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Nudging for Tax Compliance: A Meta-Analysis

Abstract

Tax compliance nudges are used increasingly by governments because of their perceived cost-effectiveness in raising tax revenue. We collect about a thousand treatment effect estimates from 45 randomized controlled trials, and synthesize this rapidly growing literature using meta-analytical methods. We show that interventions pointing to elements of individual tax morale are on average ineffective in curbing tax evasion (when evaluated against a control group of taxpayers receiving neutral communication). In contrast, deterrence nudges - interventions emphasizing traditional determinants of compliance such as audit probabilities and penalty rates - increase compliance. However, their effects are modest in magnitude increasing the probability of compliance by 1.5-2.5 percentage points more than non-deterrence nudges. Our additional results suggest that nudges i) work better on sub-samples of late payers and when delivered in-person, ii) are less effective in the long-run and in lower-income countries, and iii) are somewhat inflated by selective reporting of results.

JEL-Codes: C930, D910, H260.

Keywords: tax compliance, randomized control trials, nudging, tax morale, meta-analysis.

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1 Introduction

Recent years have seen a lot of excitement around the idea of using "nudges" with the aim to improve individual behavior. Nudges are interventions that respect the freedom of choice and leave economic incentives intact (Benartzi et al. 2017),[1] and they have been studied in many important policy areas such as taxation (Mascagni 2018), education (Dizon-Ross 2019), healthcare (Wisdom et al. 2010), consumer behavior (Costa and Kahn 2013), among others.

In the field of taxation, nudging has become widely popular in the last decade among policy makers who often claim that relative to the negligible direct cost of nudging (e.g., sending a letter) the potential payoffs involved can be extremely high.[2] Academic economists, on the other hand, have come to recognize that a large behavioral response to a simple informational update induced by a nudge is not overly consistent with expected utility theories of human behavior where tax compliance is primarily driven by fundamental economic incentives.[3] Such evidence showing that agents respond to nudges would at least demonstrate the presence of information imperfections (e.g., taxpayers misperceive the probability of being detected when evading), and would possibly hint to the existence of deviations from utility maximization (e.g., taxpayers additionally care about tax morale).[4]

In this paper, we use methods of meta-analysis and ask whether nudges are really effective in increasing tax compliance levels among individuals and small firms. In so doing we aim to present a systematic review of the literature and to provide guidance for further tax experiments

[1] Thaler and Sunstein (2008) define a nudge as an "aspect of the choices 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".

[2] For instance, Hallsworth et al. (2017) and Bott et al. (2017) report £9 million and $25 million increase in tax revenues, respectively, due to letters sent. Although, typically these letters are interpreted of being virtually costless, Allcott and Kessler (2019) argues that nudges entail significant costs for the nudge recipients and shows that the failure to take into account these costs overstates the effects of nudges on social welfare.

[3] This canonical theory of Allingham and Sandmo (1972), Yitzhaki (1974), modeled after the economics of crime literature (Becker 1968), views the individual as a rational agent with some level of risk aversion who considers tax evasion as a gamble trading off the benefits of successful evasion against the costs of detection and punishment.

[4] Motivated by mounting evidence on various behavioral biases of taxpayers, Farhi and Gabaix (2019) develops a theory of optimal taxation with behavioral agents also incorporating nudges into this framework. For a recent review of behavioral public economics literature, see, Bernheim and Taubinsky (2018).

and policy interventions. Of course, an alternative and arguably a more thorough way of surveying the literature can be done through qualitative means. Many excellent literature reviews have been written, such as Alm (2019), Andreoni et al. (1998), Slemrod (2007, 2019), Slemrod and Yitzhaki (2002) on tax compliance generally, as well as Luttmer and Singhal (2014) on tax morale, Mascagni (2018) on tax experiments and Pomeranz and Vila-Belda (2019) on tax capacity, more specifically. We extend this literature by performing a systematic empirical analysis of nudging interventions. This is an important task since as Luttmer and Singhal (2014) put it "similar interventions have produced varying results in different contexts" and "it would be useful to examine why". We are aware of two meta-studies of tax experiments (Alm et al. 2018, Blackwell 2007) which study laboratory experiments while we focus on field work.[5] Additionally, Benartzi et al. (2017), DellaVigna and Linos (2020), Hummel and Maedche (2019) perform meta-analyses of nudges, but they do so in various fields of policy rather than focusing on tax compliance as we do here.

As opposed to qualitative reviews, our meta-analysis attempts to give more systematic answers to questions like: i) Are nudges effective in curbing tax evasion? ii) If so, on which margins of compliance and by how much on average? iii) Which nudge types work more effectively? iv) Are nudges effective also in the longer horizon? v) Which groups of taxpayers are more responsive to nudges? vi) Do nudges work in specific settings (e.g., low compliance environment) or more generally?

To answer these questions we collect data on nearly 1,000 treatment effect estimates of tax compliance coming from 45 studies.[6] We divide these data into three different samples according to the measure of tax compliance employed:[7] i) extensive margin of tax compliance measured as either the probability to pay or file or report taxes, ii) intensive margin of tax

[5]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, meanwhile tax rate has no significant impact on tax compliance. Focusing on a larger set of papers, Alm et al. (2018) 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.

[6]These studies are listed in Table 1. The map in Figure 1 shows the geographic distribution of these experiments as well as of 18 ongoing interventions registered in the randomized control trial (RCT) registry of the American Economic Association.

[7]Figures 2(a), 2(b) and 3 present the distributions of these three variables.

compliance measured as the amount of reported income or reported tax, and iii) the standardized t-value behind the treatment effect estimates for all various types of tax compliance measures.

Unlike many other meta-studies in economics,[8] one advantage of this paper is that we pool together RCTs which have a relatively high degree of homogeneity in their quality of identification. This higher than usual level of comparability of treatment effects coming from different interventions makes our conclusions more meaningful.[9] Another favorable feature of this exercise is that we can study magnitudes of effects in addition to their direction since tax compliance, our dependent variable of interest, can be measured in a relatively standard form. A third advantage is that since almost all of the studies in our sample implement several interventions in their experiments, we can use study fixed effects thus obtaining within study estimates of nudges that control for all study characteristics.

Our main finding is that, in contrast to the recent excitement over nudges, the behavioral content introduced in the communication between the tax administration and the taxpayer is not as effective as often thought. We present robust evidence that on average only deterrence interventions, i.e., nudges informing about audit probabilities and potential penalties, work in increasing compliance levels. The baseline effects of behavioral letters that inform taxpayers about the importance of paying taxes for the adequate provision of public goods, about the (positive) behavior of their peers, or hint towards general appeals of paying taxes as a morale obligation are on average ineffective once these are properly compared to a control group of taxpayers who receive some neutral information. The effect magnitudes of deterrence intervention are modest as they increase compliance by 1.5 to 2.5 percentage points on the extensive margin compared to taxpayers receiving non-deterrence treatments.[10]

[8]For several recent applications, see, Card et al. (2010, 2017), Feld and Heckemeyer (2011), Gechert (2015), Heinemann et al. (2018), Lichter et al. (2015), Neisser (2017), and for a review of meta methods, see, Anderson and Kichkha (2017), Nelson and Kennedy (2009), Stanley et al. (2013), Stanley (2001), Stanley and Doucouliagos (2012).

[9]This argument is one reason behind the methods of meta-analysis being so much more popular in the field of medical sciences (which often evaluate randomized clinical trials) than in economics (Stanley 2001).

[10]These relative effects are somewhat higher in our additional sample studying responses at the intensive margin, and range from 5 to 13 percentage points on average. However, these effects are still moderate when comparing them to the effects of various other characteristics on intensive margin compliance.

A strong way of interpreting this evidence is that individual financial motives, rather than elements of tax morale[11] like social norms or reciprocity remain the first order factors behind compliance decisions. A competing interpretation, however, is that deterrence and non-deterrence nudges are not equally effective in shifting the prior beliefs of taxpayers. According to Pomeranz and Vila-Belda (2019), it may well be that nudges implemented by tax authorities are more effective in shifting perceptions of audit probabilities than perceptions of social norms. Either way, our evidence suggests that at the very least the mainstream neoclassical approach to tax evasion should take into account the possibility that taxpayers are constrained with information imperfections.

Despite these modest baseline effects, our additional findings highlight certain design aspects of experiments that may make them potentially more effective. For example, we find that nudges communicated through in-person visits deliver more powerful results in terms of compliance outcomes than nudges communicated through letters. In terms of different groups of taxpayers, we find that the practice of selecting to focus on previous non-compliers (such as late-payers) can potentially increase the effectiveness of nudges. On the other hand, we find that the effects of nudges are likely to be bound to the short-run, as well as to work less effectivelly in lower income countries. In line with DellaVigna and Linos (2020),[12] we also find evidence that these experimental estimates are inflated by selective reporting of results.[13]

2 Sample of studies

Literature search: We ran a literature search on a rolling basis throughout March to October of 2019. First, we searched for relevant papers using a defined combinations of keywords[14]

[11] See Besley et al. (2019) for theory and evidence on the interaction between individual and social motives in tax evasion.

[12] DellaVigna and Linos (2020) compare estimates of interventions presented in research studies versus the unpublished estimates obtained from trials done "at scale" by two nudge units in the US.

[13] Both in terms of p-hacking, where marginally significant treatment effects are more likely to be reported than results narrowly failing to reject the null, as well as file drawer type of bias, where the results not supporting the likely hypotheses of researchers are not reported.

[14] The keywords include: randomized controlled trial, RCT, field experiment, nudging, nudges, behavioral intervention, tax evasion, tax compliance, tax non-compliance.

Table 1: List of studies in meta-analysis sample

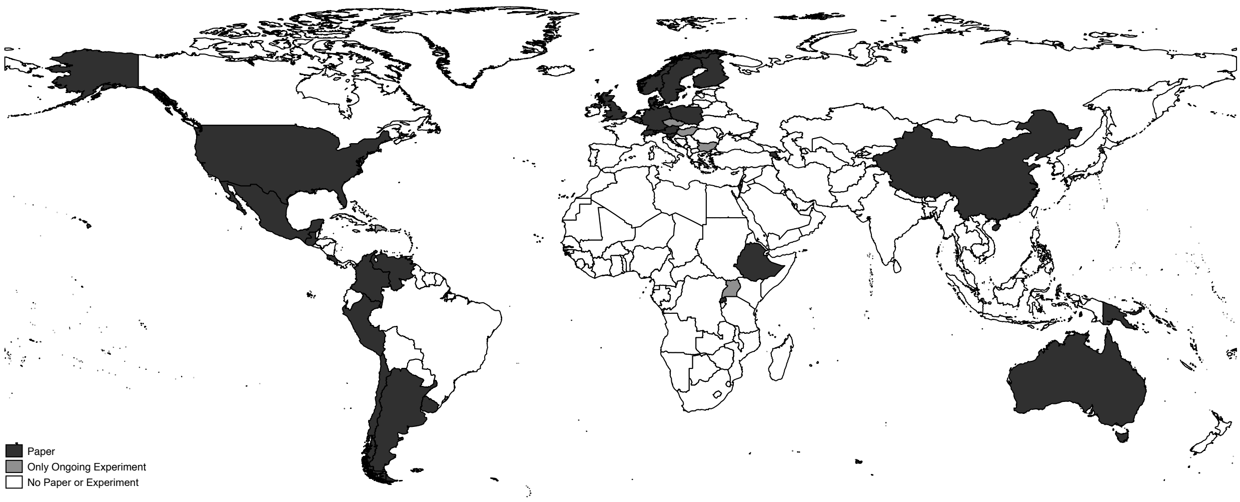
No	Author	Country	No	Author	Country
1	Antinyan and Asatryan (2020)	Armenia	24	Harju et al. (2018)	Finland
2	Antinyan et al. (2020)	China	25	Hasseldine et al. (2007)	UK
3	Appelgren (2008)	Sweden	26	Hernandez et al. (2017)	Poland
4	Ariel (2012)	Israel	27	Hiscox (Hiscox)	Australia
5	Bérgolo et al. (2017)	Uruguay	28	Hoy et al. (2020)	Papua New Guinea
6	Biddle et al. (2018)	Australia	29	Iyer et al. (2010)	USA
7	Blumenthal et al. (2001)	USA	30	John and Blume (2018)	UK
8	Boning et al. (2018)	USA	31	Kettle et al. (2016)	Guatemala
9	Bott et al. (2017)	Norway	32	Kettle et al. (2017)	Guatemala
10	Boyer et al. (2016)	Germany	33	Kleven et al. (2011)	Denmark
11	Brockmeyer et al. (2020)	Mexico	34	Mascagni et al. (2017)	Rwanda
12	Brockmeyer et al. (2019)	CostaRica	35	Mascagni et al. (2018)	Ethiopia
13	Castro and Scartascini (2015)	Argentina	36	Meiselman (2018)	USA
14	Chirico et al. (2019)	USA	37	Ortega and Sanguinetti (2013)	Venezuela
15	Coleman (1996)	USA	38	Ortega and Scartascini (2015)	Columbia
16	Del Carpio (2013)	Peru	39	Perez-Truglia and Troiano (2018)	USA
17	De Neve et al. (2019)	Belgium	40	Pomeranz (2015)	Chile
18	Doerrenberg and Schmitz (2017)	Slovenia	41	Scartascini and Castro (2019)	Argentina
19	Dwenger et al. (2016)	Germany	42	Shimeles et al. (2017)	Ethiopia
20	Eerola et al. (2019)	Finland	43	Slemrod et al. (2001)	USA
21	Fellner et al. (2013)	Austria	44	Torgler (2004)	Switzerland
22	Gillitzer and Sinning (2018)	Australia	45	Wenzel (2006)	Australia
23	Hallsworth et al. (2017)	UK			

in the main literature databases of the profession.[15] Second, to identify ongoing work, we continued the search in the programs of the main general interest conferences in economics as well as the main conferences specializing on behavioral or experimental economics and public economics.[16] Third, we carefully looked through the bibliographic information of the papers identified in the last two steps to further refine the study sample. In July of 2020 we identified and added to our data four new and relevant working papers that appeared in the meantime, and we re-visited all the working papers identified earlier to see if they have been published.

[15]The literature databases include: Econlit, Google Scholar, and Science Direct.

[16]The conferences include: American Economic Association, European Economic Association, ESA, SABE, WEAI, National Tax Association, International Institute of Public Finance.

Figure 1: Country coverage of nudging experiments



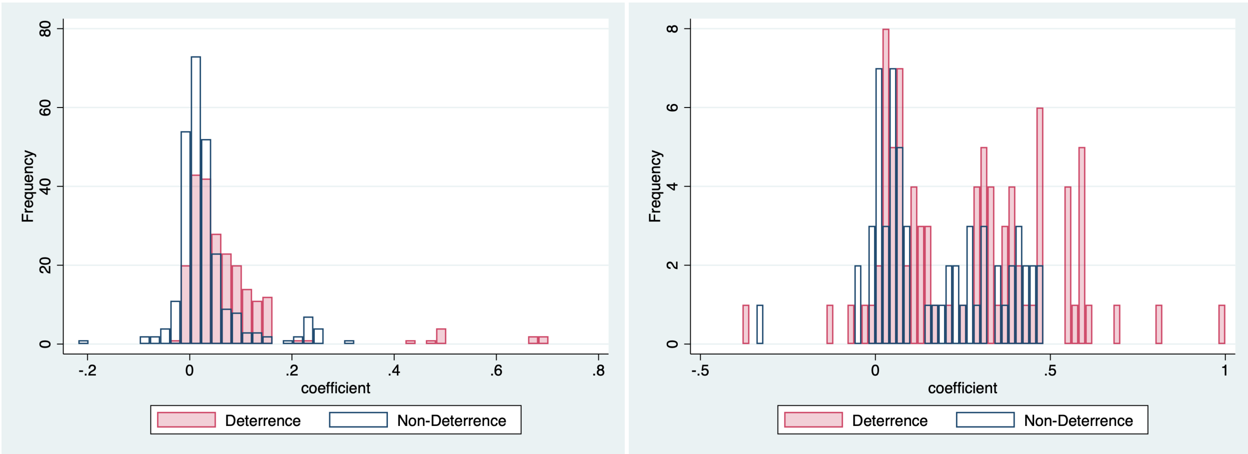
Source : Own compilation. The papers are listed in Table 1. Data on ongoing experiments is based on the RCT registry of the American Economic Association as retrieved on October 21, 2019.

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 payment behavior of the taxpayer; and iv) the resulting study reports all the relevant statistics necessary for our meta-analysis (e.g., effect sizes along with the standard errors) for at least one treatment effect estimate.

Final sample: After applying these four filters to the list of papers collected from our extensive search we arrive at an overall sample of 45 studies. These studies are listed in Table 1 in alphabetical order. These 45 experiments were performed in 28 countries situated mainly in Europe, Africa, Australia and the Americas as presented in the map of Figure 1.

Figure 2: Distribution of treatment effects of extensive and intensive margin responses

(a) Probability to comply **(b)** Log amount



Notes : Sub-figure (a) and (b) plot a histograms of treatment effects on extensive and intensive margins of compliance, respectively, which we obtained from the primary studies in our sample. We plot these treatment effect for deterrence and non-deterrence nudges separately. For visual clarity, sub-figures (a) and (b) drop, respectively, 6 and 28 outlier observations that are larger than 1.

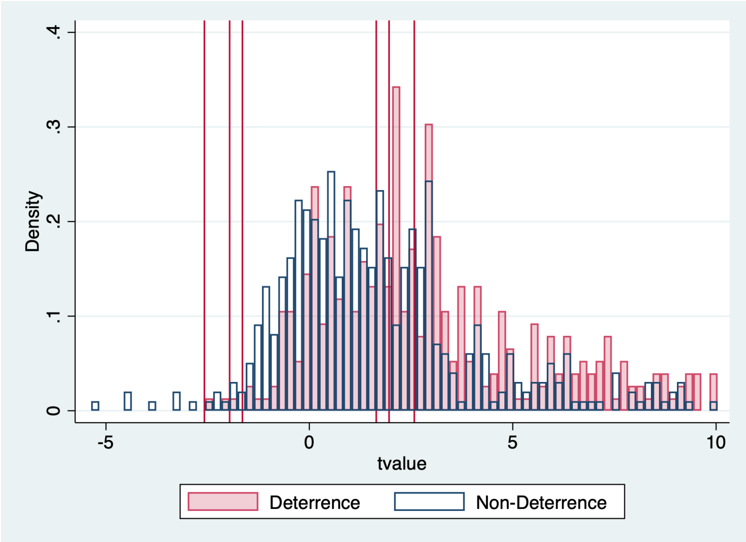
3 Data

3.1 Dependent variables

We adopt three different dependent variables that measure either the magnitude of tax compliance in two ways or the statistical significance of the treatment effects.

Extensive margin: Our main dependent variable of interest attempts to measure the magnitude of tax compliance as generally as possible. In particular, it captures treatment effect estimates that measure the impact of nudges at the extensive margin of tax compliance. This margin is defined as either the probability to pay or file or report taxes in a given time horizon. This variable is available for 488 observations coming from 31 studies. This makes about half of our full sample of estimates, owing to the fact that papers measure tax compliance differently, but it still contains sufficient variation for identification. Figure 2 (a) presents a histogram of the distribution of treatment effects on extensive margin of compliance.

Figure 3: Distribution of t-values of treatment effects in the full sample



Notes : Figure plots a histogram of t-values of treatment effects which we obtained from the primary studies in our sample. We plot t-values for deterrence and non-deterrence nudges separately. For visual clarity, we drop 117 outlier observations that lie outside the (-10, 10) range. Vertical lines denote critical values for two-sided significance tests at t-values of +/-1.645, +/-1.96 and +/-2.58.

Intensive margin: The second dependent variable collects estimates that measure the impact of nudges on intensive margin of tax compliance.[17] Studies use different measures of intensive margin responses, and the most common ones that we choose to focus on study either reported taxes or reported income. These variables are estimated either in absolute values in national currencies or on a logarithmic scale. To assure comparability across studies, we transformed those effect sizes that appear in absolute values by dividing the point estimate of the treatment effect by the average of the dependent variable of the control group (in the post-intervention period when available). We are left with only about the sixth of our total sample size which substantially reduces the precision of our estimates. Compliance measured at the intensive margin comprises of 175 observations from 17 papers, and intends to serve as a sample for testing the robustness of hypotheses in addition to our main sample. The variable is plotted in the histogram of Figure 2 (b).

[17] We do not claim that the intensive margin effect is necessarily separable from the extensive margin effect in all contexts, and rather rely on the respective specification of the primary study. For work that allows for both intensive and extensive margin responses to taxes, see, Blundell and MaCurdy (1999) and Kleven and Kreiner (2006) in the context of labor supply responses, and Almunia et al. (2019) in the context of tax deductible charitable donations.

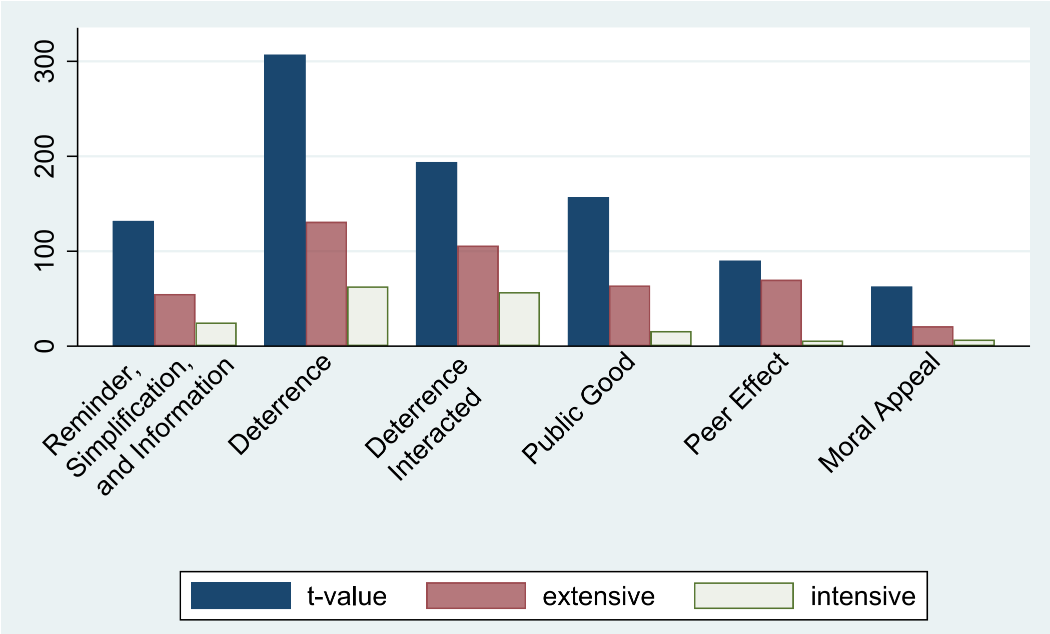
T-values: The third dependent variable comprises of the full sample of t-values of treatment effect estimates. We cannot analyze the magnitude of compliance in this sample, and rather we only study the direction and statistical significance of effects. We also caution against the fact that t-values are, of course, a function of degrees of freedom, thus making the cross-study comparisons not straightforward. The main benefit of using this variable is that t-values being standard measures are available for all 45 studies in the sample covering 997 estimates. We thus follow other applications of meta-analytical techniques in economics, such as by Baskaran et al. (2016), Card et al. (2010, 2017), Heinemann et al. (2018), Klomp and De Haan (2010), and use this sample. Our primary motivation for using the t-values is the aim to study the robustness of results coming from the main sample of extensive margin responses. Figure 3 presents a histogram of the distribution of t-values.

3.2 Types of nudges

We classify nudges into deterrence and non-deterrence categories. Each category contains about half of our sample. We then further divide the non-deterrence nudges into interventions related to reminders, simplifications and information on one side, and tax morale related nudges on the other side. Figure 4 presents a tabulation of the types of nudges with respect to the three dependent variables.

Deterrence nudges: To be considered as a deterrence nudge, the communication between tax administration and taxpayers should specifically contain a threat that highlights one of the economic factors behind the tax compliance decision as in the canonical model of tax evasion by Allingham and Sandmo (1972): mainly the possibility of audit and the potential penalty if caught evading. An example of such a 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

Figure 4: Frequency of observations per type of nudge for each dependent variable



Notes : The figure shows the frequency of non-missing observations per type of nudge for the three dependent variables. For visual clarity we drop 36 observations from the full sample that fall into other less popular categories of nudges.

Municipality can take administrative and legal action?".[18] Within the category of deterrence nudges we also distinguish deterrence nudges that additionally include a tax morale type nudge (as defined below) and call these nudges "deterrence interacted".

Tax morale related nudges: To be acknowledged as a non-deterrence nudge, the communication content between the tax administration and taxpayers should not contain a threat that has the potential to alter the taxpayers' financial motives. The first and main sub-category of non-deterrence nudges is built on the solid evidence in the literature that taxpayers can be motivated by such considerations as morality, the perception of fairness, social norms in the society, provision of public goods by the government, and the like. For convenience, we call these types as "tax morale related" nudges. The three common nudges in this sub-category are detailed below:

[18] Note that communications including both deterrence and non-deterrence components are classified as a deterrence nudge given the presence of the threat component. In an additional analysis we label these communications as mixed deterrence nudges and study their effects separately.

- Public good, which makes it clear that the taxes paid by individuals are 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. 2017).

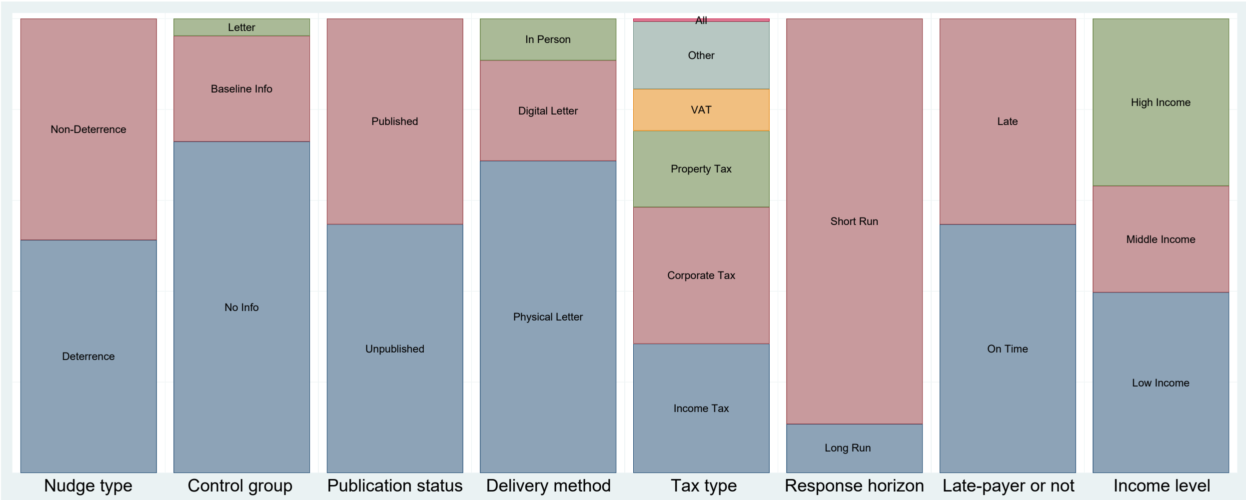
- Peer effect, which underlines that the majority of individuals in a given country/community are complying with taxes: "Nine out of ten people pay their taxes on time" (Hallsworth et al. 2017).

- Moral appeal, which appeals to morality 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).

In our analysis we study deterrence and the main non-deterrence nudges as two broad groups of nudge types, and in additional analysis we study the individual effects of these three types of non-deterrence nudges separately.

Reminder, simplification and information nudges: The second sub-category of non-deterrence nudges mainly contains manipulations that are utilized "to correct" the taxpayer non-compliance that stems from such behavioral fallacies as limited attention, procrastination and cognitive overload among others. For instance, simple reminders are sent to taxpayers to overcome the problem of limited attention (Hernandez et al. 2017, Mascagni et al. 2017). The problem of cognitive overload is usually bypassed through the simplification of the communication language, the introduction of visual stimuli or provision of information how to file the income (De Neve et al. 2019, Eerola et al. 2019). This sub-category of non-deterrence nudges also include communications which introduce various types of informational content, such as a sentence on tax deductible donations (Biddle et al. 2018), phone number for enhanced consumer service (Coleman 1996), a statement of intent by tax administration to help during the filing process (Hasseldine et al. 2007), and the like. In the analysis that

Figure 5: Study characteristics



Notes : The figure shows the distributions of categories within study characteristics for the full sample of t-values.

follows, we group all these nudges under the common umbrella name reminder, simplification and information nudges. Although, the types of nudges in this sub-category are not always coherent in the type of content they introduce, they make only about a quarter of non-deterrence nudges.

3.3 Study characteristics

We account for about a dozen characteristics related to studies in our primary sample. Most of these are study specific, and a few vary within studies. We set forth three characteristics that define a baseline nudge, and a number of additional characteristics which we use to study potential heterogeneities in the effects of nudges on compliance. Figure 5 presents a tabulation of these characteristics.

Baseline characteristics: i) type of a nudge as defined in Section 3.2, in the most general specification it classifies the nudge into deterrence or non-deterrence types; ii) the benchmark against which the interventions are evaluated, in particular, defining the control group as taxpayers who did not receive any communication (no information), received some neutral information (baseline information), or the comparison is made against another behavioral intervention (letter); and iii) publication status of the study, i.e., working paper or published article.

Additional characteristics: iv) communication channel used by the tax authority to reach out to the taxpayers, i.e., digital letters (e.g., e-mails, SMS, CAPTCHA), physical letters (e.g., letters, tax bill manipulations), and in-person visits; v) type of the tax, i.e., personal income tax, corporate income tax, property tax, VAT, all taxes and other taxes[19]; vi) response horizon of the compliance measure, i.e., a dummy variable on whether the time interval between the date on which the nudge was sent and the date when the outcome variable was measured is shorter or longer than 12 months; vii) a dummy whether the taxpayer is a late payer, i.e., did not comply in paying taxes by the official deadline; and viii) the income level of the country where the experiment was conducted, i.e. low, middle or high-income country.

4 Empirical methodology

Baseline specification: We estimate the following equation:

$$Estimate^{\tau}_{i,p} = \alpha + \beta^{\tau} Nudge_{i,p} + \gamma BaselineCharacteristics_{i,p} + \epsilon_{i,p} \tag{1}$$

where $Estimate^{\tau}_{i,p}$ is the i^{th} estimate from paper p of type τ: i) treatment effects on extensive margin responses, ii) treatment effects on intensive margin responses, and iii) t-value of primary studies. Figures 2(a) and 2(b) and 3 plot the distributions of these three dependent variables, respectively. For a more detailed description of these variables, see Section 3.1. $Nudge_{i,p}$ is a binary variable indicating whether a nudge is of deterrence or non-deterrence type, as discussed in Section 3.2. $BaselineCharacteristics_{i,p}$ additionally include a categorical variable capturing whether the control group received no letter, a baseline letter or a behavioral letter, and a dummy variable on the publication status of the study, as defined in

[19]Other taxes include country-specific taxes or fees, e.g., the church tax in Germany, wealth tax in Colombia, TV license fees in Austria.

Section 3.3. $\epsilon_{i,p}$ is the error term which is clustered at the level of papers p. We choose this level of clustering the errors, since the estimates may not be independent within studies.

The intercept α is our main coefficient of interest. It captures the average effect of a baseline intervention. As our baseline effect we understand a nudge intervention of non-deterrence type that is compared to a control group of taxpayers who received a neutral communication and which has been published.

Specification with study fixed effects: Having defined the baseline effect, we now turn to the identification of relative effects of deterrence nudges to that of non-deterrence types of nudges on tax compliance. Almost all of the studies in our sample implement several nudge interventions.[20] Therefore, unlike many other meta-analyses in economics, we can exploit the substantial within-study variation in the data and study the relative effects of deterrence and non-deterrence nudges controlling for study fixed effects. The specification is as follows:

$$Estimate^{\tau}_{i,p} = \alpha + \beta^{\tau} Nudge_{i,p} + \lambda_p + \epsilon_{i,p} \tag{2}$$

which is similar to Equation 1, but additionally includes the study fixed effects λ_p. Given that most of our characteristics do not vary within studies we drop $BaselineCharacteristics_{i,p}$.

β is the main coefficient of interest which shows the effect of deterrence types of nudges on the tax compliance measure under study compared to that of non-deterrence nudges. Note that in this equation with study fixed effects, we are not anymore interested in interpreting the intercept α which is some average value of the study fixed effects. In an additional variation of this specification, we let $Nudge_{i,p}$ to be defined more broadly as a categorical variable taking into account the several different types of nudges as defined in Section 3.2 and presented in Figure 4.

[20]Since the number of estimates across studies differs, we treat our data as an unbalanced panel. Two studies do not allow for within-study variation, that is, Blumenthal et al. (2001) with only one estimate and Torgler (2004) with one type of nudging intervention (moral suasion).

Heterogeneity in treatment effects: We are also interested in the question of whether various study-, experiment-, or country-specific characteristics drive the heterogeneity in treatment effect estimates. Most of these characteristics do not vary within studies, therefore we revert to Equation 1 and drop the study fixed effects instead of them introducing a richer set of characteristics as follows:

$$Estimate^{\tau}_{i,p} = \alpha + \beta^{\tau} Nudge_{i,p} + \gamma BaselineCh_{i,p} + \delta OtherCharacteristics_{i,p} + \epsilon_{i,p} \quad (3)$$

γ and δ are the main coefficients of interest which capture the average effects of baseline as well as the additional characteristics as defined in Section 3.3.[21]

Estimation method: In the choice of our estimation methods we follow a number of recent applications of meta-analytical techniques in economics (Card et al. 2010, 2017, Feld and Heckemeyer 2011, Gechert 2015, Heinemann et al. 2018, Lichter et al. 2015, Neisser 2017) as well as a literature reviewing these methods (Nelson and Kennedy 2009, Stanley et al. 2013, Stanley 2001, Stanley and Doucouliagos 2012). Our simplest specification relies on an OLS estimator. We present all set of results estimated with the OLS, and, in the Appendix, show the robustness of these results to estimates using two further methods.

Given that meta-analytical regressions are known to be heteroskedastic,[22] we follow the literature and as a second specification use a WLS estimator, whereas analytical weights we take the inverse of the squared standard error of the parameter estimates.[23] This weighting scheme that makes use of standard errors is standard in the literature, however, it is obviously appropriate to use only in specifications where we study the magnitudes of compliance at the extensive and intensive margins. For the sample where the dependent variable is captured by the t-value, we follow Heinemann et al. (2018) and replace the former analytical weights

[21] In an extended version of Equation 3, we interact nudge types with all characteristics in order to test the hypothesis of whether the potential heterogeneities are different between deterrence and non-deterrence nudges.

[22] One form of heteroskedasticity arises because the variance of 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.

[23] Due to their wide distribution we follow Card et al. (2017) and winsorize these analytical weights at the top and bottom deciles. Results remain very similar to alternative winsorizations at 1 or 5 percentiles.

with inverse of the share of observations per study in relation to the full sample. Additionally, as a third method, we adopt a random effects model,[24] which assumes the existence of a distribution of true effects for distinct studies and populations. Thus we relax the assumption that for each type of a nudge there exists a single "true" effect which is common to all studies under consideration.

5 Results

We start by describing the baseline effect of a typical nudge in Sub-section 5.1. Sub-section 5.2 contains our main specification with study fixed effects where we quantify the relative effects of deterrence nudges to those of non-deterrence nudges within papers. In the following Sub-section 5.3 we break down nudges into more detailed categories and study their relative effects. Sub-section 5.4 relaxes the strategy of using study fixed effects, which allows to study the role of a wider set of study characteristics in addition to types of nudges in explaining the variation in estimated treatment effects. Sub-section 5.5 adopts a number of approaches to examine traces of publication bias in our sample of RCTs.

The tables discussed in this section follow a similar structure. Each of the three columns of the tables represents one of our dependent variables as discussed in Section 3.1. These variables are tax compliance at the extensive margin as our main variable of interest, as well as compliance at intensive margin and the t-value of the treatment effect as additional variables of interest. In our main estimations we use an OLS estimator. In the Appendix we present robustness tables behind all of these estimates using two estimators that are standard in the meta literature, a weighted least squares and a random effects model as introduced in Section 4.

[24] The terminology of random effects in this context should not be confused with the study fixed effects, the inclusion of dummies for individual studies.

5.1 Baseline effect

In order to understand the baseline effect of a nudge we necessarily need to define what we understand under a baseline nudge. One way of thinking about the baseline effect of a nudge is simply to think about some average of the raw data of treatment effect estimates. According to our data, the average (median) deterrence nudge increased compliance by 7.7% (4.6%) at the extensive margin, while the average (median) non-deterrence nudge increased compliance by 3.1% (1.5%). At the intensive margin, the average (median) treatment effect is an increase of 81% (34%) and 23% (20%) for deterrence and non-deterrence nudges, respectively. However, the relatively wide distribution of estimates – as presented by the histograms of Figures 2 and 3 – as well as the multi-dimensionality of study characteristics raises the question of whether such an average gives any meaningful characterization about the baseline effect of interest.

Another, arguably a more meaningful, approach of thinking about the baseline effect is to try and define a nudge based on a combination of characteristics. As discussed in Section 3.3, we have two sets of characteristics, baseline and additional characteristics. The main approach behind thinking about a baseline nudge that we adopt defines the baseline on only three rather generic but meaningful characteristics. In so doing we remain relatively agnostic about what constitutes a "typical" nudge. The results of this main exercise are discussed below. In an additional analysis of Section 5.4, we also make an attempt of fixing both the baseline as well as the additional characteristics. Although the results turn out to be similar, we believe that this choice of characteristics is necessarily a subjective one. In addition, the fact of fixing of too many characteristics dramatically narrows down the sample the baseline effect is representing.

As discussed in Section 3.3, the three baseline characteristics are as follows. First, given the interest of this paper in studying deterrence and non-deterrence nudges, as a baseline we fix non-deterrence nudges so that we can later compare the effects of deterrence nudges to the baseline. Second, and most importantly, we fix the benchmark against which the interventions are evaluated. In particular, as baseline we take those estimates that compare

Table 2: Baseline effects of nudges

	(1) Extensive	(2) Intensive	(3) t-value
Baseline effect	-0.002	0.000	-0.099
	(0.019)	(0.226)	(2.599)
Treatment (omitted: Non-Deterrence)			
Deterrence	0.041*	0.410	5.253
	(0.019)	(0.277)	(2.708)
Baseline Comparison (omitted: Baseline Info)			
No Info	0.025	-0.034	1.164
	(0.021)	(0.197)	(1.959)
Letter	-0.027	-1.352*	-4.864*
	(0.015)	(0.596)	(2.320)
Publication Status (omitted: Unpublished)			
Published	0.034	1.073	3.756
	(0.022)	(0.558)	(3.396)
Study FE	No	No	No
Observations	471	174	977
R^2	0.112	0.164	0.094

Standard errors in parentheses

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

Regressions are estimated according to Equation 1.

the intervention versus a control group of taxpayers that received some neutral information, rather than a control group that did not receive any communication. This choice is motivated by the fact that interventions in the latter group also capture the effects of sending any communication and are not able to isolate the effects of the content of the nudge. Third, given the evidence in the past meta literature as well as our analysis of Section 5.5 on the existence of publication bias, we fix whether a study is published or not.

Table 2 plots the results from the estimation of Equation 1. The baseline effects of a nudge is represented by the estimated intercept α. Since we are interested in the baseline effects of nudges across studies, in these regressions we leave out the study fixed effects which is otherwise our preferred specification. Column (1) shows that the treatment effect of a non-deterrence nudge at the extensive margin evaluated against a control group that received neutral communication and as presented in an unpublished paper is very small and is about -0.2% on average. Similarly, column (2) shows that at the intensive margin this baseline effect is close to 0. Column (3) shows that the t-value behind the treatment effect for this non-deterrence nudge is about 0.1, and is thus far from a critical value of a significant effects. None of these average effects are statistically different from zero.

Overall, this evidence shows that the baseline effect of a non-deterrence nudge published in a paper and evaluated against a control group receiving some neutral communication is close to 0. This evidence is robust across the various margins of compliance that we consider. The evidence also remains robust when we estimate Equation 1 with a WLS estimator as presented in columns (1)-(3) of Table A2 of the Appendix.[25]

5.2 Deterrence and non-deterrence nudges

We next study the effect of deterrence nudges relative to non-deterrence nudges on the magnitudes of treatment effect estimates and their t-values. All regressions include study fixed effects, and thus use variation coming from within studies. Given that most study characteristics other than the types of nudges do not vary within studies, we do not include these characteristics in these regressions. The specification is more precisely represented by Equation 2, and its estimation results are plotted in Table 3. As before the main results are estimated using an OLS model, while Table A3 tests for the robustness of these estimates using WLS and RE estimators.

Column (1) of Table 3 shows a statistically significant 2.5 percentage point increase in the probability to comply when receiving deterrence as opposed to receiving non-deterrence nudges. Consistent with this result, column (2) indicates that deterrence nudges increase intensive margin compliance by about 12.6 percentage points more than the non-deterrence nudges. Results from the full sample of column (3) show that studies in our sample find significantly higher t-values on the treatment effects of deterrence nudges compared to those of non-deterrence nudges. These findings are robust across the two alternative estimation

[25] For symmetry, this table also presents the results from the random effects estimator, however, we note that, given the assumption of this model about the existence of a distribution of effects across studies as discussed in Section 4, the intercept does not have the same interpretation such that it would inform about the baseline effect of interest as before.

Table 3: Deterrence and non-deterrence nudges

	(1) Extensive	(2) Intensive	(3) t-value
Treatment (omitted: Non-Deterrence)			
Deterrence	0.025***	0.126*	1.420**
	(0.006)	(0.045)	(0.526)
Constant	-0.026***	0.007	-0.585
	(0.006)	(0.030)	(0.526)
Study FE	Yes	Yes	Yes
Observations	471	174	979
R^2	0.368	0.364	0.384

Standard errors in parentheses

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Regressions are estimated according to Equation 2.

methods as presented in Table A3. The WLS estimator yields smaller, but still statistically significant, effect sizes of 1.5 and 5.9 percentage points on the extensive and intensive margins, respectively, and the RE estimator yields similar results.

Overall, this evidence suggests that deterrence nudges are more effective in improving tax compliance than non-deterrence nudges. However, the magnitudes of these relative effects are in general fairly modest.

5.3 Detailed categories of nudges

In this section, instead of grouping the multiple types of behavioral interventions into deterrence and non-deterrence nudges, we study the relative effects of more detailed categories of nudges on tax compliance. Within the deterrence nudge category, we differentiate between pure deterrence and mixed deterrence nudges, where the latter type of nudges append a deterrence with a non-deterrence communication. Within the non-deterrence nudge category, we distinguish between nudges that are of "Reminder, simplification and information" type and nudges that are related to tax morale. In this latter category we further distinguish between Public Goods, Peer Effects, and Moral Appeals nudges. Section 3.2 discusses the further typological details behind the nudge categories, and Figure 4 presents a tabulation of our data over these nudges.

Table 4: Types of nudges

	(1) Extensive	(2) Intensive	(3) t-value
Treatment (omitted: Reminder, Simplification and Information)			
Deterrence	0.022*	0.061	0.427
	(0.009)	(0.105)	(0.584)
Deterrence Interacted	0.028**	0.177**	1.853**
	(0.009)	(0.050)	(0.634)
Public Good	-0.007	-0.078	-1.287
	(0.008)	(0.105)	(0.732)
Peer Effect	0.002	-0.086	-1.042
	(0.012)	(0.088)	(0.792)
Moral Appeal	-0.001	-0.032	-0.849
	(0.008)	(0.091)	(0.766)
Other	-0.005		-1.766
	(0.014)		(1.978)
Constant	-0.023*	0.077	0.408
	(0.009)	(0.102)	(0.584)
Study FE	Yes	Yes	Yes
Observations	471	174	979
R^2	0.369	0.365	0.385

Standard errors in parentheses

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

Regressions are estimated according to Equation 2.

The estimates based on a variation of Equation 2 are collected in Table 4. First, regarding the tax morale related nudges, we find that the individual effects of three main types of tax morale nudges – that are public goods, peer effects, and moral appeals – are not statistically distinguishable from "Reminder, simplification and information" nudges.[26] This result has two implications. First, the comparison of tax morale nudges to the omitted category of nudges provides further support for the conclusions of Section 5.1 that tax morale related nudges are not effective once evaluated against a control group of taxpayers who receive some neutral communication. In particular, this adds within paper evidence to the previous cross-sectional result. Indeed, this interpretation rests on the assumption that "Reminder, simplification and information" nudges can be thought of as neutral nudges, similar to the baseline nudges of Section 5.1. Second, this finding suggests that we not only fail to find evidence that an average

[26] This finding does not change if we combine the three tax morale nudges into one category.

tax morale nudge works, but we also fail to find evidence that its more detailed components – those that are related to public goods, peer effects, and moral appeals – matter for tax compliance.

Regarding deterrence nudges, once we distinguish between deterrence and mixed deterrence nudges the effects of the deterrence category of our treatment variable as compared to the omitted group become somewhat less precise although the magnitudes are robust to what we found in Section 5.2. Although the mixed deterrence nudges become strong and sizable, in an additional test where we omitted the simple deterrence category we failed to reject the null hypothesis that mixed deterrence nudges are not different from deterrence nudges. All of these findings are generally robust to the alternative estimators presented in Table A4.

5.4 Study characteristics and heterogeneity in treatment effects

The last three sub-sections have documented the finding that deterrence interventions are effective in increasing tax compliance while non-deterrence interventions are not. By accounting for the types of nudges we were able to explain 36 to 37% of the observed within study variation in treatment effect estimates of, respectively, intensive and extensive margin responses (see R^2 of Table 3).

In this sub-section we study whether and how various additional characteristics, as developed in Section 3.3 and summarized in Figure 5, drive our results. Most of these characteristics are study-specific.[27] Therefore, we relax our preferred model and do not include the study fixed effects. Taken together these study characteristics can explain an additional of 23% to 30% of the remaining heterogeneity in the treatment effect estimates of nudges (see the difference in R^2 between Table 5 and Table 2).

Before studying the heterogeneities, we note that the baseline effects on the extensive and intensive margins – as captured by the estimates of the intercepts in Table 5 – are statistically

[27] There are exemptions to this rule. For example, several papers estimate tax compliance responses across different time horizons, or a few papers vary the methof of the delivery of the nudge. However, these are rare exemptions. For the vast majority of cases characteristics do not vary within studies. For example, we do not have a single experiment in our sample that was implemented across multiple countries or across multiple tax types.

Table 5: Heterogeneity of results

	(1) Extensive	(2) Intensive	(3) t-value
Baseline effect	0.056	-0.039	1.540
	(0.036)	(0.821)	(1.356)
Treatment (omitted: Non-Deterrence)			
Deterrence	0.016	0.166**	1.405*
	(0.010)	(0.054)	(0.555)
Baseline Comparison (omitted: Baseline Info)			
No Info	0.031	0.293	2.634*
	(0.019)	(0.446)	(1.088)
Letter	-0.049**	-0.268	-5.390**
	(0.017)	(0.567)	(1.932)
Publication Status (omitted: Published)			
Unpublished	0.007	0.491	1.274
	(0.013)	(0.322)	(1.061)
Delivery (omitted: Physical Letter)			
Digital Letter	0.018	0.832*	2.183
	(0.024)	(0.310)	(1.350)
In Person	0.111**	2.180***	12.975**
	(0.040)	(0.494)	(4.533)
Tax Type (omitted: Corporate Tax)			
Income Tax	-0.016	0.675	2.473*
	(0.028)	(0.370)	(0.948)
Property Tax	-0.032	0.281	2.533
	(0.036)	(0.369)	(1.673)
VAT	-0.004	1.546***	1.959
	(0.018)	(0.319)	(1.529)
Other	0.044	1.447***	12.216***
	(0.024)	(0.243)	(1.864)
All	0.036	1.485***	4.992**
	(0.033)	(0.292)	(1.695)
Response Horizon (omitted: Short Run)			
Long Run	-0.040*	-0.023	-1.161
	(0.018)	(0.133)	(1.000)
Late-payer sample (omitted: Late)			
On Time	-0.031*	-1.140***	-6.026***
	(0.012)	(0.175)	(1.268)
Development Level (omitted: High Income)			
Low Income	-0.056*	-0.799**	-2.514
	(0.025)	(0.254)	(1.504)
Middle Income	-0.016	-0.835*	-0.169
	(0.015)	(0.302)	(1.141)
Study FE	No	No	No
Observations	467	174	963
R^2	0.338	0.474	0.379

Standard errors in parentheses

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

Regressions are estimated according to Equation 3.

not distinguishable from zero. This implies that, similar to the findings of Section 5.1, the baseline effects of non-deterrence nudges are close to zero. The baseline nudge in this model is understood as having the following eight characteristics: it is a non-deterrence nudge (i) evaluated against a control group that received neutral communication (ii) and published in a working paper (iii), and additionally is a nudge that was delivered through a physical letter (vi) to a corporate taxpayer (v) situated in a high income country (vi) who was late with her payment (vii) and whose compliance behavior is measured in a short horizon (viii). We do not claim that the combination of these characteristics defines a "representative" nudge, however this combination creates a meaningful group that is one of the more common types in our sample. Changing some of these characteritics will, of course, affect the baseline result, and we devote the remainder of this section to the discussion of the more important characteristics that seem to explain the heterogeneity in the treatment effect estimate.

Four results stand out as being quantitatively important and robust across the three sample. First, we find that a key feature of the experimental design, its delivery method, matters for compliance. Table 5 suggests that interventions delivered by in-person visits to taxpayers relative to nudges delivered through physical letters raise compliance by 11 percentage point at the extensive and by twice at the intensive margin. The full sample analysis suggests that interventions studying in-person visits are more likely to find significant treatment effect with larger t-values than interventions delivered through letters.

Second, we find that nudges are more effective when addressing sub-sample of taxpayers who missed their deadline of paying taxes. The magnitude of the effect is 3 percentage point at the extensive margin and is over 100 percentage points on the intensive margin. This finding remains robust at the full sample, in general suggesting that late-payers are more sensitive to nudges.

Third, Table 5 shows evidence for the hypothesis that the treatment effects are stronger statistically as well as in magnitude in the short-run compared to the long-run. At the extensive margin, taxpayers whose compliance is measured within 12 months after the interventions are 4 percentage point more likely to comply with taxes than taxpayers whose compliance is measured after 12 months of the intervention. The directions of the point estimates of intensive margin responses and at the full samples are consistent with this interpretation, but are small and statistically not distinguishable from zero. However, the results of Table A5 provide additional support to this interpretation.

Fourth, when comparing experimental results across countries where the RCTs were conducted, we find that experiments seem to be more effective in high-income countries compared to low- as well as middle-income countries. This finding may reflect the fact that lower-income countries have to in general operate in much lower compliance environments such that letters hardly change taxpayer behavior. However, the result may also simply be driven some other endogenous variable correlated with development.

The finding about the null effect of a baseline non-deterrence nudge as well as the results about four important variables explaining the heterogeneity of treatment effects are in general very robust to the alternative estimation methods presented in Table A5. We note, however, that these results represent correlations and can be driven by other factors unobservable to us that happen to coincide with these characteristics.

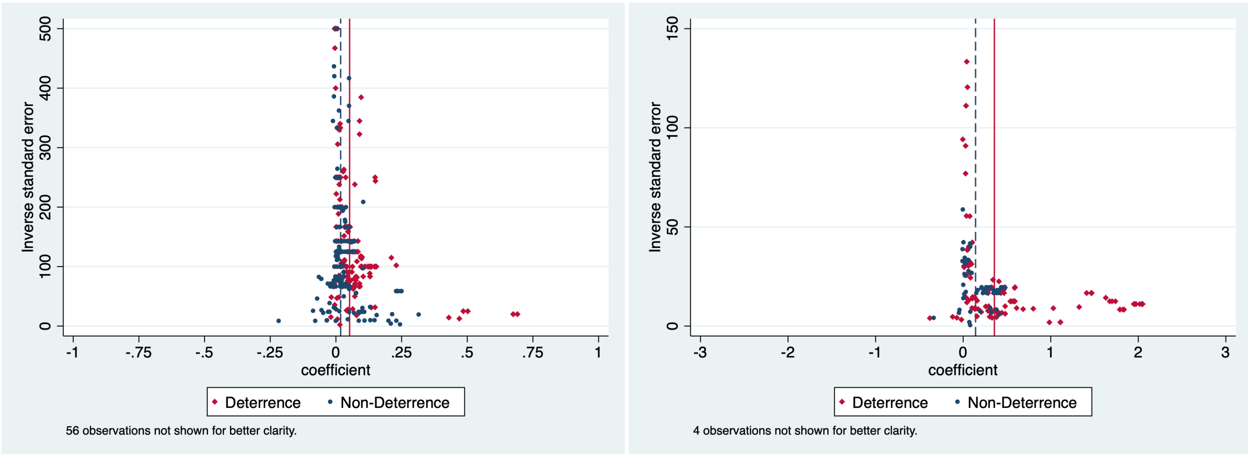
Finally, in Table A6 of the Appendix we interact all these baseline and additional characteristics, as discussed in Section 3.3, with the types of nudges. The idea is to test whether the effects we find here hold generally for all nudge types or if they are mainly driven by deterrence nudges. The evidence of Table A6 in general does not support the hypothesis that the effects of these characteristics are driven by a particular type of a nudge.

5.5 Publication selection bias

One standard question often discussed in the meta-analytical literature is that the estimated treatment effects shown in the primary studies are systematically biased towards positive and significant effects. The underlying hypothesis is that researchers tend to present results that show: i) positive effects because it is generally believed that nudges should only have positive effects (file drawer bias), and ii) statistically significant effects because of the belief that non-significant effects are harder to publish (p-hacking). Additionally, we ask whether such potential biases can be different for deterrence and non-deterrence nudges, and thus potentially explaining our result that deterrence nudges are more effective than non-deterrence ones.

Figure 6: Funnel plots

(a) Extensive margin **(b)** Intensive margin

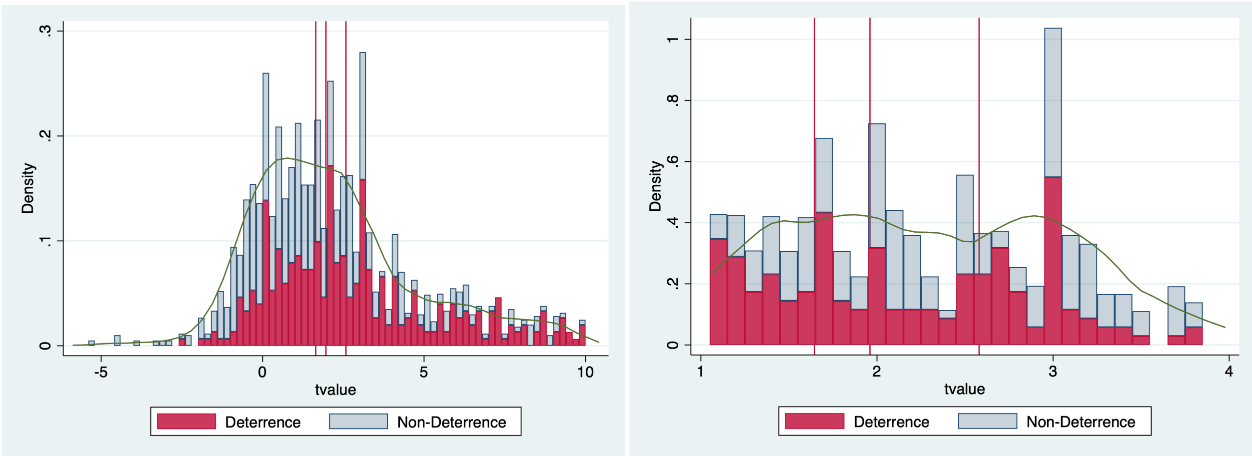


Notes : The blue circles plot deterrence and the red diamonds plot the non-deterrence nudges. Blue dashed and red undashed vertical lines show precision weighted means of the treatment effect for the two nudges, respectively. For visual clarity, sub-figure (a) drops 56 outlier observations that are larger than 500 on the y-axis, and sub-figure (b) drops 4 obervations that are larger than 5 on the x-axis.

Funnel plots are a common way of visually diagnosing meta-datasets for the file drawer bias. These plots provide visual checks for asymmetries in the relation between treatment effect magnitudes and measures of their precision. The idea is that absent publication bias very imprecise estimates should be randomly distributed around zero rather than being skewed to the right. We present funnel plots for our extensive and intensive margin samples in Figures 6 (a) and (b), where the x-axis plots the size of the treatment effect and the y-axis plots the inverse standard error of the treatment effects as a measure of precision. The vertical lines show the precision weighted means of the treatment effect. The figures separately plot deterrence and non-deterrence nudges. In both the extensive and intensive margin samples we observe that the imprecisely estimated treatment effects, i.e. those in the bottom of the funnel plot, tend to be skewed towards positive values. This visual evidence speaks for the presence of file drawer type bias in our general sample. We do not find striking evidence that the bias is present for one and absent in the second type of nudge that we study.[28]

[28] More formally, we use the method of Egger et al. (1997) to test for funnel-plot asymmetry in our meta samples. In the extensive margin sample, we fail to reject the null hypothesis that there is no publication bias for both deterrence as well as non-deterrence nudges. In contrast in the intensive margin sample, we reject the null seperately for both deterrence and non-deterrence nudges. Thus we continue maintaining the hypothesis that at least part of our sample has publication bias.

Figure 7: Distribution of t-values in the full sample



Notes : Figures plot histograms of t-values of treatment effects of deterrence and non-deterrence nudges stacked together. The kernel density line is estimated for the total sample of both nudges according to an Epanechnikov function. For visual clarity, the left sub-figure drops 117 outlier observations that lie outside the (-10, 10) range. The right sub-figure plots the same data as in the left sub-figure but zooming in to the range (1 , 4). Vertical lines denote critical values for two-sided significance tests at t-values of 1.645, 1.96 and 2.58.

One approach used to study p-hacking type of bias is to check for unusual patterns around critical values in the distribution of t-values. Such evidence is presented by Brodeur et al. (2016). The paper uses a large data comprising of tests published in top economics journals, and shows a disproportionately large share of tests that narrowly reject the null hypothesis. We follow this test and plot the distribution of t-values in Figure 7. We are working with much small sample sizes to make definitive conclusions, but this evidence suggest some bunching in the number of observations of t-values situated just right to the three critical values which are denoted by vertical lines. We also observe corresponding missing masses on the left sides of the critical values. This evidence suggests that part of the studies in our sample select to report results that are statistically significant at conventional level, and ignore treatment effect estimates that narrowly miss to reject the null hypothesis. Figure 7 shows the relative contribution of deterrence and non-deterrence nudges to bunching at critical values. Although, again the sample is too small to make definitive conclusions, we do not see overwhelming evidence that one nudge types dominates the other type at close vicinity to critical values.

Thus, we do not think that p-hacking bias may completely explain away explain the earlier result that deterrence nudges are more effective than non-deterrence nudges.

Third, a related but separate idea is that, in addition to researchers selecting to report stronger results in working papers, the publication process will either exacerbate or mitigate this selection (see, e.g., Andrews and Kasy 2019). Both directions of selection are plausible. If journals have preferences similar to those of researchers, the publication process may amplify the selection bias. On ther other hand, if the peer-review process effectively serve as a check against the behavior of researchers to report results selectively, the publication process will mitigate the selection effect. In either case, this discussion leads to the testable hypothesis that biased significant and positive treatment effects in working paper versions of studies will tend to different in published studies. We test this hypothesis by including a dummy for the publication status in Tables 5 and A5. We do not find evidence that either the magnitude or the statistical significance of treatment effects are different in working papers as compared to published papers. In Table A6 we fail to find any evidence for the hypothesis that this specific type of bias is more pronounced in one of the two types of nudges of interest.

Overall we conclude that our sample is likely to be biased both due to file drawer as well as to p-hacking type bias. We do not find evidence for positive or negative selection bias at the publication stage, and we find it unlikely that the existing biases can explain the difference in the effects of deterrence and non-deterrence nudges that we document. In general, this evidence for selective reporting of results that we find is similar to the findings of many other meta-analytical applications in economics. This suggests that empirical studies implementing RCTs, which are otherwise believed to have relatively sound methodologies, are not immune to biased reporting of results.[29]

[29]See Brodeur et al. (2018) for evidence on how publication selection bias differs by the identification method used.

6 Conclusions

Policy interventions that nudge taxpayers with the aim of increasing compliance have become an attractive tool among many governments due to their ease of implementation and low monetary costs. This easy adoption of the policy is demonstrated, for example, by Hjort et al. (2019) who inform Brazilian mayors about research on the positive tax compliance effects of reminder letters in an experimental setting 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 summarize the knowledge accumulated so far from 45 nudging interventions in a systematic way. We show that, unlike the general excitement over nudges in policy and academic circles, communications informing taxpayers about the morale aspects of paying taxes are not very effective in increasing compliance. Although, nudges that threaten taxpayers with audit probabilities and other elements of deterrence can be effective, the magnitudes of these effects are fairly small and are likely to be bound to the short-run.

Our evidence in general warns against the widespread and unconditional adoption of tax nudges in practice. However, this is not to say that nudges are useless. Our evidence on the particular design features of interventions that make them more effective (e.g., sending deterrence rather than only non-deterrence letters) combined with the identification of the sub-populations of taxpayers that are likely to be more sensitive to nudges (e.g., focusing on late-payers) provide guidance for potentially more effective policy interventions in the future. Note that the nudges we study are arguably the most common types of behavioral interventions, 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. (2019) and Dwenger and Treber (2018),[30] 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 a broader sense of the word.

[30] For a welfare analysis of a wide class of social recognition policies, see, Butera et al. (2019).

This review also highlights a number of opportunities for researchers by directing attention towards gaps in the literature where the evidence has been weak so far. For example, only few papers test whether nudges work in the longer run, and when implemented repeatedly. Evidence on the question of whether the strength of deterrence (e.g., different audit probabilities or fine rates) and non-deterrence (e.g., different degrees of public goods) nudges matters is also lacking. Importantly, we do not have much knowledge on whether 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 due to methodological concerns in comparing different experiments. Such an analysis in our paper would be additionally constrained due to the fact that interventions so far have mainly focused on Europe and the Americas leaving us with little cross-sectional variation to exploit. Future interventions, possibly ones that span across borders, could try to study i) whether non-deterrence nudges work more effectively in contexts of higher levels of trust, and ii) if deterrence nudges work better in uncorrupt environments where audits can be enforced more credibly compared to institutionally less mature environments.

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Appendix

Table A1: Summary statistics

	Full sample		Extensive margin		Intensive margin	
Variable	Obs	Mean	Obs	Mean	Obs	Mean
Dependent variable						
Treatment effect	979	4.954	471	0.054	174	0.633
Nudge type						
Deterrence (0) and non-det. (1)	979	0.486	471	0.495	174	0.310
Baseline characteristics						
Baseline comparison	977	1.309	471	1.344	174	1.454
Publication status	979	0.454	471	0.561	174	0.402
Additional characteristics						
Delivery method	979	0.403	471	0.344	174	0.391
Tax type	979	1.539	471	1.599	174	1.891
Response horizon	965	0.892	467	0.891	174	0.851
Taxpayer type	979	0.726	471	0.643	174	0.897
Late payer sample	974	0.454	471	0.584	174	0.609

Table A2: Baseline effects of nudges: Robustness to methods

	WLS(se)		WLS(n)	Random Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
	Extensive	Intensive	t-value	Extensive	Intensive	t-value
Baseline effect	0.010	0.067	1.644	0.027***	0.783***	3.605
	(0.011)	(0.155)	(1.192)	(0.007)	(0.188)	(2.297)
Treatment (omitted: Non-Deterrence)						
Deterrence	0.026*	0.126	1.517	0.036***	0.224	5.473**
	(0.010)	(0.147)	(1.098)	(0.007)	(0.117)	(1.825)
Baseline Comparison (omitted: Baseline Info)						
No Info	0.007	-0.075	1.729	0.015	-0.014	1.304
	(0.012)	(0.124)	(1.066)	(0.008)	(0.141)	(2.144)
Letter	-0.023*	-0.468	-1.196	-0.027	-0.915**	-4.716
	(0.009)	(0.361)	(0.889)	(0.017)	(0.296)	(5.045)
Publication Status (omitted: Published)						
Published	0.006	0.351	-0.367	-0.021**	-0.676***	-3.811*
	(0.015)	(0.350)	(1.454)	(0.007)	(0.147)	(1.831)
Study FE	No	No	No	No	No	No
Observations	447	174	969	447	174	955
R^2	0.188	0.090	0.035	0.231	-0.105	0.374

Standard errors in parentheses

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

Regressions are estimated according to Equation 1.

Table A3: Deterrence and non-deterrence nudges: Robustness to methods

	WLS(se)		WLS(n)	Random Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
	Extensive	Intensive	t-value	Extensive	Intensive	t-value
Treatment (omitted: Non-Deterrence)						
Deterrence	0.015**	0.059**	1.896*	0.026***	0.101	1.482
	(0.005)	(0.020)	(0.737)	(0.007)	(0.083)	(1.635)
Constant	-0.016**	0.012	-1.062	-0.027	0.008	-0.647
	(0.005)	(0.014)	(0.737)	(0.034)	(0.104)	(6.812)
Study FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	447	174	971	447	174	957
R^2	0.519	0.563	0.461	0.517	0.735	0.674

Standard errors in parentheses

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

Regressions are estimated according to Equation 2.

Table A4: Types of nudges: Robustness to methods

	WLS(se)		WLS(n)	Random Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
	Extensive	Intensive	t-value	Extensive	Intensive	t-value
Treatment (omitted: Reminder, Information and Omission)						
Deterrence	0.009	-0.013	0.530	0.015	0.014	0.338
	(0.007)	(0.032)	(1.165)	(0.011)	(0.120)	(2.574)
Deterrence Interacted	0.019*	0.185*	1.100	0.029*	0.154	2.029
	(0.008)	(0.064)	(1.677)	(0.013)	(0.151)	(3.451)
Public Good	-0.011	-0.090	-2.480	-0.016	-0.105	-1.453
	(0.010)	(0.064)	(1.704)	(0.012)	(0.149)	(2.914)
Peer Effect	-0.005	-0.130**	-1.354	-0.007	-0.128	-1.164
	(0.008)	(0.036)	(1.557)	(0.012)	(0.213)	(3.431)
Moral Appeal	-0.001	-0.046	-1.374	-0.009	-0.059	-0.957
	(0.007)	(0.029)	(1.380)	(0.016)	(0.226)	(3.807)
Other	-0.005		-2.765	-0.012		-1.872
	(0.009)		(2.076)	(0.017)		(5.514)
Constant	-0.010	0.090*	0.305	-0.016	0.100	0.496
	(0.007)	(0.039)	(1.165)	(0.035)	(0.146)	(7.180)
Study FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	447	174	971	447	174	957
R^2	0.524	0.570	0.466	0.500	0.744	0.665

Standard errors in parentheses

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

Regressions are estimated according to Equation 2.

Table A5: Heterogeneity of results: Robustness to methods

	WLS(se)		WLS(n)	Random Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
	Extensive	Intensive	t-value	Extensive	Intensive	t-value
Treatment (omitted: Non-Deterrence)						
Deterrence	0.009	0.058*	1.665	0.014*	0.042	1.402*
	(0.005)	(0.026)	(0.871)	(0.006)	(0.059)	(0.676)
Baseline Comparison (omitted: Baseline Info)						
No Info	0.019	0.303	3.740*	0.024*	-0.203	2.889**
	(0.015)	(0.245)	(1.540)	(0.010)	(0.194)	(1.033)
Letter	-0.034***	-0.157	-0.508	-0.041***	-0.657*	-5.652**
	(0.008)	(0.323)	(2.075)	(0.012)	(0.257)	(1.881)
Publication Status (omitted: Published)						
Unpublished	0.025*	0.579*	1.355	0.016**	0.339*	1.322
	(0.010)	(0.253)	(0.922)	(0.006)	(0.131)	(0.753)
Delivery (omitted: Physical Letter)						
Digital Letter	0.025	0.836*	-2.217	0.024**	1.098***	1.776
	(0.027)	(0.367)	(1.735)	(0.009)	(0.125)	(0.978)
In Person	0.059***	1.211***	6.463	0.063***	1.061***	13.398***
	(0.003)	(0.085)	(3.640)	(0.011)	(0.104)	(1.294)
Tax Type (omitted: Corporate Tax)						
Income Tax	-0.001	0.688*	0.476	-0.014	0.388**	2.156*
	(0.009)	(0.260)	(1.577)	(0.010)	(0.127)	(1.068)
Property Tax	0.002	0.380	1.961	-0.028**	0.089	2.101*
	(0.018)	(0.270)	(2.067)	(0.010)	(0.164)	(1.061)
VAT	-0.002	1.091***	0.974	-0.023	0.866***	1.963
	(0.020)	(0.233)	(1.615)	(0.015)	(0.141)	(1.268)
Other	0.034**	1.074***	4.896	0.028*	0.816***	11.771***
	(0.010)	(0.219)	(3.119)	(0.011)	(0.122)	(1.234)
All	0.066***	1.057***	4.235	0.040	0.771***	4.898
	(0.010)	(0.223)	(2.241)	(0.137)	(0.207)	(3.915)
Response Horizon (omitted: Short Run)						
Long Run	-0.025*	-0.163	-0.327	-0.038***	-0.211	-1.355
	(0.012)	(0.096)	(1.058)	(0.008)	(0.110)	(1.044)
Late-payer sample (omitted: Late)						
On Time	-0.018***	-0.511***	-5.899***	-0.019**	-0.470***	-6.227***
	(0.004)	(0.119)	(1.631)	(0.007)	(0.111)	(0.769)
Development Level (omitted: High Income)						
Low Income	-0.041*	-0.324	0.165	-0.045***	-0.306*	-2.742**
	(0.015)	(0.159)	(1.689)	(0.009)	(0.131)	(1.000)
Middle Income	-0.013	-0.545**	-0.553	-0.015*	-0.732***	-0.332
	(0.012)	(0.137)	(1.331)	(0.007)	(0.189)	(0.850)
Constant	0.016	-0.408	1.762	0.042***	0.325	1.990
	(0.011)	(0.550)	(2.045)	(0.011)	(0.338)	(1.412)
Study FE	No	No	No	No	No	No
Observations	443	174	955	443	174	941
R^2	0.579	0.626	0.274	0.798	0.775	0.952

Standard errors in parentheses

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

Regressions are estimated according to Equation 3.

Table A6: Heterogeneity of results behind deterrence and non-deterrence nudges

	(1) Extensive	(2) Intensive	(3) t-value
Treatment (omitted: Non-Deterrence)			
Deterrence	-0.017	-1.974**	-3.598
	(0.059)	(0.647)	(3.715)
Baseline Comparison (omitted: Baseline Info)			
No Info	0.043*	0.048	1.131
	(0.021)	(0.079)	(0.761)
Letter	-0.055*	-0.025	-6.770**
	(0.022)	(0.113)	(2.275)
Unpublished	0.012	0.229***	2.086**
	(0.014)	(0.041)	(0.722)
Delivery (omitted: Physical Letter)			
Digital Letter	-0.001	1.212***	2.206
	(0.034)	(0.220)	(1.579)
In Person	-0.025	0.816***	3.961**
	(0.048)	(0.037)	(1.424)
Tax Type (omitted: Corporate Tax)			
Income Tax	-0.057	0.116**	-0.814
	(0.049)	(0.036)	(0.928)
Property Tax	-0.085	0.333	-1.123
	(0.051)	(0.378)	(1.175)
VAT	-0.032	0.168***	-0.193
	(0.033)	(0.039)	(0.810)
Other	0.032	0.117***	5.769
	(0.077)	(0.026)	(3.763)
All	-0.007	0.103*	0.532
	(0.049)	(0.039)	(1.021)
Response Horizon (omitted: Late)			
Short Run	0.036**	0.107	2.012***
	(0.013)	(0.107)	(0.539)
Late-payer sample (omitted: On Time)			
Late	0.029	0.175*	3.842**
	(0.016)	(0.062)	(1.112)
Middle Income	0.036	0.309***	2.549
	(0.042)	(0.073)	(1.482)
High Income	0.058	0.418***	3.576*
	(0.041)	(0.037)	(1.551)
Deterrence × No Info	-0.040	0.457	2.389
	(0.026)	(0.537)	(1.811)
Deterrence × Letter	0.015	0.074	4.532
	(0.029)	(0.562)	(3.086)
Deterrence × Unpublished	-0.006	0.804**	-1.310
	(0.018)	(0.207)	(1.314)
Deterrence × Digital Letter	0.046	0.000	-0.160
	(0.043)	(.)	(1.784)
Deterrence × In Person	0.187***	1.966***	12.998**
	(0.051)	(0.197)	(3.837)
Deterrence × Income Tax	0.063	0.573	4.699**
	(0.053)	(0.427)	(1.619)
Deterrence × Property Tax	0.098	0.000	6.034**
	(0.056)	(.)	(2.195)
Deterrence × VAT	0.030	1.405***	3.312
	(0.036)	(0.320)	(1.819)
Deterrence × Other	-0.000	1.474***	5.530
	(0.079)	(0.242)	(4.165)
Deterrence × All	0.069	1.421***	6.635**
	(0.054)	(0.267)	(2.144)
Deterrence × Short Run	0.006	-0.066	-1.439
	(0.030)	(0.273)	(1.451)
Deterrence × Late	0.002	0.973***	3.684**
	(0.021)	(0.204)	(1.234)
Deterrence × Middle Income	-0.007	-0.204	0.072
	(0.046)	(0.337)	(1.818)
Deterrence × High Income	-0.027	0.688**	-0.646
	(0.045)	(0.231)	(2.143)
Constant	-0.031	-0.623***	-5.221*
	(0.042)	(0.022)	(2.007)
Observations	467	174	963
R^2	0.385	0.496	0.407

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation 3.