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Comparing transparent and covert nudges: A meta-analysis calling for more diversity in nudge transparency research☆

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ABSTRACT

Do transparent and non-transparent nudges have similar effects? The question is central in recent research on behavioural public policy, as it leads to ethical and practical implications regarding policy-maker responsibility, citizen agency, and nudge design. We meta-analysed results from 23 publications designed to compare transparent to covert nudges including 117 effect sizes and found a positive effect of transparency on behavioural outcomes, but no effect on non-behavioural outcomes. The moderator analyses revealed that studies conducted online, manipulating the decision structure, and conducted in the domain 'other' tended to exhibit significantly positive transparency effects for behavioural outcomes. We note that all but two studies were conducted online or in the lab, and that there is an over-representation of research on default nudges (88 % of total effects), severely limiting the generalizability of the findings. Thus, we call for an improvement of research conducted on transparent nudges and the inclusion of more nudge types, preferably in a field setting. We also stress the importance of defining the form of transparency that societies require for respecting their citizen's autonomy.

1. Introduction

Policymakers around the world increasingly rely on behavioural insights to address a wide array of policy issues (Whitehead et al., 2019). Behavioural insights put human behaviour at the centre of evidence-based policymaking, aiming to improve existing policies and reveal alternative approaches to policy problems. The core of the paradigm consists of interventions called nudges. Nudges seek to systematically change behaviour by affecting the choice environment for decision-makers, without using financial incentives or significant restrictions (Thaler & Sunstein, 2008). The application of nudges has been one of the most impressive developments in public policy in the last 15 years (Hallsworth, 2023), as nudges are supposedly effective, low-cost, and respectful towards people's freedom of choice (Thaler & Sunstein, 2003).

However, not everyone agrees with this assessment. In a critical review, Bovens (2009) delivered a detailed critique of nudges from an ethical standpoint. He argued that most nudges would be ethically permissible only if they allow people to maintain control over their behaviour. He argued for transparency in nudges "to ensure that everyone can unmask the manipulation if they wish to do so" (Bovens, 2009, p. 218). However, he also assumed that the mechanisms by which nudges operate "work best in the dark" (Bovens, 2009, p. 217). Hence, his theorizing implied the presence of a trade-off between introducing transparency to covert nudges on the one hand, and their effectiveness on the other.

This position quickly sparked the interest of the scientific community, which ideally informs policymakers on how to create both freedom-preserving and effective policies. The main question is whether transparent nudges produce similar effects to more covert ones. The answer is uncertain: on the one hand, people strive for self-determination (Deci & Ryan, 1985), and once they realize they have

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been nudged, they can perceive this as a limitation to their freedom of choice (Brehm, 1966). Hence, they could deliberately ignore or resist a nudge to reinstate that freedom (Paunov et al., 2019). On the other hand, the "third person effect" (Perloff, 1993) implies that people may not worry too much about being manipulated, potentially moderating such resistance. In the narrow category of default nudges, Michaelsen and Sunstein (2023)) argue that the presence of a transparent message may induce people to cognitively engage with the task and reduce inertia, one mechanism of default nudges. On the other hand, how the transparency message is formulated likely affects whether the default is perceived as a recommendation, another mechanism driving default effects (Michaelsen & Sunstein, 2023).[1]

Recent experiments have compared the impact of transparent and non-transparent nudges with respect to different outcomes, defined either as the adoption of a given behaviour or as participants' perceptions. Such perceptions pertain to issues ranging from possible restrictions to freedom imposed by a nudge, to how the implementer of the nudge is viewed. Michaelsen (2024), Michaelsen & Sunstein (2023), De Ridder et al. (2022) as well as Marchiori et al. (2017) review the literature and conclude that transparency does not seem to reduce nudge effects, while Sunstein (2016)) provides evidence that the public has a similar perception. Very recently, Michaelsen & 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." (Michaelsen & Sunstein, 2023, p. 37). However, they only considered behavioural outcomes of nudges.

While such qualitative reviews can be informative, they are often prone to risks of incomplete, selective, or subjective coverage of the literature. The field of behavioural public policy can benefit from a rigorous quantitative analysis, which systematically summarizes the available empirical evidence and draws conclusions about its strengths and limitations. This can guide future research and suggest best intervention practices. It is worth noting that a solid majority of European and American citizens support nudges (Reisch & Sunstein, 2016; Sunstein, 2016), and public support for overt nudging is also greater than for covert nudging. Accordingly, it is important to take stock in a systematic and rigorous manner of how, if at all, nudges coupled with a transparency message – thus presumably alleviating most ethical concerns (Michaelsen, 2024) – differ from non-transparent nudges with respect to different outcomes, domains and types.

The main objective of this paper is to conduct a quantitative meta-analysis of the difference between transparent and covert nudges,[2] regarding behavioural (a choice was made by the participants) and non-behavioural (perceptions, reactions, and intentions) outcomes, and to provide useful contributions to the general debate about the mechanisms behind nudges. We address the latter by pointing at the most critical gaps in the literature and lessons for future developments, and by conducting a rigorous summary of existing studies in this area.

Overall, our results reveal a rich and complex empirical picture. For the strand of the literature that examines behavioural outcomes, we find a robust medium-sized positive effect of transparency on nudge effectiveness. There is no support for effects on either desirable or undesirable non-behavioural outcomes. Our study also reveals effect heterogeneity and major limitations in the literature. Effect moderation is limited to behavioural outcomes.[3] Studies conducted online, studies manipulating the decision structure, and studies conducted in the domain "other" exhibited stronger transparency effects. The domain effect is interesting, since "domain of examination" does not seem to make a difference (the moderator is nonsignificant) in domains for which nudges are often recommended, i.e., environmental, food, health, and prosocial domains. The main effects on behavioural and non-behavioural outcomes were not robust to an alternative analysis limited to studies including no-nudge conditions.[4]

Regarding the revealed limitations of the literature, while our 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. Furthermore, most studies focused on online environments with weak incentives, and only two were conducted in the field with a consequential nudge outcome. Moreover, a large majority of studies used a default choice as a nudge, severely limiting generalizability. Overall, the existing literature does not support the view that defaults need to operate "in the dark", indicating that overt nudges may have similar effects as covert ones. However, a systematic and differentiated agenda for examining the robustness and generalizability of these limited results is urgently needed.

2. Methods

We followed the PRISMA guidelines (Page et al., 2021) to conduct this meta-analysis. Inclusion criteria were designed to encompass studies that featured both a transparent nudge condition[5] and an explicit comparison with a non-transparent nudge condition. These studies had to measure outcome variables — whether behavioural or otherwise — in each condition. By allowing for various types of outcome variables, we facilitate a comprehensive assessment of transparency, which is crucial to understanding whether transparent nudges are viable policy interventions. It is not just the potential effects of transparency on nudge effectiveness that matter, but also its impact on how citizens perceive transparent nudges—as respecting their autonomy and being ethically defensible.

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",

[1] Experimental evidence suggests different mechanisms to explain default effects. They can function as a reference value (Dinner et al., 2011; Samuelson & Zeckhauser, 1988), anchor (for preference construction) (Dhingra et al., 2012), social norm (Everett et al., 2015), persuasion attempt (Brown & Krishna, 2004), implicit recommendation (McKenzie et al., 2006), coordination device (Cappelletti et al., 2014), and through inertia (by imposing costs on default-deviation) (Madrian & Shea, 2001). The meta-analysis by Jachimowicz et al. (2018) reveals a considerable effect of defaults, but with considerable variation. They partially explain this heterogeneity by the domain to which they were applied and the mechanism through which they operated. See Reisch & Sunstein (2016) for a review on default mechanisms.

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

[3] 'Desirable' outcomes are defined as variables in which social welfare is increasing (from a social planner's perspective). Such outcomes may include, for example, behaviours that the nudge is explicitly targeting to induce, or positive perceptions, such as trust in the source of a nudge. 'Negative' outcomes are defined analogously. Examples could include negative perceptions, for instance concerning limitations to freedom imposed by the nudge.

[4] In robustness checks placed in our Appendix, we examined whether our findings carry through if we take a stricter view of what it means to examine the impact of transparency on nudge effects. If we interpret a 'nudge effect' as the difference in outcomes between a no-nudge condition and a nudge condition, then we may look on the impact of transparency on this difference. When we conduct this analysis, the sample size is greatly reduced – because only a few transparency studies include a 'no nudge' control condition – and we do not find any significant transparency effects.

[5] 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 (Bruns & Paunov, 2021).

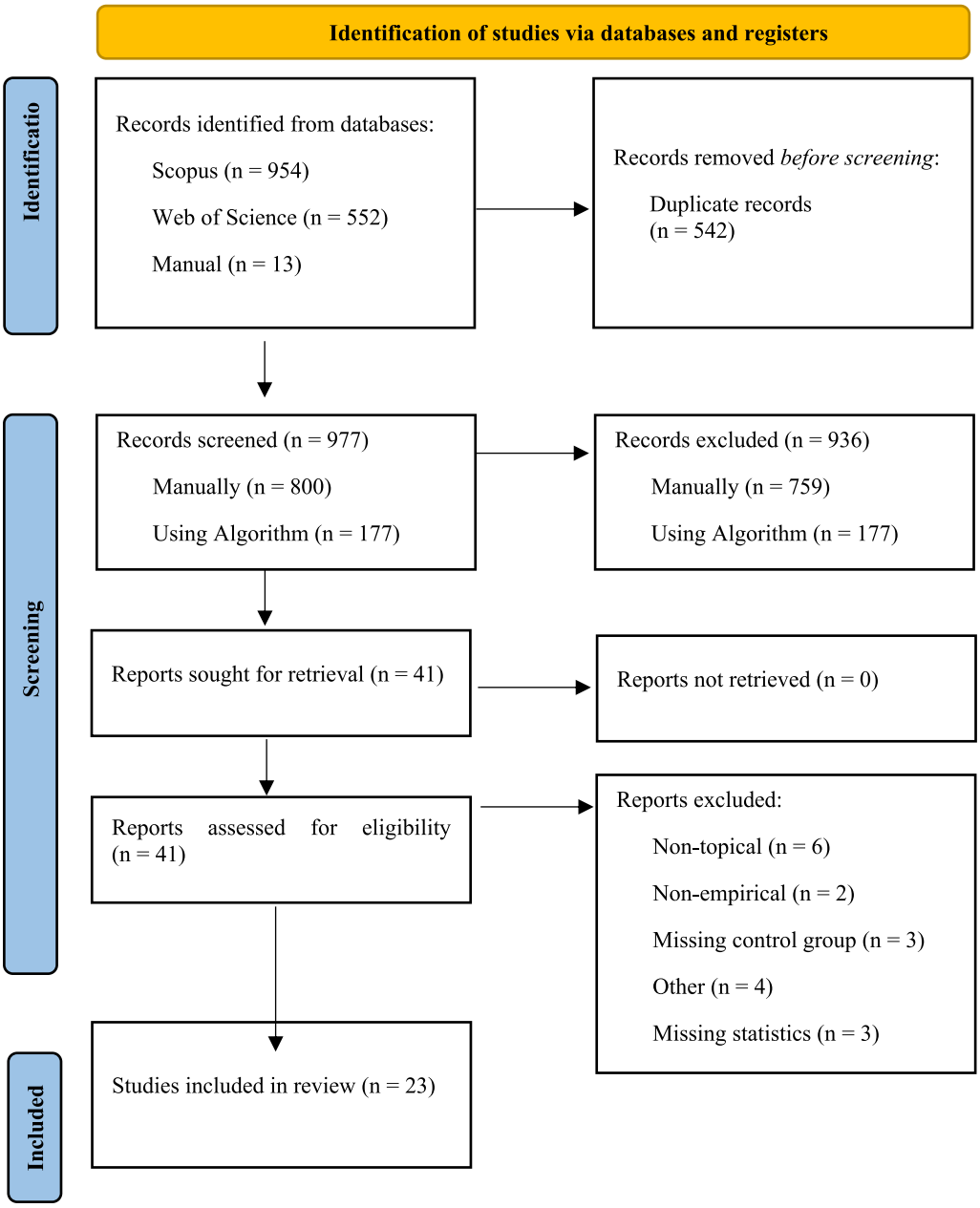


Fig. 1. PRISMA flowchart of included studies.

"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 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.[6] 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 empirical studies that tested for a difference between a transparent and non-transparent nudge on an outcome, and excluded correlational designs, observational studies, narrative reviews, and commentaries. In terms of outcomes, the included studies measured behavioural and various non-behavioural variables. We excluded studies that failed to report the crucial statistics necessary for a meta-analysis or for which we could not produce 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 the data were unclear, to verify that the

[6] 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)

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

Nr	Reference	Domain	N for main contrast by experiment	Country	Experiment Type	Outcome type, no of outcomes, valence
1	Bruns et al. (2018)	Environment	Exp. 1: 333 Exp. 2: 210 Exp. 3:212	Germany, The Netherlands	Lab	B: 3 D NB: 6 U
2	Cheung et al. (2019)	Food	Exp. 1: 589 Exp. 2: 1090	Netherlands	Field	B: 2 D
3	Dranseika & Piasecki (2020)	Health	Exp. 1:187 Exp. 2: 181	UK	Online	B: 2 D
4	Gråd et al. (2021)	Pro-social	Exp. 1: 323 Exp. 2: 311 Exp. 3: 312	UK, US, Portugal, Poland, others	Online	B: 3 D
5	Große Hokamp & Weimann (2022)	Environment	120	Germany	Lab	B: 1 D
6	Hallez et al. (2021)	Health	64	Belgium	Lab	B: 1 D NB: 2 D
7	Kantorowicz-Reznichenko & Kantorowicz (2021)	Other	370	UK	Online	B: 2 D
8	Leimstädtner et al. (2023)	Other	143	MTurk	Online	B: 1 D
9	Liu et al. (2022)	Health	545	China	Online	NB: 4 D NB: 1 U
10	Liu et al. (2023)	Other	760	China	Online	B: 1 A NB: 3 D NB: 1 U
11	Michaelsen et al. (2020)	Pro-social	Exp. 1: 289 Exp. 2: 702	MTurk worldwide	Online	B: 1 D NB: 4 D NB: 5 A
12	Michaelsen et al. (2021)	Other	Exp. 1: 415 Exp. 2: 373*	US	Online	B: 3 D NB: 2 D
13	Michaelsen et al. (2024)	Environment, Pro social	Exp. 1: 188 Exp. 2: 217 Exp. 3: 400 Exp. 4: 402	MTurk worldwide	Online	B: 4 D NB: 8 D NB: 6 U
14	Michels et al. (2021)	Health	200	UK	Online	B: 2 D
15	Michels et al. (2023)	Health	201	UK	Online	B: 2 D
16	Paunov et al. (2018)	Other	Exp. 1: 165 Exp. 2: 158 Exp. 3: 117	Online panel, English-speaking	Online	B: 3 D NB: 1 D NB: 1 U
17	Y. Paunov et al. (2019)	Other	Exp. 1: 110 Exp. 2: 103 Exp. 3: 108	Online panel, English-speaking	Online	B: 3 D
18	Paunov et al. (2020)	Other	131	Online panel, English-speaking	Online	B: 1 D
19	Paunov et al. (2022)	Other	Exp. 1: 128 Exp. 2: 131 Exp. 3: 113 Exp. 4: 110	Online panel, English-speaking	Online	B: 4 D
20	Steffel et al. (2016)	Health	Exp. 1: 222 Exp. 2: 200 Exp. 3: 104 Exp. 4–7: 195 Exp. 8–11: 204	US	Field	B: 11 D NB: 11 D
21	van Rookhuijzen et al. (2023)	Other	Exp. 1: 857 Exp. 2: 840	UK, Poland	Online	B: 2 D
22	Wachner et al. (2020)	Other	Exp. 1: 449** Exp. 2: 424**	UK	Online	B: 2 D NB: 4 D NB: 2 U
23	Zhuo et al. (2023)	Environment	1219	UK	Online	B: 1 D

Notes: B: Behavioural outcome; NB: Non-behavioural outcome; D: Desirable outcome; U: Undesirable outcome; A: Ambiguous outcome; Exp.: Experiment. * 371 for outcome "Choice to participate voluntarily in future survey". ** 420 for behavioural outcome.

information was correct.[7] 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 employed no empirical (experimental, data-driven) method, or that had no appropriate experimental setup (including an appropriate control group). Finally, we included articles published until 20 July 2024. Studies that met our criteria were included in the dataset.[8] The full inclusion process can be found in Fig. 1, and a full list of included articles can be found in Table 1.

When available, the descriptives (means, standard deviations, or counts) were recorded and transformed into Cohen's d to standardize the effect sizes, then corrected for low sample sizes using Hedge's g (Higgins et al., 2023). 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

[7] We reached out to the main authors of ten papers that we had identified by August/September 2022. We asked them to a) check if the information we retrieved on their paper was correct, and to provide us with missing information, if needed. We received replies from seven authors, providing complementary data, which we took into account. In one case, the additional data still did not suffice to make the required calculations in the meta-analysis.

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

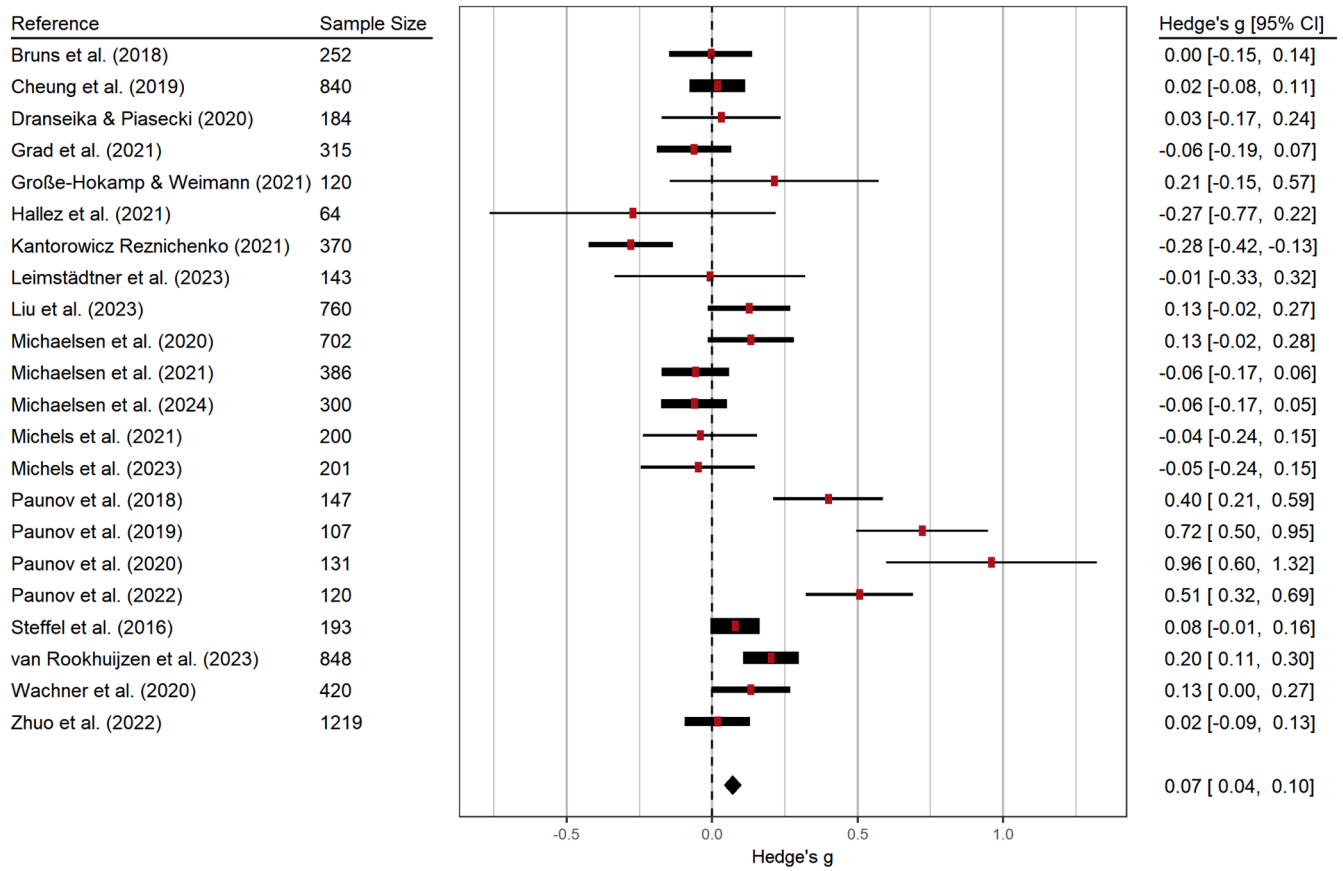


Fig. 2. Forest plot of studies containing a behavioural effect ($n = 22$) included in the meta-analysis with their corresponding 95 % confidence intervals.
Notes: Sorted alphabetically. The thickness of each line indicates the weight of the study in the meta-analysis. The figure containing the results of the 3-level meta-analysis can be found in the appendix. Effects were merged by reference using the aggregate function of the Metafor Package.

accounted by using a three-level meta-analysis. Within all papers, we identified 117 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.

For the 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) as moderators. This classification is detailed in Szaszi et al. (2018)'s review on nudge effectiveness. We also examined the impact of the type of experiment, namely whether the experiment took place in a laboratory, in the field, or online, characteristics than have been shown to influence nudge effectiveness (DellaVigna & Linos, 2022). Additionally, we examined the effects of transparency on different types of outcome variables (choice, nudge reaction, nudge perception, choice satisfaction, source perception, intention). We further separate these outcome variables into normatively desirable, undesirable, or ambiguous from the social planner's perspective. Thus, we make sure that potential transparency effects are compared only when they are comparable. Not doing this could lead to an underestimated meta-analytic effect size. For example, aggregating the effect of transparency on perceived threat to freedom (undesirable) and perceived informativeness (desirable) in one effect size would be biased. More information on how we addressed this is provided in the Appendix, where we also describe 11 cases where we inverted the sign of effect sizes to account for specifics in the experimental design. Finally, we applied several publication bias tests (for instance, examining funnel plot asymmetry or receiving estimates with correction for bias).

3. The main analysis

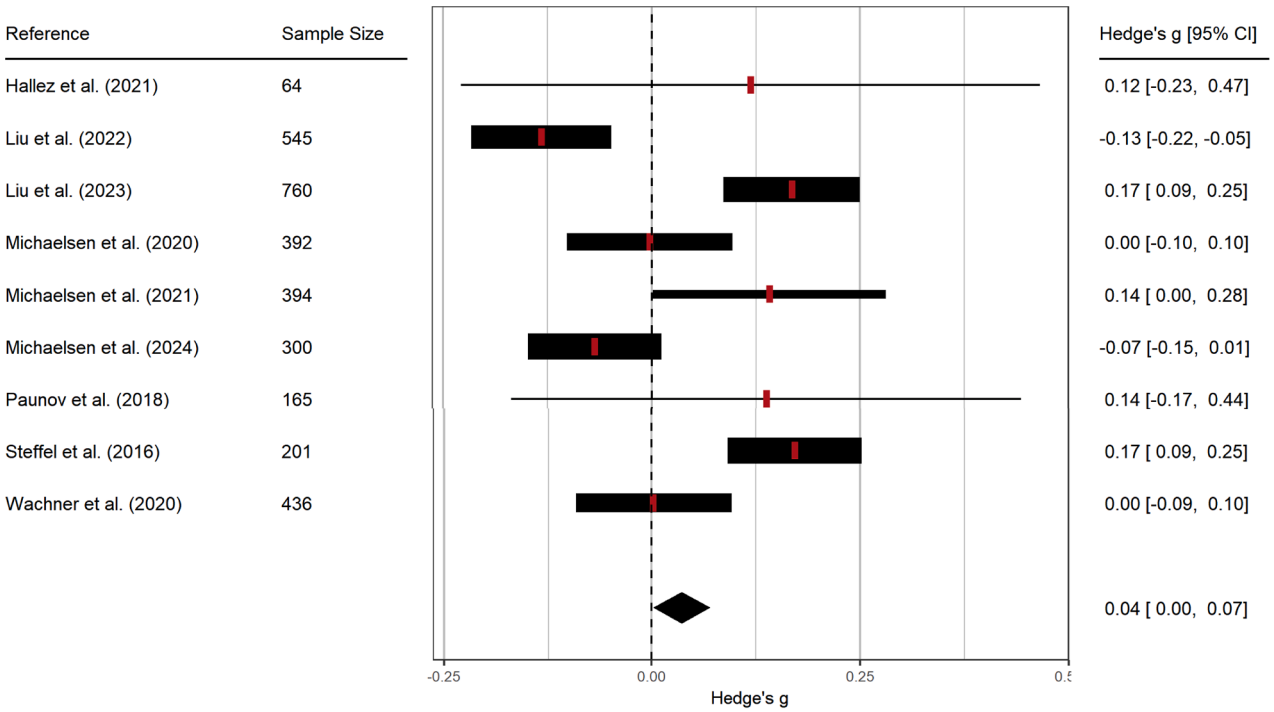
As outlined above, to ensure comparability of effects, we divided our meta-analysis of 117 effect sizes from 23 publications (total number of subjects is $n = 15{,}855$) according to whether effects were behavioural or non-behavioural. We divided the latter into, from the social planner's perspective, normatively desirable, undesirable, and ambiguous. The three-level meta-analysis, considering variance within studies, supported the notion that, behaviourally, transparent nudges were more effective than covert nudges ($n = 22$, $k = 55$, Hedges' g = 0.12, 95 % CI [0.01, 0.23], $p = .039$). See

Fig. 2 for a forest plot of all behavioural effects.

For non-behavioral outcomes, we found no support for transparency effects on desirable, ($k = 40$, Hedges' g = 0.04, 95 % CI [-0.05, 0.13], $p = .35$), undesirable ($k = 17$, g = 0.05 [-0.01, 0.10], $p = .10$), or ambiguous outcomes ($k = 5$, g = 0.09 [-0.01, 0.18], $p = .07$). Fig. 3 presents forest plots for desirable and undesirable non-behavioural effects.

For the behavioural measures, the part of variance not attributable to sampling error was high, with $Q(54) = 208.77$, $p < .001$, $I^2 = 83.20$ %, split into within-study heterogeneity (28.10 %) and between-study heterogeneity (55 %). Heterogeneity was also high for desirable non-behavioural outcomes ($Q(39) = 81.96$, $p < .001$, $I^2 = 56.10$ %, split into within-study heterogeneity (26.10 %) and between-study heterogeneity (30 %)). On the other hand, heterogeneity was low for undesirable outcomes (Q (17) = 16.62, $p = .41$). Since the ambiguous condition comprises too few effects, we did not investigate it further, but the relevant analysis can be found in the supplementary materials.

We performed publication bias analysis to estimate the probability that bias influenced the results (Fig. 4). Visual inspection of the relationship between effect sizes, their corresponding standard errors, and



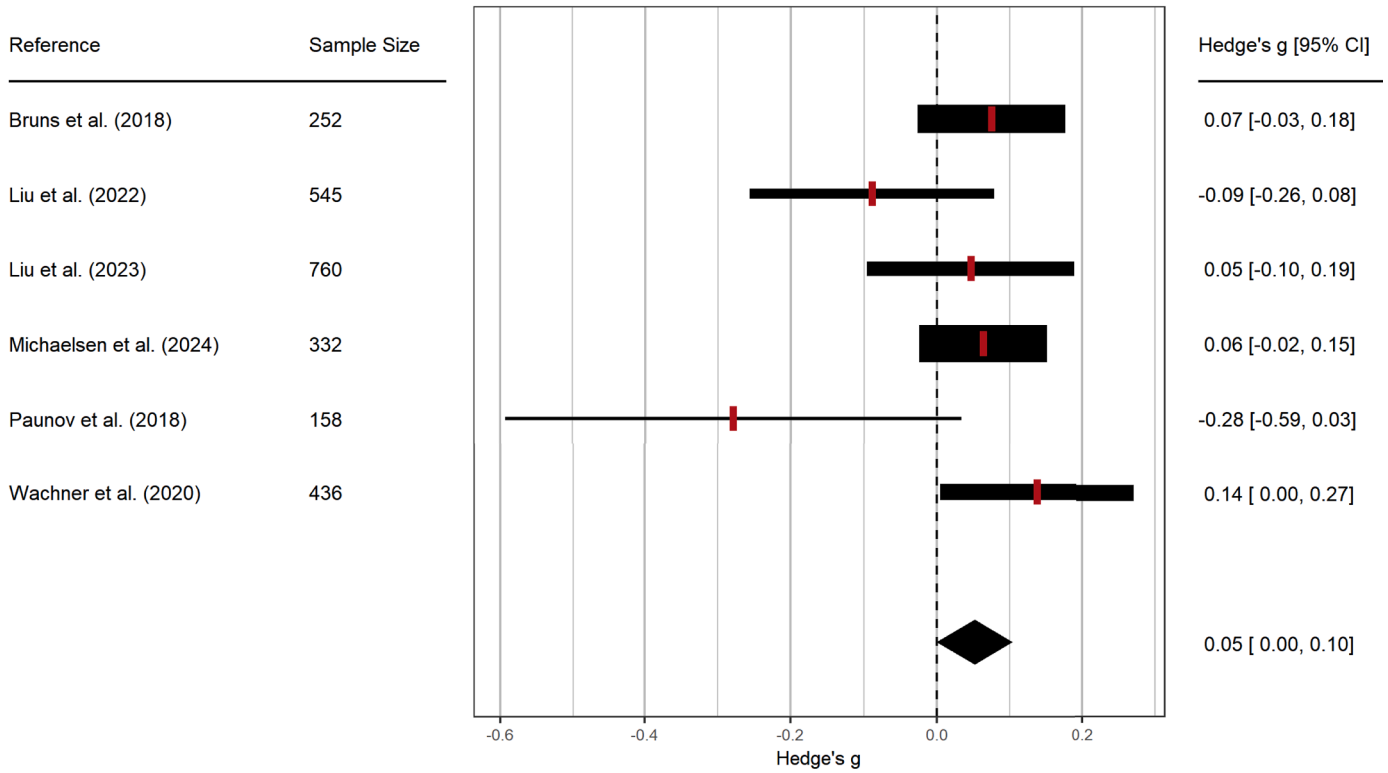


Fig. 3. Forest plots of studies containing a desirable (top; $n = 9$) and undesirable (bottom; $n = 6$) non-behavioural effect included in the meta-analysis with their corresponding 95 % confidence intervals.
Notes: Sorted alphabetically. The thickness of each line indicates the weight of the study in the meta-analysis. The three-level meta-analysis forest plot can be found in the appendix.

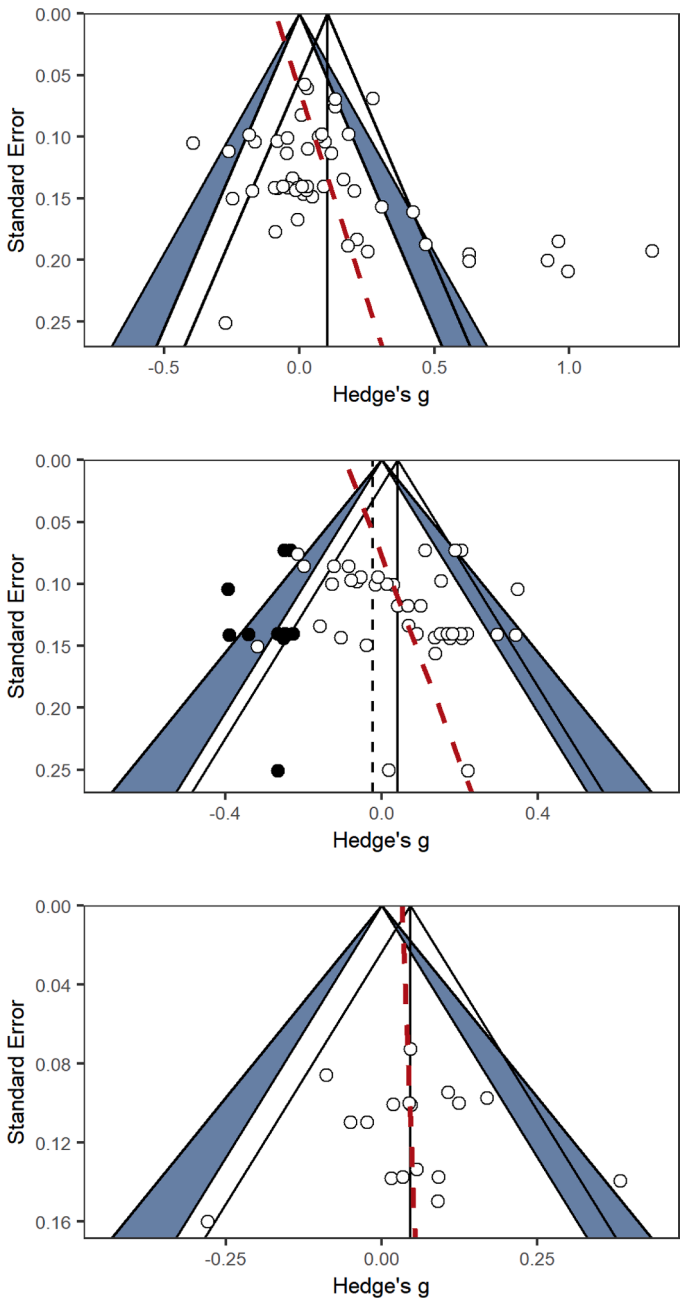


Fig. 4. Funnel plot displaying each observation as a function of its effect size and standard error for behavioural outcomes (top), desirable non-behavioural (middle) and undesirable (bottom) outcomes.
Notes: The dashed line represents the Egger test of asymmetry. A vertical line indicates no asymmetry, as for undesirable non-behavioural outcomes (right), whereas the line indicates an asymmetry favouring positive results with high standard errors for behavioural and desirable non-behavioural outcomes (left and middle). Black dots represent the added effects from the TRIM-and-FILL test.

Egger's test revealed asymmetric effects for behavioural (b = -0.30 [-0.53, -0.07] and desirable non-behavioural outcomes (b = *-0.23* [-0.37, -0.08]), while there is symmetry around an effect of 0 for undesirable non-behavioural outcomes (b = 0.03 [-0.20, 0.28]). Kendall's τ, measuring the correlation between the effect size and variance, was significant for behavioural outcomes (τ = 0.22, p = .002) and for desirable non-behavioural outcomes (τ = 0.24, p = .03), whereas it was nonsignificant for undesirable outcomes (τ = -0.04, p = .84). Having symmetry in the funnel plot is important, because it implies that the more precise the effect, the closer it is to the effect reported, and that there is no sign of bias driven by a "small-studies effect" (Schwarzer et al., 2015). This series of funnel plot analyses provides some evidence that our main transparency effects for the behavioural and desirable non-behavioural outcomes, but not the effect for the normatively undesirable outcome variables are subject to a small studies effect.

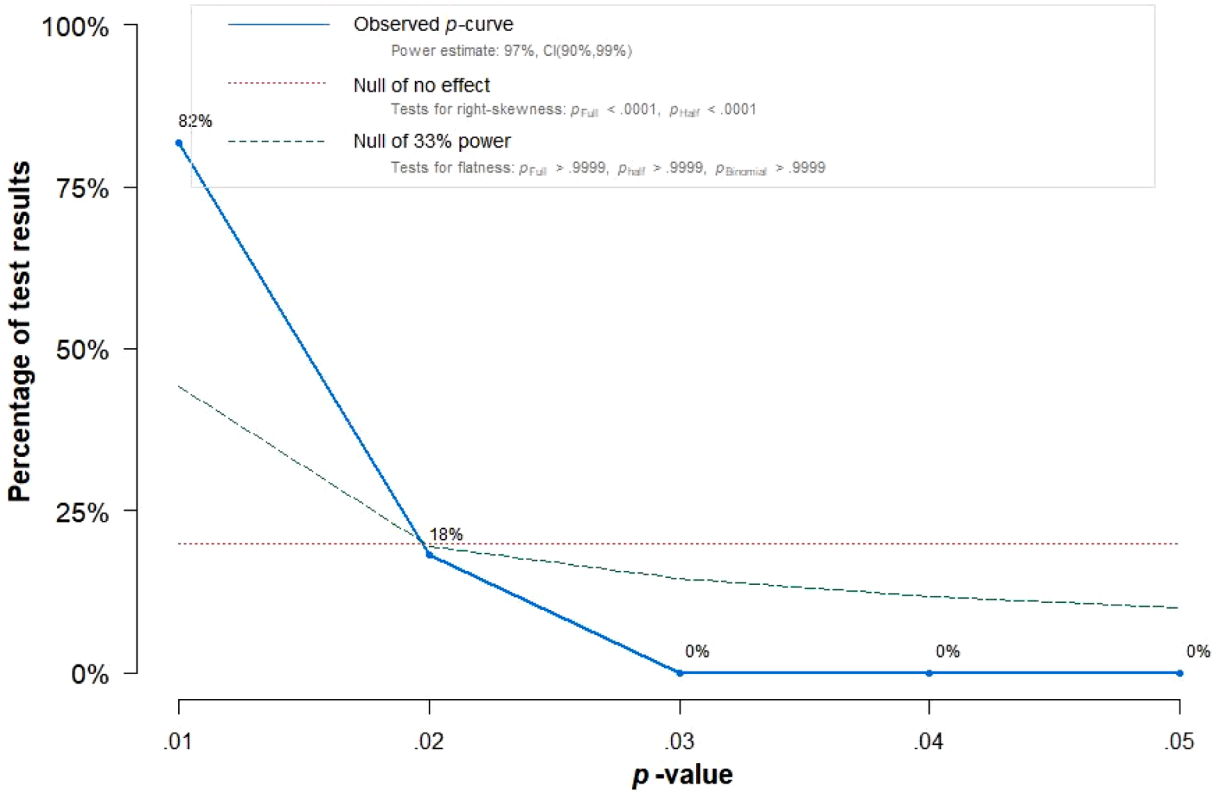
Statistical tests for publication bias indicated a bias-corrected effect size including the null, or a very small effect for both desirable and undesirable outcomes. A precision-effect test regressing effect size on its standard error indicated an effect size corrected for publication bias including zero for the behavioural (PET g = -0.09 [-0.27, 0.08]) and desirable non-behavioural outcomes (PET g = -0.09[-0.28, 0.09]). A precision-effect estimate with standard error (PEESE) regressing effect size on the square of the standard error was significant neither for the behavioural (PEESE g = -0.04 [-0.13, 0.06]) nor for the desirable non-behavioural outcomes (PEESE g = -0.02 [-0.11, 0.07]). For the undesirable non-behavioural outcomes, the PET and PEESE are consistent with the null effect found in the main analysis (PET g = 0.03 [-0.24, 0.31] and PEESE g = 0.05 [-0.10, 0.19].

A Three-Parameter Selection Model (3PSM) indicated that, for the behavioural outcomes, the adjusted model for publication bias performed better than the unadjusted model ($\chi^2(1) = 15.4$, $p < .001$). The adjusted model indicated a medium effect (3PSM g = 0.40 [0.16; 0.63]). This supports the notion that our meta-analytic result for behavioural outcomes exhibits a lack of significant studies and is biased by studies with very high effect sizes, compared to the mean effect size we identified. For desirable non-behavioural outcomes, the unadjusted model is preferred ($\chi^2(1) = 0.02$, $p = .90$). The unadjusted model indicated a non-significant effect (3PSM g = 0.04 [-0.01; 0.09]). This result is important because in a recent study, the 3PSM produced the better estimate under high heterogeneity (Carter et al., 2019). For the effects on undesirable non-behavioural outcomes, the unadjusted model is preferred ($\chi^2(1) < 0.01$, $p > .9$), with g = 0.05 [-0.01, 0.10].

We also conducted a p-curve analysis (Simonsohn et al., 2014), which did not indicate that false positives played an important role for the case of behavioural outcomes. However, we found some evidence for possible p-hacking for desirable non-behavioural outcomes (Fig. 5). 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 biased reporting. This is the case in our analysis for desirable non-behavioural outcomes. For the effect on undesirable outcomes, we could not conduct a p-curve analysis, as only one effect is significant.

We then investigated moderators to provide further nuance to the results. We present the moderator analysis for behavioural outcomes in Table 2 and the moderator analysis for desirable and undesirable non-behavioural outcomes in Table 3. For behavioural outcomes, the moderator variables associated with online studies, studies using a nudge modifying the decision structure, and studies in the domain "other", tended to exhibit significantly positive transparency effects. These three modalities are also the most studied in the literature. On the contrary, we found no significant moderation on desirable or undesirable non-behavioural outcomes. Overall, there were 55 effect sizes for behavioural and 62 effect sizes for non-behavioural outcomes (40 desirable and 17 undesirable), indicating that both were equally interesting to contributors in the literature.

We further note that most of the effects are from online studies (k = 102, 53 for behavioural, 38 for desirable non-behavioural, and 11 for undesirable non-behavioural outcomes). Moreover, only a few effects are from a laboratory setting (k = 13, 5 for behavioural, 2 for desirable and 6 for undesirable non-behavioural outcomes) and even less are from a field setting (k = 2, both for behavioural outcomes). In terms of domain, only two studies were conducted in the food domain, and they correspond to the two "field" studies, with an effect size of g = 0.02



Note: The observed *p*-curve includes 11 statistically significant ($p < .05$) results, of which 11 are $p < .025$.
There were 44 additional results entered but excluded from *p*-curve because they were $p > .05$.

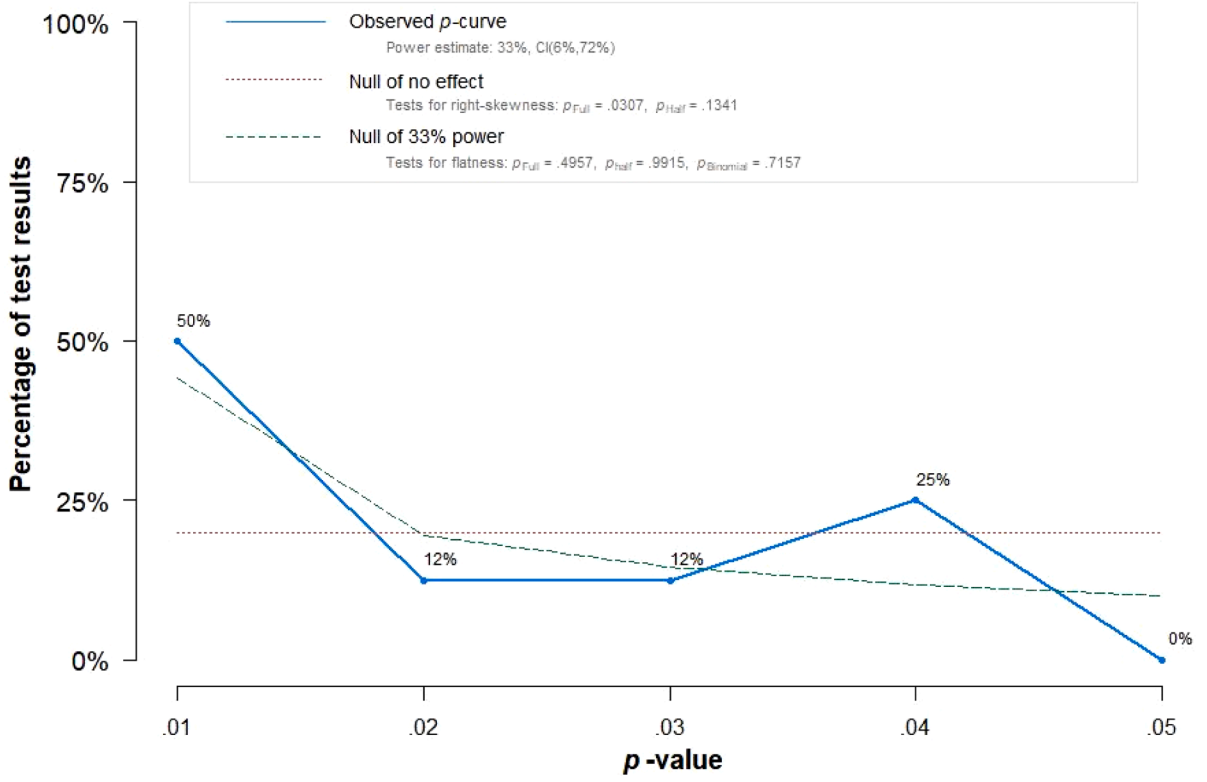


Fig. 5. P-curve analyses for effects on behavioural outcomes (top) and normatively desirable outcomes (bottom).
Notes: As there are no significant effects on undesirable outcomes, we cannot conduct a p-curve analysis.

Table 2
Moderator analysis for behavioural outcomes.

Effect	*k*	g	95 % CI	Test statistic	*P*
Type of Experiment				$F(1, 53) = 0.33$	0.72
Online	48	0.13	[0.01, 0.26]*		
Laboratory	5	0.01	[-0.12, 0.14]		
Intervention Category				$F(1, 53) = 0.002$	0.96
Decision Structure	45	0.16	[0.04, 0.28]*		
Decision Information	9	-0.06	[-0.19, 0.06]		
Domain				$F(1, 53) = 3.01$	0.08
Environment	15	0.01	[-0.06, 0.07]		
Health	8	0.05	[-0.18, 0.28]		
Other	24	0.23	[0.03, 0.43]*		
Pro-social	6	0.02	[-0.09, 0.13]		
Pre-registration				F(1, 53) = 1.21	0.27
Pre-registration	14	0.04	[-0.05, 0.13]		
No pre-registration	41	0.16	[-0.00, 0.32]		

Notes: Following Mertens et al. (2022)'s Taxonomy, we defined Decision Structure as the "alteration of the utility of choice options through their arrangement in the decision", Decision information as the "increase in the availability, comprehensibility, and/or personal relevance of information", and Decision Assistance as the "facilitation of self-regulation". More information is available in Table 1 of Mertens et al. (2022) manuscript. Variable categories with <3 observations were excluded from the table.

[-0.08, 0.12]. Twenty tests (17 %) out of the total number of tests were pre-registered, and while preregistration is not a significant moderator, we note that it is associated with a lower average effect size compared to non-preregistered studies. Finally, most of the studied effects belong to the "decision structure" category, the most promising explanatory category according to Mertens et al. (2022). This can be explained by the predominance of defaults in our data set (around 88 % of total effects), which are prime examples of interventions changing the decision structure. On the other hand, only one effect size concerns decision assistance, with an effect of g = -0.05 [-0.11, 0.01]

In the Appendix, we conducted a series of robustness checks based on the subset of our studies where a "no nudge" control is included. The goal was to assess whether transparency in nudging provides an additional benefit beyond the general effectiveness of nudges. To achieve this given our data, we contrasted the aggregate effect size of opaque nudges versus no nudge (50 effects) with the effect of transparent nudges versus no nudge (52 effects). We used a z-test to compare these two meta-analytic estimates and found no difference for behavioural outcomes ($z = 0.69$, $p = .76$), desirable non-behavioural outcomes ($z = 0.16$, $p = .56$) and undesirable non-behavioural outcomes ($z = 0.04$, $p = .52$). This suggests that, within the subset of studies using a control group without nudges, transparent nudges did not yield a statistically significant improvement over opaque nudges for any of the outcome variables under this alternative statistical approach. This suggests that the observed positive transparency effect in the main meta-analysis may be sensitive to the selection of studies or the analytical approach.

4. Discussion

Our meta-analysis reveals that the evidence does not support the claim expressed in Bovens (2009), namely that transparent nudges lead to worse outcomes than covert nudges. This provides some support to previously discussed informal reviews on the effects of transparency. We show that, irrespective of whether nudges work or not, the ethical defensibility of nudges can be improved – via transparency – without negative consequences. Our analysis reveals that transparency increases the effect of nudges on behavioural outcomes, but has no impact on desirable and undesirable non-behavioural outcomes. Hence, introducing transparency to nudging seems to benefit policy compliance, whereas no effect of transparency can be detected for non-behavioural constructs, such as behavioural intentions or policy acceptance.

Regarding moderators, studies conducted online, nudges manipulating the decision structure, and nudges applied in the domain "other" tended to exhibit significantly positive transparency effects for behavioural outcomes. No significant moderators were found for non-behavioural outcomes of any valence. Robustness checks considering whether nudges are effective in the first place provide insignificant transparency effects throughout, not corroborating the positive behavioural transparency effect.

Our analysis revealed major limitations of the current literature: the available evidence lacks diversity in types of nudges, research teams and origins, and investigated settings. Almost all included studies concern

Table 3
Moderator analysis for normatively desirable and undesirable non-behavioural outcomes.

	Desirable non-behavioural outcomes					*Undesirable non-behavioural outcomes*				
Effect	*k*	g	95 % CI	Test statistic	*P*	*k*	g	95 % CI	Test statistic	*P*
Type of Experiment				$F(1, 38) = 0.13$	0.72				F(1,15) = 0.10	0.76
Online	38	0.03	[-0.05, 0.12]			11	0.02	[-0.07, 0.12]		
						6	0.06	[-0.05, 0.16]		
Intervention Category				$F(1, 38) = 0.13$	0.71				F(16) = 16.62	0.41
Decision Structure	38	0.03	[-0.05, 0.12]			17	0.05	[-0.01, 0.10]		
Domain				$F(1, 38) = 0.33$	0.57				F(1,15) = 0.04	0.84
Environment	16	0.04	[-0.22, 0.30]			8	0.06	[-0.03, 0.15]		
Health	6	-0.07	[-0.28, 0.15]			1	-0.09	[-0.12, -0.06]		
Other	10	0.11	[-0.02, 0.25]			4	0.01	[-0.19, 0.21]		
Prosocial	8	-0.05	[-0.20, 0.10]			4	0.06	[-0.04, 0.16]		
Pre-registration				F(1, 38) = 0.03	0.85				F(16) = 16.62	0.41
Preregistration	6	0.03	[-0.20, 0.26]							
No preregistration	34	0.04	[-0.06, 0.15]			17	0.05	[-0.01, 0.10]		
Outcome subtype				F(1, 38) = 3.08	0.08				F(16) = 16.62	0.41
Nudge Reaction	9	0.00	[-0.14, 0.15]			17	0.05	[-0.01, 0.10]		
Nudge Perception	13	0.00	[-0.16, 0.17]							
Choice Satisfaction	6	-0.08	[-0.20, 0.05]							
Source Perception	9	0.13	[-0.01, 0.27]							
Intention	3	-0.05	[-0.27, 0.16]							

Notes: Following Mertens et al. (2022)'s Taxonomy, we defined Decision Structure as the alteration of the utility of choice options through their arrangement in the decision, Decision information as the increase in the availability, comprehensibility, and/or personal relevance of information, and Decision Assistance as the facilitation of self-regulation. Note that the reason for the test statistic F(16)=16.62 being identical for Intervention Category, Pre-registration, and Outcome sub-type is that there are 17 studies with undesirable non-behavioural effects, and none of them are preregistered and all belong to the decision structure category. Also, since they all have one experiment per study except Michaelsen et al. (2024), we conducted a two-level analysis for all of them except the domain where we have two different effects from Michaelsen et al. (2024). Variable categories with <3 observations were excluded from the table.

default nudges (17 out of the 23 studies employed defaults, accounting for 103 out of 117 effect sizes). The concentration of studies in narrow domains and categories significantly reduces the generalizability of the current literature in assessing transparency effects in other domains and categories. Hence, the effects of transparency need to be rigorously examined with other types of choice architecture, as well as in a wider variety of domains (see Mertens et al., 2022). Furthermore, the significance of the "other" domain effect may be associated with the inclusion of several studies investigating transparency disclosures that explicitly request to help the researchers, leading to large positive effects. This could amplify the observed transparency effect.

Moreover, the composition of the literature appears rather concentrated, with eight studies involving UK samples, three involving US samples, several countries with two studies each (Germany, the Netherlands, China, and Poland), and two countries with one study each (Belgium and Portugal). Considering the increasing role of mega-studies in improving the knowledge generated and hence the impact of applied behavioural science (Milkman, Gromet, et al., 2021; Milkman, Patel, et al., 2021), future studies should focus on how transparency works for nudging in cross-cultural settings or investigate which cultural factors influence the role of transparency. In addition, replications play a key role in building a solid body of evidence, and the results in the transparency literature need to be confirmed by replication studies, for future meta-analyses to be able to express greater confidence in the overall effect of transparency. Moreover, the number of studies that rigorously assess the phenomenon is relatively low, and a limited number of researchers and research teams participate in large fractions of the literature. Relatedly, while preregistration is increasingly becoming the norm, we also note that only seven of the included 23 studies preregistered their hypotheses. Hopefully, this number will increase for future larger-scale field experiments, where preregistrations are very common.

Moreover, almost all studies appear hypothetical or for small stakes, and only two of them involve field interventions with real consequences. While there may be risks in introducing transparency conditions in actual nudge policies, the data are promising enough to warrant the exploration of the effect of transparency in large-scale field experiments. Considering recent evidence suggesting smaller nudge effect sizes in policy settings compared to laboratory settings (DellaVigna & Linos, 2022), such investigations appear even more warranted. Implementing transparent disclosures in large interventions can also enhance the methodological rigor of the literature.

In broader terms, our finding that transparency effects depend on other variables, and are thus context-dependent aligns with recent calls to understand nudge moderators (Bryan et al., 2021). Our findings substantiate prior calls for further examination of the determinants of nudge effects (Marchiori et al., 2017), emphasizing that only a small number of nudge studies examine boundary conditions or underlying mechanisms of effectiveness. A lack of relevant evidence may render knowledge accumulation and theoretical development difficult, as demonstrated by several recent controversies over the effectiveness of nudging (Maier et al., 2022; Mertens et al., 2022; Szaszi et al., 2022). Hence, we strongly recommend that future research makes a deliberate effort to uncover more of the processes responsible for the effectiveness of different transparent nudges. One of these key conditions could be "nudgeability", as proposed by De Ridder et al. (2022, a concept expressing the degree to which individuals are susceptible to a nudge, based on their preexisting preferences for the behaviour targeted by the nudge). Moreover, future studies on transparency could elicit measures of trust in expertise or science, and test whether it moderates people's reaction to transparency.

Finally, we argue that different social structures (including the scientific community) might benefit from defining clearly what form of transparency is required to respect citizen's autonomy. A few taxonomies are already available: De Ridder et al. (2022) distinguished between two forms of transparency: transparency related to the source of the nudge (e.g., government or private parties) and transparency related to the behaviour induced by the nudge (simple behaviours vs. complex social issues). Bruns & Paunov (2021) proposed eight distinct types of transparency inferred from the related literature. Once ethically defensible transparency types are identified, the robustness of their effects can be examined further.

CRediT authorship contribution statement

Hendrik Bruns: Writing – review & editing, Validation, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Adrien Fillon:** Writing – review & editing, Writing – original draft, Visualization, Methodology, Investigation, Formal analysis. **Zacharias Maniadis:** Writing – review & editing, Writing – original draft, Validation, Methodology, Investigation. **Yavor Paunov:** Writing – review & editing, Validation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.socec.2025.102350.

Data availability

All materials, data, and code are available at https://osf.io/2u4ae/.

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