" (Schwarzer et al., 2015).

**[FIGURE 1 HERE]**

**[TABLE 1 HERE]**

Statistical tests for publication bias indicated an effect size including the null, or a very small effect. A Precision Effect Test, in which effect size is regressed on its standard error, did not indicate an effect of transparency (PET $g$ = -0.09 [-0.21,0.04]). A Precision-Effect Estimate with Standard Error, in which effect size is regressed on the square of the standard error, led to the same conclusion (PEESE $g$ = -0.02 [-0.08,0.04]). A Three-Parameter Selection Model (3PSM) indicated that the unadjusted model performed better than the model adjusted for publication bias ($\chi^2(1) = 0.74$, p = 0.4). The unadjusted model indicated a small but non-null effect (3PSM $g$ = 0.05 [0.02; 0.09]). This result is important because in a recent study, the 3PSM was the better estimate under high heterogeneity (Carter et al., 2019).

We also conducted a P-curve analysis (Simonsohn et al., 2014) which did not indicate evidence for false positives, even though we note an increase around p-values of 0.04 (Figure 4). Conditional on a true effect different from zero, we should expect p-values to be highly concentrated below 0.01, then distributed evenly between 0.01 and 0.05. The shape of the P-curve is exponential at zero. If more p-values are observed just below the 0.05 threshold, and the distribution between 0.01 and 0.05 is uneven, this may indicate some biased reporting. This is the case in our analysis, even though the proportion of effects at the 0.04 and 0.05 values is not high enough to indicate strong publication bias.

We then investigated moderators that can explain the heterogeneity in the results. Table 2 displays all moderator statistics and effect sizes and as can be seen, none of the moderators reveals a non-null effect size. We also note that most of the effects are from online studies ($k$ =

99), while only a few are from a laboratory setting ($k$ = 10) and even less from a field setting ($k$ = 2). In terms of domain, only two studies were conducted in the food domain, and they are the same as the two "field" studies. Finally, most of the effects were found via the 'decision structure' category, the most promising explanatory category according to Mertens et al. (2022).

**[FIGURE 2 HERE]**

**[FIGURE 3 HERE]**

**[FIGURE 4 HERE]**

**[TABLE 2 HERE]**

# 4 Discussion

As our meta-analysis revealed, the existing rigorous evidence does not statistically support the claim expressed in Bovens (2009), namely that transparency has a detrimental effect on the effectiveness of nudge interventions. This provides some support to previously discussed informal reviews on the effects of transparency. Hence, our first result (proof of principle) is that, based on what we currently know, the ethical defensibility of nudges could be improved – via transparency – without sacrificing efficacy.

However, our analysis also reveals the restrictive nature of the current evidence, and points to directions for future exploration and research. First, the number of studies that rigorously assess the phenomenon is relatively low, and some researchers and research teams participate in large fractions of the literature. Moreover, almost all studies are hypothetical or for small stakes, since only two of them involve actual nudge field interventions with real consequences. Whereas there may be potential risks in introducing transparency conditions in actual nudge policies, the data reveal enough promise to make it worthwhile to explore the effect of transparency in actual large interventions. Considering recent evidence suggesting smaller nudge effect sizes in applied compared to laboratory settings (DellaVigna and Linos, 2022), such investigations appear even more warranted. Implementing the transparency conditions in such studies will also enhance the methodological rigor of the literature. While pre-registration is more and more becoming a norm, we noted that only 7 studies included already pre-registered their hypotheses. This number will increase for future larger-scale field experiments, where preregistration will likely be even more prominent. In addition, a few

studies reveal remarkable positive effects of transparency in specific domains, and the mechanisms for this deserve to be carefully examined and reproduced.

Another important limitation is that the current evidence is concentrated on particular domains and types of nudging. Almost all the studies included concern default nudges (14 out of the 19 studies included concern defaults, accounting for 103 out of 114 unique effect sizes included). Therefore, the effects of transparency need to be examined in alternative types of choice architecture, such as 'social reference', 'reminder' or 'translation' (see Mertens et al., 2022). In addition, the composition of the literature seems rather concentrated, with seven studies with US samples, seven with UK samples, two from Germany, two from the Netherlands, and one study from China. Future studies could focus also on how transparency works for nudging in a cross-cultural sense. In addition, replications play a key role in building a solid body of evidence, and the results in the transparency literature need to also be affirmed by replication studies, for future meta-analyses to be able to express greater confidence on the overall effect of transparency.

We also need to make a general note. As mentioned before, we did not find meaningful moderator effects. Insufficient understanding of moderators of nudge effects appears to be a general problem (Bryan, Tipton, and Yeager, 2021). Marchiori, Adriaanse and de Ridder (2016) call for further examination of the determinants of nudge effects, and De Ridder, Kroese and van Gestel (2022) emphasize that only a small number of nudge studies examine boundary conditions or underlying mechanisms of effectiveness. One may worry that the noted lack of structure may render knowledge accumulation and theoretical development difficult, as evinced by a number of recent controversies over the effectiveness of nudging in a meta-analytic sense (Mertens et al., 2022, Maier et al., 2022). The focus has been on the effect of heterogeneity of different interventions, which renders drawing general conclusions almost impossible. Some scholars read the evidence as supportive of the effect of nudges, while others increasingly emphasize its low effect sizes.[7] Accordingly, we also note the need to examine further boundary conditions and mechanisms for nudge effectiveness (which may also inform us about the effect

[7] DellaVigna and Linos (2022) show that nudge interventions are subject to large voltage effects, whereby the effect greatly falls at scale. In the end, even pioneers in the nudge literature are being increasingly sceptical (Chater and Loewenstein, 2022).

of transparency). One of these key conditions is ‘nudgeability’, proposed by De Ridder, Kroese and van Gestel (2022). This concept concerns the degree to which individuals are susceptible to a nudge, based on their pre-existing preferences for the behaviour targeted by the nudge. For instance, future studies examining transparency could elicit measures of trust in expertise or science. Does such trust moderate people’s reaction to learning about the existence of a nudge informed by behavioral theory (with some public policy objective)? Presumably, people’s general stance to evidence-based policy could inform their reaction to the evidence-based nudge.

Finally, societies need to clearly define what form of transparency they require in order to respect their citizen’s autonomy. As long as this is not achieved, there is too much noise with little progress. When societies agree on what type of transparency is ethically defensible, studies can then examine the robustness of nudge effects on such exact types of transparency.

# Tables and Figures

**Figure 1:** *PRISMA Flowchart of included studies*

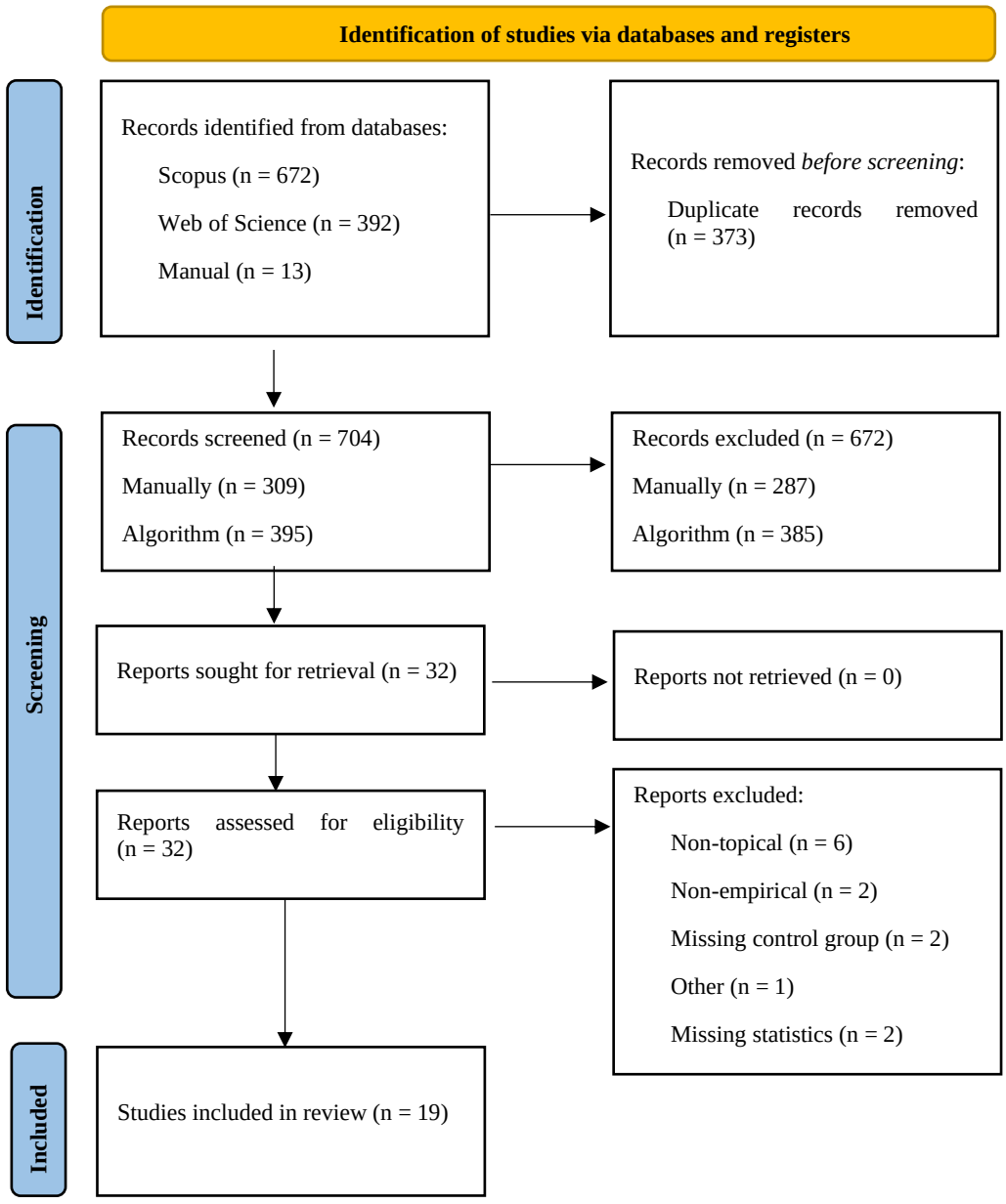


**Table 1:** *All studies/datasets included in the meta-analysis*

| | Reference | Context | Domain | *N* | Country | Type |
|---|---|---|---|---|---|---|
| 1 | Bruns et al. (2018) | Environment | Environment | 498 | Germany; The Netherlands | Lab experiment |
| 2 | Cheung et al. (2019) | Diet | Food | 589 | The Netherlands | Field experiment |
| 3 | Dranseika & Piasecki (2020) | Health | Health | 368 | UK | Online experiment |
| 4 | Grad et al. (2021) | Charity | Pro-social | 1098 | UK, US, PT, PL, others | Online experiment |
| 5 | Große-Hokamp & Weimann (2021) | Environment | Environment | 200 | Germany | Lab experiment |
| 6 | Hallez et al. (2021) | Health | Health | 94 | Belgium | Online experiment |
| 7 | Kantorowicz-Reznichenko & Kantorowicz (2021) | Lottery | Other | 748 | UK | Online experiment |
| 8 | Liu et al. (2022) | Health | Health | 1926 | CN | Online experiment |
| 9 | Michaelsen et al. (2020) | Charity | Pro-social | 1580 | MTurk worldwide | Online experiment |
| 10 | Michaelsen et al. (2021) | Environment, Charity | Environment, Pro social | 2399 | MTurk worldwide | Online experiment |
| 11 | Michaelsen, Nyström et al. (2021) | Survey | Other | 1173 | US | Online experiment |
| 12 | Michels et al. (2021) | Health | Health | 399 | UK | Online experiment |
| 13 | Paunov et al. (2018) | Education | Other | 756 | NA | Online experiment |
| 14 | Paunov et al. (2019) | Survey | Other | 265 | Online respondent panel, English speaking | Online experiment |
| 15 | Paunov et al. (2020) | Survey | Other | 256 | Online respondent panel, English speaking | Online experiment |
| 16 | Steffel et al. (2016) | Health | Health | 2809 | US | Field experiment |
| 17 | van Rookhuijzen et al. (2023) | Survey | Other | 2781 | UK, PL | Online experiment |
| 18 | Wachner et al. (2020) | Survey | Other | 630 | UK | Online experiment |
| 19 | Zhuo et al. (2022) | Environment | Environment | 1842 | UK | Online experiment |

**Figure 2:** *Forest plots of all effect sizes (n = 19, k = 114) included in the meta-analysis with their corresponding 95% confidence intervals. Effect sizes are arranged from the smallest to the highest sample size.*

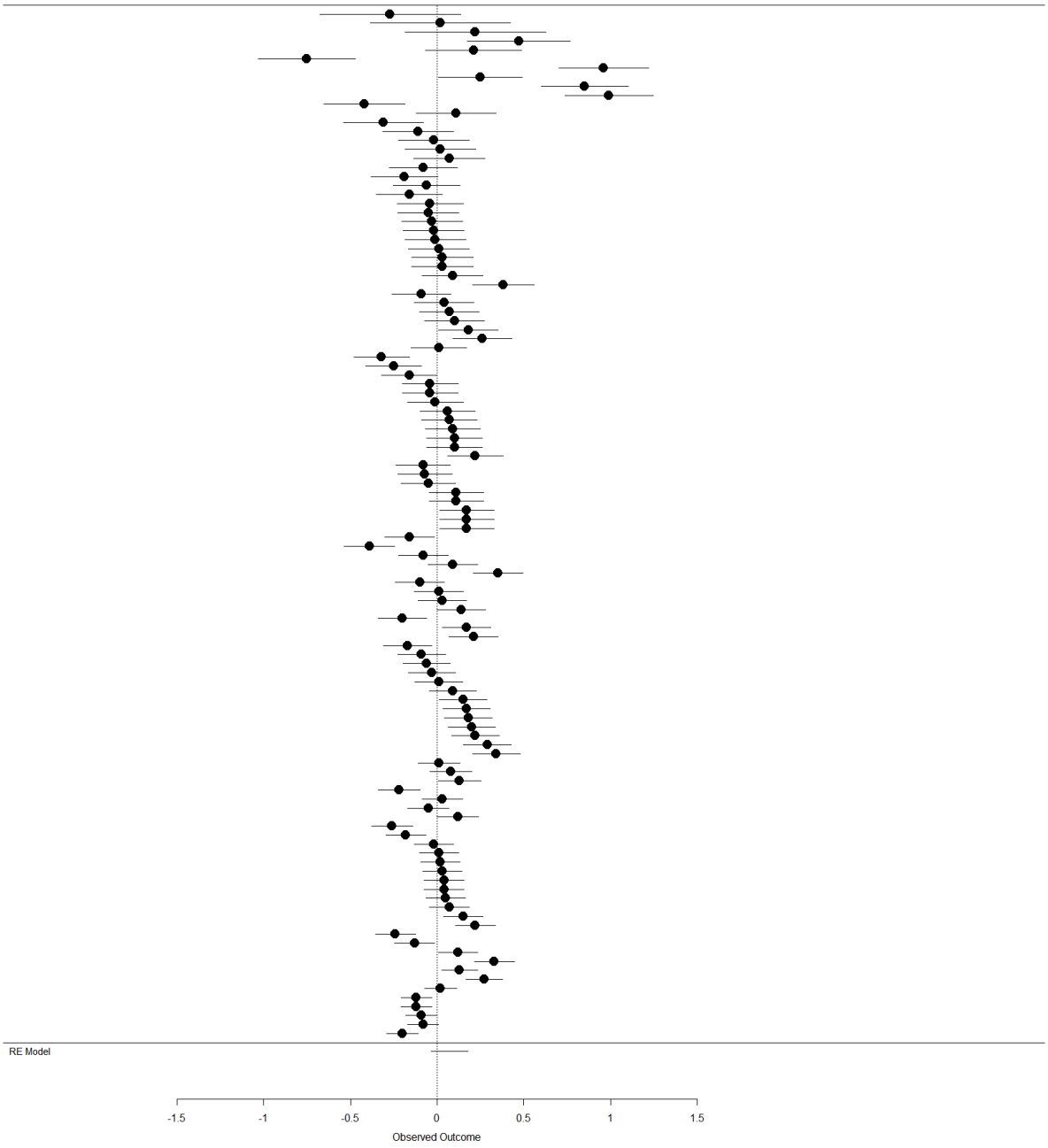


**Figure 3:** *Funnel plot displaying each observation as a function of its effect size and SE.*

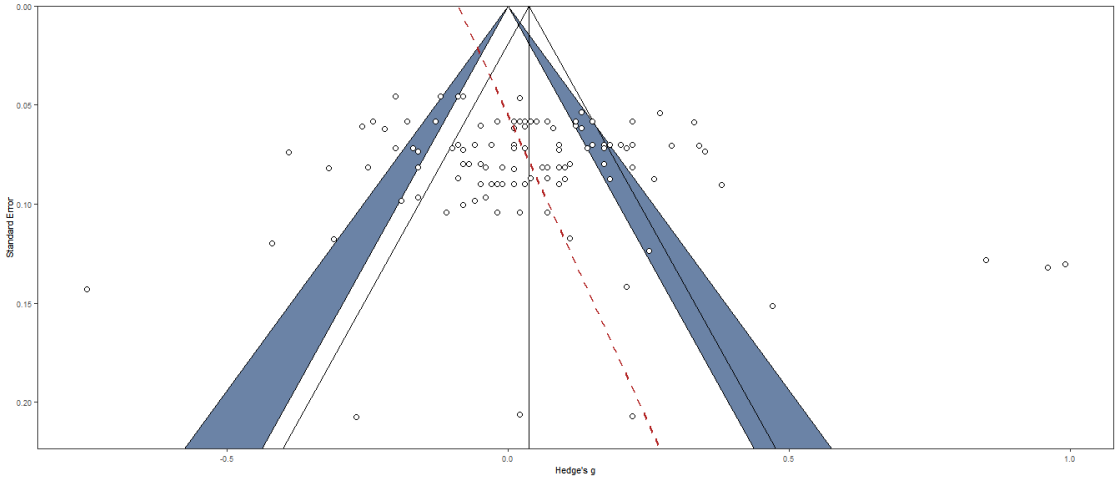
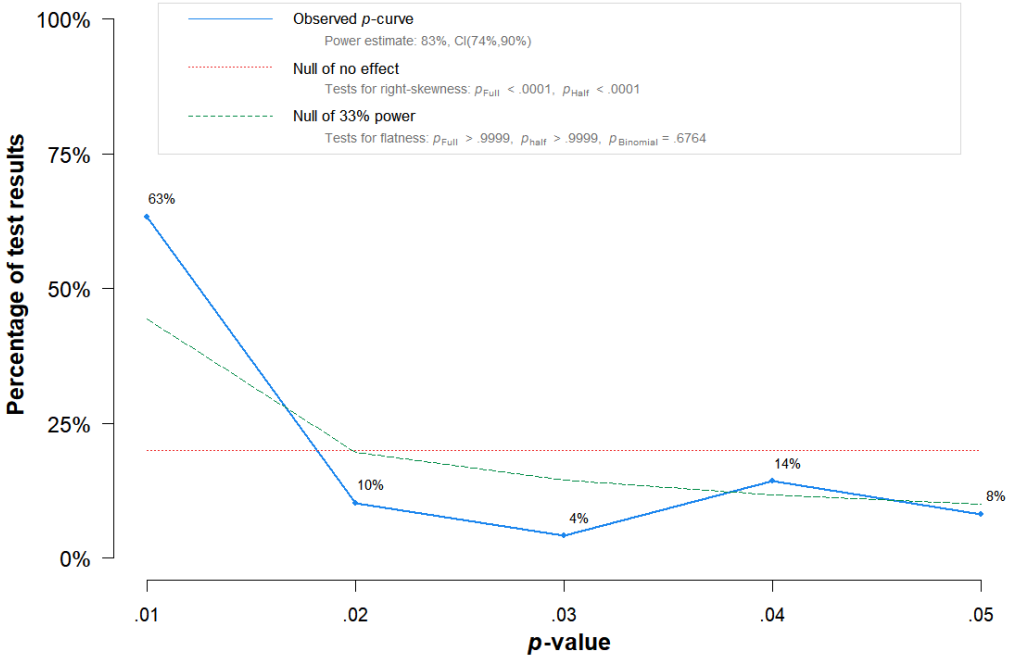



**Figure 4:** *P-curve analysis*




Note: The observed *p*-curve includes 49 statistically significant ($p < .05$) results, of which 36 are $p < .025$.
There were 65 additional results entered but excluded from *p*-curve because they were $p > .05$.

**Table 2:** *Moderator analysis*

| Effect | *k* | *g* | 95% CI | Test statistic | *P* |
|---|---|---|---|---|---|
| Type of Experiment | | | | $F(2, 108) = 0.03$ | 0.96 |
| Online | 99 | 0.08 | [-0.06, 0.21] | | |
| Laboratory | 10 | 0.06 | [-0.02, 0.14] | | |
| Field | 2 | 0.02 | [-0.07, 0.12] | | |
| Intervention Category | | | | $F(1, 112) = 0.14$ | 0.71 |
| Decision Structure | 105 | 0.10 | [-0.02, 0.22] | | |
| Decision Information | 8 | -0.06 | [-0.21, 0.09] | | |
| Decision Assistance | 1 | -0.05 | | | |
| Domain | | | | $F(1, 112) = 0.01$ | 0.92 |
| Environment | 43 | 0.04 | [-0.01, 0.10] | | |
| Food | 2 | 0.02 | [-0.07, 0.12] | | |
| Health | 14 | -0.18 | [-0.43, 0.07] | | |
| Other | 27 | 0.17 | [-0.10, 0.43] | | |
| Pro-social | 28 | 0.03 | [-0.02, 0.08] | | |

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