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DISCUSSION PAPER

// ARMENAK ANTINYAN AND ZAREH ASATRYAN

Nudging for Tax Compliance: A Meta-Analysis

Nudging for Tax Compliance: A Meta-Analysis

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Abstract

Governments increasingly use nudges to improve tax collection. We synthesize the growing literature that evaluates nudging experiments using meta-analytical methods. We find that simple reminders increase the probability of compliance by 2.7 percentage points relative to the baseline where about a quarter of taxpayers are compliant. Nudges that commonly refer to elements of tax morale increase compliance by another 1.4 percentage points. Deterrence nudges, which inform taxpayers about enforcement parameters, increase compliance the most, amounting to an additional 3.2 percentage points increase on top of reminders. Our additional findings highlight the conditions where nudges are more effective, such as their potential when targeting sub-population of late-payers, and also suggest that even this sample of randomized trials may be susceptible to selective reporting of results. Overall, our findings imply that taxpayers are biased by various informational and behavioral constraints, and that nudges can be of some help in overcoming these frictions.

JEL codes: C93, D91, H26.

Keywords: Tax compliance, Tax evasion, Randomized control trials, Nudging, Reminders, Tax morale, Deterrence, Meta-analysis, Publication selection bias.

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1 Introduction

Recent years have seen much excitement around the idea of using “nudges” with the aim of improving individual behavior. Nudges are interventions that respect freedom of choice and leave economic incentives intact ([Benartzi et al., 2017](#)), and they have been studied in many policy areas such as education ([Dizon-Ross, 2019](#)), healthcare ([Wisdom et al., 2010](#)), environment ([Costa and Kahn, 2013](#)), finance ([Handel, 2013](#)), savings decisions ([Blumenstock et al., 2018](#), [Karlan et al., 2016](#)), and welfare benefits ([Finkelstein and Notowidigdo, 2019](#), [Linos et al., 2022](#)), among others.

In the field of taxation too, nudging has become quite popular in the last decade. This holds both among academics, who strive to understand why people pay taxes, and among policy makers who often claim that the potential payoffs of nudges can be very large in terms of raised revenue. In tax experiments, nudges occasionally take the form of reminders similar to other contexts, and more often they are designed to appeal to either moral motives behind paying taxes or to deterrence reasons behind paying taxes such as threats of audits. In light of the growing number of studies in this field, our paper aims to present a quantitative review of the literature and to provide guidance for further (policy) interventions.

In particular, our meta-analysis attempts to give more systematic answers to questions such as: i) Are nudges effective in curbing tax evasion? ii) If so, by how much on average? iii) Which nudge types work more strongly? iv) Are nudges also effective over a longer time horizon? v) Which groups of taxpayers are more responsive to nudges? vi) Do nudges work only in specific settings (e.g., low-compliance environments) or more generally?

To answer these questions we collect data on intention to treat (ITT) estimates of nudging interventions on tax compliance from studies that implement randomized control trials (RCTs). Our largest sample consists of up to 73 RCTs that include 887 estimates, while our baseline sample – which focuses only on measurements of extensive margin tax compliance and what we think of main rather than all estimates within papers – consists of 53 papers and 270 estimates. Our analysis starts with a synthesis of this literature. This appraisal provides a taxonomy of nudging interventions in the field of tax compliance, in particular highlighting the main experimental designs used, the common types of nudges studied, the customary measures of tax compliance used and the important contextual characteristics that define nudging interventions. We then apply meta-analytical techniques to identify the quantitative impact of various types of nudges on tax compliance.

Our main results are threefold. First, our evidence suggests that reminders increase extensive margin compliance, i.e., the share of compliant taxpayers, by 2.7 percentage points compared to a control group of taxpayers not receiving any treatments. Second, we find that non-deterrence nudges, i.e., interventions commonly referring to elements of tax morale, increase extensive margin compliance by another 1.4 percentage points in addition to the reminder effect. Third, we show that deterrence nudges, that is interventions that inform taxpayers about potential audit probabilities and fine rates when caught cheating, increase compliance by an additional 3.2 percentage points on top of reminders.

To put these effects into perspective, we compare them to underlying levels of compliance, that is to the share of compliant taxpayers in the control group that received no communication. In the sample where this information is reported, only about 25% of taxpayers not receiving any nudges are compliant on average, which

is low but not surprisingly so, since more than half of estimates in our sample work with samples of taxpayers which were late in paying their taxes. Our estimates suggest that, compared to this underlying level of compliance, the reminder effect increases the probability of compliance by 10.8% on average, tax morale and other non-deterrence nudges raise compliance by 16.4%, and deterrence nudges are most effective, increasing tax compliance by 23.6%. Thus, in an average experiment, the most comprehensive of nudges, those sending reminders in combination with warning about deterrence, are able to increase the share of compliant taxpayers from 25% in the control group with no communication to about 31%. These headline numbers are based on a sample of 44 papers and 218 estimates which is a sub-sample of our baseline sample of 53 papers and 270 estimates but restricted to papers which report the underlying levels of compliance in the control group. The results are generally robust to these and other sample definitions as well as to alternative estimators and measures of tax compliance.

These results are consistent with the idea that taxpayers are biased by various informational and behavioral constraints, and that nudges can help overcome these frictions. Whereas reminders help overcome limited attention, tax morale and deterrence nudges operate by updating taxpayers' beliefs or preferences on the moral and deterrence motives behind paying taxes. As far as the stronger compliance effect of deterrence nudges relative to non-deterrence nudges is concerned, one interpretation is that individual financial motives are more important for compliance decisions than elements of tax morale. However, it is also plausible that nudges implemented by tax authorities are simply more effective at updating perceptions of audit probabilities than perceptions of the various tax morale elements. In terms of the types of tax morale nudges, we consider three main groups – nudges which highlight the importance of paying taxes for the adequate provision of public goods, those about the (positive)

behavior of the majority of taxpayers, and a third group hinting at general appeals of paying taxes as a moral obligation – and show that neither of them stands out to be as important driver of compliance as deterrence nudges.

Although we are tempted to make comparisons between our results and that of nudges in contexts going beyond tax compliance, such comparisons are not straightforward given the heterogeneity in the behavioral outcomes that nudges are used to target. [Benartzi et al. \(2017\)](#), [Hummel and Maedche \(2019\)](#), [Mertens et al. \(2022\)](#) provide meta-analyses of nudging interventions in various fields, albeit almost always neglecting the tax compliance studies. As suggested by these papers, reminders are one of the more popular nudges in the literature. Deterrence nudges, on the other hand, are used in more special cases such as in law enforcement related contexts. Finally, among the morale nudges, social norm nudges are the ones that are somewhat popular in other fields (for a review of the role of social norms in shaping attitudes and behaviors, see, [Bursztyn and Yang, 2022](#)). However, large heterogeneities in both the treatments and outcomes studied in these papers do not allow for meaningful comparisons of effect magnitudes across the different types of nudges. In addition, a better assessment of whether the effects of nudges that we have identified are small or large requires an understanding of the welfare impacts of nudges as well as an idea of how these effects relate to those of traditional policy tools. Despite the popular belief that the sending of nudges is essentially costless, several papers – such as, [Damgaard and Gravert \(2018\)](#) on reminders for charitable giving, [Huck and Rasul \(2010\)](#) on transaction costs again in the context of giving, [Allcott and Kessler \(2019\)](#) on social norms in energy savings, [Bernheim et al. \(2015\)](#) on default options for savings decisions, [Bhattacharya et al. \(2015\)](#) on commitment devices in health choices, among others – show that nudges may entail significant costs, and suggest that the failure to take these costs

into account will overstate the welfare effects of nudges. In addition, [List et al. \(2023\)](#) studies the welfare effects of policies that combine nudges with more traditional price instruments.

Our additional findings highlight certain design aspects of RCTs that may make nudging more or less effective for tax compliance. We find that nudges are more effective in the very short-run, when targeting late-payers, when communicated through in-person visits, and when implemented in higher income countries. Our final set of results focuses on publication bias. Consistent with [DellaVigna and Linos \(2022\)](#)¹ and [Brodeur et al. \(2020\)](#), we find evidence that the results of this literature, despite being identified through RCTs, are likely to be driven by selection effects both on the basis of statistical significance and also based on the sign of reported treatment effects.

The remainder of the paper is structured as follows. Section 2 presents a taxonomy of tax compliance nudges based on our synthesis of the literature. Section 3 describes the sample of papers and estimates that we collect. Section 4 discusses the meta-analysis framework that we use for estimation. Section 5 presents our main results, and Section 6 discusses our additional results on publication bias. Section 7 concludes with a summary and some suggestions for future research.

¹[DellaVigna and Linos \(2022\)](#) compare the impact of nudges in RCTs conducted by nudge units with those found in RCTs conducted and published by academics. The authors find the average impact of nudges to be 1.4 percentage points or 8% in the nudge unit trials, which is one-sixth the magnitude found in academic trials, and explain a large part of this difference to be driven by publication bias.

2 A taxonomy of tax compliance nudges

2.1 Definition of nudges

Unlike standard economic policy interventions, nudging interventions neither prohibit individuals from undertaking a certain action, nor they affect the economic incentives of these individuals (Sunstein, 2014, Thaler and Sunstein, 2008). Thaler and Sunstein (2008) define a nudge as an “aspect of the choice architecture that alters people’s behavior in a predictable way without forbidding any options or significantly changing their economic incentives”. They continue that for an intervention “to count as a mere nudge, the intervention must be easy and cheap to avoid”. Sunstein (2014) provides a list of popular nudges in various fields. In the context of taxation, experiments are usually conducted in collaboration with the tax authorities of a given jurisdiction. Not only academics, but also dedicated nudge units (e.g., Mind, Behavior, and Development Unit of the World Bank, the Behavioral Insights Team) conduct such experiments.

In a typical nudging experiment, the agents are randomized into one or several treatment arms, which are exposed to a nudge or nudges of various types, and a control arm in which the agents are either not exposed to any intervention or are exposed to a neutral intervention. The behavior of agents in the treatment group or groups is then compared with that of agents in the control group some time after the intervention. The exact measurement of taxpayer behavior, the length of the time horizon over which this behavior is studied as well as the delivery method of the nudge can vary across experiments. While some experiments may study one type of behavior (e.g., probability to pay) over one time horizon after sending the nudge using a certain delivery method (e.g., digital letters), others may study multiple types of behaviors (e.g., probability to pay and probability to file) measured over multiple time horizons (e.g., one month

and three months) of nudges delivered through multiple methods (e.g., digital letters and in-person visits), or some combination of these. On the other hand, the taxpayer type (e.g., individual or business), the specific tax (e.g., income tax, property tax or indirect tax) and the country are typically fixed in a given experiment.

2.2 Literature on nudging for tax compliance

The literature on tax compliance is centered around the questions of why taxpayers pay (or do not pay) taxes, and on the effectiveness of enforcement policies in enhancing tax compliance. These questions are of central importance in public economics as the level and nature of tax compliance may have implications for the efficiency and distributive effects of taxes (see, e.g., [Slemrod and Gillitzer, 2014](#)), and what they can say about the level of public good provision. Several excellent qualitative reviews have been written on this extensive literature. Reviews by [Andreoni et al. \(1998\)](#), [Slemrod and Yitzhaki \(2002\)](#) and [Slemrod \(2007\)](#) and, more recently, by [Slemrod \(2019\)](#) and [Alm \(2019\)](#) discuss the literature on the economics of tax compliance. More specifically, [Luttmer and Singhal \(2014\)](#), [Mascagni \(2018\)](#) and [Pomeranz and Vila-Belda \(2019\)](#) review the literatures on the roles of, respectively, tax morale, tax experiments and tax capacity in tax compliance.

We perform a systematic quantitative analysis of the literature on the impact of nudging interventions on tax compliance for the first time.² Unlike the recent qualitative reviews, we not only study the question of whether the nudging interventions

²We are aware of two other meta-studies of tax experiments by [Blackwell \(2007\)](#) and [Alm and Malézieux \(2020\)](#), but both study laboratory experiments while we focus on field work. [Blackwell \(2007\)](#) concludes that increasing the penalty rate, the marginal per capita return to the public good and the probability of audit lead to higher tax compliance, while the tax rate has no significant impact on tax compliance. Focusing on a larger set of papers, [Alm and Malézieux \(2020\)](#) illustrate that audit probability increases tax compliance on the extensive margin, while audit probability and the tax rate influence tax compliance negatively on the intensive margin.

are effective or not, but we also provide an estimate of the average effects of nudges on tax compliance. This is important, since, despite the relative similarity of these nudging experiments, summarize their results as having “produced varying results in different contexts”. We also aim to understand which nudge types work best in increasing compliance, and what are the important contextual characteristics that potentially matter for the effectiveness of these nudges.

The three most distinctive features of nudges in the existing literature are their potential to boost tax compliance, first, by referring to deterrence factors such as the threat of audit and fines, second, by using reminders, and third, by appealing to morale elements such as altruism and fairness.³ [Luttmer and Singhal \(2014\)](#) Below we discuss these three groups of nudges one by one.

2.3 Types of nudges

Deterrence nudges: Tax compliance may be driven by a cost-benefit calculation reflecting on the trade-off between higher retained income due to evasion and costs potentially incurred if caught evading. This is the essence of the so-called deterrence approach to tax compliance. The workhorse model dates back to [Allingham and Sandmo \(1972\)](#) and, following the economics of crime literature ([Becker, 1968](#)), articulates that taxpayers are rational utility maximizers who compare the benefits of tax evasion against the costs of detection and punishment when deciding to comply

³The nudges we study are arguably the most common types of behavioral interventions in taxation, but governments can nudge in other ways too. For example, policies that publicly recognize the top taxpayers and shame the tax delinquents, as studied by [Slemrod et al. \(2022\)](#) and [Dwenger and Treber \(2018\)](#), or ones that use third-party information reports to pre-fill tax returns, as studied by [Fochmann et al. \(2018\)](#), [Gillitzer and Skov \(2018\)](#), [Kotakorpi and Laamanen \(2016\)](#), might as well be considered as nudges in the broader sense of the word.

with taxes ([Alm, 2012](#)).⁴ This puts forth the fine rate and the audit probability as the two most important policy instruments for enforcing tax compliance ([Alm, 2019](#)). The idea behind deterrence nudges is then to refer to these deterrence factors with the aim of increasing tax compliance, of course without changing the audit probability or the fine rate. A large body of previous evidence, both from the field and the lab, has confirmed that audit and penalty rates do matter for compliance decisions (see, e.g., [Slemrod, 2019](#)). Thus, deterrence nudges can affect compliance by making the audit and penalty rates more salient to taxpayers, or by updating the magnitudes of already salient beliefs.

Consequently, to be considered as a deterrence nudge, the communication between tax administration and taxpayers should contain elements of enforcement. More specifically, the communication should include a threat that highlights the possibility of an audit or the potential penalty if caught evading (or both). A typical example of a deterrence nudge is the following one used by [Castro and Scartascini \(2015\)](#): “Did you know that if you do not pay the CVP on time for a debt of AR\$ 1,000 you will have to disburse AR\$ 268 in arrears at the end of the year and the Municipality can take administrative and legal action?”⁵

Reminder nudges: Tax compliance behavior may depend on the simple behavioral fallacy of limited attention. Limited attention may lead individuals to forget about the tax payment deadline and simple reminders can help them overcome this issue. [Antinyan et al. \(2021\)](#), [Hernandez et al. \(2017\)](#), [Mascagni et al. \(2017\)](#) discuss the

⁴Recent extensions of this theory include, among others, the possibility that agents are sometimes unable to cheat because of withholding and third-party reporting rules ([Kleven et al., 2011, 2016](#)), or that agents face substantial uncertainties with regards to the (perceived) probabilities of being caught ([Snow and Warren, 2005](#)).

⁵Note that communications including both deterrence and non-deterrence components are classified as a deterrence nudge, given the presence of the threat component.

role of reminders in the tax compliance context. Reminder nudges have also, of course, been studied in other policy areas, such as health (Altmann and Traxler, 2014), savings (Calzolari and Nardotto, 2017) and investment (Karlan et al., 2016) decisions.

The nudges in this sub-category are mainly utilized to “correct” taxpayer non-compliance that stems from limited attention. These nudges use neutral language to remind the taxpayers to comply with taxes, for example: “RRA would like to inform you that your CIT tax return is due by 31st of March 2016. For more information about the filing process and payment methods, contact the call centre (3004) or visit the RRA website (<http://www.rra.gov.rw>)” (Mascagni et al., 2017).

Tax morale nudges: A number of moral factors such as intrinsic motivation, social norms, altruism, reciprocity, and fairness, among others, may affect the tax compliance decision. “Tax morale” is an umbrella term that encompasses these factors. Individuals may be intrinsically motivated to pay taxes without any enforcement (Torgler, 2003), and feel shame or guilt in cases of tax evasion (Coricelli et al., 2010, Dulleck et al., 2016). Individuals may also be guided by concerns of reciprocity and comply with taxes if the state effectively provides public goods or treats the taxpayers fairly (Kirchler et al., 2008). The tax compliance behavior of the majority may create a prevailing social norm of compliance and suppress one’s decision to evade taxes. Altruistic concerns, such as improving the welfare of others, may also influence the compliance decision (Bosco and Mittone, 1997).

We distinguish between the following three types of tax morale related nudges: public goods, social norms and moral appeals. Public good nudges make it clear that the taxes paid by individuals are effectively used to finance public goods and services: “Your tax payment contributes to the funding of publicly financed services in education,

health and other important sectors of society” (Bott et al., 2020). Social norm nudges stress that the majority of individuals in a given country/community are complying with taxes: “Nine out of ten people pay their taxes on time” (Hallsworth et al., 2017).⁶ Moral appeal nudges aim to appeal to morality, fairness, and altruism to influence taxpayer behavior: “If the taxpayers did not contribute their share, our commune with its 6226 inhabitants would suffer greatly. With your taxes you help keep Trimbach attractive for its inhabitants” (Torgler, 2004).

Other nudges: The sub-category of other nudges includes communications that are relatively rare and are not coherent in the type of content they introduce. Studies introduce distinct types of information content such as sentences on tax-deductible donations (Biddle et al., 2018), instructions on how to file returns (Eerola et al., 2019), various other textual and visual communications (De Neve et al., 2021, Schächtele et al., 2023), among others.

2.4 Tax compliance measures

Extensive margin compliance: Our main dependent variable of interest is the extensive margin of tax compliance. This captures whether a taxpayer is compliant with taxes or not, and is measured with binary outcome variables. More specifically, the studies measure the probability of taxpayers in paying, filing or reporting their taxes,⁷ and they may also distinguish between whether the compliance was full or partial. Following DellaVigna and Linos (2022), we focus on extensive margin of compliance as

⁶As such, our social norm label refers to the descriptive social norm that depicts what most people in a group (or in a society) usually do.

⁷Few studies consider other compliance measures such as whether the taxpayers registered for TV tax, revised the submitted report, or made agreements to pay these taxes.

our main measure for three main reasons. First, this is the most popular measure of compliance used in the literature we study. Second, the binary nature of the outcome variable allows us to measure the impacts of nudges with a common metric, which is the percentage point difference in the outcome relative to the control group. Third, for a meaningful interpretation of the magnitudes of effects, we not only need to measure the effect of the treatment versus the control group, but we also want to have a metric for the level of compliance in the baseline against which these effects can be judged. This metric, labelled as the underlying compliance level, is based on the extensive margin concept and informs us about the share of compliant taxpayers in the control group at the end of the intervention.

Other measures of compliance: Tax compliance can also be measured by focusing on the intensive margin of compliance, which measures the extent or the intensity of compliance. The intensive margin is, however, typically context-dependent and the effect magnitudes are generally not comparable across studies. What is possible instead is to compare the direction and statistical significance of these effects by collecting data on the t-values of treatment effect estimates. The main benefit of this variable is that t-values are available and comparable for all outcomes studied in the literature, including outcome measures at both intensive and extensive margins. We follow other applications of meta-analytical techniques in economics, such as those by [Card et al. \(2010, 2017\)](#), and, in a robustness exercise, study t-values.

2.5 Treatment effect estimates

Experimental designs: The literature uses two main experimental designs to identify the treatment effects of nudges on tax compliance. These design differences have

to do with how the control group is defined. One approach does not treat the taxpayers in the control group in any way, i.e., the taxpayers in this group do not receive any communication from the authority. Another approach always treats the taxpayers in the control group with reminder letters. These two experimental designs are shown in columns (2) and (3) of Table 1, respectively, where we provide a stylized summary of the framework we are in.

Experiments with no communication sent to the control group: The first design typically, but not necessarily, will have a treatment arm that sends reminder letters as shown in column (2) of Table 1. The comparison of this treatment to the control group of taxpayers not receiving any communication will be informative about the reminder effect, r . In these studies, non-deterrence and deterrence nudges are again compared to the control group that did not receive any communication. Therefore, these comparisons lead to treatment effect estimates that also capture the reminder effect, i.e., one treatment effect estimate for reminder and non-deterrence nudges, $r + n$, and another treatment effect estimate for reminder and deterrence nudges, $r + d$.

Experiments with reminders sent to the control group: Studies using the second experimental design always send reminder letters to the control group of taxpayers, as shown in column (3) of Table 1. These studies are not informative about the effects of reminders. Since studies following this design compare non-deterrence and deterrence nudges to the control group that receives reminders, they will be informative about the effects of non-deterrence and deterrence nudges net of the reminder effects, that is n and d , respectively.

Table 1: Stylized summary of the framework

(1)	(2)	(3)	(4)	(5)
Treatment nudge	Treatment effect estimates		Post-treatment compliance levels	
	Control group:		In addition to c	In relation to c
	No letter	Reminder	(% of population)	(% change)
Reminder	r	—	$c + r$	$(r)/c$
(Reminder &) Non-deterrence	$r + n$	n	$c + r + n$	$(r + n)/c$
(Reminder &) Deterrence	$r + d$	d	$c + r + d$	$(r + d)/c$

The parameters r, n, d of columns 2 and 3 represent the treatment effect estimates collected from the underlying studies. The c of columns 4 and 5 stands for the underlying compliance level, that is the share of compliant taxpayers in the control group at the end of the intervention, as discussed in Section 2.4.

Effect magnitudes: To measure the magnitudes of effects, the literature typically reports underlying compliance levels, c , that is the share of compliant taxpayers in the control group at the end of the intervention. We collect this data and calculate the reminder, r , reminder and non-deterrence, $r + n$, and reminder and deterrence, $r + d$, effects compared to the underlying compliance levels. As shown in columns (4) and (5) of Table 1, we calculate this post-treatment compliance levels both in terms of level and relative terms.

2.6 Study characteristics

Basic characteristics: The two basic characteristics that all experiments have are: i) the type of nudges, as defined in Section 2.3, in the most general specification classifying nudges into deterrence or non-deterrence types; and ii) the experimental design, in particular the composition of the control group against which nudges are evaluated as defined in Section 2.5, that is whether the control group received a reminder letter or did not receive any communication.

Additional characteristics: We identify seven additional characteristics as being the defining features of nudging RCTs as follows: iii) late payer sample, i.e., whether the taxpayer is identified as being late in paying her taxes by the official deadline or not; iv) the response horizon of the compliance measure, which we define as a binary variable capturing whether the time interval between the date when the nudge was sent and the date when the outcome variable was measured is shorter or longer than 2 months; v) the year and publication status of the study, i.e., a working paper or a published article; vi) the delivery method used by the tax authority to reach out to the taxpayers, i.e., digital letters, physical letters, or in-person visits; vii) the type of tax being studied, i.e., personal income tax, corporate income tax, property tax, VAT, or other taxes;⁸ viii) the taxpayer type in the sample, i.e., individuals, businesses or a sample mixing both individuals and businesses; and, finally, ix) the income level of the country where the experiment was conducted, i.e., low-, middle- or high-income country.

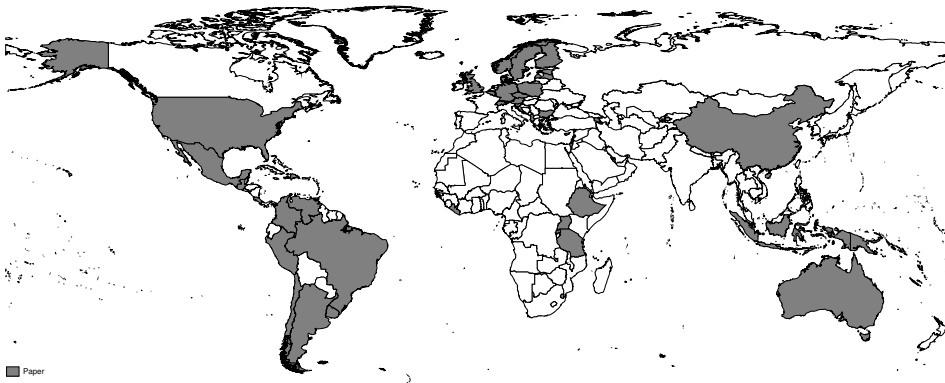
3 Sample of studies and estimates

3.1 Sample of studies

Literature search: We ran a literature search on a rolling basis throughout 2019 to 2023. First we searched for relevant papers using a defined combination of keywords in the main literature databases of the profession. Second, to identify ongoing work, we continued our search in the programs of the main general-interest conferences in economics as well as the main conferences specializing in behavioral or experimental

⁸Other taxes include country-specific taxes or fees, e.g., church tax in Germany, wealth tax in Colombia, TV license fees in Austria, etc.

Figure 1: Country coverage of nudging experiments



Notes: The studies where these experiments come from are listed in Table [A1](#).

economics and public economics.⁹ Third, we carefully looked through the bibliographic information in the papers identified in the last two steps to further refine the study sample. Fourth, we also considered papers sent to us directly by scholars working in the field. In October 2023, we re-visited all of the working papers identified earlier to check their publication status.

Study inclusion criteria: For a paper to be included in our sample, all of the following four criteria need to be fulfilled: i) the study is based on a RCT performed at the level of taxpayers (i.e., individuals or firms rather than, e.g., regions); ii) the trial introduces a nudging intervention which closely follows the definition of [Thaler and Sunstein \(2008\)](#); iii) the dependent variable of interest is the tax compliance behavior of the taxpayer (either the extensive or the intensive margin or both); and iv) the resulting study reports all of the relevant statistics necessary for our meta-analysis (e.g., effect sizes along with the standard errors) for at least one treatment effect estimate.

⁹The keywords include: randomized controlled trial, RCT, field experiment, nudging, nudges, behavioral intervention, tax evasion, tax compliance, and tax non-compliance. The literature databases include: Econlit, Google Scholar, and Science Direct. The conferences include ones organized by: AEA, EEA, ESA, SABE, WEAI, NTA, and IIPF.

After applying the four filters to the list of papers collected from our extensive search we arrive at an overall sample of 73 studies. These studies are listed in alphabetical order in Table A1. As presented in the map of Figure 1, these experiments were performed in around 40 countries situated mainly in Europe and the Americas, and fewer of them in the developing countries of Africa and Asia.

3.2 Sample of estimates

Treatment effect estimate inclusion criteria: After having defined the sample of studies, we need to decide which treatment effect estimates to collect from these studies. One approach would be to select all estimates that authors report. However, the studies included in our sample differ starkly in the number of estimates, and this approach would run the risk that papers reporting very many estimates, for example from multiple robustness tests, would drive our results. Therefore, we decided to perform our baseline analysis on the “main” estimates reported in studies, while using the full (i.e., main and non-main) sample of treatment effect estimates for robustness checks.¹⁰

We apply the following seven rules when collecting the estimates from studies and when defining which of these represent the main estimates. First, both in the main and in the full samples, we only consider those estimates that compare the effect of a nudging intervention to the average compliance in the control group. Thus, we do not consider those estimates that compare the effect of nudging interventions across different treatments. Second, as main estimates, we collect treatment effects utilizing

¹⁰The only exception is that we use the full sample of treatment effect estimates in Section 6. Since in this section we are interested in the potential selection effects in reported estimates, we choose to use the full sample of estimates that papers report, rather than the main sample of estimates which is chosen by us.

the full sample of taxpayers, while those focusing on certain sub-samples, such as for purposes of heterogeneity analysis, are classified among non-main estimates. Third, we consider ITT as the main estimate.¹¹ Fourth, when studies report results from specifications with (possibly several sets of different) control variables or none at all, we consider as main estimates the latter specifications that do not include any control variables. Fifth, we restrict the main estimates to include only the effects measured in a time horizon of up to around 12 months after the intervention. Sixth, when studies report that their estimates are contaminated by other enforcement activities by tax authorities, we exclude them from both the main and the non-main samples if the time horizon is less than 12 months,¹² and include them in the non-main sample when the horizon is 12 months or longer. Seventh, if studies report effects measured over many time horizon after the intervention, we select three of these effects as our main estimate by taking the effects measured at the shortest, the longest and at the middle time horizon.

Dimensions of main estimates within papers: After applying the filters defined above, we arrive at a sample of 55 studies that contain main estimates measured at the extensive margin of compliance. Table A2 reports the number of observations with which each study contributes to the sample, along with several other important study-level characteristics. The number of estimates per study ranges between 1 estimate

¹¹Studies in our sample either report ITT estimates only, or both ITT and ToT estimates. ITT estimates include every subject that is randomized according to randomized treatment assignment, disregarding non-compliance, protocol deviations, withdrawal, and anything that happens after randomization (Gupta, 2011). ToT estimates present the treatment effect on the group of taxpayers who received the communication from the tax administration, using the treatment assignment as an instrumental variable (IV) for the actual treatment (Mascagni, 2018). The only exception is Mogollón et al. (2021), where the experiment was stopped and failed to treat everyone it intended to. For this specific paper ToT is counted as the main estimate.

¹²For example, 48-day and 70-day estimates in Hallsworth et al. (2017) are contaminated by external letters sent by tax authorities.

Table 2: Summary statistics

	(1)		(2)	
		Extensive margin		T-values
Dependent Variable				
Treatment Effect (mean)	270	0.035	475	3.230
Control Group				
Reminder vs No Letter	270	123	475	188
Nudge Type				
Deterrence	270	126	475	228
Public Good	270	39	475	78
Social Norm	270	40	475	52
Moral Appeal	270	16	475	43
Other	270	49	475	74
Late-Payers				
General Sample (vs Late-Payers)	270	115	475	255
Response Horizon				
Short Run	270	138	475	226
Publication Status				
Published	270	171	475	278
Year of Publication (mean)	270	2015	475	2015
Delivery				
Letter	270	175	475	289
Digital	270	81	475	161
In Person	270	14	475	25
Tax Type				
Income Tax	270	116	475	198
Corporate Tax	270	16	475	31
Property Tax	270	76	475	105
VAT	270	11	475	45
Other	270	33	475	72
Multiple	270	18	475	24
Taxpayer Type				
Individual	270	169	475	244
Business	270	65	475	162
Individual and Business	270	36	475	69
Development Level				
Low Income	270	27	475	46
Middle Income	270	108	475	195
High Income	270	135	475	234

Summary statistics show the total number of observations, and the number of observations satisfying the respective criteria. For the dependent variable, its mean values are shown.

to a maximum of 28 estimates per study, and they are determined by the following three dimensions. First, as discussed in Section 2.4, studies may use several outcome variables when measuring tax compliance at the extensive margin. In particular, they can report up to four compliance measures – probability to pay, file, report or other as represented in column “compliance measure” of Table A2 – also distinguishing whether the compliance was full or partial as shown in the column “full compliance”. Second, studies will typically have more than one treatment arm in their experimental design. This is primarily driven by the types of nudges, as discussed in Section 2.3, which can be up to five in a given study as shown in the column “number of nudge types”. The treatment arm can also vary due to the method of delivering the nudge, which can be up to three – physical letter (L), digital letter (D) or in-person visit (P) – as shown in the column “delivery method”. Third, studies will also tend to report the effects of nudges measured at different time horizons. As explained above, we restrict the baseline sample to not more than three time horizons. These horizons are shown in months for every study in column “time horizons”.¹³

Thus, the largest possible number of estimates per paper is given by interacting all possibilities provided by these three dimensions. In practice, however, the studies will naturally use much fewer and also different combinations of these dimensions. Taking the example of Chirico et al. (2019), the study that contributes with the most number of observations to our sample, it has 28 main estimates as shown in Table A2. These treatment effect estimates are collected from Table 2 of the study, and are available for two outcome variables (full or partial compliance), seven treatment effects that

¹³Time horizons are approximated for several studies since the exact timing of nudge dispatch and treatment effect measurement are not discussed.

include five nudge types, and for effects measured over two time horizons (1 month, 3 months after intervention).

Baseline and other samples of estimates: To conclude, we have 270 treatment effect estimates in our baseline sample coming from 53 studies. These are the compliance effects measured at the extensive margin and represent only the estimates that we consider to be the main ones. Note that this baseline sample only includes the treatment effects of type n , $r + n$, d and $r + d$ of Table 1, and leaves out the effects of reminder nudges, r . Column 1 of Table A1 presents the list of studies in this sample along with the number of estimates coming from each study. The summary statistics for this baseline sample are presented in Table 2.

The robustness analysis of Section 5.3 is performed on three further samples. First, for the robustness analysis of the reminder effect we consider again the reminder nudges, r . Two papers only send reminder nudges leading to the sample increasing to 55 papers, but since we focus here only on the experiments that do not treat the control group with any communication this sample consists of 32 studies and 172 estimates. Column "reminder nudges" of Table A2 shows the studies where these treatment effects r come from. Second, when considering compliance effects measured by their t-values which contain estimates not only of extensive margin but also of intensive margin compliance, our sample increases to 67 papers and 475 estimates. These studies are shown in column (2) of Table A1. Third, when considering both main and non-main estimates, the sample of extensive margin estimates increases to 535 estimates from 54 papers, and the sample of t-values increases to 887 estimates from up to 73 papers. These are presented in columns (3) and (4) of Table A1. The publication bias analysis of Section 6 employs this largest sample.

4 Empirical design

Baseline specification: We estimate the following equation:

$$ComplianceEffect_{i,p} = \alpha Reminder_p + \beta Deterrence_{i,p} + \gamma NonDeterrence_{i,p} + \epsilon_{i,p} \quad (1)$$

Dependent variable: $ComplianceEffect_{i,p}$ is the i^{th} estimate of extensive margin treatment effect from paper p . The distribution of this variable averaged by study is plotted in Figure 2. In all of our analyses, we winsorize the dependent variable at its 5th and 95th percentiles.¹⁴ In additional results described in Section 5.3, we run this specification taking the t-values of all treatment effects as an alternative dependent variable.¹⁵

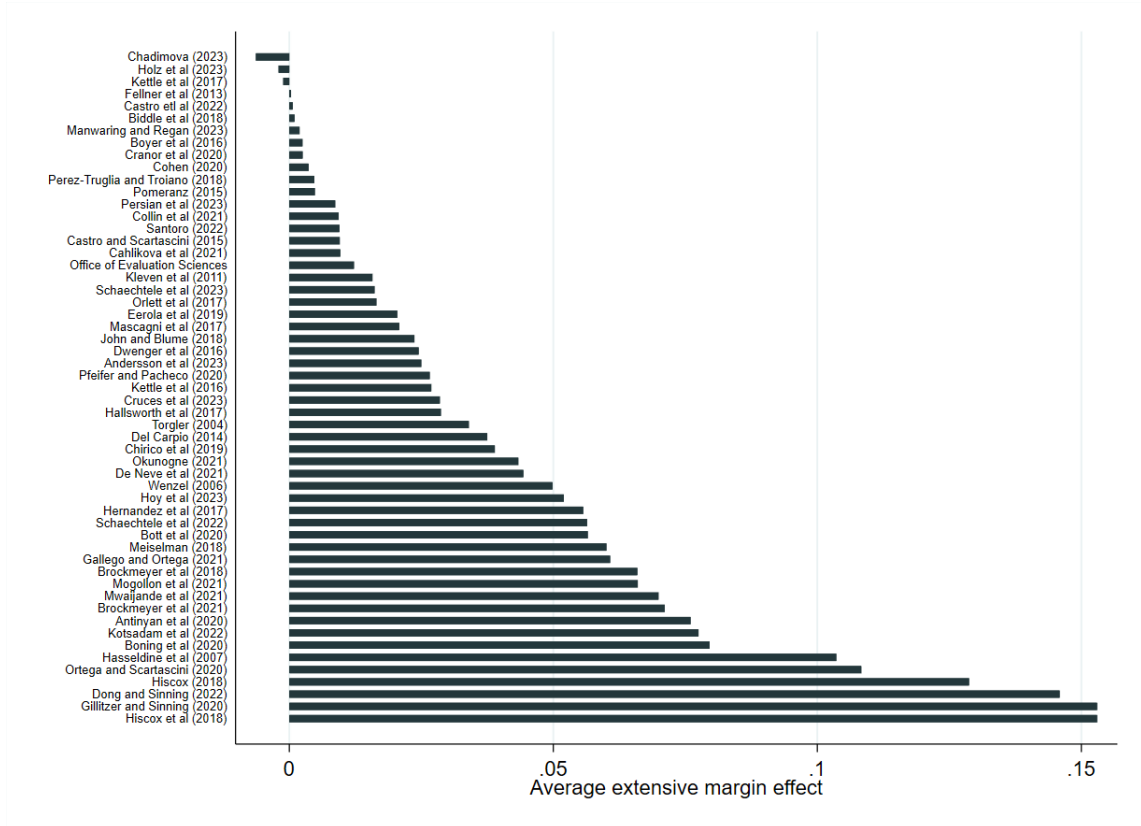
Independent variables: $Reminder_p$ is a binary variable capturing whether, in a given experiment, the control group received a reminder letter or no communication at all, as defined in Section 2.5. Our sample has about the equal number of these two types of estimates. Consequently, $\hat{\alpha}$ compares the treatment effect estimates across the two experimental designs fixing for the type of the nudge that was sent.¹⁶ Since we do not have studies that utilize both types of experiments, the reminder effect is

¹⁴This winsorization strategy is not uncommon in the literature, and is motivated by the possibility of having errors in the data as well as with the aim of reducing the effect of outliers on the average estimates. For example, Card et al. (2017) winsorize the data at the 10th and 90th percentiles. Table 6 provides a robustness exercise by running our baseline specifications on non-winsorized data.

¹⁵In instances where the standard errors of the estimates are either not reported or are reported as rounded to 0, we take advantage of the reported significance levels of these estimates (e.g., indicated by stars in the regression tables) and replace the missing values with the most conservative t-values that pass the respective significance threshold. When analyzing publication bias, values derived in such a way are excluded in order to avoid artificial bunching at critical significance thresholds.

¹⁶If a non-deterrence nudge was sent $\hat{\alpha} = r + n - n = r$, and if a deterrence nudge was sent $\hat{\alpha} = r + d - d = r$.

Figure 2: Distribution of average treatment effect estimates across studies



Notes: The figure depicts the average treatment effect estimate on extensive margin compliance among main estimates by paper in ascending order of average effect magnitude.

only identified from across study variation. In Section 5.3, we present an alternative estimator for the reminder effect which uses within study variation. However, this is possible to do only using the sample of experiments that have a control group receiving no communication and a send a reminder nudge that is compared to that control group.

$Deterrence_{i,p}$ is again a binary variable equal to one if a nudge is of deterrence type and equal to zero if a nudge is of non-deterrence type. We have about as many deterrence as non-deterrence nudges in the sample. Consequently, $\hat{\beta}$ represents the average treatment effect of a deterrence nudge when the respective control group receives a reminder letter, thus identifying the effect d . Since most studies in our

sample send both deterrence and non-deterrence type nudges, we are able to, in Section 5.3, provide an alternative estimator for the deterrence effect identified from within study variation using study fixed effects.

Conversely, $\hat{\gamma}$ captures the average effect of non-deterrence type nudges compared to a control group of taxpayers receiving reminders, that is the effect n . As with the reminder and deterrence effects, we provide further evidence on these non-deterrence nudges in Section 5.3, in particular by dividing them into more detailed types of tax morale nudges.

Error term: $\epsilon_{i,p}$ is the error term. Since the compliance effect estimates may not be independent within studies, we cluster the error term at the level of studies p .

5 Results

5.1 Baseline

Table 3 shows the estimation results of Equation 1. Column (1) runs the regression for the baseline sample of 270 estimates coming from 53 papers, and column (2) repeats the analysis for the sub-sample of this where the underlying compliance level, c , is available leading to a reduced sample of 218 estimates coming from 44 papers. First, regarding the reminder effect, $\hat{\alpha}$ suggests a 2.6 percentage point difference between the treatment effects of the experiments sending no communication to the control group and those sending reminder letters to the control group. Second, $\hat{\beta}$ suggests that deterrence nudges increase the probability of compliance by 3.1 percentage points compared to the control group receiving reminder letters. And third, $\hat{\gamma}$ suggests that non-deterrence nudges increase the probability of compliance by 1.2 percentage points

compared to the control group receiving reminder letters. All of these effects are statistically distinguishable from zero at least at the 5% level. Post-estimation tests across the three parameters suggest that neither deterrence nor non-deterrence effects are statistically distinguishable from the reminder effect, but the deterrence effect is distinguishable from the non-deterrence effect. The results from the reduced sample where the underlying compliance levels are observed are plotted in column (2). These are very similar to those from the baseline sample of column (1). We present our headline calculations on the post-treatment compliance levels using the estimates from this latter sample.

Table 3: Baseline results

	(1) Baseline sample	(2) Reduced baseline (where c is observed)
Reminder $\hat{\alpha}$	0.026*** (0.009)	0.027*** (0.010)
Non-Deterrence $\hat{\gamma}$	0.012** (0.005)	0.014** (0.006)
Deterrence $\hat{\beta}$	0.031*** (0.007)	0.032*** (0.008)
Observations	270	218
Adjusted R^2	0.496	0.507
Papers	53	44
Postestimation p-values:		
Deterrence = Non-Deterrence	0.012	0.026
Deterrence = Reminder	0.702	0.745
Reminder = Non-Deterrence	0.273	0.349

Regressions are estimated according to Equation 1. Column (2) restricts the baseline sample to observations where the underlying compliance level, c , is observed.

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Effect magnitudes

To understand the magnitudes of the effects identified so far, we compare them to underlying levels of compliance in our sample, c . The average share of compliant taxpayers in the sample which have not received any communication is 25%. As discussed earlier this number is low and also, with a standard deviation of 21.8, masks quite some heterogeneity. However, this is not surprising, since more than half of RCTs in our sample work with samples of late-payers where compliance is close to zero. Another reason is that about one-third of RCTs in our sample were conducted in low- and middle-income countries, where baseline compliance rates can be quite low.

Table 4 summarizes the discussion on the magnitudes of our effects, building on the structure of Table 1. Column (2) shows how the estimates we identify map into the treatment effect estimates of the underlying studies. Column (3) shows the three treatment effects using the parameters estimated in Table 3: Reminder effect, r , is identified by $\hat{\alpha}$, the effect of reminder and non-deterrence nudges, $r + n$, is identified by $\hat{\alpha} + \hat{\gamma}$, and the effect of reminder and deterrence nudges, $r + d$, is identified by $\hat{\alpha} + \hat{\beta}$. Columns (4) and (5) then show the post-treatment compliance levels in addition and in relation to the underlying compliance level. Compared to the share of compliant taxpayers of 25%, reminders shift the compliance level to 27.7% of the population or increase compliance by 10.8% relative to the underlying average, while reminders combined with non-deterrence and deterrence nudges increase compliance to, respectively, the levels of 29.1% and 30.9% of the population or by 16.4% and 23.6% relative to the baseline.

Table 4: Effect magnitudes

(1)	(2)	(3)	(4)	(5)
Treatment effect	Estimator	Estimated effects Table 3 col. 2 (percentage points)	Post-treatment compliance levels In addition to c (% of population)	In relation to c (% change)
Reminder	$r = \hat{\alpha}$	=2.7p.p	27.7%	10.8%
Rem. & Non-det.	$r + n = \hat{\alpha} + \hat{\gamma}$	=4.1p.p	29.1%	16.4%
Rem. & Deterrence	$r + d = \hat{\alpha} + \hat{\beta}$	=5.9p.p	30.9%	23.6%

Parameters r, n, d of column 2 represent the reminder, non-deterrence and deterrence effects, as discussed in Table 1. The effects, $\hat{\alpha}$, $\hat{\beta}$ and $\hat{\gamma}$ are estimated according to Equation 1 in Table 3 (column 2). Columns (4) and (5) show the post-treatment compliance levels in addition and in relation to the underlying compliance level, c .

5.3 Robustness tests

Reminder effect: In our baseline approach we identify the reminder effect using variation across papers. An alternative strategy is to focus on the sub-sample of experiments that do not treat the control group with any communication, include the estimates on the effects of reminder nudges r in the data and compare their effects to the control group of untreated taxpayers. The specification is as follows:

$$ComplianceEffect_{i,p} = \alpha_1 ReminderNudge_{i,p} + \beta_1 Deterrence_{i,p} + \gamma_1 NonDeterrence_{i,p} + \epsilon_{i,p}^1 \quad (2)$$

where $\hat{\alpha}_1$, $\hat{\gamma}_1$ and $\hat{\beta}_1$ respectively identify the effects of reminder r , non-deterrence $r + n$ and deterrence $r + d$ nudges compared to the control group that receives no communication. Column (1) of Table 5 runs Equation 2 on this sub-sample of experiments. The finding of a 2.2 percentage point effect of reminders is similar to the baseline results on the effects of reminders presented in Table 3.¹⁷

¹⁷The deterrence and non-deterrence effects are likewise similar to the baseline results. Note that $\hat{\beta}_1$ should be compared to $\hat{\alpha} + \hat{\beta}$ and $\hat{\gamma}_1$ to $\hat{\alpha} + \hat{\gamma}$.

Table 5: Robustness tests for reminder, deterrence and tax morale nudges

	(1) Reminder	(2) Deterrence	(3) Non-Deterrence
Reminder $\hat{\alpha}_1$	0.022*** (0.004)		
Non-Deterrence $\hat{\gamma}_1$	0.033*** (0.008)		
Deterrence $\hat{\beta}_1$	0.061*** (0.009)		
Det-vs-NonDet $\hat{\beta}_2$		0.024*** (0.006)	
Non-Deterrence Effect $\hat{\gamma}^\kappa$ of type:			
Public Good			-0.029*** (0.007)
Social Norm			-0.023*** (0.006)
Moral Appeal			-0.028*** (0.007)
Other			-0.019*** (0.006)
Paper FE	No	Yes	Yes
Observations	172	270	270
Adjusted R^2	0.534	0.647	0.645
Papers	32	53	53
Postestimation p-values:			
Public Good = Social Norm			0.175
Public Good = Moral Appeal			0.922
Social Norm = Moral Appeal			0.294

Regressions of columns (1), (2) and (3) are estimated according to Equations 2, 3 and 4.

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Deterrence effect: Almost all of the studies in our sample implement several nudge interventions. Therefore, we can exploit the substantial within-study variation in the data and, as a robustness exercise, study the effects of deterrence nudges compared to that of non-deterrence nudges on tax compliance within studies. The specification is as follows:

$$ComplianceEffect_{i,p} = \beta_2 Deterrence - vs - NonDeterrence_{i,p} + \lambda_p + \epsilon_{i,p}^2 \quad (3)$$

where λ_p is a new term capturing the study fixed effects. $\hat{\beta}_2$ is the main coefficient of interest which identifies the effect of deterrence nudges on tax compliance compared to that of non-deterrence nudges. Column (2) of Table 5 suggests that deterrence nudges increase the probability of compliance by 2.4 percentage points compared to the effects of sending non-deterrence nudges controlling for study fixed effects. This estimate is consistent with the baseline results of Table 3 where the deterrence effect compared to the non-deterrence, i.e. $\hat{\beta} - \hat{\gamma}$, was 1.9 percentage points and statistically different from zero.

Tax morale effects: Instead of grouping nudges into the general deterrence and non-deterrence categories, we unpack non-deterrence nudges according to the detailed elements of tax morale that they target. Following our definitions of Section 2.3, we divide non-deterrence nudges into public good, social norm, moral appeal and other nudges. As shown in Table 2 public good, social norm and moral appeal nudges are about as numerous as deterrence nudges. Additionally, a tenth of the nudges are grouped into the category of other nudges.¹⁸ Since the exact definitions of these

¹⁸Several interventions mix different types of non-deterrence nudges. Our analysis classifies such instances in the following hierarchical order: deterrence, moral appeal, social norm, and public good.

detailed types of nudges can vary substantially across studies, we run this specification using study fixed effects as introduced in Equation 3. The variable *NonDeterrence – vs – Deterrence* now equals zero for deterrence nudges and serves as the omitted category, and it has separate categories for the four types of detailed non-deterrence nudges identified by γ_2^κ .

$$ComplianceEffect_{i,p} = \gamma_2^\kappa \sum_{\kappa=1}^4 NonDeterrence-vs-Deterrence_{i,p}^\kappa + \lambda_p + \epsilon_{i,p}^2 \quad (4)$$

Column (3) of Table 5 presents the estimation results. They suggest that the individual effects of public good, moral appeal and social norm nudges are smaller than those of deterrence nudges. The magnitudes of these three effects are tested for equality in the bottom of the table. These post-estimation tests do not find robust differences across the three types of non-deterrence nudges. Overall, this evidence suggests that not only the average tax morale nudge but also its three sub-categories – public good, social norm and moral appeal nudges – do not stand out as being as important drivers of tax compliance as deterrence nudges.

Alternative estimators: Our baseline specification is estimated using an ordinary least squares (OLS) estimator. In columns (1) and (2) of Table 6, we follow a number of recent applications of meta-analytical techniques in economics (see, e.g., [Card et al., 2010](#), [2017](#), [Heinemann et al., 2018](#), [Lichter et al., 2015](#), [Neisser, 2021](#)) and the literature reviewing these methods (see, e.g., [Stanley, 2001](#), [Stanley and Doucouliagos, 2012](#)), and show the robustness of the OLS results to those using weighted least squares (WLS) and random effects estimators. We use a WLS estimator since meta-analytical

That is, if, for example, a message contains both a moral appeal and a public good nudge, we classify it as a moral appeal nudge.

regressions are known to be heteroskedastic.¹⁹ As analytical weights we take the inverse of the squared standard error of the parameter estimates, which, unlike the OLS approach, yields to precision-weighted estimates. We also adopt a random effects model. This estimator assumes the existence of a distribution of true effects for distinct studies and populations. Thus, we relax the assumption that for each nudge type there exists a single “true” effect which is common to all studies under consideration. In general, the results are similar to the baseline findings.²⁰

Table 6: Robustness to alternative estimators, samples and compliance measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Estimator		Sample definition			Compliance measure			Main &
	WLS (s.e.)	Ran- dom effects	Non- winsor- ized	Non- nega- tive	<90th %tile	T- value	Pr. to pay	Pr. to file	non-main esti- mates
Reminder $\hat{\alpha}$	0.017* (0.009)	0.023* (0.013)	0.029*** (0.010)	0.020** (0.009)	0.030** (0.012)	1.451 (0.934)	0.029** (0.012)	0.016 (0.026)	0.031*** (0.011)
Non-Det $\hat{\gamma}$	0.010* (0.005)	0.015 (0.010)	0.012** (0.006)	0.021*** (0.005)	0.016** (0.007)	1.528** (0.690)	0.015** (0.007)	0.013 (0.021)	0.010 (0.006)
Det $\hat{\beta}$	0.024*** (0.007)	0.036*** (0.012)	0.030*** (0.008)	0.040*** (0.007)	0.029*** (0.009)	3.247*** (0.725)	0.035*** (0.010)	0.036 (0.025)	0.029*** (0.009)
Obs.	257	257	270	228	185	475	157	58	535
R^2	0.379		0.455	0.565	0.505	0.457	0.579	0.428	0.500
Papers	52	52	53	53	47	67	31	12	54

Regressions are estimated according to Equation 1. Column 1 estimates a weighted least squares model, where the inverse of squared standard errors are the weights. Column 2 estimates a Random Effects model. Column 3 uses non-winsorized data as the outcome variable. Column 4 excludes negative values from the outcome variable. Column 5 excludes estimates from studies that present 12 (90th percentile) or more estimates. Column 6 uses t-values instead of extensive margin treatment effect estimates as the outcome variable. Column 7 and 8 focus on the sub-sample where the outcome variable is either the probability to pay or the probability to file. Column 9 includes non-main estimates in addition to main ones. Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sample definitions: We present four additional tests for the sensitivity of our results to the choice of the sample. First, in column (3) of Table 6, we replicate the baseline results using non-winsorized data. Second, in column (4) Table 6, we test whether

¹⁹One form of heteroskedasticity arises because the variance in the individual estimates is negatively related to the size of the underlying sample and this correlation is likely to be different between the primary studies.

²⁰Columns 1 and 2 of Table 6 have fewer observations than in the baseline because for some estimates in our data the standard errors are missing.

the effects we have identified are driven by the few negative treatment effects in our sample. Third, in column (5) of Table 6, we drop papers which present extraordinarily many treatment effects estimates to test whether a few papers that have unusually many estimates drive our results. To that end, six papers are excluded that have more than 12 estimates which is the 90th percentile in the distribution of the number of estimates. Fourth, in Figure B1 we implement jackknife-type robustness tests by excluding papers one-by-one from the sample. Overall, these tests suggest that for all of our three effects of interest, the confidence intervals estimated in the robustness exercises always include the average estimate of the baseline approach.

Tax compliance measures: We perform two exercises. First, we study a larger sample of estimates that use heterogeneous measurements of compliance, and, second, we narrow down the measurement of extensive margin compliance to two more specific definitions. As discussed earlier, the magnitudes of the estimates that measure compliance at the intensive margin cannot be compared to each other due to the heterogeneity in measurements. However, we can compare their direction and statistical significance by using the t-values of treatment effect estimates as our dependent variable of interest. The main benefit of this exercise is that t-values are available for all estimates increasing our baseline sample from 270 to 475 estimates. This approach is not uncommon in the field of meta-analysis, and is used by many applications in economics (Card et al., 2010, 2017), sometimes even as their primary outcome variable of interest (Baskaran et al., 2016, Heinemann et al., 2018).

The results presented in column (6) of Table 6 suggest that the direction of all three main effects, that is of reminder, deterrence and non-deterrence nudges, are robust in this larger sample. Columns (7) and (8) of Table 6 then narrow down

the measurement of extensive margin compliance to two more specific definitions of compliance: probability to pay, which make about 60% of our data, and probability to file, which makes another about 20% of the data. Although we lose statistical power in the latter and reduced sample, the estimates are not dissimilar to the baseline findings.

Main and non-main estimates: Our baseline results use the sample of main treatment effect estimates, and here we extend the analysis to using the full sample of treatment effect estimates. The sample increases from 270 to 535 treatment effect estimates. The results from running the baseline regressions on this sample are presented in column (9) of Table 6. Overall, these results are not different from the baseline findings of Table 3.

5.4 Study characteristics

We are interested in the role of a number of important study characteristics in explaining the heterogeneity in the estimates of the compliance effect $ComplianceEffect_{i,p}$. To do so, we introduce vector $\mathbf{X}_{i,p}$ of these study characteristics to Equation 1 on top of the baseline regressors.

$$ComplianceEffect_{i,p} = \alpha Reminder_p + \beta Deterrence_{i,p} + \gamma NonDeterrence_{i,p} + \delta \mathbf{X}_{i,p} + \epsilon_{i,p} \quad (5)$$

The study characteristics enter the regression first one-by-one and then jointly.²¹ These seven characteristics are defined in Section 2.6. Table 2 shows that in our sample more

²¹This strategy follows DellaVigna and Linos (2022) for example, and is motivated by the fact that the inclusion of all characteristics is too demanding of a specification given our data such that, due to multi-collinearity of regressors, we are likely to be left with little variation to exploit.

than half of estimates deal with samples of taxpayers who were late in paying their taxes; that about half of the responses are measured in the short time horizon of less than two months after treatment; that about two-third of the estimates come from papers that have been published already; that the mean study year is 2015; that most of the communication takes place through either physical or digital letters as opposed to through few in person visits; that estimates are most likely to come from experiments undertaken with personal income taxpayers but substantial part also coming from property taxpayers, and fewer corporate income and VAT taxpayers; that about two-third of our sample interventions target individuals; and that half of estimates come from experiments conducted in high-income countries and fewer come from middle-income and especially low-income countries.

The regressions are presented in Table 7. The basic characteristics are always included. Reassuringly, we find that the point estimates on these characteristics, that is $\hat{\alpha}$, $\hat{\beta}$ and $\hat{\gamma}$, remain robust to the inclusion of the additional study characteristics as control variables. Taken together, these study characteristics can explain an additional 14 percentage points of the heterogeneity in the treatment effect estimates of nudges.²²

Our findings are as follows. First, nudges are more effective when addressing sub-samples of taxpayers who missed their deadline for paying taxes. The magnitude of the effect is 2.7 percentage points. Second, the treatment effects are stronger in the short-run, that is within a horizon of two months after the intervention, compared to effects measured two months after the intervention. Third, we do not find evidence for significant differences between working papers and published papers, and nor by the year of the study. Fourth, nudges communicated by in person visits to taxpayers

²²This refers to the difference in adjusted R^2 in regressions without and with the additional study characteristics. Adjusted R^2 in Table 3, column (1) is 49.6% and in Table 7, column (8) it is 63.2%.

Table 7: Role of study characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reminder $\hat{\alpha}$	0.028*** (0.008)	0.029*** (0.009)	0.026*** (0.008)	0.027*** (0.009)	0.025*** (0.008)	0.023*** (0.009)	0.030*** (0.009)	0.027*** (0.008)
Non-Deterrence $\hat{\gamma}$	0.024*** (0.007)	0.016** (0.006)	0.012* (0.006)	0.012* (0.006)	0.045 (0.029)	0.012** (0.005)	0.019*** (0.006)	0.033 (0.029)
Deterrence $\hat{\beta}$	0.040*** (0.008)	0.038*** (0.008)	0.031*** (0.007)	0.029*** (0.008)	0.066** (0.029)	0.031*** (0.008)	0.041*** (0.009)	0.052* (0.029)
Late-Payers (omitted: Late-Payer Sample)								
General Sample	-0.027*** (0.008)							-0.017** (0.008)
Response Horizon (omitted: Short Run)								
Long Run		-0.014* (0.009)						-0.012 (0.007)
Publication Status (omitted: Published)								
Unpublished			0.001 (0.010)					0.004 (0.009)
Year			-0.002 (0.001)					-0.001 (0.001)
Delivery (omitted: Physical Letter)								
Digital Letter				-0.004 (0.009)				0.018* (0.009)
In Person				0.026* (0.015)				0.057*** (0.012)
Tax Type (omitted: VAT)								
Income Tax					-0.032 (0.031)			-0.008 (0.027)
Corporate Tax					-0.064** (0.030)			-0.035 (0.026)
Property Tax					-0.034 (0.030)			-0.005 (0.029)
Other					-0.035 (0.031)			-0.020 (0.027)
Multiple					-0.017 (0.041)			-0.010 (0.029)
Taxpayer Type (omitted: Individual)								
Business						0.012 (0.012)		0.024** (0.011)
Individual and Business						-0.008 (0.011)		-0.003 (0.010)
Development Level (omitted: High Income)								
Low Income							-0.023* (0.013)	-0.043*** (0.015)
Middle Income							-0.019* (0.010)	-0.022*** (0.008)
Observations	270	270	270	270	270	270	270	270
Adjusted R^2	0.553	0.510	0.508	0.505	0.527	0.505	0.524	0.632
Papers	53	53	53	53	53	53	53	53

Regressions are estimated according to Equation 5.

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

relative to nudges delivered through physical letters have 2.6 percentage point larger effects on tax compliance. Fifth, we study whether the effects differ across tax types and, relatedly, across individuals or businesses. We do not find robust evidence for such differences. Sixth, and finally, when comparing experimental results across countries where the RCTs were conducted, we find that experiments seem to be less effective in low- and middle-income countries compared to high-income countries. We note that these findings remain suggestive since our inference is correlational and is often based on small samples. Future work may try to use experiments to study the role of these characteristics more directly. This can be done by designing interventions that go beyond short horizons, tax bases and country borders, among other dimensions.

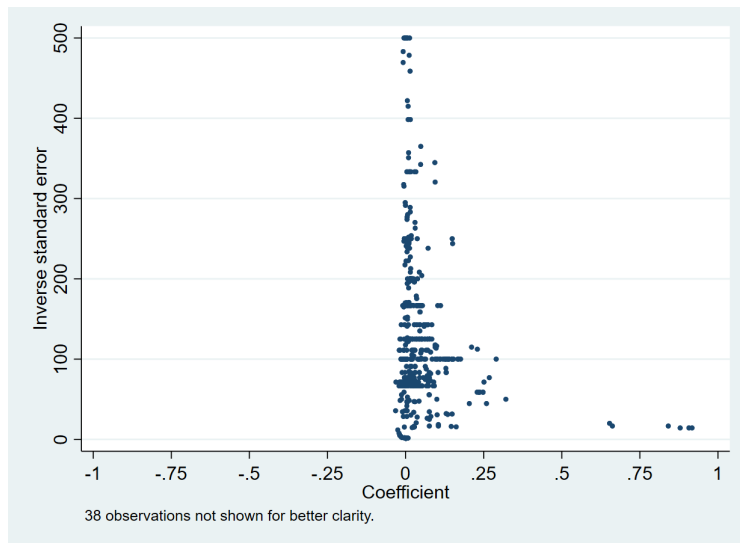
6 Publication selection bias

We study whether treatment effects reported by the studies in our sample are systematically selected towards having the “right” sign and towards being more statistically significant. The two underlying hypotheses are that researchers tend to present results that show: i) positive effects because it is generally expected, either according to theory or due to conventional beliefs, that nudges should only have positive effects (sign bias), and ii) statistically significant effects because of a predisposition to treat significant results more favorably, for example, due to the belief that non-significant effects are harder to publish (p-hacking).

6.1 Sign bias

We use a funnel plot to provide visual checks for asymmetries in the relationship between treatment effect directions and magnitudes on the one hand, and measures

Figure 3: Funnel plot



Notes: For visual clarity, the figure drops outliers larger than 500 on the y-axis, arriving at a sample of 495 estimates.

of their precision on the other hand. The idea is that, absent publication bias, very imprecise estimates should be randomly distributed around zero rather than being skewed to one direction, resembling an inverted funnel. In our particular case, the hypothesis is that the sign bias will lead to imprecise estimates being skewed to the right, that is towards positive treatment effect estimates. We present a funnel plot in Figure 3 where the x-axis plots the size of the treatment effect at the extensive margin and the y-axis plots the inverse standard error of the treatment effect as a measure of precision. We observe that the imprecisely estimated treatment effects, i.e., those at the bottom of the funnel plot, tend to be skewed towards positive values. This visual evidence provides suggestive evidence for the presence of sign bias in our sample.

More formally, we use the method of Egger et al. (1997) to test for funnel-plot asymmetry. Table B1 regresses the normalized coefficient, i.e., the point estimate divided by the standard error, on a measure of its precision, i.e., the inverse of the

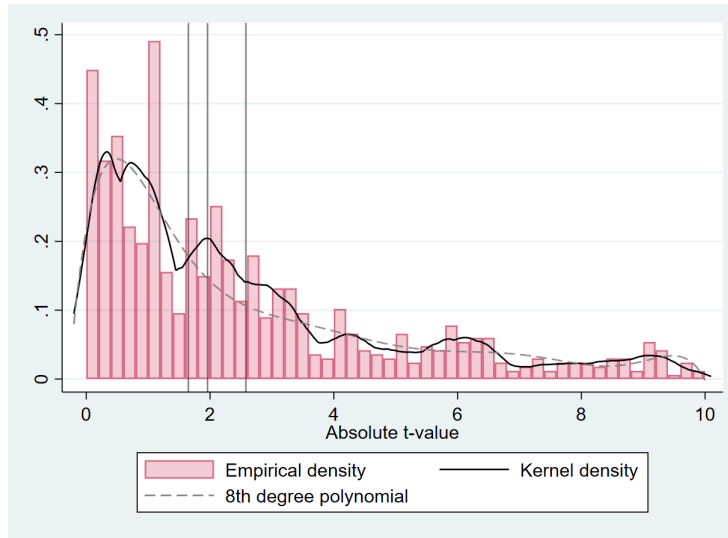
standard errors, and an intercept. The coefficient of interest is the intercept, which provides a measure of asymmetry. If there is asymmetry, with smaller studies showing effects that differ systematically from larger studies, the intercept will be different from zero. Our results reject the null hypothesis that there is no publication bias of this type. This evidence suggests that the published estimates on the treatment effects of nudges are systematically selected towards showing the “right” sign, that is towards showing effects that increase tax compliance and ignoring those that decrease tax compliance.

6.2 P-hacking bias

We now turn to the study of p-hacking, and check for unusual patterns in the distribution of t-values around their critical values. [Brodeur et al. \(2016\)](#) use a large dataset comprising of tests published in top economics journals, and show a disproportionately large share of tests that narrowly reject the null hypothesis. We follow this approach and plot the distribution of absolute t-values in Figure 4. This first visual evidence suggests some bunching in the number of observations of t-values situated just to the right of the three critical values (which are denoted by vertical lines).

More formally, we follow [Brodeur et al. \(2020\)](#) and implement three exercises. First, in Figure 4 we estimate a counterfactual distribution by fitting a polynomial function to data that drops t-values in the region around the three critical values, and contrasting this to the kernel density estimated on the whole distribution. This exercise shows visually the presence of excess mass in the density around the area of the critical values. Second, to statistically confirm the visually observable discontinuities, we use the randomization test, and check for discontinuities in the probability of a t-value appearing above or below the critical values. The idea is that, absent p-hacking, the

Figure 4: Distribution of absolute t-values



Notes: This figure plots a histogram of absolute t-values for treatment effect estimates in 0.2 wide bins. The kernel density line is estimated according to an Epanechnikov function with bandwidth 0.2. The 8th degree polynomial is estimated for the counterfactual case by dropping absolute t-values in the [1.5, 3] range. For visual clarity, the figure drops outliers outside the (-10, 10) range and includes 676 observations. Vertical lines denote critical values for two-sided significance tests at t-values of 1.645, 1.96 and 2.58.

probability of being just above versus just below any threshold should be equal. Panel A of Table B2 performs this test using the data of t-values centered around the three significance thresholds and several local bandwidths going from 0.075 to 0.1575 (the largest value before we cross the next closest critical value). The test shows that there are discontinuities in the distribution of t-values, with over 60% of observations in these bandwidths being skewed towards showing statistically significant effects. In columns (4) to (6) of Table B2, we implement a version of this test that studies several bandwidths around the 5% significance threshold rather than centering the data around the three thresholds. We find similar results. This evidence suggests that the studies in our sample choose to report results that are statistically significant at conventional levels, and ignore reporting treatment effect estimates that narrowly fail to reject the

null hypothesis. Third and finally, we use the caliper tests again following [Brodeur et al. \(2020\)](#), to study whether p-hacking is more likely to take place in certain sub-samples. We consider the type of the nudge and the experimental design, and also compare published papers versus working papers, new versus old papers (split at the median study year in our sample) and main estimates versus other estimates. The coefficients shown in Panel B of Table [B2](#) represent increases in the probability of finding a statistically significant effect relative to the baseline category. We do not find robust evidence for large differences in p-hacking patterns across any of these dimensions.

Overall, our evidence suggests that our sample is biased due to both sign as well as p-hacking type biases. We find it unlikely that the existing biases can explain the difference in the effects of reminder and deterrence nudges that we document. This analysis suggests that empirical studies implementing RCTs, which are otherwise believed to have relatively sound methodologies, may not be immune to biased reporting of results.

7 Conclusions

Policy interventions that nudge taxpayers with the aim of increasing compliance have become a popular tool among many governments owing to their ease of implementation and low monetary costs. This easy adoption of the policy is demonstrated, for example, by [Hjort et al. \(2021\)](#), who inform randomly selected Brazilian mayors about research on the positive tax compliance effects of reminder letters and find that the treated municipalities are more likely to implement nudging interventions. However, little is

known about the effectiveness of nudges beyond the evidence presented in individual experiments carried out in different contexts.

In this paper we quantitatively summarize the knowledge accumulated from tax nudging interventions in a systematic way. We estimate the average effects of reminder, tax morale, and deterrence nudges on tax compliance. We find positive effects of all these nudges and provide precise numbers on their magnitudes. These estimates may help policymakers form more realistic expectations about the impact of nudges rather than having to rely on the outcomes of individual studies. Our evidence on the particular design features of interventions that make them more or less effective can provide further guidance for potentially more effective policy interventions in the future.

This review highlights a number of opportunities for researchers by directing attention towards gaps in the literature where the evidence has been weak so far. Few papers test whether nudges work in the longer run, and when implemented repeatedly. Although it is plausible that nudges shift decisions from the future to the present, we are not aware of studies that identify such potential inter-temporal (crowding) effects of nudges. Having better measurements about taxpayers' priors, perhaps by borrowing techniques from the literature on survey experiments (for a review, see, [Fuster and Zafar, 2022](#)), would help understand the mechanism through which nudges operate more exactly. We are also not aware of studies that try to measure and then take into account the costs of nudging in the tax compliance context. Although the direct or implementation related costs are probably negligible, the indirect costs of nudges – such as the annoyance costs of reminders, the psychological costs of tax morale nudges, or the potentially risk-aversion inducing effects of “intimidating” deterrence nudges – can be substantial, thereby degrading the positive effects of nudges from

a welfare perspective. Finally, we do not have much knowledge of how interventions interact with the context they operate in. This is not surprising given that randomized control trials tend to narrowly focus on local environments where the context is fixed. Cross-study comparisons such as the one adopted in this paper, on the other hand, are limited owing to methodological concerns when comparing different experiments. Future interventions, possibly ones that span borders or institutional environments, could study whether, for example, tax morale nudges work more effectively in contexts with higher levels of trust, and whether deterrence nudges work better in uncorrupted environments where audits can be enforced more credibly than in institutionally less mature environments.

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Online Appendix

Appendix A: Sample of studies

Table A1: Samples of studies

No.	Paper	Country	(1)	(2)	(3)	(4)
			Main estimates Extensive	T-value	All estimates Extensive	T-value
1	Andersson et al. (2023)	Sweden	5	5	10	10
2	Antinyan and Asatryan (2020)	Armenia	0	0	3	9
3	Antinyan et al. (2021)	China	0	0	0	0
4	Appelgren (2008)	Sweden	0	0	0	0
5	Ariel (2012)	Israel	0	8	0	8
6	Bergolo et al. (2023)	Uruguay	0	6	0	9
7	Biddle et al. (2018)	Australia	3	6	3	6
8	Blumenthal et al. (2001)	USA	0	4	0	4
9	Boning et al. (2020)	USA	2	6	8	24
10	Bott et al. (2020)	Norway	3	6	12	24
11	Boyer et al. (2016)	Germany	2	4	2	4
12	Brockmeyer et al. (2019)	Costa Rica	12	20	24	32
13	Brockmeyer et al. (2021)	Mexico	2	4	4	8
14	Cahlikova et al. (2021)	Czech Republic	12	12	12	12
15	Castro and Scartascini (2015)	Argentina	9	9	18	18
16	Scartascini and Castro (2019)	Argentina	0	3	0	9
17	Castro et al. (2022)	Peru	12	12	48	48
18	Chadimova (2023)	Czech Republic	6	6	18	18
19	Chirico et al. (2019)	USA	24	36	36	54
20	Cohen (2020)	Uganda	2	4	4	8
21	Coleman (1996)	USA	0	8	0	8
22	Collin et al. (2021)	Tanzania	4	8	8	16
23	Cranor et al. (2020)	USA	6	6	15	15
24	Cruces et al. (2023)	Argentina	6	6	6	6
25	De Neve et al. (2021)	Belgium	3	3	5	5
26	Del Carpio (2013)	Peru	3	3	6	6
27	Doerrenberg and Schmitz (2017)	Slovenia	0	2	0	4
28	Doerrenberg et al. (2023)	Bulgaria	0	18	0	18
29	Dong and Sinning (2022)	Australia	2	2	4	4
30	Dwenger et al. (2016)	Germany	1	2	1	2
31	Eerola et al. (2019)	Finland	3	9	6	18
32	Fellner et al. (2013)	Austria	3	3	3	3
33	Gallego and Ortega (2022)	Venezuela	3	6	6	12
34	Gillitzer and Sinning (2020)	Australia	3	6	6	12
35	Hallsworth et al. (2017)	UK	13	13	31	31
36	Harju et al. (2020)	Finland	0	2	0	2

37	Hasseldine et al. (2007)	UK	5	9	5	9
38	Hernandez et al. (2017)	Poland	9	27	18	54
39	Hiscox (2018)	Australia	4	6	4	6
40	Hiscox et al. (2018)	Australia	1	2	1	2
41	Holz et al. (2023)	Dominican Rep.	5	10	5	10
42	Hoy et al. (2024)	Papua New G.	2	4	2	4
43	Iyer et al. (2010)	USA	0	0	0	0
44	John and Blume (2018)	UK	3	2	3	2
45	Karver et al. (2022)	Albania	0	4	0	8
46	Kettle et al. (2016)	Guatemala	8	16	32	64
47	Kettle et al. (2017)	Guatemala	6	30	6	30
48	Kleven et al. (2011)	Denmark	2	4	2	4
49	Kotsadam et al. (2022)	Norway	2	4	2	4
50	Manwaring and Regan (2023)	Uganda	5	10	10	20
51	Mascagni et al. (2017)	Rwanda	8	10	16	20
52	Meiselman (2018)	USA	5	5	10	10
53	Mogollón et al. (2021)	Colombia	2	3	8	12
54	Mwaijande et al. (2021)	Tanzania	4	4	4	4
55	Office of Evaluation Sciences (2022)	USA	4	6	4	6
56	Okunogbe (2021)	Liberia	8	12	12	20
57	Orlett et al. (2017)	USA	0	0	0	0
58	Ortega and Sanguinetti (2013)	Venezuela	0	4	0	12
59	Ortega and Scartascini (2020)	Colombia	9	12	36	48
60	Perez-Truglia and Troiano (2018)	USA	4	0	4	0
61	Persian et al. (2023)	Indonesia	5	5	5	5
62	Pfeifer and Pacheco (2020)	Brazil	5	5	5	5
63	Pomeranz (2015)	Chile	2	4	2	4
64	Santoro (2024)	Eswatini	12	12	24	24
65	Saulitis and Chapkovski (2023)	Latvia	0	1	0	5
66	Schächtele et al. (2022)	Brazil	1	1	2	2
67	Schächtele et al. (2023)	Argentina	2	2	10	10
68	Shimeles et al. (2017)	Ethiopia	0	2	0	4
69	Slemrod et al. (2001)	USA	0	3	0	3
70	Torgler (2004)	Switzerland	1	1	2	2
71	Vainre et al. (2020)	Estonia	0	2	0	2
72	Wenzel and Taylor (2004)	Australia	0	3	0	3
73	Wenzel (2006)	Australia	2	2	2	2
			270	475	535	887

The table presents the list of all studies used in this paper along with the country where the RCT comes from and the number of estimates coming from each study. Column 1 (main estimates on the extensive margin) represents our baseline sample. The samples of columns 2, 3 and 4 are described in Section 3.2.

Table A2: Study characteristics

Paper	No of estimates	No of reminder nudges <i>r</i>	Average estimate	Full compliance	Compliance measure	Control group (<i>Reminder_{CTP}</i>)	No. of nudge types	Country	Published	Avg. compliance	Time horizons in months	Share late payer	Delivery methods	Tax type	Tax-payer type
Andersson et al. (2023)	5	0	0.025		PTP	Baseline letter	2	Sweden	1	0.66	1; 3	1.00	D	Income tax	Ind.
Antinyan et al. (2021)	1	1	0.076	Full	PTP	No letter	1	China	0	0.04	2	1.00	D	Property tax	Ind.
Biddle et al. (2018)	3	0	0.001	Partial	Other	Baseline letter	3	Australia	1	0.58	3	0.00	L	VAT	Bus.
Boning et al. (2020)	2	0	0.080	Partial	PTP	No letter	1	USA	1	0.20	3	0.00	L; P	Other	Bus.
Bott et al. (2020)	3	0	0.057	Partial	PTP	Baseline letter	2	Norway	1	0.20	.75	0.00	L	Income tax	Ind.
Boyer et al. (2016)	2	0	0.002	Partial	PTP	Baseline letter	2	Germany	1	0.02	4.6	0.00	L	Other	Ind.
Brockmeyer et al. (2019)	12	0	0.066	Partial	PTP; PTF; PTP	No letter	1	Costa Rica	1	0.03	3.75	1.00	D	Income tax	Bus.
Brockmeyer et al. (2021)	2	0	0.071	Partial	PTP	No letter	2	Mexico	0	0.06	1.3	1.00	L	Property tax	Ind.
Chahlikova et al. (2021)	12	0	0.010	Full	PTP	Baseline letter	4	Czech Republic	0	0.18	1.3; 3; 3.8	1.00	L	Other	Ind.
Castro and Scartascini (2015)	9	0	0.010	Full	PTP	Baseline letter	3	Argentina	1	0.04	.33; 1; 2	0.00	L	Property tax	Ind.
Scartascini and Castro (2019)	12	0	0.001	Partial	PTP	Baseline letter	3	Peru	1	0.18	.5; 5.5; 12.5	0.00	D	Income tax	Ind.
Chadimova (2023)	6	0	-0.006	Full	PTP	Baseline letter	1	Czech Republic	0	0.37	1; 2	1.00	L	Other	Ind.
Chirico et al. (2019)	28	4	0.039	Full; Partial	PTP	No letter	5	USA	1	0.37	1; 3	1.00	L	Property tax	Ind.
Cohen (2020)	3	1	0.004	Partial	PTP	No letter	3	Uganda	0	0.05	3.1	0.00	D	Income tax	Bus.
Collin et al. (2021)	6	2	0.009	Partial	PTP	No letter	3	Tanzania	0	0.09	.33; 1.33	0.00	D	Property tax	Ind.
Crano et al. (2020)	6	0	0.003	Full; Partial	PTP	Baseline letter	2	USA	1	0.19	1	1.00	L	Income tax	Ind.
Cruces et al. (2023)	6	0	0.029	Partial	PTP	No letter	1	Argentina	0	0.20	.15; 1	0.50	L	Property tax	Bus.
De Neve et al. (2021)	3	0	0.044	Partial	PTF; PTP	Baseline letter	1	Belgium	1	0.50	.46; 7; 2	0.67	L	Income tax	Ind.
Del Carpio (2013)	4	1	0.038	Partial	PTP	No letter	3	Peru	0	0.29	1	1.00	L	Property tax	Ind.
Dong and Siming (2022)	2	0	0.146	Partial	PTP	No letter	2	Australia	1	0.04	1.5	1.00	L	Income tax	Ind.
Dwenger et al. (2016)	1	0	0.024	Partial	PTP	Baseline letter	1	Germany	1	0.21	5	0.00	L	Other	Ind.
Eerola et al. (2019)	4	1	0.020	Partial	PTP	No letter	2	Finland	0	0.75	1	0.00	L	Income tax	Ind.
Fellner et al. (2013)	3	0	0.000	Partial	Other	Baseline letter	3	Austria	1	0.07	1.6	0.00	L	Other	Ind.
Gallego and Ortega (2022)	3	0	0.061	Full	PTP	No letter	1	Venezuela	1	0.02	1.5	1.00	D	Other	Ind.; Bus.
Glitzner and Sinning (2020)	3	0	0.153	Partial	PTP	No letter	1	Australia	1	0.50	.79; 1.05; 1.28	1.00	L	VAT	Bus.
Hallsworth et al. (2017)	14	1	0.029	Partial	PTP	Baseline letter	4	UK	1	0.34	.75	1.00	L	Income tax	Ind.
Hasseldine et al. (2007)	5	0	0.104	Partial	PTP	No letter	3	UK	1	0.40	1	0.00	L	Income tax	Ind.
Hernandez et al. (2017)	9	0	0.056	Partial	PTP	Baseline letter	5	Poland	1	0.40	1	1.00	L	Income tax	Ind.
Hiscox (2018)	4	0	0.129	Partial	PTF; PTP	No letter	2	Australia	0	0.29	.5; .7	1.00	D	Multiple	Bus.
Hiscox et al. (2018)	1	0	0.153	Partial	Other	No letter	1	Australia	0	0.01	2	0.00	L	Income tax	Ind.
Hoy et al. (2024)	5	0	-0.002	Partial	PTF	Baseline letter	2	Dominican Republic	1	0.51	4	0.00	D	Corporate tax	Bus.
John and Blume (2018)	3	0	0.052	Full	PTF	Baseline letter	1	Papua New Guinea	1	0.21	.467	1.00	D	VAT	Bus.
Kettle et al. (2016)	10	2	0.027	Partial	PTF; PTP	No letter	2	UK	1	0.43	1	0.00	L	Property tax	Ind.
Kettle et al. (2017)	6	0	-0.001	Partial	PTF	No letter	3	Guatemala	0	0.25	2.5	1.00	L	Income tax	Ind.; Bus.
Kleven et al. (2011)	2	0	0.016	Partial	Other	No letter	1	Guatemala	1	0.14	0	0.00	D	Multiple	Ind.; Bus.
Kotsadam et al. (2022)	2	0	0.078	Partial	Other	No letter	1	Denmark	1	0.14	4; 10	0.00	L	Income tax	Ind.
Manwaring and Regan (2023)	5	0	0.002	Partial	PTP	No letter	4	Norway	0	0.04	1.4	0.00	D	Income tax	Ind.; Bus.
Mascagni et al. (2017)	12	4	0.021	Partial	PTF	Baseline letter	3	Uganda	0	0.04	2	0.00	D; P	Property tax	Bus.
Meiselman (2018)	6	1	0.060	Partial	PTF	No letter	3	Rwanda	0	0.00	2.4	1.00	L	Income tax	Ind.
Mogollón et al. (2021)	2	0	0.066	Full; Partial	PTP	No letter	1	Colombia	1	0.05	2	1.00	L	Income tax	Ind.
Mwaijande et al. (2021)	4	0	0.070	Partial	PTP	No letter	1	Tanzania	0	0.04	.47	0.00	D	Property tax	Ind.; Bus.
Office of Evaluation Sciences (2022)	4	0	0.012	Partial	Other	No letter	2	USA	0	0.29	4	0.00	L	Income tax	Ind.
Okunogbe (2021)	8	0	0.043	Partial	PTP; PTF; Other	Baseline letter	1	Liberia	0	0.08	6	1.00	P	Property tax	Ind.; Bus.
Oriet et al. (2017)	8	8	0.016	Partial	PTF	Baseline letter	1	USA	0	0.60	1.5	1.00	L	Income tax	Ind.
Ortega and Scartascini (2020)	9	0	0.108	Full; Partial	PTP	No letter	1	Colombia	1	1	3; 4	1.00	L; D; P	Other	Bus.
Perez-Truglia and Trolano (2018)	4	0	0.005	Partial	Other	Baseline letter	2	USA	1	0.66	1.15; 2.5	1.00	L	Income tax	Ind.
Persian et al. (2023)	5	0	0.009	Partial	PTP	No letter	3	Indonesia	1	0.66	9	0.00	D	Income tax	Ind.
Pfeifer and Pscheco (2020)	5	0	0.027	Partial	PTF	Baseline letter	4	Brazil	0	0.49	1	1.00	L	Property tax	Ind.
Pomeranz (2015)	2	0	0.005	Partial	PTP	No letter	2	Chile	1	0.16	4	0.00	L	VAT	Bus.
Santoro (2024)	12	0	0.009	Partial	PTF	No letter	4	Eswatini	1	0.01	3	0.00	L	Income tax	Ind.; Bus.
Schächtele et al. (2022)	1	0	0.056	Full	PTP	No letter	1	Brazil	1	0.77	2	1.00	D	Property tax	Ind.
Schächtele et al. (2023)	2	0	0.016	Full	PTP	Baseline letter	2	Argentina	1	0.89	2	0.00	L	Income tax	Ind.
Torgler (2004)	1	0	0.034	Partial	PTP	No letter	1	Switzerland	1	0.46	1.15	1.00	L	VAT	Bus.
Wenzel (2006)	2	0	0.050	Partial	PTF	Baseline letter	2	Australia	1	0.46	1.15	1.00	L	VAT	Bus.

Appendix B: Additional tables

Table B1: Test of asymmetry

	(1)
Inverse of standard error	0.006 (0.006)
Constant	2.909*** (1.074)
Observations	490
Adjusted R^2	0.015

Normalized coefficients are regressed on inverse standard errors and an intercept following Egger et al. (1997).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

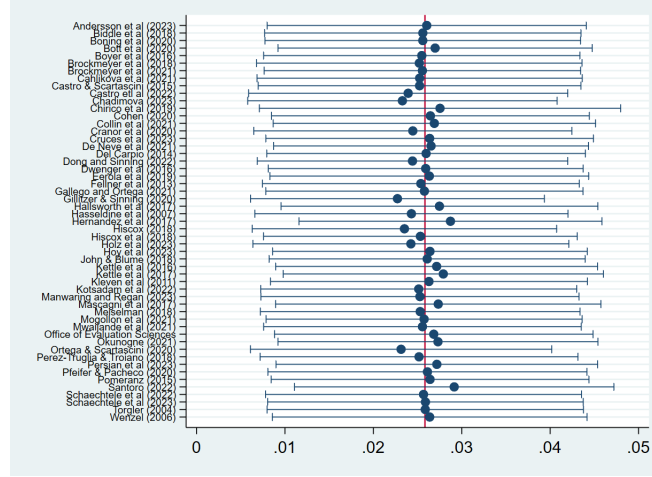
Table B2: P-hacking in t-values around significance threshold

	Normalized t-values			5% significance threshold		
	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth	0.1575	0.1	0.075	0.25	0.2	0.1
Number of tests in bandwidth	141	103	82	82	67	40
Panel A: Randomization test of discontinuities in t-values						
Share significant	0.652	0.641	0.634	0.610	0.642	0.675
p-value (one-sided)	0.000	0.003	0.010	0.030	0.014	0.019
Panel B: Caliper tests						
Deterrence vs non-deterrence	0.007	-0.062	-0.084	0.071	-0.077	-0.160
p-value	0.903	0.392	0.348	0.502	0.403	0.103
Reminder vs no letter	-0.087	-0.154	-0.179	-0.066	-0.101	-0.052
p-value	0.119	0.141	0.229	0.559	0.414	0.669
Published vs working paper	0.056	0.161	0.179	-0.113	-0.015	0.118
p-value	0.481	0.117	0.125	0.263	0.861	0.278
Main vs other estimate	-0.112	-0.219	-0.165	0.019	0.054	-0.121
p-value	0.029	0.008	0.097	0.855	0.547	0.195
New vs old paper	-0.197	-0.176	-0.193	-0.037	-0.043	0.041
p-value	0.001	0.040	0.106	0.742	0.696	0.691

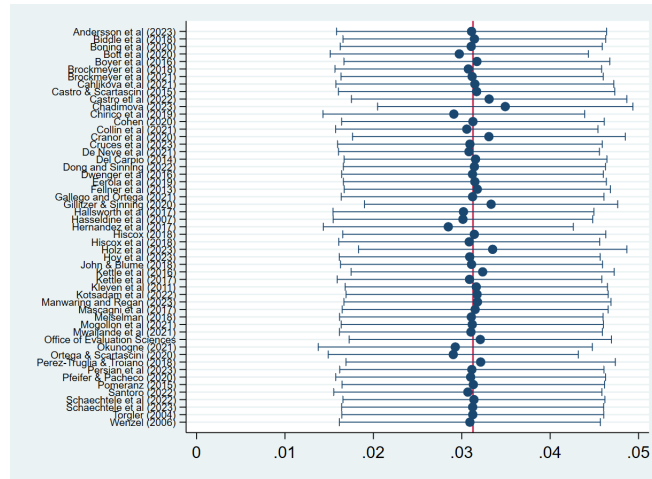
The table shows two exercises in Panels A and B on detecting p-hacking for three different bandwidths around normalized significance thresholds in columns 1-3 and around the 5% significance threshold in columns 4-6. Panel A tests whether the empirical distribution corresponds to a binomial distribution with equal probability below and above the significance threshold. Panel B presents marginal effects from a probit regression of a significance dummy on a set of control variables. Standard errors are clustered at the level of papers.

Figure B1: Jackknife exercise

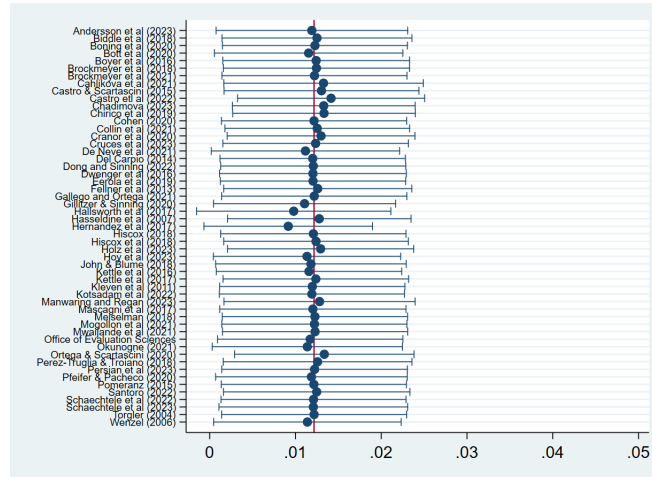
(a) Reminder, $\hat{\alpha}$



(b) Deterrence, $\hat{\beta}$



(c) NonDeterrence, $\hat{\gamma}$



Notes: The figures presents jackknife-type robustness tests by excluding papers one-by-one from the sample. The specification follows Equation 1. The exercise is performed for the three coefficients – $\hat{\alpha}$, $\hat{\beta}$ and $\hat{\gamma}$ – separately in the three sub-figures. Horizontal lines represent 95% confidence intervals.



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