Tax Audits as Scarecrows

Evidence from a Large-Scale Field Experiment

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This Draft: May 2021. First Draft: April 2017.

Abstract

The canonical model of Allingham and Sandmo (1972) predicts that firms evade taxes by optimally trading off the costs and benefits of evasion. However, there is no direct evidence that firms react to audits in this way. We conducted a large-scale field experiment in collaboration with Uruguay's tax authority to address this question. We sent letters to 20,440 small- and medium-sized firms that collectively paid more than 200 million U.S. dollars in taxes per year. Our letters provided exogenous yet nondeceptive signals about key inputs for their evasion decisions, such as audit probabilities and penalty rates. We measured the effect of these signals on their subsequent perceptions of the auditing process using survey data, as well as on the actual taxes paid using administrative data. We find that providing information about audits had a significant effect on tax compliance but in a manner that was inconsistent with Allingham and Sandmo (1972). Our findings are consistent with an alternative model of risk-as-feelings, in which messages about audits generate fear and induce probability neglect. According to this model, audits may deter tax evasion in the same way that scarecrows frighten off birds.

JEL Classification: tax, evasion, audits, penalties, frictions.

Keywords: C93, H26, K34, K42, Z13.

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*Corresponding author: ricardotruglia@berkeley.edu, 545 Student Services Building #1900, Berkeley, CA 94720. We thank the Uruguay's national tax administration (Dirección General Impositiva) for their collaboration. We thank Gustavo Gonzalez for his support, without which this research would not have been possible. We thank Joel Slemrod for his valuable feedback, as well as that of seminar participants at University of Berkeley, University of Michigan, University of California San Diego, Dartmouth University, Universidad Di Tella, Universidad de la Republica, Universidad de Santiago de Chile, Universidad Católica de Chile, UADE, Corporación Andina de Fomento (Buenos Aires), Banco Central del Uruguay, LACEA 2017, the 2017 NBER Public Economics Fall Meeting, the 2017 RIDGE Public Economics Conference, the 2017 Zurich Center for Economic Development Conference, the 2017 Advances with Field Experiments Conference, the 2018 PacDev Conference, the 2018 AEA Annual Meetings, the 2018 LAGV Conference, the 2018 IIPF Annual Congress, the 2019 LACEA BRAIN Conference and the 2019 National Tax Association meeting. This project benefited from funding by CEF, CEDLAS-UNLP and IDRC. The AEA RCT registration number is AEARCTR-0004593.

1 Introduction

Tax audits are a standard tool that most tax administrations have used throughout history. Audits increase tax revenues directly, because firms caught evading must pay taxes on the hidden income and corresponding penalties. However, with the exception of large taxpayers, these direct revenues are insufficient to make audits cost-effective. Audits play a central role in the deterrence paradigm of tax evasion: the threat of being audited in the future, being caught evading and having to pay penalties deters firms from evading taxes in the present.

Audits may be useful in fostering tax compliance, but there is no direct evidence on how firms react to them. The Allingham and Sandmo (1972) model (hereafter referred to as *A&S*) is the canonical model of tax evasion in economics. This model is an application of Becker (1968), in which selfish individuals choose whether to engage in criminal activities based on the trade-off between expected costs and benefits. In *A&S*, firms choose the optimal amount of income to hide from the tax authority so that the marginal benefits (i.e., the lower tax burden) equal the marginal costs (i.e., the penalties they will be required to pay if caught). This intuition is so deeply ingrained in economic thought that most economists take it for granted. However, surprisingly little causal evidence exists on whether real firms react to audits in this profit-maximizing fashion (Alm et al., 1992; Luttmer and Singhal, 2014; Slemrod, 2018). In this study, we provide direct tests of the *A&S* model based on a high-stakes, large-scale field experiment.

We study small- and medium-sized firms in Uruguay that are subject to the Value Added Tax (VAT), which provides a context in which taxpayers should care about the threat of being audited. This is not always the case: tax agencies can sometimes use third-party reporting to automatically detect and rectify tax evasion regardless of whether the taxpayer is audited or not, thus making the audit threat irrelevant. For instance, the U.S. Internal Revenue Service uses their electronic records to compare the wage amount reported by an individual to the amount reported by the individual's employer. This algorithm automatically rectifies the discrepancies in reporting and sends a notification to the taxpayer with the updated tax amount to be paid. Because the evasion will be caught through the third-party reporting regardless of whether the individual is audited or not, taxpayers should not care about the threat of being audited in such a context (Kleven et al., 2011). On the contrary, in our context of the VAT in a developing country, such automatic cross-checking and rectification does not exist. The VAT paper trail, which consists of non-electronic invoices, can only be scrutinized in the event of an audit.[1] Thus, tax authorities must still rely heavily on the

[1] While the VAT requires a paper trail, which is a form of third-party reporting, this paper trail is subject to significant limitations. Most importantly, there is no simple algorithm that automatically detects tax evasion. Moreover, the paper trail breaks down when reaching the consumer (Naritomi, 2019). Finally, firms

threat of audits to discourage VAT evasion (Gomez-Sabaini and Jimenez, 2012; Bergman and Nevarez, 2006).

We collaborated with Uruguay's Internal Revenue Service (hereafter referred to as 'IRS') to conduct a natural field experiment with a sample of 20,440 small- and medium-sized firms that are subject to the VAT. For our study, the IRS mailed four different types of letters with information about audits to the owners of each of these firms.[2] Some of the information contained in each of these letters was randomly assigned, with the goal of testing predictions of *A&S*. Using IRS administrative records, we measured the subsequent effects of the information contained in the letters on the firms' compliance with the VAT and other tax liabilities in the following year. Additionally, we collaborated with the IRS to conduct a post-mailing survey to capture the effect of this information on these firms' subsequent perceptions about audits.

The first part of the experimental design, following the seminal work by Slemrod et al. (2001), measures how informing taxpayers about tax enforcement affects their tax compliance. Firms were randomized into four different letter types: *baseline*, *audit-statistics*, *audit-endogeneity*, and *public-goods*. The *baseline* letter type included brief and generic tax information that the IRS often includes in its communications with firms. The *audit-statistics* letter type was identical to the *baseline* letter, with additional information about the probability of being audited and the penalty rate based on tax administration statistics. The relevant hypothesis is that adding the *audit-statistics* message to the *baseline* letter will deter tax evasion and thus increase post-treatment tax payments. We also can compare the effects of this *audit-statistics* message with the effects of other types of messages. The *audit-endogeneity* letter type provided information about a different feature of the auditing process. It was identical to the *baseline* letter, with an additional message about how evading taxes increases the probability of being audited. The *public-goods* letter type was designed to provide a benchmark for a message that might increase tax compliance but does not provide information about tax audits. It was identical to the *baseline* letter, with an additional message describing the social costs of evasion by detailing the set of public goods that could be provided if tax evasion was lower.

We show that, consistent with Slemrod et al. (2001) and the subsequent literature, informing firms about tax enforcement increases their tax compliance. We find that adding the *audit-statistics* message to the *baseline* letter increases tax payments by about 7.0% in the first post-treatment year; in the second post-treatment year the effects are still present,

can also collude with each other to tamper with the paper trail (Pomeranz, 2015).

[2]Throughout the paper, for simplicity, we refer to firms' perceptions and behavior as a shorthand for the perception and behavior or the firms' owners or managers.

but they are half as large and no longer statistically significant. This effect is economically significant: the estimated average VAT evasion rate in Uruguay is 26% (Gomez-Sabaini and Jimenez, 2012). While the tax base is not necessarily fully comparable, this figure implies that the 7.0% increase equates to a 27% reduction in VAT evasion. The effect of the *audit-statistics* message (increased tax payments 7.0% in the first year) is similar in magnitude to the effect of the other message related to tax audits (7.1%, for *audit-endogeneity*) and larger and more persistent than the effect of the *public-goods* message (5.1% in the first year, but negligible in the second year).

The second and most important part of the experimental design tests the hypothesis that firms react to information about audits as predicted by *A&S*. We provide two tests of *A&S*. The first test exploits survey data on perceptions about audits. If the *audit-statistics* letter increased average compliance, to be consistent with *A&S*, it must be true that this message increased the perceived probability of being audited or the perceived penalty rate. To test this hypothesis, we designed a survey, which was sent months after the firms received the *audit-statistics* and *audit-endogeneity* letters, which elicited perceptions about the probability of being audited and the penalty rate.

The second test of *A&S* is based on heterogeneity in the signals provided in the letters. We included exogenous, non-deceptive variation in the information about audit probabilities and penalty rates in the *audit-statistics* letter. To generate this information, we computed the average probabilities and penalty rates using a series of random samples of 50 firms. This sample size was small enough to introduce non-trivial sampling variation in the average probabilities and fines shown to the subjects. Specifically, a given firm could receive a letter saying that the audit probability is 8%, 10%, or 15%, depending on the sample of similar firms chosen for that particular letter. These random variations in probabilities and penalties shown to the firms allow us to test whether firms evade less when they face higher audit probabilities and higher penalty rates, as predicted by *A&S*.

The second part of the results suggests that the effects of the *audit-statistics* letter are not consistent with *A&S*. The results for the first test, based on the survey data, indicate that the *audit-statistics* message reduced the perceived probability of being audited. According to *A&S*, a reduction in the perceived probability of being audited should have reduced tax compliance. On the contrary, we find that the *audit-statistics* message increased average compliance.

The second test shows that, contrary to the *A&S* prediction, the effect of the *audit-statistics* message does not change with the signals of audit probability and penalty rates included in the letter. The estimated elasticity of tax compliance with respect to audit probabilities and penalty rates is close to zero and precisely estimated. Moreover, we compare our experimental estimates to the results from calibrations of *A&S*. We reject the null hypothesis of *A&S* even under conservative assumptions about how much firms learned from the *audit-statistics* message. These results suggest the presence of probability neglect – i.e., that firms react similarly to the threat of being audited regardless of the actual probability of that happening or the penalties involved.

As a complement to the *audit-statistics* treatment arm, we designed a separate treatment arm that created exogenous variation in expected audit probabilities in a more direct way. The *audit-threat* letter type was sent to a separate sample of firms that were pre-selected by the IRS for auditing. We randomly divided this set of firms into two groups, one with a 25% probability of being audited and the other with a 50% probability of being audited. The *audit-threat* letter informed firms of the audit probability that was assigned to them. Consistent with the *audit-statistics* treatment arm, we find probability neglect in the *audit-threat* arm too.

In sum, we find that informing firms about tax audits increased their tax compliance, but this reaction to the information was inconsistent with the optimal reaction predicted by *A&S*. On average firms reduced, rather than increased, their perceived probability of being audited. Furthermore, firms did not react more when facing a higher probability of being audited or a higher penalty rate. These results suggest that firms may comply with taxes because of the threat of being audited but not necessarily in the optimal way as predicted by *A&S*.

This leaves open the question of which alternative model best explains the firms' reactions to audits. Models of salience (Chetty et al., 2009) and prospect theory (Kahneman and Tversky, 1979) can explain some, but not all, findings. Agency issues and information frictions within VAT-paying firms subject to third-party reporting have implications for evasion that differ from the context of individuals paying an income tax, as highlighted in recent models of firm evasion (Kleven et al., 2016), but these issues are not consistent with our evidence either. Instead, our preferred interpretation is based on the model of risk-as-feelings (Loewenstein et al., 2001). The models used for choice under risk are typically cognitive; that is, people make decisions using some type of expectation-based calculus. The risk-as-feelings model proposes that responses to fearsome situations may differ substantially from cognitive evaluations of the same risks. When fear is involved, the responses to risks are quick, automatic and intuitive, and thus neglect the underlying probabilities (Sunstein, 2003; Zeckhauser and Sunstein, 2010). This model of risk-as-feelings can reconcile all of our key findings. Moreover, we present anecdotal and survey evidence indicating that the fear of being audited does indeed play a significant role in tax compliance. We also discuss policy implications for increasing tax capacity.

Our study relates to various strands of literature. First, it belongs to a recent but growing literature that uses field experiments in partnership with tax authorities to study the decisions of individuals to pay taxes. In a seminal contribution, Slemrod et al. (2001) showed that, for a sample of U.S. self-employed individuals, those who were randomly assigned to receive a letter from the Minnesota Department of Revenue with an enforcement message reported higher income in their tax returns. Similar messages about tax enforcement have been shown to have positive effects on tax compliance in other contexts (for recent reviews, see Pomeranz and Vila-Belda, 2018; Slemrod, 2018; Alm, 2019).[3] One standard interpretation in this literature is that taxpayers react to the information about tax enforcement tools and, in line with *A&S*, reduce their evasion to re-optimize their behavior. However, there is no direct evidence in favor of or against this interpretation. Our contribution is to fill this gap in the literature.

This paper is closely related to a group of studies testing the predictions of *A&S* in a laboratory setting. For example, Alm et al. (1992) conducted a laboratory experiment in which undergraduate students play a tax evasion game. Subjects can hide income from the experimenter, but some subjects are randomly selected to be audited and must pay a penalty if they are caught evading. The authors show that tax compliance in the game increases significantly with audit and penalty rates, but these effects are economically small and smaller than those predicted by optimizing behavior in the context of *A&S*. The laboratory experiment setting of Alm et al. (1992) and similar studies have a number advantages, such as their full control over the rules of the game and freedom in the selection of the model parameters. However, these laboratory experiments have two main limitations. First, the subjects are typically undergraduate students playing the tax game for the first time and with no prior experience of paying taxes in the real world. In contrast, subjects in our field experiment are experienced firm owners who have been registered with the tax agency, and thus paying taxes, for an average of 15 years. Second, subjects from laboratory experiments typically pay taxes in the game amounting to less than USD 10. In contrast, subjects in our field experiment paid USD 11,800 per year in taxes on average, which is in the same order of magnitude as the country's GDP per capita.[4] We contribute to this literature in two ways. First, we show that *A&S* does not fare substantially better in a natural context with experienced subjects and high stakes. Second, we show that audit threats may still be useful for the tax agency even if the individuals do not react optimally to them.

Our findings also contribute to the more general debate about the determinants of tax

[3] The following are some examples: Slemrod et al. (2001); Kleven et al. (2011); Fellner et al. (2013); Pomeranz (2015); Castro and Scartascini (2015); Dwenger et al. (2016); Perez-Truglia and Troiano (2018).

[4] More specifically, the firms in our sample paid an average of USD 7,770 in VAT and USD 4,070 in other taxes in the 12 months before our experiment. In comparison, the GDP per capita in Uruguay was about USD 15,000 in 2015.

compliance. One of the main puzzles in the literature is that evasion rates seem too low, given the low detection probabilities and penalty rates, especially among smaller firms and self-employed individuals (Luttmer and Singhal, 2014). One traditional explanation for this puzzle is based on tax morale: firms and individuals do not evade taxes because they feel morally obliged to comply (Luttmer and Singhal, 2014). Our evidence suggests an alternative explanation for the puzzle: due to the emotional nature of the decision, taxpayers overreact to the threat of audits. In other words, audits may scare taxpayers into compliance in the same way that scarecrows scare birds. Indeed, this interpretation can provide an explanation for the paradox that, despite the low audit probabilities and penalty rates, most taxpayers report the threat of audits as a major reason for why they report their taxable income truthfully (United States Internal Revenue Service, 2018).

The paper is organized as follows. Section 2 discusses the experimental design. Section 3 presents the data sources and discusses the implementation of the field experiment. Section 4 presents the results on the average effect of the *audit-statistics* message, and Section 5 presents the two tests of *A&S*. Section 6 discusses the interpretation of the findings. The final section concludes.

2 Experimental Design

Our experiment consisted of a mailing campaign from Uruguay's IRS with multiple treatment arms and sub-treatments. Rather than comparing firms that received a letter to firms that did not, all of our analyses are based on comparisons between firms that received letters with subtle variations in their content. We can thus control for the potential effects of simply receiving a letter from the tax authority, which might induce compliance on its own – for instance, as a reminder to report taxable income.

The letters consisted of a single sheet of paper with the name of the recipient in the header, the official letterhead of the IRS, and the scanned signature of the IRS General Director. These letters were folded, sealed in an envelope with the official letterhead of the IRS on the outside, and sent by certified mail, which guarantees direct delivery to the recipient, who must sign upon receipt. Panel (a) of Figure 1 presents a diagram with the sample sizes for the different treatment arms that are detailed below.

2.1 *Baseline* Letter

The *baseline* letter contained some information about the goals and responsibilities of the tax authority, which the IRS routinely includes in its communications with firms. It explained that the individual was randomly selected to receive this information, that the letter was for informational purposes only, and that there was no need to reply or to provide any documentation to the IRS. Figure 2 provides a sample of the *baseline* letter, with the addition of a placeholder box with the word "MESSAGE" written inside it.[5] This box was empty in the *baseline* letter but it contained a different message (printed in larger type size and boldface) in each of the other letter types.

2.2 ***Audit-Statistics*** Letter

According to the Allingham and Sandmo (1972) model, we expect risk-averse firms to be interested in information about the audit process, because it helps them optimize their evasion decisions and potentially increase their bottom line.[6] Furthermore, these figures should be particularly valuable in a context where information about audits is limited. For instance, it is easy to find online data about factors potentially relevant for firms' decision-making, such as prices, inflation and exchange rates. However, information about tax audit probabilities (and, to a lesser extent, actual penalties paid by evading firms) is much harder to come by. Tax authorities seem to prefer to conceal this information.

In the *audit-statistics* letter type, we added the following paragraph to the *baseline* letter that provided information about the audit probabilities (p) and penalty rates (θ) for a random sample of firms that were similar to the recipient:[7]

> "On the basis of historical information on similar businesses, there is a probability of *[p%]*that the tax returns you filed for this year will be audited in at least one of the coming three years. If, pursuant to that auditing, it is determined that tax evasion has occurred, you will be required to pay not only the amount previously unpaid, but also a fee of approximately *[θ%]* of that amount."

Note that we communicated the probability that firms will be audited in at least one of the three following years, because IRS experts stated that this was the relevant probability for firms' decision-making. Uruguay's tax law indicates that tax audits should cover the previous three years of tax returns and, as a result, the probability that the current year's tax report

[5]For a full-page sample of the letter without this placeholder, see Appendix A.1. For the corresponding samples of the *audit-statistics*, *audit-threat*, *audit-endogeneity and public-goods* letter types, see Appendices A.2, A.3, A.4, and A.5, respectively.

[6]We assume that firms in our sample are risk-averse, which is plausible since we deal mainly with small- and medium-sized firms. However, *A&S* has also been generalized to settings with risk-neutral agents (Reinganum and Wilde, 1985; Srinivasan, 1973).

[7]To make the information on the audit probability and the penalty rate clear and salient, we provided all figures as round numbers.

will be audited is roughly equal to the probability that the firm will be audited at least once over the following three years.

In our sample, the average value of p is 11.7%, and the average value of θ is 30.6%. Tax agencies in most countries do not publish data on the values of p and θ, which makes it difficult to compare the Uruguayan case to other contexts. In the United States, for which some comparable data are available, these two parameters are on the same order of magnitude: self-employed individuals face a p of 11.4% and a base θ of 20%.[8]

The goal of this treatment arm was to generate exogenous variation in the firms' perceptions about audit probabilities and penalty rates. Because of legal considerations and other constraints, we could not assign different firms to different sets of information about these factors. We instead induced non-deceptive, exogenous variation in messages that may affect these perceptions by exploiting the sampling variation in statistics about audits and penalties.

More specifically, we divided the firms into five groups of "similar firms," corresponding to the five quintiles of total VAT payments in the fiscal year before our intervention. For each firm, we then drew a random sample of 50 other firms from the same quintile (i.e., similar firms), from which we computed the averages of p and θ. This randomization strategy generated 940 different combinations of p and θ. These estimates of p and θ were unbiased and consistent with the explanation given in a footnote that we included in the letter, meaning that the information provided to recipients was nondeceptive. The footnote explained how we estimated the values of p and θ:

> "Estimates are based on data from the 2011–2013 period for a group of firms with similar characteristics, for instance, in terms of total revenue. The probability of being audited was calculated as a percentage of audited firms in a random sub-sample of firms. The rate of the fee was estimated as an average of a random sub-sample of audits."

The values of p ranged from 2% to 25%, with an average of about 11.7%. The values of θ ranged from 15% to 68%, with an average of about 30.6%. Figure 3 presents the audit probability and penalty rate distribution across five groups by firm size (one in each row) and the distribution of the generated within-group parameters. The vertical line denotes the average audit probability or penalty rate based on all members of the group. If we based

[8]First, there is a 2.1% probability of being audited in any given year, according to the ratio of returns examined for businesses with no income tax credit and with a reported income between USD 25,000 and 200,000 (Table 9a of IRS, 2014). Each audit covers the previous 3 to 6 years, which implies that the the probability that the current year's tax filing will be eventually audited ranges from 5.88% to 11.42%. Second, IRS usually imposes a basic penalty of θ=20%, although the penalties can be higher depending on the specific situation.

our estimates of p and θ included in the letter on the population of firms, every member of the group would have received the same signal (the vertical line). As we computed p and θ from samples of 50 firms, the sampling variation implies that different members of each group received different signals. For example, panel (a.1) of Figure 3 shows that in group 1 (i.e., the first quintile of firms ranked by total VAT payments), the average p for all group members is 8.2%, whereas the histogram depicts the different signals actually sent to firms within the group. These signals center around the average of 8.2%, but they range anywhere from 2.5% to 25%.[9]

2.3 *Audit-Threat* Letter

To complement the evidence from the *audit-statistics* sub-treatment, we implemented an alternative way of randomizing perceptions about audit probabilities using an *audit-threat* letter. We devised a treatment arm that randomly assigned firms to groups with different probabilities of being audited in the following year.

The *audit-threat* letter was identical to the *baseline* letter, with the following additional paragraph:

> "We would like to inform you that the business you represent is one of a group of firms pre-selected for auditing in 2016. A *[X%]* of the firms in that group will then be randomly selected for auditing."

This *audit-threat* treatment arm was applied to a separate experimental sample, a group of high-risk firms selected by the IRS audit department. The recipients of the *audit-threat* letter thus cannot be compared to recipients of the *baseline* letter. Instead, we randomly assigned the firms in this treatment arm to two groups, one with a 25% probability of being audited in the following year (X=25%) and another with a 50% probability of being audited (X=50%). These messages were non-deceptive: the IRS audit department provided a commitment that they would conduct audits according to these probabilities in the following year.

[9] The within-group average p is different across each of the give different groups, increasing monotonically from 8.2% in the bottom quintile to 13.4% in the top quintile. This implies that some of the variation in the values of p and θ included in the letter was non-random. To estimate the causal effects of the signals p and θ, we must isolate the random variation when analyzing the data. In any case, this aspect of the design is not so important in practice, as most of the variation in signals is indeed due to the sampling variation: for example, regressing p on the pre-treatment VAT quintile dummies results in $R^2 = 0.118$; and regressing θ on the same dummies results in $R^2 = 0.007$.

2.4 *Audit-Endogeneity* Letter

The *audit-statistics* and *audit-threat* treatment arms conveyed quantitative information about audit probabilities and penalty rates. We also wanted to incorporate into our research design a message about a different aspect of the audit process. Most tax agencies, including Uruguay's, account for firm characteristics when deciding which ones to audit. They assign higher audit probabilities to firms with higher evasion risk. As a result, evading taxes typically increases the probability of being audited. This factor was incorporated as a special case in *A&S*, in which audit probabilities were determined endogenously. If unsuspecting firms learn about the endogenous nature of their audit probabilities, they should revise their tax evasion decisions and reduce the amount of tax evaded.[10]

We used this insight from economic theory to devise the *audit-endogeneity* message about the nature of the audit process. We asked our counterparts at the IRS to use their evasion-risk scores to divide a small sample of firms into two groups: those suspected of evading taxes and those not suspected of evading taxes. We then computed the difference in audit rates from 2011–2013 between the two groups: the rates were approximately twice as high for the high evasion risk group. We used this information to create the message in the *audit-endogeneity* letter type, which was identical to the *baseline* letter with the addition of the following paragraph:

> "The IRS uses data on thousands of taxpayers to detect firms that may be evading taxes; most of its audits are aimed at those firms. Evading taxes, then, doubles your chances of being audited."

2.5 *Public-Goods* Letter

We also devised a treatment arm to provide a benchmark for the effect of messages intended to increase tax compliance without directly mentioning audits. We designed a non-pecuniary message based on the suggestions by the IRS staff and authorities (i.e., on what they expected to be most effective at increasing compliance). This message provided information about the cost of evasion in terms of the provision of public goods, in the spirit of the model of Cowell and Gordon (1988).[11] The *public-goods* letter is identical to the *baseline* letter, with the

[10]Konrad et al. (2016) present suggestive evidence of this mechanism in the context of a laboratory experiment, finding that taxpayers who face a situation where suspicious attitudes toward tax officers increase the probability of being audited increase their tax compliance by 80%.

[11]This message is also related to the laboratory experiment from Alm et al. (1992), which finds that one of the reasons for why people decide to pay taxes is their valuation of the public goods provided by means of the tax revenues.

addition of the following message:[12]

> "If those who currently evade their tax obligations were to evade 10% less, the additional revenue collected would enable all of the following: to supply 42,000 portable computers to school children; to build 4 high schools, 9 elementary schools, and 2 technical schools; to acquire 80 patrol cars and to hire 500 police officers; to add 87,000 hours of medical attention by doctors at public hospitals; to hire 660 teachers; to build 1,000 public housing units ($50m^2$ per unit). There would be resources left over to reduce the tax burden. The tax behavior of each of us has direct effects on the lives of us all."

2.6 Survey Design

We designed a survey to be conducted with a sample of owners from our main subject pool several months after they received the letters. The IRS, with the support of the Inter-American Center of Tax Administrations and the United Nations, had previously administered a survey on the costs of tax compliance for small- and medium-sized firms. We collaborated with the tax authority on the design and implementation of a new survey, which included a specific module tailored to our research design. The survey also included seven additional modules, designed by the IRS, about the costs of tax compliance and other topics.[13] We partnered with local and international universities to increase respondent confidence and to highlight the fact that the survey was part of a scientific study, and not an audit or compliance exercise by the IRS.

To further ensure trustworthy responses, the IRS assured potential respondents that the survey responses would remain anonymous and that they could not be traced back to specific individuals or firms. To measure the effect of our experiment on these survey responses, we embedded a code in the survey link to identify the treatment arm of the experiment which the recipient was assigned to. These codes did not uniquely identify any single firm, but they allowed us to link treatment arms and completed questionnaires while maintaining the anonymity of responses.

In our survey module, we assessed whether the *audit-statistics* message shifted the perceptions of the recipients of our letters by means of the following two questions:[14]

[12]The content of the message was based on estimates from the following governmental agencies: Administracion Nacional de Educacion Publica (ANEP), CEIBAL, Ministerio de Salud Publica (MSP), Ministerio del Interior (MI), Ministerio de Vivienda, Ordenamiento Territorial y Medio Ambiente (MVOTMA).

[13]A sample of the email with the invitation to participate in the online survey is presented in Appendix A.6.

[14]A screenshot of our survey module is presented in Appendix A.7.

Perceived Audit Probability: "In your opinion, what is the probability that the tax returns filed by a company like yours will be audited at least in one of the next three years (from 0% to 100%)?"

Perceived Penalty Rate: "Let us imagine that a company like yours is audited and that tax evasion is detected. What, in your opinion, is the penalty (in %) as determined by law that the firm must pay in addition to the originally unpaid amount? For example, a fee of X% means that, for each $100 not paid, the firm would have to pay those original $100 plus $X in penalties."

After each question, we elicited how certain the subject felt about his or her response on a 1–4 scale, from "Not sure at all" (1) to "Very sure" (4).[15]

3 Data Sources and Implementation of the Field Experiment

3.1 Institutional Context

Uruguay is a South American middle-income country (the annual GDP per capita was about USD 15,000 in 2015). Our main focus in terms of studying tax evasion is the VAT, which represents the largest tax liability for firms in Uruguay and also the largest source of tax revenue. At the time of the study, the VAT rate was 22%,[16] and VAT revenues accounted for nearly half of the total tax revenues.[17] Uruguay is not atypical in terms of tax evasion. According to estimates from Gomez-Sabaini and Jimenez (2012), evasion of VAT in Uruguay was around 26% in 2008. This is the third-lowest rate among the nine Latin American countries included in the study and comparable to evasion rates in more developed economies. For example, the evasion rate for Italy in 2006 was estimated as 22% (Gomez-Sabaini and Moran, 2014).[18] Uruguay is not an outlier in terms of tax morale either. According to data from the 2010–2013 wave of the World Values Survey, 77.2% of respondents from Uruguay stated that evading taxes is "Never Justifiable," whereas this proportion is 68.2% for all other

[15]For the sake of completeness, we also included a question in the survey to measure the subject's awareness of the endogeneity of audit probabilities. You can find a screenshot of this question in Appendix A.7.

[16]A small number of products considered basic necessities either had a 10% rate or were exempt from the tax.

[17]Own calculations based on data from the Central Bank of Uruguay and from the Internal Revenue Service. Other sources of tax revenues include the personal income tax, the corporate income tax, and some specific taxes of consumption, businesses and wealth.

[18]Gomez-Sabaini and Jimenez (2012) compute those rates by applying the "indirect" method to estimate tax evasion. This method is based on the comparison of collected VAT with aggregate consumption data from the System of National Accounts (SNA).

Latin American countries on average (weighted by population) and 70.9% for the United States.

In some contexts, tax authorities do not need to rely on audits to mitigate tax evasion. For example, the U.S. Internal Revenue Service uses their electronic records to compare the wage amount reported by a taxpayer to the amount reported by his or her employer. This algorithm automatically rectifies the discrepancies in reporting and sends a notification to the taxpayer with the updated tax amount to be paid. Because the evasion will be caught through third-party reporting regardless of whether the individual is audited or not, the probability of being audited should be irrelevant for taxpayers (Kleven et al., 2011). Instead, we focused on a context in which tax authorities must still rely heavily on the threat of audits to discourage evasion. There is some third-party reporting for the VAT, consisting of the paper trail of invoices for sales and purchases.[19] However, this type of third-party reporting is highly imperfect. Most importantly, there is no automatic cross-checking and rectification of VAT payments. This limitation arises because the paper trail is non-electronic and thus can only be scrutinized by the tax agency in the event of an audit.[20] Additionally, the VAT paper trail has other limitations documented in the literature; for example, it breaks down at the consumer end (Pomeranz 2015; Naritomi 2019). The tax agency has access to other enforcement tools, such as tax withholding, but those other tools have limitations too. As a result, audits are still one of the main ways in which the tax agency can detect tax evasion in our context (Gomez-Sabaini and Jimenez, 2012; Bergman and Nevarez, 2006).

3.2 Subject Pool and Randomization

Our experiment was conducted in collaboration with the IRS of Uruguay. As of May 2015, there were 120,142 firms registered in the agency's database. A sub-sample of 4,597 firms, pre-selected by the IRS, was put aside for the *audit-threat* sample, which we call the secondary experimental sample. We followed a series of criteria to select our main experimental sample from the remaining firms. First, we excluded some firms on request of the IRS. For instance, we excluded very small or very large firms that were subject to special regimes for VAT payments. We also restricted the experimental sample to firms that had made VAT payments in at least three different months in the previous 12 month period and firms with a total

[19] Firms may credit VAT paid on input costs (i.e., imports and purchases from their suppliers) against the total sales of goods and services to their costumers (i.e., "tax debit"). They pay VAT to the IRS only on the excess of the total "tax debit" over the tax credit. If the tax credit exceeds the debit, the excess may be carried over for future tax years. While the VAT should in theory be similar in its effects to a retail sales tax, in practice the two types of taxes differ in some significant aspects (Slemrod, 2008).

[20] In other countries, the use of standardized electronic invoicing systems may facilitate and automatize the cross-checking of the VAT trail to detect evasion.

value of at least USD 1,000.[21]

To maximize the impact of our information provision experiment, we did our best to ensure that the letters would be delivered to the firms' owners.[22] Moreover, in very large firms, the effect of the information could be substantially diluted, as it may not reach the owner or the individuals making decisions about tax compliance. Thus, we excluded from our subject pool firms with a total value exceeding USD 100,000 during the previous 12 months.

These criteria left a subject pool of 20,471 firms for the main experimental sample. All firms were randomly assigned to receive one of the four letter types according to the following distribution: 62.5% were assigned to the main treatment arm (*audit-statistics* letter), and 12.5% were assigned to each of the three remaining letter types (*baseline*, *audit-endogeneity*, and *public-goods*).[23] After removing the 19.9% of letters that were returned by the postal service, the final distribution of letter types was as follows: 10,272 received *audit-statistics*; 2,064 received *baseline*; 2,039 received *audit-endogeneity*; and 2,017 received *public-goods* letters (total N = 16,392). The 4,597 firms in the secondary sample were assigned to receive the *audit-threat* letter. Half were randomly assigned to the message of a 25% audit probability, and the other half to the 50% audit probability. After excluding the 12% of letters returned by the postal service, we were left with 2,015 firms in the 25% probability group and 2,033 firms in the 50% probability group (total N = 4,048).

Table 1 allows us to compare the balance of pre-treatment characteristics between firms assigned to the different letter types. Columns (1) through (4) correspond to firms in the main experimental sample. For each characteristic, column (5) presents the p-value of the test of the null hypothesis that the averages for these characteristics are the same across all four letter types. As expected, the differences across letter types are economically small and statistically insignificant. Columns (6) through (8) of Table 1 present a similar balance test for the secondary sample used for the *audit-threat* arm. Again, the characteristics are balanced across the two sub-treatments in the *audit-threat* treatment arm.[24]

[21]The sample selection was conducted in May 2015, so this 12 month period spans from April 2014 to March 2015.

[22]In some cases, owners provide the address of external accountants instead of their own or their firm's. We removed from the sample firms that were registered with an accountant's mailing address as their own (the IRS keeps records of addresses for all registered accountants).

[23]The randomization of letter types was stratified by the quintiles of the distribution of VAT payments over the fiscal year before our intervention.

[24]Appendix B.1 provides descriptive statistics for the firms in our subject pool. Moreover, Appendix B.2 shows that the rate of non-delivered letters are mostly balanced across treatments, with only minor and not economically significant differences in missing delivery status for the *public-goods* and *audit-endogeneity* treatment arms with respect to *audit-statistics* and *baseline*.

3.3 Outcomes of Interest

The letters were handed to Uruguay's postal service on August 21, 2015. The vast majority of the letters were delivered to taxpayers during the month of September, and therefore we set September as the last month of the pre-treatment period and October as the first month of the post-treatment period. The main outcome of interest in our study is the total amount of VAT liabilities remitted by taxpayers in the 12 months after receiving the letter.[25] To test for the persistence of our treatment effects, we define a second period of observation between October 2016 and September 2017 (i.e., up to two years after the intervention). Panel (b) of Figure 1 depicts a timeline of the experiment and the data collection.

VAT represented 64.7% of total taxes paid by these firms in the fiscal year that preceded our treatment. The corporate income tax represented 25.4% of total taxes paid, the wealth tax 6.5%, and the personal income tax withholding only 3.3%. In this context of sole proprietorships, micro enterprises and small enterprises, the VAT is the firms' main tax liability, which is why it is our main focus. Nonetheless, we also obtained data on the other taxes paid by the firms from the IRS, which allows us to assess whether firms effectively changed their overall tax compliance or if they simply substituted the evasion of VAT for that of other taxes.

It should be noted that the firms in our sample are mostly small. On average, the total amount of VAT paid by firms that received the baseline letter in the 12 month pre-treatment period was about USD 7,700, whereas the amount for the corresponding post-treatment period was approximately USD 6,500. This negative trend in VAT payments can be explained by the fact that this sample contains a high share of small firms with a high turnover rate. The size of post-treatment VAT payments varied substantially, ranging from USD 400 at the 10th percentile to USD 16,550 at the 90th percentile.[26]

We can further break down firms' VAT payments according to their timing. We can observe the date of transfer to the IRS as well as the month for which the payment was imputed. Firms can back-date payments to cover liabilities from previous periods. As firms typically make VAT payments on a monthly basis, they normally cover the current and previous months, which we call concurrent payments. We classified payments covering liabilities incurred two or more months ago as retroactive payments – that is, adjustments for revisions in past liabilities. About 99.4% of firms made at least one concurrent payment in the 12 month pre-treatment period, whereas only 23.8% of firms made at least one retroactive

[25]This variable includes all VAT payments, including direct VAT payments and indirect VAT withholdings.

[26]Appendix B.3 presents detailed descriptive statistics about the distribution of pre- and post- treatment payments for firms that received the *baseline* letter type.

payment over this same period.[27]

3.4 Survey Implementation

The IRS communicates mainly by postal mail, and therefore it has mailing addresses for all registered firms. It also keeps records of email addresses for a subset of firms that have used their online services. We emailed invitations to all firms in the main experimental sample with a valid email address (N=3,867). While we wanted to roll out the survey shortly after the mail interventions, for reasons beyond our control we were not able to roll it out until May 2016, nine months after the intervention.[28] We find that firms which we invited to the survey were similar in characteristics to the broader set of firms in the main experimental sample.[29]

Our purpose was to elicit the beliefs of firm owners. We did not include email addresses that were repeated more than three times in the full sample, as these most likely belonged to accounting firms representing multiple small- and medium-sized firms. Even after applying this criterion, the IRS records could not ensure that the registered email address belonged to the firms' owners. We thus asked the survey respondent to self-identify as one of the following five types: owner, internal accountant, external accountant, manager, or other employee. From the 3,867 recipients that we invited to participate in the survey, 948 started to answer the survey (response rate of 24.5%).[30] Of these 948, 68.9% self-identified as an owner, 23.5% as a non-owner, and the remaining 7.6% did not provide a response to this question.[31] Our baseline specification excludes respondents who self-identified as non-owners, but the results are similar if we include only those who actively identified as owners.[32] Per an IRS request, respondents could skip as many questions as they wanted. We find that 6.6% and 8.6% of respondents skipped the audit probability and penalty questions respectively, which is comparable to the average rate (6.1%) at which they skipped other questions in the

[27] It should be noted that the retroactive payments do not reflect delinquency or outstanding debts to the tax authority. Overall VAT liabilities are computed on a yearly basis, and firms make monthly payments according to their provisional receipts on a pay-as-you-go basis to avoid a large bill at the end of the fiscal year. Thus, retroactive payments reflect changes in past liabilities. For instance, a firm may have "forgotten" to declare a sale in the past and thus need to send a retroactive payment corresponding to the gap between the original and the updated accounting.

[28] While we would have preferred a shorter turnaround between the experiment and the survey, several departments of the tax authority were involved in its design, which created some delays.

[29] Results reported in Appendix B.1.

[30] In this calculation, we require that respondents had answered at least the first two questions of the survey.

[31] The non-owner responses are distributed as follows: 6.1% self-identified as an internal accountant, 8.3% as an external accountant, 2.7% as a manager, 6.3% as other employee.

[32] Results reported in Appendix B.4.2.

survey.[33]

4 Results: Average Effect of Messages

4.1 Effect of the *Audit-Statistics* Message

Our first set of hypotheses concern whether providing letters with information related to tax enforcement increases tax compliance. We start by describing the effects of our main treatment, the *audit-statistics* message. Findings in previous literature suggest that this message is expected to have a positive effect on tax compliance because it teaches the taxpayer about tax enforcement.[34]

Figure 4 summarizes the raw data before conducting any regression analysis. Panel (a) of Figure 4 corresponds to the effect of the *audit-statistics* message. More precisely, this graph shows the percentage difference in VAT paid between the individuals who were randomly assigned to the *audit-statistics* letter and the individuals who were assigned to the *baseline* letter.[35] This figure shows the difference for each bi-monthly period covering all the months that we have data for, including three pre-treatment years (October 2012 to July 2015) and two post-treatment years (October 2015 to September 2017).[36] By construction, period 0 is defined as the period during which the letters were being delivered (August-September, 2015), which is highlighted in the figure with the vertical dashed line. Following Pomeranz (2015), we normalize by the average difference during the entire pre-treatment period.

Panel (a) of Figure 4 shows that the *audit-statistics* message had economically significant effects on VAT payments. Prior to the delivery of the letters, and due to random assignment, we would not expect to see any differences between individuals who were assigned to one type of letter versus another. In other words, individuals cannot be possibly affected by messages that they have not received yet. As expected, the differences in VAT payments between the

[33]This skip rate is the probability of providing a missing answer conditional on reaching that question in the survey. Appendix B.4 presents a series of robustness tests and analyses of differential response rates by treatment group, among other tests.

[34]As long as the enforcement messages do not affect the taxpayers' true income, the changes in the total amount of VAT paid measure changes in tax evasion. However, given the presence of real effects, our estimates provide a lower bound for the impact of information about enforcement on tax compliance. Although real effects are possible in our setting, most of the public finance literature provides evidence that real effects are normally zero or small relative to reporting effects (see for example Saez et al., 2012).

[35]For more details, Appendix B.5 discusses the evolution of the VAT payments for the treatment and control groups separately.

[36]Since a number of firms are required to pay VAT bi-monthly and there is a strong seasonal pattern, we group the data into bins of two months. This is balanced across treatment and control groups and does not affect our results at all. In all the results the amounts are top-coded at the 99.99% percentile to avoid the contamination of the results by outliers.

audit-statistics and *baseline* letter recipients hover around zero in the pre-treatment period. After the letter delivery, our hypothesis predicts that there will be a positive wedge between individuals in the *audit-statistics* and *baseline* letter groups. As expected, a positive gap in VAT payments between the two groups arises right after the letters are delivered. More precisely, in the first couple of months after the letter delivery, the difference in VAT payments between the *audit-statistics* and *baseline* letter recipients jumps to 10.4%, and then hovers between 4.0% and 10.5% in the rest of the first post-treatment year. During the second year, the effect becomes weaker over time.

While these results suggest that the *audit-statistics* message increased subsequent VAT payments, we need to use a more formal framework for statistical inference. Since we observe the outcome variable before and after the intervention, we can use that information to reduce the variance of the error term and thus gain statistical power through a difference-in-differences specification which compares treated firms to control firms and the pre-treatment period to the post-treatment period (McKenzie, 2012). With that goal in mind, we follow the econometric specification from Pomeranz (2015). Consider the sample of firms assigned to either the *baseline* letter or the *audit-statistics* letter. Let i index firms and $t = \{1, 2\}$ denote time, where $t = 1$ corresponds to the 12 months pre-treatment and $t = 2$ corresponds to the 12 months post-treatment. Let Y_{it} be the outcome variable by taxpayer i in period t (e.g. $Y_{i,2}$ could be the total VAT payments by firm i in the 12 months post-treatment period). D_i^1 is a dummy variable that takes value 1 if i was assigned to receive the *audit-statistics* letter and 0 if it was assigned to receive the *baseline* letter instead. Let $Post_t$ be a dummy variable that takes the value 1 if $t = 2$ (i.e., after the letters were delivered) and 0 if $t = 1$. The regression of interest is the following:

$$Y_{it} = \alpha_0 + \gamma_1 \cdot D_i^1 \cdot Post_t + \alpha_1 \cdot D_i^1 + \alpha_2 \cdot Post_t + \epsilon_{it} \tag{1}$$

The coefficient of interest is γ_1, which measures the differential effect between the *audit-statistics* letter and the *baseline* letter. When the dependent variable is the amount of taxes paid, we estimate a log-linear model, also known as a Poisson regression. The reasons for using this specification are twofold. First and foremost, the Poisson regression allows effects to be proportional – indeed, the coefficients can be readily interpreted as semi-elasticities.[37] Second, the Poisson regression naturally accounts for the bunching at zero of the dependent variable. Note that the Poisson regression can be used for a continuous non-negative variable

[37]The Poisson model can be expressed as follows: $\log(Y_X) = \alpha + \beta X + \varepsilon$. The effect of a unit change in X can be re-expressed in log-units of the dependent variable, $\beta = \log(Y_{X=x+1}) - \log(Y_{X=x})$. Provided this coefficient is small enough, it can be approximated as a percent-change effect: $\beta = \log(Y_{X=x+1}) - \log(Y_{X=x}) \approx \dfrac{Y_{X=x+1} - Y_{X=x}}{Y_{X=x}}$.

and we do not have to rely on additional functional form assumptions such as equidispersion thanks to the quasi-MLE estimator.[38] In any case, we find that the results are robust to alternative regression models (OLS, Tobit and Probit).[39] Standard errors are always clustered at the firm level.

Table 2 presents the baseline regression results. We start with Panel (a), which corresponds to the comparison between the *audit-statistics* and *baseline* letters. In column (1), the dependent variable corresponds to the effect on VAT paid. The post-treatment coefficient corresponds to the effect during the 12 months after the delivery of the letter (October 2015 to September 2016), as measured by the coefficient γ_1 from equation (1) above. The post-treatment coefficient of *audit-statistics* (in column (1) of panel (a)) is positive (0.070) and highly statistically significant (p-value = 0.001). This coefficient is economically significant too: it suggests that the *audit-statistics* message increased VAT payments in the 12 months after the intervention by about 7.0%.[40] To better grasp the magnitude of the effects, we can compare it to some basic benchmarks. The estimated average evasion rate for VAT in Uruguay is 26% (Gomez-Sabaini and Jimenez, 2012), and while the tax base is not necessarily comparable, it provides a benchmark: the 7% increase in VAT payments amounts to a reduction in the evasion rate of 27% ($= \frac{7.0\%}{26\%}$). In terms of previous findings in the literature, the effects of our *audit-statistics* treatment are not directly comparable to those of the audit message from Pomeranz (2015) because the messages differed in content, and because the two studies cover firms from different countries and with different characteristics. Nevertheless, Table 4 from Pomeranz (2015) indicates that the deterrence letter in that study led to an increase in VAT payments of 7.6%, which is similar in magnitude and statistically indistinguishable from the 7.0% effect of our *audit-statistics* message. Moreover, our results are qualitatively consistent with a broader literature that finds effects of messages about enforcement on tax compliance in a variety of contexts: self-employed income in the United States (Slemrod et al., 2001), wage income taxes in Denmark (Kleven et al., 2011), individual TV license fees in Austria (Fellner et al., 2013), individual municipal taxes in Argentina (Castro and Scartascini, 2015), an individual church tax in Germany (Dwenger et al., 2016), and tax delinquencies in the United States (Perez-Truglia and Troiano, 2018).

Table 2 presents a number of robustness checks, which we discuss below. First, we present falsification tests in the spirit of event-study analysis. The pre-treatment coefficients from

[38]For more details, see for example Chapter 19 of Wooldridge (2010).

[39]The results from these robustness checks are presented in Appendix B.6.1.

[40]This percent-effect is based on the following approximation: $\beta = \log(Y_{X=x+1}) - \log(Y_{X=x}) \approx \frac{Y_{X=x+1} - Y_{X=x}}{Y_{X=x}}$. For simplicity, in the rest of the paper we interpret all of the Poisson coefficients using this same approximation. Alternatively, the exact percent-effect can be calculated exactly using the exponential transformation. For example, the 0.070 coefficient corresponds exactly to a 7.25% effect ($= 100 \cdot \left(e^{0.070} - 1\right)$).

Table 2 are estimated with a specification identical to the one used for the post-treatment coefficients, except it uses a "placebo" date for the delivery of the letters: i.e., we pretend that the letters were delivered during August and September of 2014, and estimate the "effects" on the VAT paid in the subsequent 12 months (i.e., October 2014 to September 2015). Since the letters had not really been delivered on that date, we would expect the "effect" of the *audit-statistics* message to be close to zero and statistically insignificant. A finding to the contrary would be suggestive of potential concerns with the specification or the random assignment. As expected, the pre-treatment coefficients for the *audit-statistics* message (column (1) in panel (a) of Table 2) is close to zero (0.009), statistically insignificant (p-value=0.658), and as precisely estimated as the corresponding post-treatment coefficient.

Over time, individuals may forget the information conveyed in the letter, or it may become less salient. Individuals may also update their beliefs and perceptions for other reasons, for instance due to new events such as audits and information campaigns. To assess the persistence of the effects, column (2) in Table 2 replicates the analysis for the second year after the treatment (October 2016 to September 2017). Consistent with the arguments provided above, the effect of the treatment is half as large as the effect during the first year, and is no longer statistically significant. These estimates are consistent with the pattern of effects by quarter depicted in panel (a) of Figure 4, which shows that the effect falls gradually over time and is about half as large in the second year as in the first year. The timing of the effects is furthermore consistent with previous evidence on the effects of tax enforcement messages. For instance, Pomeranz (2015) shows that the effects of her main intervention were also substantially higher in the first 12 months after the intervention, fell substantially thereafter and almost disappeared by the 18th month.[41]

Table 2 also presents results for complementary outcomes. As discussed in the previous section, firms in Uruguay make payments for their current liabilities, but also for taxes that correspond to previous periods because they revise their accounts and correct past mistakes, or because they impute invoices that were not available at the time of the original payment. When firms that engage in tax evasion face a heightened threat of being audited, we can expect them to increase their tax payments (i.e., reduce their evasion) in the future, but we can also expect them to retroactively revise their payments for previous time periods to reduce or eliminate their past evasion. To shed light on this question, columns (3) and (4) in panel (a) of Table 2 split the effects during the first year between retroactive and concurrent payments. The *audit-statistics* message had an economically and statistically significant effect on both retroactive and concurrent payments: the coefficient corresponding to the *audit-statistics* message is 0.383 (p-value=0.006) for retroactive payments and 0.053

[41] See for example Figure 2 in Pomeranz (2015).

(p-value=0.012) for concurrent payments.[42]

We have so far established that firms in the *audit-statistics* treatment arms increased their VAT payments compared to recipients of the *baseline* letter. Our analysis focuses on VAT liabilities, which represent the largest fraction of tax payments by firms in our sample. However, our letters referred to taxes in general and did not specifically mention VAT or any other tax in particular. Given the presence of other tax liabilities, the effects we reported on VAT may not represent a net increase in tax payments: firms may increase their evasion (i.e., reduce their payments) of other taxes they are liable for, thereby crowding out payments or substituting evasion to other taxes. On the other hand, firms may need to declare higher income in order to declare higher VAT, and thus they could be obliged to pay more, rather than less, in non-VAT taxes. The results in columns (5) and (6) of Table 2 shed light on these issues. Column (5) presents the effects on all other taxes paid (mostly the corporate income tax). The effects on the payments of other taxes are as economically and statistically significant as those on VAT payments: the *audit-statistics* message had an effect of 8.6% (p-value=0.019) on other tax payments. Column (6) shows that the results are robust if we look at the effect on the sum of VAT and other taxes: the *audit-statistics* message increased this outcome by a statistically significant 7.3% (p-value<0.001).[43]

4.2 *Audit-Endogeneity* Message

The first benchmark for the *audit-statistics* message is the *audit-endogeneity* message, which is similar in that it provides information about the enforcement of taxes through audits. Panel (b) of Figure 4 shows the raw evolution of VAT payments in the *audit-endogeneity* treatment relative to the *baseline* treatment. The results suggest that the *audit-endogeneity* message also induced a significant increase in VAT payments, which was similar in timing and magnitude to the effects of the *audit-statistics* message depicted in panel (a) of Figure 4. For a more formal statistical analysis, the regression results are presented in panel (b) of Table 2.[44] The coefficient in column (1) indicates that the *audit-endogeneity* message increased

[42] The effect size is larger for retroactive payments than for concurrent payments, but these differences in magnitude have to be taken with a grain of salt because there are large differences in baseline levels between the two outcomes. For example, in the *baseline* letter group, firms paid an average of USD 300 in retroactive payments versus USD 7,370 in concurrent payments in the post-treatment period.

[43] Appendix B.7 presents a finer analysis that breaks down the effects of "other taxes" separately by each of its three components and a series of additional robustness checks, such as alternative estimation methods (Appendix B.6.1), alternative specifications of the dependent variable (Appendix B.6.1) and heterogeneity analysis based on firm characteristics such as size, age and sector (Appendix B.8). Overall, we find that the effects are qualitatively and quantitatively similar across the board.

[44] The results from panels (b) and (c) in Table 2 are based on a regression specification equivalent to the one from equation (1) above which was used to obtain the estimates in panel (a).

subsequent VAT payments by 7.1% (p-value of 0.009).[45] This 7.1% is similar in magnitude and statistically indistinguishable (p-value=0.950) from the corresponding 7.0% effect of the *audit-statistics* message reported in panel (a).[46] The effects of the *audit-endogeneity* message also pass the same robustness checks presented above for the effects of the *audit-statistics* message: for example, the "effect" on the pre-treatment year is close to zero (-0.005) and statistically insignificant (p-value=0.868), and the effects during the second post-treatment year are about half as large as the effects during the first year.

4.3 ***Public-Goods*** Message

Panel (c) of Figure 4 shows the effects of the *public-goods* message on VAT payments. The time series data suggest that the *public-goods* message had a positive effect on tax compliance too, but it dissipates a lot more quickly than the effects of the *audit-statistics* message. The corresponding regression results are presented in panel (c) of Table 2. The *public-goods* message increased VAT payments in the first post-treatment year by 5.1%, and the effect is statistically significant (p-value=0.043). On the other hand, the effects of the *public-goods* message in the second post-treatment year were close to zero (0.4%) and statistically insignificant (p-value=0.906). There is mixed evidence on the effects of moral messages, with evidence that they work in some contexts (Bott et al., 2020; Nathan et al., 2020; Hallsworth et al., 2017) but not in others (Blumenthal et al., 2001; Fellner et al., 2013; Castro and Scartascini, 2015; Dwenger et al., 2016; Meiselman, 2018; Perez-Truglia and Troiano, 2018). The most related study, Pomeranz (2015), included a message of moral suasion that had a positive effect on subsequent VAT payments, although it was not as large as the effect of the deterrence message and statistically insignificant. Our findings on moral suasion fall closest to the findings from the experiment with Norwegian taxpayers reported in Bott et al. (2020): they find that the message of moral suasion increased tax compliance in the short term, but the effects dissipated completely the following year.

5 Tests of ***A&S***

The results presented in the previous section are broadly consistent with the evidence in the literature that providing information about audits significantly increases tax compliance.

[45]The effect of the *audit-endogeneity* message is somewhat less precisely estimated than the corresponding effect of the *audit-statistics* message, but that is expected due to the differences in sample sizes.

[46]This is an equality test between two coefficients based on the same data but different regressions. To allow for a nonzero covariance between these two coefficients, we estimate a system of seemingly unrelated regressions. In the remainder of the paper, when comparing coefficients from the same data but different regressions, we always use this method.

In this section, we present additional evidence to establish whether the effects of the *audit-statistics* treatment are driven by the *A&S* mechanism.

5.1 First Test of *A&S*: Effects on Perceptions

According to *A&S*, the *audit-statistics* message should have a positive effect on tax compliance if it increased the perceived probability of being audited, the perceived value of the evasion fine, or both. We explore this hypothesis by utilizing data from our post-treatment survey. This survey data consists of 365 firms in the *audit-statistics* group and 137 in what we refer to as the pooled control group with individuals who did not receive information related to audits.[47]

Panels (a) and (b) of Figure 5 depict the distributions of perceptions about audit probabilities and penalty rates respectively, as elicited from the survey. The shallow bars with solid borders correspond to the perceptions of firms that received the *audit-statistics* message. The shaded gray bars depict the distribution of perceptions for firms in the pooled control group. The red dashed curve, in turn, corresponds to the distribution of signals sent to firms in the *audit-statistics* letters. The comparison between the shaded bars and the red curve from panel (a) of Figure 5 suggests that, on average, respondents in the control group substantially overestimated the probability of being audited. While our administrative data on audits indicates a probability of being audited of about 11.7%, the mean perception for the control group is 40.7% (p-value<0.001 for the difference). This finding of an overestimation of audit probabilities is consistent with prior survey evidence (Harris and Associates 1988; Erard and Feinstein 1994; Scholz and Pinney 1995).[48] In contrast, the comparison between the shaded bars and the red curve from panel (b) of Figure 5 suggests that the average belief about the penalty rates was unbiased: the actual average penalty computed from administrative data for the experimental sample is 30.6%, while the mean perceived penalty is 30.5% in the control group.

The positive bias in the perceived audit probability could be explained by the availability heuristic bias (Kahneman and Tversky, 1974). According to this model, individuals judge the probability of an event by how easily they recall instances of it. Even though audits are

[47]The survey sample size was substantially smaller than that of our experimental sample. To increase the statistical power of our test, we defined this control group by pooling subjects from the *baseline* and the *public-goods* groups, since both received messages with no specific information about audit probabilities or fines. Appendix B.4.2 shows that the results are similar, but less precisely estimated, when we only use recipients of the *baseline* letter for the control group.

[48]However, the prior survey evidence was based on responses from wage earners, for whom the misperception of audit probabilities is mostly inconsequential due to widespread third-party reporting (Kleven et al., 2011). On the contrary, the financial stakes of misperceiving audit probabilities can be substantial in our context.

rare, the fact that they may be salient in memory or discussed by colleagues and the media may induce firms to perceive them to be more frequent than they actually are. Indeed, there is evidence that individuals overestimate the probabilities of a wide range of rare events of a similar nature such as the probabilities of dying in a terrorist attack or in a plane crash (Lichtenstein et al., 1978; Kahneman et al., 1982).

The survey data indicates that the effects of the *audit-statistics* message are inconsistent with the *A&S* predictions. According to *A&S,* if taxpayers overestimate the audit probabilities on average, the *audit-statistics* message should have *reduced* the average tax compliance. On the contrary, the results presented in the previous section show that the *audit-statistics* message *increased* the average compliance.[49] To strengthen this argument, we show that the *audit-statistics* letter indeed had a negative effect on the perceived audit probability.[50] The shallow bars with solid borders in panel (a) of Figure 5 show the distribution of perceptions for respondents who received the *audit-statistics* letter. An inspection of panel (a) of Figure 5 indicates that recipients of the *audit-statistics* letter reported a lower perceived probability of being audited on average, from an average of 40.7% in the pooled control group to an average of 35.2% in the *audit-statistics* group (p-value of the difference 0.03).[51] The same mechanisms behind the *audit-statistics* message are also relevant for the interpretation of the *audit-endogeneity* message: subjects may have found out that the audits were endogenous through the *audit-endogeneity* message and re-optimized their tax evasion accordingly; or they could have had a knee-jerk reaction to any information about audits, even if they knew about the endogeneity component already. Consistent with the evidence on the *audit-statistics* message, we find that the effect of the *audit-endogeneity* message was probably not due to an update of recipients' beliefs, because recipients were already aware of this endogeneity.

[49] One caveat for the test presented in this section, and also for the test presented in the section below, is that we are estimating the effects on the average firm. The fact that the average firm does not behave as *A&S* predicts does not imply that none of the firms behave as *A&S* predicts. For instance, it is possible that some firms updated their perceived probability upwards because of the information contained in the letter and increased their tax payments as a consequence. However, the fact that the average effects are so far from the *A&S* prediction suggests that, if anything, the firms behaving as *A&S* predicted must have been a minority.

[50] One caveat to this interpretation is that a reduction in the self-reported probability of being audited could also be caused by an increase in tax compliance due to the endogenous nature of p with respect to tax evasion.

[51] Additionally, panel (b) of Figure 5 shows that the *audit-statistics* message had a small effect on the perceived penalty rate, decreasing it from an average of 30.5% for the pooled control group to an average of 29.9% for the *audit-statistics* group, with this difference being statistically insignificant (p-value of 0.82). For more details, see also Appendix B.4.3.

5.1.1 Addressing Concerns with Survey Data

This first test relies on survey data, and as such it is subject to some common challenges with this type of data. In this section, we discuss and address some of those challenges.

One potential concern is that the responses about audit probabilities and penalty rates mostly reflect measurement errors because the questions were not incentivized. There are several pieces of evidence suggesting otherwise. First, the fact that the survey beliefs changed depending on the information provided in the letters suggests that these responses contained some truthful information. Second, while there is a large positive bias in the perception of audit probabilities, the average perception of the penalty rate (30.5%) is extremely close to the actual probability computed from administrative data (30.6%). The fact that beliefs are so accurate for penalty rates suggests that individuals responded honestly and thoughtfully. Additionally, individuals were better informed about penalty rates than about audit probabilities, which is also consistent with the fact that there is more readily available information about penalty rates: audits are relatively rare events and their probabilities are not advertised and are not common knowledge, whereas evasion penalties are more openly disclosed by the tax agency.

Another potential issue is that respondents were aware that they were misinformed, so they would never have acted on their biased beliefs anyways. Our survey data provides evidence to the contrary. Even though their estimates were substantially off, survey participants reported being confident about their responses. For example, only 16.2% of those in the control group reported being "Not sure at all" about their perceived probability of audit (on a four point scale, ranging from "Not sure at all" to "Very sure"); and a similar share (18.1%) reported being "Not sure at all" about their guess of the penalty rate. Even for the subgroup of individuals from the control group who reported to be "Very sure" about the audit probability, their average belief was, if anything, slightly more biased: they reported a percieved audit probability of 42.1%, which is still substantially higher than the actual probability of 11.7%.

Another concern is that our subjects may be confused about the questions, for instance if they did not understand the definition of an "audit." Among the 145 responses from the pooled control group, 10.3% of firms reported that they had been audited in the past three years. This share (10.3%) is close to the actual share of firms that were audited (11.7%), thus suggesting that respondents understood the definition of an audit correctly. Moreover, if firms use their own audit history to form their beliefs about auditing probabilities, the firms who had been audited recently should report a higher perceived probability of being audited. Indeed, we found that to be the case: firms who had recently been audited reported a substantially higher average perceived probability of being audited in the future (63.9%) than firms that had not recently been audited (38.1% – p-value of the difference<0.001). Likewise, one may worry that subjects have cognitive limitations when responding to questions about percentages and probabilities. However, this should be a minor concern in our subject pool, which is comprised of business owners who should be familiar with fractions and probabilities. While we do not have administrative data to verify this, the anecdotal evidence indicates that this is a highly educated subgroup of the population. And, at the very least, these business owners need some rudimentary arithmetic and understanding of percentages to compute the VAT and other tax liabilities.

Another potential concern is that some respondents may report a probability of 50% as a way of expressing their uncertainty (Bruine de Bruin et al., 2002; Bruine de Bruin and Carman, 2012). Responses of exactly 50% are somewhat common in our data: among individuals in the pooled control group, 41.61% of responses about the perceived audit probability and 13.5% of responses about the penalty rate are exactly equal to 50%. However, our data indicates that most of these responses of exactly 50% are not a product of uncertainty: individuals who provided an answer of 50% are somewhat less confident, but not dramatically less so, than individuals who provided answers different to 50%.[52] Moreover, even if we ignore the 50% responses, the main result would still be robust: individuals in the control group still substantially overestimate the probability of being audited (average perception of 31.5%, compared to the actual probability of 11.7%).[53]

As an additional validation of the survey data, we can measure the effect of the signals about p and θ from the *audit-statistics* sub-treatments. However, we have limited power for this exercise due to the small sample size, as we only have 365 survey responses in the *audit-statistics* group. With that caveat in mind, the survey data suggests that a percentage point increase in the signal about the audit probability provided in the letter increased the perceived audit probability nine months later by 0.397 (SE 0.288) percentage points.[54] While imprecisely estimated and thus statistically insignificant at conventional levels, the magnitude of this point estimate is consistent with the updating in other information-provision experiments (discussed in more detail in Section 5.2.1 below). Most importantly, the true effects of the signals provided in the letter on beliefs were probably stronger than what the above estimates suggest, due to different sources of non-compliance.[55]

[52]In the pooled control group, the average certainty for perceived audit probability is 2.05 (in the 1-4 scale from "Not sure at all" (1) to "Very sure" (4)) for individuals who responded with a value of exactly 50%, and 2.57 for individuals who responded with a different value (p-value of difference=0.001). For the responses about the perceived penalty rate, the average certainty is 2.05 for individuals who responded with 50%, and 2.50 for those who responded with another value (p-value of difference=0.06).

[53]For more details, see Appendix B.4.2.

[54]For more details, see Appendix B.4.4.

[55]While we are confident that our certified letters reached the firms' owners, we cannot be as confident

We further provide an alternative to this test that does not rely on survey data.[56] We assume that firms form prior beliefs about the audit probabilities based on their own exposure to audits, due the the paucity of information available about the auditing process. Take for instance two firms that have been paying taxes for 10 years and, by chance, one of those firms was audited in the past while the other was not. As a result, the firm that was audited in the past will have a higher perceived probability of being audited in the future. The results from this alternative test provide evidence against the *A&S* mechanism too.

5.2 Second Test of *A&S*: Heterogeneity with Respect to Signals

The second test is based on the differential effects of the values of the signals provided in the letters. According to *A&S*, the effects of the *audit-statistics* message should be increasing in the signals about the audit probability (p) and the penalty rate (θ). The random variation we introduced in the p and θ conveyed in our *audit-statistics* letters allows us to test this hypothesis directly. We first present our estimates of these elasticities, and then compare them with values obtained from calibrations of the *A&S* model.

5.2.1 Elasticities with respect to p and θ

For a less parametric look at the data, Figure 6 estimates the effect of the *audit-statistics* message on VAT payments, but broken down by decile of the signals included in the letter.[57] Panel (a) of Figure 6 presents the effect of the *audit-statistics* message by decile of the signal of p shown in the letter. In the *A&S* framework, we should expect that very low signals of p should reduce tax compliance (since they most likely reduce the firms' perceived probability of audits), whereas the effect should become larger, and turn positive at some point, as we increase the signal of p. The coefficients plotted in panel (a) of Figure 6, however, indicate that the effect of the *audit-statistics* letter is not related to the value of p included in the letter. The coefficients are similar in magnitude for the whole range of values from $p = 2\%$

about whether the owner was the same person who received the email invitation to complete the survey. And while we wanted to conduct the survey shortly after the mailing campaign, we were not able to roll out the survey until nine months after the intervention for reasons beyond our control. In this type of information-provision experiment, the effect of information on beliefs tends to decay substantially in a matter of a few months – recipients may forget the information provided in the letters, or acquire additional information in the meantime. For example, Cavallo et al. (2017) show that the effect of information on beliefs decays by about half in a matter of just three months, and similar findings are reported by Bottan and Perez-Truglia (2017) and Fuster et al. (2018). All these factors will lead to an underestimation of the effects of the letter on beliefs.

[56] For more details, see Appendix B.9.

[57] These results are based on the same specification used for Table 2, except that it also includes dummies for the quintiles of pre-treatment VAT payments as additional controls, from which we drew the sample to calculate p_i and θ_i.

all the way up to $p = 25\%$. Moreover, the resulting linear relationship (shown as a dashed red line) has a slope that is close to zero and statistically insignificant. Panel (b) of Figure 6 provides a similar analysis for the heterogeneity by penalty rates (θ) provided in the letter. According to *A&S*, we should expect a positive relationship between the effect of the *audit-statistics* letter and the value of θ included in the letter. Panel (b) of Figure 6 shows evidence to the contrary: the coefficients are similar for the whole range of values from $\theta = 15\%$ to $\theta = 68\%$, and the slope is close to zero and statistically insignificant.

Using a more parametric approach, we can quantify the effects of the *audit-statistics* sub-treatments in a way that can be contrasted with the quantitative predictions of *A&S*. For the *audit-statistics* treatment arm, we use the following model:

$$
\begin{aligned}
Y_{it} = \alpha_0 + \gamma_p \cdot p_i \cdot Post_t + \gamma_\theta \cdot \theta_i \cdot Post_t + \alpha_1 \cdot p_i + \alpha_2 \cdot \theta_i + \alpha_3 \cdot Post_t + \\
+ \sum_{g=2}^{5} \alpha_{4,g} \cdot I_{\{i \in g\}} + \sum_{g=2}^{5} \alpha_{5,g} \cdot I_{\{i \in g\}} \cdot Post_t + \epsilon_{it}
\end{aligned} \tag{2}
$$

where $p_i \in (0, 1)$ is the signal about the audit probability included in the letter sent to firm i, and $\theta_i \in (0, 1)$ is the signal about the penalty rate included in the letter sent to the same firm. The $I_{\{i \in g\}}$ variables correspond to a set of dummies for the quintiles of pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p_i and θ_i. Including these controls ensures that we only exploit the exogenous variation in p_i and θ_i – that is, the heterogeneity due to sampling variation. $Post_t$ is a dummy variable that takes the value 1 if the observation corresponds to the post-experiment period, and 0 otherwise. Since we are using a Poisson regression model, γ_p and γ_θ can be directly interpreted as elasticities. For instance, an estimate of $\gamma_p = 1$ would imply that a 1 percentage point increase in the audit probability conveyed in the letters increased VAT payments by 1%. *A&S* predicts that $\gamma_p > 0$ and $\gamma_\theta > 0$ – i.e., that firms' tax payments are increasing in the perceived probability of audit and evasion penalty rates.

Panel (a) in Table 3 presents the results from the econometric model of equation (2). Column (1) of Table 3 presents estimates of the elasticities of VAT payments with respect to the values of p and θ conveyed in the *audit-statistics* sub-treatments. The elasticity with respect to the audit probability in the first post-treatment year is -0.063 (p-value=0.796). This means that increasing p by 1 percentage point would decrease VAT payments by a mere 0.063%. The elasticity with respect to the penalty rate is -0.033 (p-value=0.782), which implies that increasing θ by 1 percentage point would decrease VAT payments by 0.033%. The estimates are close to zero, statistically insignificant at standard levels and precisely estimated. The precision implies that we can rule out even moderate elasticities: the 95%

confidence interval for the audit probability excludes elasticities above 0.411, and the 95% confidence interval for the penalty rate excludes elasticities above 0.198.

It should be noted that the pre-treatment falsification test does not yield any statistically significant effects, and that the results are similar for the other specifications: for the second post-treatment year (column (2)), by payment timing (columns (3) and (4)), and by type of tax (columns (5) and (6)). The estimates in all cases are close to zero, precisely estimated and statistically insignificant.

One potential confounding factor for the lack of heterogeneity in signals about p and θ is that some subjects might have interpreted the *audit-statistics* message *per se* as a signal that their firms were under the IRS radar, above and beyond the factual information conveyed in the message. We were careful to mitigate this concern in the design of our mailings. For instance, we highlighted the fact that the letter recipients were randomly selected. Nevertheless, some individuals may have ignored or overlooked this cue. Even if the recipients learned something from the receipt of the *audit-statistics* message, there is no reason why they should not learn about the content of the message as well. In other words, the test presented above continues to be valid, as *A&S* would still predict that the *audit-statistics* message should have a differential effect depending on the values of p and θ.

To address this concern more directly, we use the *audit-threat* treatment arm, in which the tax agency made an explicit threat to every recipient and thus is not subject to this concern. Panel (d) of Figure 4 depicts the difference in the evolution of VAT payments over time between the two sub-treatments in the *audit-threat* arm, corresponding to audit probabilities of 50% and 25%. We mostly find no systematic difference between the two groups in post-treatment VAT payments. We can also provide a more parametric test based on an econometric model similar to that of equation (2) for firms assigned to the *audit-threat* letter:

$$Y_{it} = \alpha + \tau_p \cdot p_i + \delta \cdot Post_t + \gamma_p \left(p_i \cdot Post_t\right) + \epsilon_{it} \tag{3}$$

where $p_i \in \{0.25, 0.50\}$ is the audit probability included in the *audit-threat* letter sent to firm i. Again, *A&S* predicts $\gamma_p > 0$. The results are presented in panel (b) of Table 3. While the estimated coefficient based on *audit-threat* messages implies an elasticity of 0.217 with respect to p in the first year post-treatment, this estimate is not statistically significant (p-value=0.128) and is economically small. Taking into account the precision and power concerns, the evidence from panel (d) of Figure 4 and panel (b) of Table 3 further reinforces our result. Contrary to the prediction of *A&S*, tax compliance does not seem to depend on the probability of being audited, even with a direct and credible threat of an audit (rather than just providing information about audit probabilities).

Taken together, we find robust evidence that firms did not react to the values of p and θ shown in our letters.[58] Since we devoted a large fraction of our subject pool to this treatment arm, these elasticities are precisely estimated. It is not clear, however, whether the estimates are small enough, and precisely estimated enough, to rule out the values of the elasticities predicted by *A&S*. We address this question below.

5.2.2 *A&S* Calibration

For a quantitative test of *A&S*, we need to obtain quantitative predictions from *A&S*. In this section we present results from different calibrations of the model, and compare the calibration results to the experimental results in the following section.

Let Y be the total value-added amount and let $\tau = 0.22$ be the value added tax rate. Let E be the amount to be under-reported (so $\tau \cdot E$ is the amount evaded). Each firm has a utility from income given by a Constant Relative Risk Aversion (CRRA) utility function with risk parameter σ. Let p be the probability that the tax return for a given year will be audited sometime in the future, and θ the penalty rate applied over the amount evaded when caught (both of these parameters are defined as in the *audit-statistics* treatment).

Given any reasonable value for the CRRA parameter, the basic *A&S* model predicts 100% evasion. As a result, we need to use one of the extensions discussed in the literature to accommodate the 26% evasion rate observed in practice (as estimated by Gomez-Sabaini and Jimenez, 2012). We consider the following extensions: endogenous audit probabilities (Allingham and Sandmo, 1972; Yitzhaki, 1987), third-party reporting and whistle-blowing (Acemoglu and Jackson, 2017), misperceptions about audit parameters (Alm et al., 1992) and social preferences (Luttmer and Singhal, 2014).

The probability of being audited can be broken down as $p = p_0 + p_1 \frac{E}{Y}$.[59] The parameter $p_1 > 0$ represents the endogeneity of the audit process, whereby firms that evade more are more likely to be audited (in the original *A&S* model, the audit probability is exogenous so $p_1 = 0$). We can also allow firms to be caught evading due to some non-audit technology such as third-party reporting or whistle-blowing. We represent this with an effective probability of being caught of $p + \epsilon$, where the parameter epsilon represents the additional monitoring tool. To allow for misperceptions, we can simply calibrate p and θ to take the average

[58] Appendix B.6.2 present a series of robustness checks (alternative specifications based on OLS, Tobit and Probit models, and using an alternative data source for the dependent variable), and the results are similar. An additional robustness check, presented in the same Appendix, shows that results are robust if, instead of estimating the elasticities with respect to p and θ separately, we estimate the elasticity with respect to $p * \theta$ (i.e., the expected penalty per dollar evaded).

[59] Note that we implicitly assume that, conditional on being audited, evasion is detected in full. In practice, this probability may be smaller than 1. If anything, this would only make the *A&S* result more puzzling: firms should be even less worried about being audited.

perceptions reported in the survey (instead of the values calculated from the administrative records of the tax agency). To allow for social preferences, we assume that individuals get some direct utility from paying taxes equal to the fraction α of the amount paid. This social responsibility parameter α can take values from 0 to 1, where a higher value denotes higher social responsibility (in the original *A&S*, $\alpha = 0$).

The optimal evasion choice is given by maximizing the expected utility:

$$\max_{E \in [0,Y]} \frac{1 - p\left(\frac{E}{Y}\right) - \epsilon}{1-\sigma}\Big(Y - \alpha\tau(Y-E)\Big)^{1-\sigma} + \frac{p\left(\frac{E}{Y}\right) + \epsilon}{1-\sigma}\Big(Y - \alpha\tau(Y-E) - (1+\theta)\tau E\Big)^{1-\sigma} \tag{4}$$

Given a set of parameters, it is straightforward to find the optimal value of E that solves this maximization problem.[60] Table 4 presents the calibration results. Each row corresponds to a different calibration of *A&S*. The first seven columns correspond to the parameter values, while the last three columns indicate the corresponding predictions: $\frac{E}{Y}$ is the evasion rate, $\frac{\partial log(\tau(Y-E))}{\partial p}$ is the elasticity with respect to the audit probability and $\frac{\partial log(\tau(Y-E))}{\partial \theta}$ is the elasticity with respect to the penalty rate.[61]

All the parameters are calibrated so that the predictions always match the average evasion rate ($\frac{E}{Y}$) of 26% (Gomez-Sabaini and Jimenez, 2012). In the first row, we assume a CRRA of 4 and set the audit probability and penalty rate equal to those estimated from the administrative records ($p_0 = 0.117$ and $\theta = 0.306$). To match the 26% evasion rate, we allow for a non-audit detection rate of $\epsilon = 0.581$. The resulting elasticity with respect to p is 4.548 and the elasticity with respect to θ is 3.475. This is the simplest extension to the *A&S* model, and given that its predictions are in the middle range of all our calibrations, we consider this our preferred specification. The remaining rows present results under alternative calibrations of the model. Even though the models are quite different, the predicted elasticities are in the same order of magnitude as our preferred specification.

In the second row, instead of accommodating the evasion rate of 26% by introducing the non-audit detection rate, we assume a social responsibility parameter of $\alpha = 0.195$. This different approach yields different predicted elasticities, but they are still in the same order of magnitude: the elasticity with respect to the audit probability is 9.422, and the elasticity with respect to the penalty rate is 1.208. The third row follows a similar specification from the second row, but we augment it by allowing for an endogenous audit probability. We let $p_0 = p_1 = 0.0896$, which accommodates two important features of the audit probabilities: the effective audit probability turns out to be equal to the observed average probability of 11.7%,

[60] For more details, see Appendix C.

[61] These two elasticities are defined exactly as in the econometric model from equations (2) and (3), to facilitate the comparison between the regression results and the calibrations.

and, consistent with the content of the *audit-endogeneity* message, a firm that does not evade taxes ($\frac{E}{Y} = 0$) would double its audit probability if it decided to evade taxes ($\frac{E}{Y} = 1$). Since this endogeneity parameter is not enough on its own to fit the observed evasion rate, we again rely on the social responsibility parameter to fit the data by setting $\alpha = 0.229$. This specification shows that introducing the endogenous nature of the audit probabilities does not change the elasticity with respect to the audit probability by much (it is 4.386, similar to the 4.548 from the first specification), but it does substantially reduce the elasticity with respect to the penalty rate (to 0.592). The fourth row follows a similar specification as in the second row, but we extend it by allowing individuals to have biased perceptions about the audits: $p_0 = 0.407$ and $\theta = 0.305$. These biases would not be enough on their own to fit the observed evasion rate, so we again set the social responsibility parameter to $\alpha = 0.590$. Again, we obtain elasticities that are of the same order of magnitude as in the other specifications: the elasticity with respect to the audit probability is 4.021 and the elasticity with respect to the penalty rate is 1.642. The specifications in the second set of four rows are identical to those in the first set of four rows, except that we assume a CRRA of 2 instead of a CRRA of 4. The results indicate that assuming a higher risk aversion leads to elasticities that are even larger in magnitude.

5.2.3 Comparison between Experimental Results and the *A&S* Calibration

We can test the null hypothesis that the elasticities with respect to p and θ in the main specification of the *audit-statistics* presented in column (1) of Table 3 ($\gamma_p = -0.063$ and $\gamma_\theta = -0.033$) are equal to those in our preferred *A&S* calibration. We can reject the null hypothesis that the elasticity is 4.55 for the audit probability and that it is 3.48 for the penalty rate (both tests with p-values$<$0.001). More precisely, the 95% confidence interval for the estimate of γ_p in column (1) of Table 3 is $[-0.536, 0.411]$, and the corresponding interval for γ_θ is $[-0.264, 0.198]$: the calibrated elasticities are substantially above these intervals. These calibrated elasticities are also well beyond the confidence intervals for the value of γ_p estimated for the *audit-threat* model ($[-0.062, 0.496]$, from column (1) of Table 3).

One potential concern with the above comparison is that it is based on the implicit assumption that a letter conveying the message of a 1 percentage point higher signal of p or θ will increase the perception of the parameter by the recipient by 1 percentage point. This is probably a strong assumption: some individuals may not have read the letter in its entirety, they may have not entirely believed in the content of our message, or they may not have updated their prior by the full value of the signal. To establish a benchmark, we can compare our setting with studies of learning from economic variables, such as the inflation rate (Cavallo et al., 2017), the cost of living (Bottan and Perez-Truglia, 2017), and housing prices (Fuster et al., 2018). These studies find that for each percentage point increase in the feedback given to subjects, the average individual updates their beliefs by about half a percentage point. If we assume this rate of learning, then we should double the elasticities estimated in our regressions before comparing them to the calibrations of *A&S*. Under this assumption, we can still reject the null hypothesis that the estimated and the calibrated elasticities of tax compliance with respect to the audit probability are equal (p-value<0.001 for both γ_p and γ_θ).

The effect of the differential values of p and θ in the *audit-statistics* treatments, presented in Section 5.1 above, can provide a direct estimate of the learning rate in our context. The survey data suggests that a percentage point increase in the signal about the audit probability provided in the letter increased the perceived audit probability nine months later by 0.397 percentage points (SE 0.288). Even if it is imprecisely estimated and thus statistically insignificant at conventional levels, it is reassuring that this point estimate suggests a learning rate consistent with other studies of learning. Moreover, we must keep in mind that due to multiple sources of noncompliance, this is probably an underestimate of the true learning rate.[62] Moreover, we can reproduce the analysis under an extremely conservative assumption about the magnitude of the learning rate: even if we assumed that for each percentage point difference in the letter individuals only adjusted their beliefs by one tenth of a percentage point, we would still reject the null hypothesis that the estimated elasticities are equal to those in the *A&S* calibration (p-values of 0.032 and 0.001 for the the audit probability and for the penalty rate, respectively).

5.2.4 Comparison to Related Studies

We can also compare our estimated elasticities to those from related studies. There is a literature that uses laboratory experiments to study tax evasion. These experiments often randomize the probability of being audited by the experimenter and the penalties involved. Consistent with our results, those laboratory studies find evidence of probability neglect. For example, Alm et al. (1992) find an elasticity of 0.169 with respect to the audit probability (comparable to our estimate of -0.063), and an elasticity of 0.037 with respect to the penalty rate (comparable to our estimate of -0.033). Indeed, these elasticities are statistically indistinguishable from those obtained in our study (p-values of the differences are 0.338 and 0.555 respectively).

We can also compare our findings to the results of a couple of related field experiments. Dwenger et al. (2016) conducted a field experiment in the context of a local church tax in Germany for which enforcement was extremely lax. While their experiment was not designed to

[62]For a discussion of the sources of non-compliance, see footnote 55.

test the *A&S* model, it did include one treatment arm where the message mentioned different audit probabilities ($p = 0.1$, $p = 0.2$, or $p = 0.5$). Their results are qualitatively consistent with our finding of probability neglect: the effects of all these probability messages are statistically indistinguishable from each other. Another related experiment, Kleven et al. (2011), included a treatment arm with two different audit probabilities. Consistent with our results, they find economically negligible differences in tax compliance between individuals assigned to different audit probabilities.[63] However, the evidence from Kleven et al. (2011) would still be consistent with *A&S* because their subjects face automatic third-party reporting, which our subjects do not. The authors conducted their experiment with wage earners in a country where tax evasion is automatically detected through third-party reporting regardless of audits. As a result, *A&S* predicts that, consistent with their evidence, wage earners should report their earnings truthfully regardless of the probability of being audited.

6 Discussion: Risk-as-Feelings

In this section, we summarize the findings and discuss their potential interpretations and implications.

We present three main findings. First, we documented ***increased compliance***: on average, the *audit-statistics* message had a positive effect on tax compliance. Second, we reported ***reduced subjective probability***: on average, the *audit-statistics* message decreased the perceived probability of being audited. Third, we documented ***probability neglect***: the effect of the *audit-statistics* message did not depend on the audit probability included in the letter or on the firm's prior belief about this probability. Jointly, these three findings are inconsistent with the predictions of *A&S*. This begs the question of what framework might provide a better fit for these results.

One natural candidate is that of salience (Chetty et al., 2009). In this framework, firms behave as if the probability of detection and the penalty rate are zero unless these parameters are made salient to them. This explanation could reconcile the findings of increased compliance and reduced perceived audit probability: even if firms who were sent the messages adjusted their perceived audit probabilities downwards, they would have behaved as if those probabilities were zero if they had not received those signals. The messages made the audit probabilities salient and thus caused them to not be perceived as zero. However, the salience

[63] In one of their treatments, they send letters to individuals stating large audit probabilities of $p = 50\%$ and $p = 100\%$. Compared with a group that did not receive any letter, they find that the letters had a positive and significant effect on declared income and tax liability. The differential effect between these two conditions, while statistically significant, is economically negligible: an increase in the signal of the probability of audit from 50% to 100% increases reported income by 0.025% and taxes paid by 0.05%.

model fails to fit other features of our findings. First, by definition, salience models imply short-lived effects. A reminder about a non-salient tax should affect the behavior of an agent only at the time of receiving the information, but not days or months later. Effectively, salience models predict a rapid decay of the effect of information over time. This prediction contradicts our evidence about the persistent increased compliance from firms that received our *audit-statistics* letter. The effect, while decaying, remained in force for months after the messages were transmitted.[64] Salience models are also inconsistent with our finding of *probability neglect* – making a high audit probability salient should have a stronger impact than making a low audit probability salient.[65]

A second natural explanation can be found in agency issues within the firm: for instance, the person receiving the letter may differ from the person deciding how much tax to evade. This type of agency issues would generate insensitivity to the information received (or at least an attenuation effect). However, we find that firms do react to the information received, but in the opposite direction to that predicted by *A&S* (i.e., information on low audit probabilities reduces, rather than increases, evasion). Moreover, agency and information frictions should be weaker in smaller firms, and those in our experimental sample are small: over 75% of firms in our sample have 5 or fewer employees.[66] Moreover, the heterogeneity analysis indicates no substantial or statistically significant difference for firms below and above the median number of employees (1 to 3 employees versus the rest), which further reinforces our intuition that principal-agent and information frictions might not be the decisive factor at play in our context.[67]

Our preferred interpretation is based on the model of risk-as-feelings (Loewenstein et al., 2001). The models used for choice under uncertainty are cognitive in that agents make decisions using some type of expectation-based calculus. The risk-as-feelings model proposes that responses to fearsome situations may differ substantially from cognitive evaluations of the same risks (Loewenstein and Lerner, 2003).[68] When fear is involved, agents tend to neglect

[64]The informational treatment may still increase salience and cause an instantaneous effect with lasting consequences, for instance if it induces a change in the way the firm deals with evasion in transactions. However, such a change would imply a constant effect over time, whereas we find a substantial decline in evasion over the year following the intervention.

[65]Another model from behavioral economics that could be considered here is that of prospect theory (Kahneman and Tversky, 1979; see for example Dhami and al Nowaihi, 2007). This extension of the model, however, is unlikely to explain our findings. For example, prospect theory is unlikely to explain our finding of *probability neglect*: although differences between extremely low probabilities can be ignored under prospect theory, the range of probabilities in our context was far from what is normally considered extremely low (e.g., in the *audit-threat* arm, the probabilities were 25% versus 50%).

[66]More precisely, 29.1% of the firms have a single employee, 46.2% have between 2 and 5 employees, and 15% have between 6 and 10 employees.

[67]Results reported in Appendix B.8.

[68]A related concept, the affect heuristic, corresponds to the quick, automatic and intuitive evaluations of

the cost-benefit calculus and instead make quick, automatic and intuitive responses to the risks. A key prediction of this model is that feelings about risk will be mostly insensitive to changes in probability, which is known in the literature as probability neglect (Sunstein, 2002; Zeckhauser and Sunstein, 2010), or the fear that makes individuals focus on the downside of outcomes and thus ignore the underlying likelihoods. There is evidence of probability neglect in a range of fearsome situations involving electric shocks, arsenic, abandoned hazardous waste dumps, pesticides, and anthrax (Sunstein, 2003; Zeckhauser and Sunstein, 2010).

This model of risk-as-feelings can reconcile our three key findings. The model can reconcile the findings of *increased compliance* and *reduced subjective probability*: even if the perceived probability of an audit decreased among the treated subjects, they may still be scared into paying more taxes because they did not rely on cognitive evaluations of probabilities. The risk-as-feelings model predicts *probability neglect* and thus fits out third finding too.

The model of risk-as-feeling suggests that taxpayers overreact to the threat of audits.[69] This interpretation can provide an explanation for the paradox that, despite the low audit probabilities and penalty rates, most taxpayers still report the threat of audits as a major reason for why they report their taxable income truthfully. For example, a survey by the United States Internal Revenue Service (2018) indicates that 61% of U.S. taxpayers claim that a "fear of audits" exerts a significant influence on their tax compliance decisions.[70] In comparison, audits are perceived to be as strong of a deterrent as third-party reporting: 66% of respondents identified "third-party reporting (e.g., wages, interest, dividends)" as an important factor for their tax compliance. Moreover, there is some direct evidence that, consistent with the risk-as-feelings model, taxpayers have an emotional reaction when thinking of tax audits and the tax authority more generally. Some of the evidence comes from laboratory experiments. For instance, Coricelli et al. (2010) conducted a tax evasion game in the laboratory and measured how emotional arousal affected tax evasion decisions. They showed that the intensity of emotional arousal predicts whether individuals evade and by how much. In a related laboratory study, Dulleck et al. (2016) showed a significant correlation between tax compliance and physiological markers of stress during the tax reporting decision. Instances of fear of tax audits can also be found in the media. For example, a

risky situations based on emotions, which might be used as a shortcut for more complex evaluations of risk (Slovic et al., 2004). Borrowing Kahneman's (2003) terminology for the dual system model of the human mind, emotions might influence the intuitive system.

[69] Indeed, this excessive caution has been documented in other contexts (Loewenstein et al., 2001). For example, a fear of terrorist attacks can make people choose other, more dangerous forms of transport; and a fear of shark attacks can lead to unnecessary legislation (Sunstein, 2002, 2003; Zeckhauser and Sunstein, 2010).

[70] More precisely, 32% of respondents claim that a "fear of audits" exerts "a great deal of an influence" and 29% "somewhat of an influence" on whether they honestly report and pay their taxes.

Washington Post (2016) article claims that "a lot of people are super scared of the Internal Revenue Service" and that its powers "can instill a lot of fear," and The New York Times (2009) reported cases where the fear of the tax authority is so extreme that it is considered a phobia.

In other areas of public policy, the risk-as-feelings heuristic can be a problem, because it distorts facts and promotes irrational judgment, leading to suboptimal decisions from a pure risk-assessment perspective. For example, Zeckhauser and Sunstein (2010) and others discuss cases involving regulation of nuclear power, vaccines, and other emotion-arousing issues. For tax collection, however, these behavioral biases might have positive implications for the tax authority's goals. Indeed, there is anecdotal evidence that tax authorities use these tactics to foster tax compliance. In the United States, for example, a disproportionately large number of tax enforcement press releases covering criminal convictions and civil injunctions are released during the weeks immediately preceding Tax Day, presumably to scare taxpayers into preparing compliant returns (Morse, 2009; Blank and Levin, 2010). Some tax experts even claim that the IRS "likes [targeting] celebrities because they get the most bang for their buck in terms of publicity" to "scare the public into complying" (Forbes, 2008).

In particular, the risk-as-feelings framework indicates that vivid imagery can be used to instill fear and bias risk evaluations (Slovic et al., 2004; Zeckhauser and Sunstein, 2010). Coincidentally, tax agencies seem to resort to vivid images in some of their advertising campaigns. A TV advertisement in the United States showed the IRS as "something like poltergeist coming out of a TV set and the world falling apart," followed by the phrase, "Have you filed your income tax?" (United Press International, 1988). The U.K. tax agency used advertisement campaigns that also rely on fear imagery. One poster features a pair of eyes peeking threateningly through a gash in the paper and reads, "If you've declared all your income you have nothing to fear."[71] This anecdotal evidence suggests that some tax administrations may be leveraging fear for tax collection.[72]

7 Conclusions

The canonical model of Allingham and Sandmo (1972) predicts that firms evade taxes by optimally trading off the costs and benefits of evasion, but it is unclear whether real-world

[71] This poster is reproduced in Appendix D.

[72] Whether these fear tactics should be used by tax agencies or not, however, is outside the scope of this paper. For example, these tactics may be ethically questionablein the extent to which they rely on deception. Moreover, actively promoting fear could have unintended negative effects, such as imposing negative psychological stress on taxpayers. For a discussion on the ethical and practical issues with the use of communication efforts to increase tax compliance, see for example Morse (2009).

firms react to audits in this way. We designed a large-scale field experiment in collaboration with Uruguay's tax authority to assess the factors behind firms' evasion behavior and their reactions to audits. Our findings indicate that firms do increase their tax compliance when informed about the auditing process. However, we do not find this reaction to be consistent with the predictions of *A&S*. For example, the information about audits decreased (rather than increased) the perceived probability of being audited; moreover, the effects of our messages about audit probabilities were independent of the signal we conveyed and of the firms' prior beliefs. Models of salience are consistent with the increased compliance and the reduced perceived perception of audit probabilities that we observed, but they are not consistent with our findings of probability neglect. We argue that all three findings can be reconciled by the risk-as-feelings model, which highlights the role of emotions in decision-making and which predicts that agents might exhibit probability neglect in dreaded or feared situations, like paying taxes.

Our findings also contribute to the more general debate about the determinants of tax compliance. One of the main puzzles in the literature is that evasion rates seem too low. Third-party reporting can explain high compliance for some sources of income, such as wage income (Kleven et al., 2011). However, we would expect much higher evasion rates in other contexts, such as self-employed income, where there is limited third-party reporting and low detection probabilities and penalty rates. One traditional explanation for this puzzle is based on tax morale: firms and individuals do not evade taxes because it is the right thing to do (Luttmer and Singhal, 2014). Our evidence suggests an alternative explanation for the puzzle: due to the emotional nature of the decision, audits scare taxpayers into compliance in the same way that scarecrows scare birds.

We conclude by discussing some policy implications. In the traditional framework of *A&S*, the relevant policy lever is the number of audits: the tax agency must find the point at which the marginal cost of an additional audit equals the expected marginal benefit (i.e., higher tax revenues). Our findings suggest that small and medium firms face significant information and optimization frictions when reacting to audits. These frictions introduce new levers for policy-making. For example, tax agencies can decide whether to be transparent about the auditing process,[73] whether to contact taxpayers to remind them of it, and whether to make the costs of being a tax cheat salient and vivid through advertisement campaigns.[74] Indeed, we

[73]On the one hand, our evidence indicates that increasing transparency about the audit probability would reduce the average perceived probability of being audited, which could reduce tax compliance. On the other hand, our finding of probability neglect suggests that, in the end, the reduction in perceived audit probability may not affect tax compliance.

[74]For a practical discussion on how to implement this type of policy, including the drawbacks, see Morse (2009). Furthermore, this same principle can be used to improve compliance with other laws. For instance, Dur and Vollaard (2019) provide experimental evidence to show that the salience of law enforcement can be

discussed anecdotal evidence that some tax agencies may already have a working knowledge of these policy levers. For example, some tax agencies seem to avoid transparency about the auditing process while increasing visibility of enforcement actions around tax day. Some even refer to fear in their advertisement campaigns. However, there is no direct evidence on whether these policies effectively increase tax compliance or whether they have unintended effects, such as instigating so much fear in taxpayers that their anxiety and unhappiness trump the positive effects of increased tax revenues. As stated by Alm (2019) in a recent review of the literature, "the role of emotions in tax compliance decisions remains largely unexamined." Our results highlight the need for more research on probability neglect in the decision to pay taxes. Moreover, additional research should examine the role of emotions on other important economic choices beyond tax compliance.

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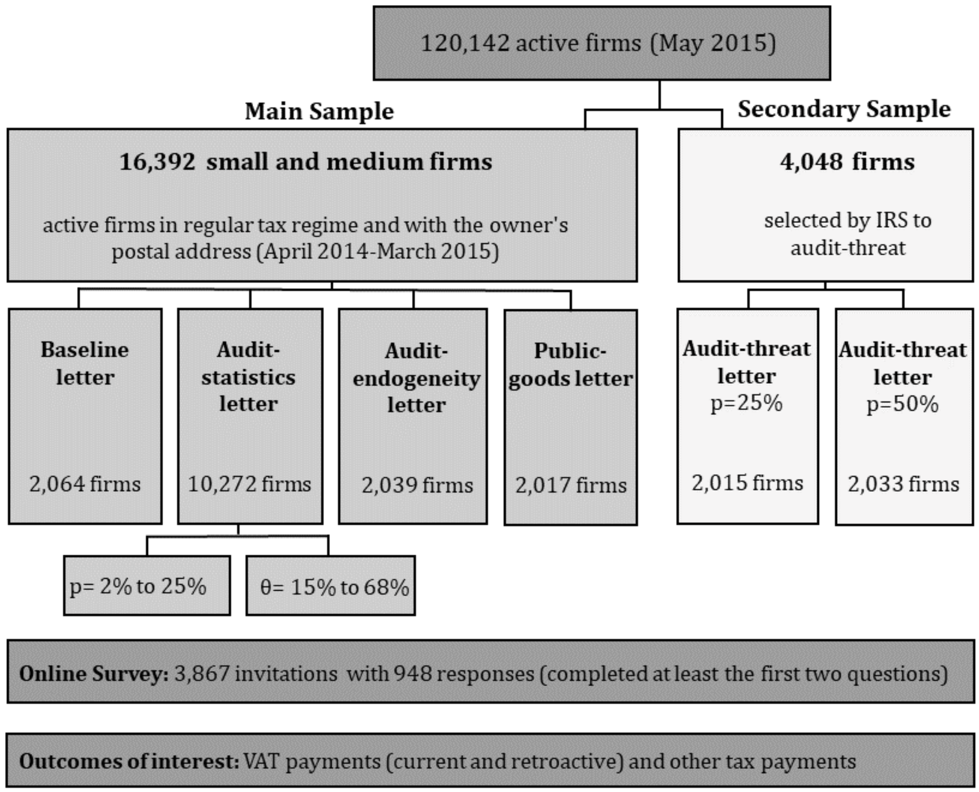
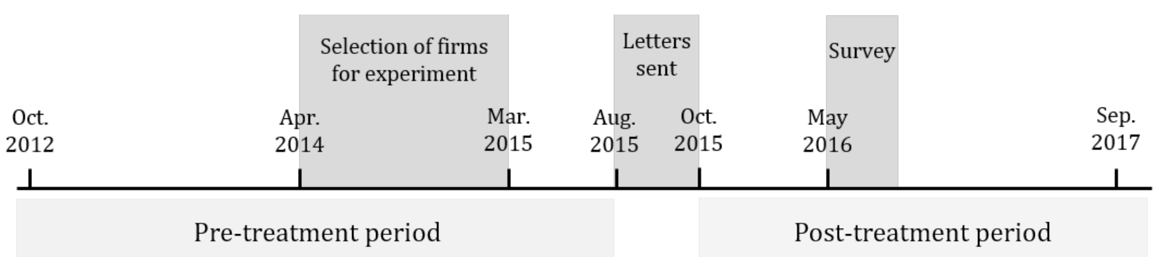
Figure 1: Structure of the Field Experiment

a. Samples and Treatment Arms



b. Timeline



Notes: Panel (a) reports the key features of the experimental design. Panel (b) reports the key dates of the field experiment and the survey.

Figure 2: Sample Letter

DGI | DIRECCIÓN GENERAL IMPOSITIVA

Montevideo, August 20th 2015

Mr../Ms. Taxpayer:

The DGI has the authority to perform inspections (see Art. 68 of the tax code) and routine audits of taxpayers on the basis of crosschecks and assessment of data compiled to detect oversights and inconsistency on tax returns as well as pending tax debts.

MESSAGE

The aim of the DGI, and the primary challenge it faces, is to ensure the collection of revenue to sustain life in society. Additionally, its task is to generate a framework of fair and transparent competition where the failure of some to meet their obligations does not have an unfavorable impact on honest taxpayers. In order to meet these goals, inspections are performed in a routine fashion.

Your micro, small, or medium-sized business has been randomly selected to receive this information. *It is solely for your information and its receipt does not require you to present any documentation to the DGI offices.*

We ask you to comply with your tax obligations for the sake of the country we all want, a more and more developed Uruguay with greater and greater social cohesion.

Sincerely,

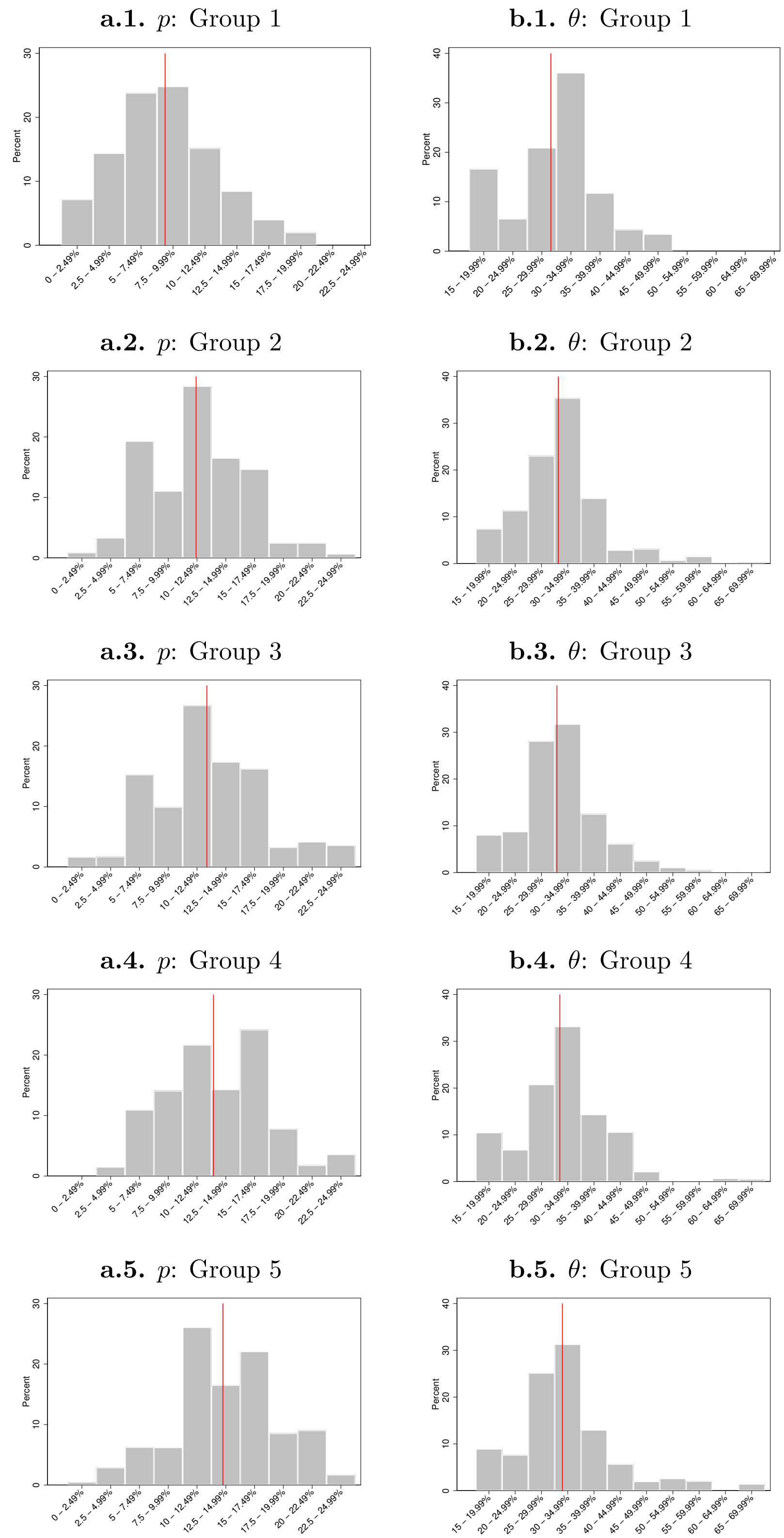
El Director General de Rentas

Lic. Joaquín Serra

Collection and Controls Division
Internal Revenues Services

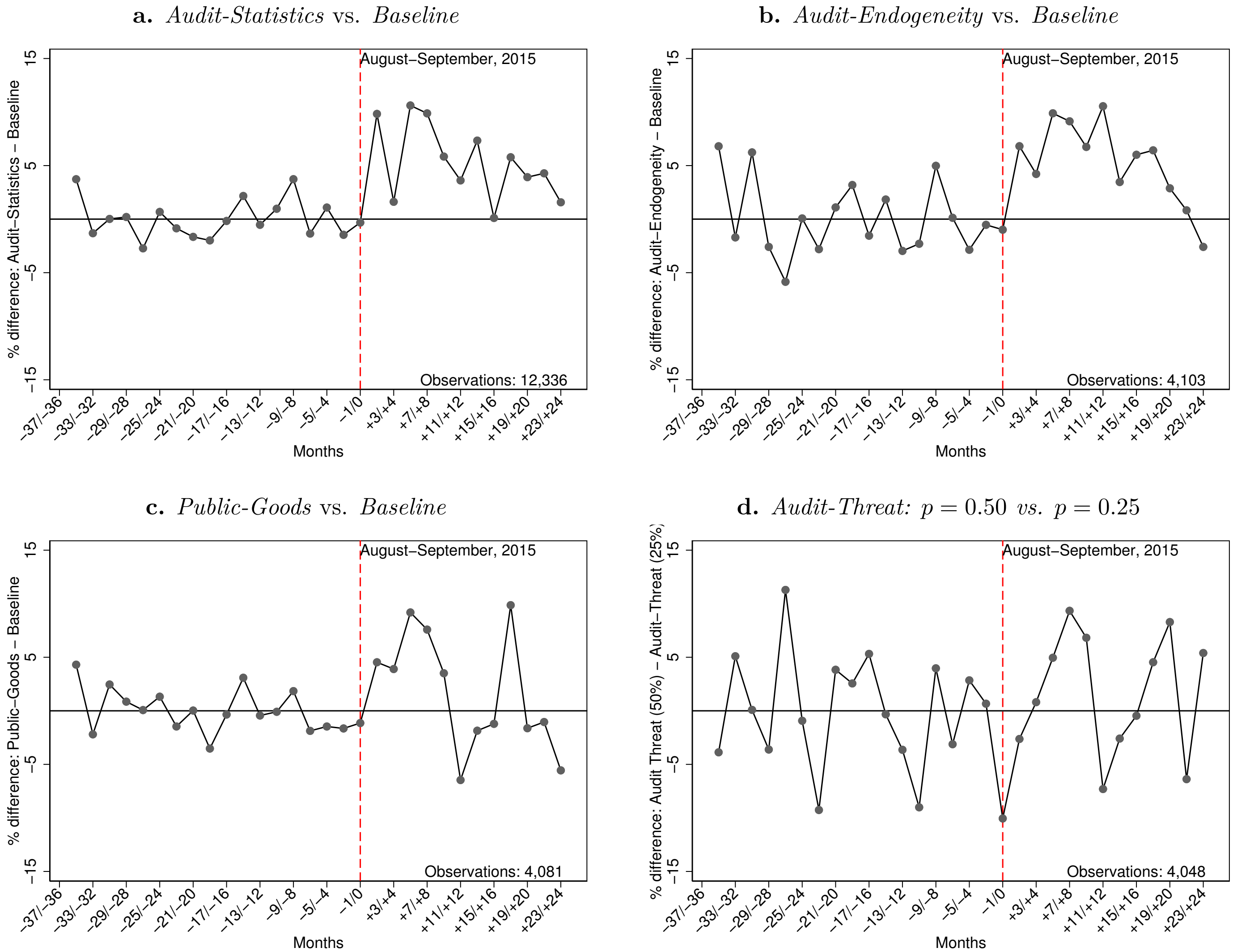
Notes: The *baseline* letter contains information about the goals and responsibilities of the tax authority. In the space with the text MESSAGE, the baseline letter is empty (See A.1 for the full letter). In the *audit-statistics* letter, we added a paragraph to the baseline letter that provided information about the audit probabilities and tax evasion penalty rates (Appendix A.2). In the *audit-threat* letter, we randomly assigned firms to groups with different probabilities (25% and 50%) of being audited in the following year (Appendix A.3). The *audit-endogeneity* letter included information about how evading taxes typically doubles the probability of being audited (Appendix A.4). Finally, the *public-goods* letter included a message providing information about the cost of evasion in terms of the provision of public goods (Appendix A.5).

Figure 3: Distribution of Statistics Shown in *Audit-Statistics* Letters by VAT Payment Quintiles



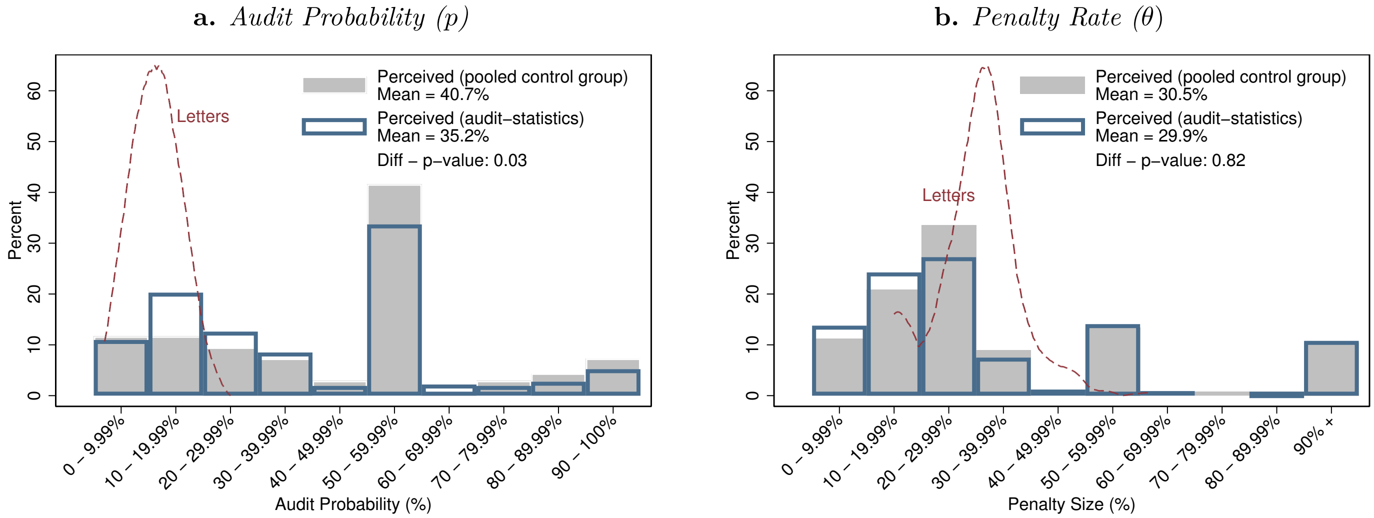
Notes: N=10,272. These panels show the information provided in the *audit-statistics* letter, including the probability of being audited (p, in panel (a)) and the penalty rate (θ, in panel (b)). Groups 1 through 5 correspond to each of the pre-treatment VAT payment quintiles (group 1 being the bottom quintile and group 5 being the top quintile). In each panel, the red vertical line denotes the average audit probability or penalty rate for all the members in the group.

Figure 4: Effects of *Audit-Statistics*, *Audit-Endogeneity*, *Public-Goods* and *Audit-Threat* Messages on VAT Payments



Notes: These figures plot the percentage difference in bi-monthly total VAT payments between treatment and control groups, normalized by the average pre-treatment percentage difference (i.e. between months -35 and 0) for the same outcome. The data covers the period from October 2012 to September 2017. The months of August and September 2015 – when most of the letters were delivered – are defined as the reference bi-monthly period (and marked with the dashed vertical line). Panel (a) presents the effect of the *audit-statistics* message (i.e., the difference between *audit-statistics* and *baseline* letters), while panel (b) represents the effect of the *audit-endogeneity* message and panel (c) depicts the effect of the *public-goods* message. Panel (d) presents the difference between being assigned a 50% probability of being audited (p =50%) and a 25% probability of being audited (p =25%) in the *audit-threat* letters. For each pair of months, VAT payments are top-coded at the 99.99% percentile to avoid the contamination of the results by outliers.

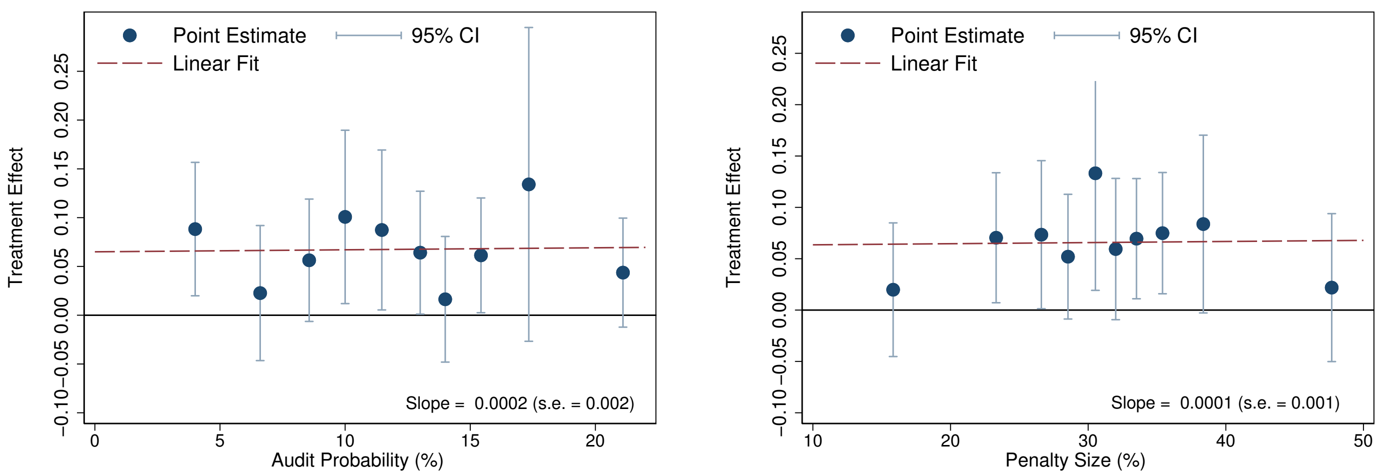
Figure 5: Survey Results: Perception of Audit Probabilities and of Tax Evasion Penalty Rates by Treatment Group



Notes: The histograms are based on the survey responses of individuals who did not self-identify as non-owners in the survey. *Perceived (pooled control group)* (N=137) refers to to survey respondents who received the *baseline* (N=69) or the *public-goods* (N=68) letters during the experimental stage (neither of those two letters contained any information about audit probabilities or penalty rates). *Perceived (audit–statistics)* refers to respondents who received the *audit-statistics* letters (N=365). In panel (a) the x-axis represents the probability of being audited; in panel (b) it represents the average penalty rate. We report the mean responses and the p-value of the difference between the two groups. The answers correspond to questions Q2 and Q4 in the survey (see full survey questionnaire in Appendix A.7). The red line represents the density function of the information displayed in the *audit-statistics* letters, measured in the right y-axis (hidden for the sake clarity).

Figure 6: Effect of *Audit-Statistics* vs. *Baseline* by Deciles of p and θ

a. *Audit Probability (p)* **b.** *Penalty Rate* (θ)



<u>Notes</u>: Panel (a) plots the effect of the *audit-statistics* letter on total VAT payments by decile of p in the first year post-treatment (October 2015 – September 2016), while panel (b) reports the results from the same regressions by decile of θ (N=10,272). In both panels, each dot represents the estimated treatment effect for each decile of the parameter considered. These effects are estimated using a regression similar to the one reported in equation (1) but with two differences. First, instead of including a single treatment variable, we include 10 dummy variables, one for each decile of p or θ. These dummies take the value of 1 if the signal specified in the letter belongs to the corresponding decile in the p or θ distribution, and 0 if the signal corresponds to a different decile, or if the firm was assigned to the *baseline* treatment. Second, we include an additional set of dummies for quintiles of the pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p and θ, and the corresponding interactions with the post-treatment indicator. All effects are depicted with a 95% confidence interval. The results are based on Poisson regressions, so the coefficients can be interpreted directly as semi-elasticities. Confidence intervals are computed with standard errors clustered at the firm level. The dashed line represents the linear fit that results from regressing the treatment effect on the average signal within the decile.

Table 1: Balance of Firm Characteristics across Treatment Groups

	Main Sample					Secondary Sample		
	Audit Statistics (1)	Public Goods (2)	Audit Endogeneity (3)	Baseline (4)	p-value test (5)	Audit Threat (25%) (6)	Audit Threat (50%) (7)	p-value test (8)
Share paid VAT (3 months pre-mailing)	0.925	0.939	0.926	0.928	0.181	0.897	0.891	0.538
	(0.003)	(0.005)	(0.006)	(0.006)		(0.007)	(0.007)	
Amount of VAT paid (3 months pre-mailing)	1.872	1.963	1.926	1.906	0.557	1.739	1.748	0.950
	(0.027)	(0.067)	(0.069)	(0.059)		(0.097)	(0.092)	
Years registered with tax agency	15.338	14.746	15.704	15.009	0.268	19.453	19.425	0.944
	(0.170)	(0.224)	(0.538)	(0.225)		(0.285)	(0.286)	
Share audited between 2013-2015	0.106	0.097	0.089	0.101	0.302	0.134	0.147	0.382
	(0.004)	(0.009)	(0.009)	(0.009)		(0.010)	(0.010)	
Number of employees	4.814	4.658	4.880	5.089	0.962	4.835	4.880	0.795
	(0.264)	(0.538)	(0.566)	(0.635)		(0.126)	(0.117)	
Share filed comprehensive tax return in 2013	0.682	0.687	0.691	0.687	0.871	0.999	1.000	0.558
	(0.005)	(0.010)	(0.010)	(0.010)		(0.001)	(0.000)	
Share no retail goods sector	0.289	0.293	0.283	0.300	0.621	0.431	0.434	0.845
	(0.004)	(0.010)	(0.010)	(0.010)		(0.011)	(0.011)	
Share retail goods sector	0.218	0.219	0.214	0.227	0.775	0.334	0.322	0.398
	(0.004)	(0.009)	(0.009)	(0.009)		(0.011)	(0.010)	
Share services sector	0.493	0.488	0.504	0.473	0.232	0.235	0.244	0.482
	(0.005)	(0.011)	(0.011)	(0.011)		(0.009)	(0.010)	
N	10,272	2,017	2,039	2,064		2,015	2,033	

Notes: Averages for different pre-treatment firm-level characteristics, disaggregated by treatment group and type of sample (robust standard errors are reported in parentheses). The main sample includes all firms selected as described in section 3.2. The secondary sample includes high-risk firms selected by the IRS for the *audit-threat* treatment. The last column of each sample reports the p-value of a test in which the null hypothesis is that the mean is equal for all the treatment groups. Data on the VAT amount and firm characteristics comes from administrative tax records (including monthly payments, annual tax returns and auditing registers). The amount of VAT reported in row 2 is expressed in constant thousands of U.S. dollars as of August 2015.

Table 2: Average Effects of *Audit-Statistics*, *Audit-Endogeneity* and *Public-Goods* Messages on VAT and Other Tax Payments by Time Horizon and Payment Timing

	By Time Horizon		By Payment Timing		By Tax Type	
	First Year (1)	Second Year (2)	Retroactive (3)	Concurrent (4)	Non-VAT (5)	VAT + Non-VAT (6)
a. *Audit-Statitstics* (10,272 firms) vs *Baseline* (2,064 firms)						
Post-Treatment	0.070***	0.032	0.383***	0.053**	0.086**	0.073***
	(0.021)	(0.027)	(0.140)	(0.021)	(0.037)	(0.020)
Pre-Treatment	0.009	0.004	-0.048	0.012	0.008	0.014
	(0.020)	(0.026)	(0.118)	(0.020)	(0.043)	(0.021)
b. *Audit-Endogeneity* (2,039 firms) vs *Baseline* (2,064 firms)						
Post-Treatment	0.071***	0.032	0.264*	0.061**	0.090*	0.078***
	(0.028)	(0.036)	(0.160)	(0.028)	(0.054)	(0.028)
Pre-Treatment	-0.005	-0.009	0.097	-0.010	0.056	0.017
	(0.028)	(0.035)	(0.164)	(0.028)	(0.055)	(0.028)
c. *Public-Goods* (2,017 firms) vs *Baseline* (2,064 firms)						
Post-Treatment	0.051**	0.004	0.208	0.043*	0.067	0.056**
	(0.025)	(0.032)	(0.170)	(0.025)	(0.043)	(0.024)
Pre-Treatment	-0.003	-0.017	-0.088	0.001	-0.038	-0.015
	(0.024)	(0.033)	(0.163)	(0.024)	(0.054)	(0.026)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (1) which compares treated firms to control firms and pre-treatment to post-treatment periods, using yearly aggregated variables. The results are based on Poisson regressions, so the coefficients can be interpreted directly as semi-elasticities. Panel (a) compares the *audit-statistics* message with the *baseline* letter, while panels (b) and (c) replicate the comparison for the *audit-endogeneity* and *public-goods* messages respectively. In the first row of each panel ("Post-Treatment"), the coefficient reported corresponds to a comparison between a post-treatment period and a pre-treatment period. The second row ("Pre-Treatment") presents a falsification test where two pre-treatment periods are compared. Columns (1) and (2) report the effect of treatment by time horizon. The post-treatment effect reported in column (1) corresponds to the difference-in-differences estimate that compares October 2015 – September 2016 to October 2014 - September 2015. The post-treatment effect reported in column (2) is analogous but uses the second year after the treatment as the post-treatment period (i.e., October 2016 – September 2017). For the falsification tests, column (1) is based on a comparison between October 2014 – September 2015 and October 2013 - September 2014, while column (2) compares October 2014 – September 2015 to October 2012 - September 2013. Columns (3) and (4) present the first year effect of treatment on retroactive (3) and concurrent (4) VAT payments. Columns (5) and (6) report the first year results by type of tax. Column (5) presents the effect of the treatment on other (non-VAT) tax payments, while column (6) reports the effect on the total amount of taxes paid by the firms during the same period. In all cases, we restrict the analysis to firms that effectively received the letter as reported by the postal service.

Table 3: Elasticities of Tax Payments with Respect to Audit Probability and Penalty Rate, *Audit-Statistics* and *Audit-Threat* Sub-Treatments

	By Time Horizon		By Payment Timing		By Tax Type	
	First Year (1)	Second Year (2)	Retroactive (3)	Concurrent (4)	Non-VAT (5)	VAT + Non-VAT (6)
a. *Audit-Statitstics* (10,272 firms)						
Audit Probability (%)						
Post-Treatment	-0.063	0.076	0.009	-0.040	0.109	0.038
	(0.242)	(0.232)	(1.103)	(0.249)	(0.240)	(0.208)
Pre-Treatment	0.141	0.018	-1.709	0.229	-0.035	0.063
	(0.164)	(0.203)	(1.118)	(0.162)	(0.230)	(0.147)
Penalty Size (%)						
Post-Treatment	-0.033	-0.175	0.928	-0.098	0.061	-0.001
	(0.118)	(0.134)	(0.763)	(0.114)	(0.103)	(0.092)
Pre-Treatment	-0.128	-0.163	0.204	-0.145	0.018	-0.078
	(0.108)	(0.127)	(0.524)	(0.111)	(0.119)	(0.087)
b. *Audit-Threat* (4,048 firms)						
Audit Probability (%)						
Post-Treatment	0.217	0.250	-0.347	0.205	0.002	0.233**
	(0.142)	(0.175)	(0.676)	(0.209)	(0.176)	(0.111)
Pre-Treatment	-0.185	-0.193	-0.432	-0.149	-0.067	-0.257
	(0.157)	(0.171)	(0.676)	(0.125)	(0.148)	(0.164)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (2) which compares treated firms that received different signals about p and θ. In all cases, we include an additional set of dummies for quintiles of the pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p and θ, and the corresponding interactions with the time variable. The results are based on Poisson regressions with variables expressed in percentage terms, so the coefficients can be interpreted directly as elasticities. Panel (a) presents the effect of providing different information regarding p and θ in the *audit-statistics* message. Panel (b) compares the two *audit-threat* messages, i.e. the 50% threat of audit vs. the 25% threat of audit. For example, rows (1) and (3) of panel (a) present the effect of an additional percentage point of p and θ (respectively) in the information included in the letters on post-treatment VAT payments. In the "Post-Treatment" rows, the coefficient reported corresponds to a comparison between a post-treatment period and a pre-treatment period. In the "Pre-Treatment" rows we present a falsification test where two pre-treatment periods are compared. Columns (1) and (2) report the effect of treatment by time horizon. The post-treatment effect reported in column (1) corresponds to the difference-in-differences estimate that compares October 2015 – September 2016 to October 2014 - September 2015. The post-treatment effect reported in column (2) is analogous but uses the second year after the treatment as the post-treatment period (i.e., October 2016 – September 2017). For the falsification tests, column (1) is based on a comparison between October 2014 – September 2015 and October 2013 - September 2014, while column (2) compares October 2014 – September 2015 to October 2012 - September 2013. Columns (3) and (4) present the first year effect of treatment on retroactive (3) and concurrent (4) VAT payments. Columns (5) and (6) report the first year results by type of tax. Column (5) presents the effect of the treatment on other (non-VAT) tax payments, while column (6) reports the effect on the total amount of taxes paid by the firms during the same period. In all cases, we restrict the analysis to firms that effectively received the letter as reported by the postal service.

Table 4: Predicted Elasticities under Different *A&S* Calibrations

Setup							Predictions		
σ	τ	p_0	p_1	ϵ	θ	α	$\frac{E}{Y}$	$\frac{\partial log(\tau(Y-E))}{\partial p}$	$\frac{\partial log(\tau(Y-E))}{\partial \theta}$
4	0.22	0.117	0	0.581	0.306	1	0.26	4.548	3.475
4	0.22	0.117	0	0	0.306	0.195	0.26	9.422	1.208
4	0.22	0.0896	0.0896	0	0.306	0.229	0.26	4.386	0.592
4	0.22	0.407	0	0	0.305	0.590	0.26	4.021	1.642
2	0.22	0.117	0	0.620	0.306	1	0.26	9.854	6.600
2	0.22	0.117	0	0	0.306	0.169	0.26	18.857	2.111
2	0.22	0.0896	0.0896	0	0.306	0.202	0.26	5.556	0.664
2	0.22	0.407	0	0	0.305	0.534	0.26	8.038	2.829

Notes: Each row corresponds to a different calibration of the extended *A&S* model presented in Section 5.2.2. The first seven columns correspond to the parameter values. The last three columns correspond to the predictions of the model under those parameter values. The predicted evasion rate ($\frac{E}{Y}$) is always 26% because all the specifications were calibrated to match that rate.

Online Appendix: For Online Publication Only

Tax Audits as Scarecrows: Evidence from a Large-Scale Field Experiment

Marcelo Bergolo, Rodrigo Ceni, Guillermo Cruces, Matias Giaccobasso and Ricardo Perez-Truglia

04/11/21

A Letters and Survey

This appendix presents samples of the five types of letter in our experiment: the *baseline* letter (A.1), the *audit-statistics* letter (A.2), the *audit-threat* letter (A.3), the *audit-endogeneity* letter (A.4) and the *public goods* letter (A.5). Additionally, Appendix A.6 presents a sample of the invitation sent by email by the IRS to complete the online survey, and Appendix A.7 presents the questionnaire module about perceptions of audit probabilities and penalty rates that we designed and that was included in the survey.

A.1 Sample Letter: *Baseline* Letter

DGI | Dirección General Impositiva

Montevideo, August 20th 2015

Mr../Ms. Taxpayer:

The DGI has the authority to perform inspections (see Art. 68 of the tax code) and routine audits of taxpayers on the basis of crosschecks and assessment of data compiled to detect oversights and inconsistency on tax returns as well as pending tax debts.

The aim of the DGI, and the primary challenge it faces, is to ensure the collection of revenue to sustain life in society. Additionally, its task is to generate a framework of fair and transparent competition where the failure of some to meet their obligations does not have an unfavorable impact on honest taxpayers. In order to meet these goals, inspections are performed in a routine fashion.

Your micro, small, or medium-sized business has been randomly selected to receive this information. *It is solely for your information and its receipt does not require you to present any documentation to the DGI offices.*

We ask you to comply with your tax obligations for the sake of the country we all want, a more and more developed Uruguay with greater and greater social cohesion.

Sincerely,

El Director General de Rentas

Lic. Joaquín Serra

Collection and Controls Division
Internal Revenues Services

A.2 Sample Letter: *Audit-Statistics* Letter

DGI | Dirección General Impositiva

Montevideo, August 20th 2015

Mr../Ms. Taxpayer:

The DGI has the authority to perform inspections (see Art. 68 of the tax code) and routine audits of taxpayers on the basis of crosschecks and assessment of data compiled to detect oversights and inconsistency on tax returns as well as pending tax debts.

On the basis of historical information on similar businesses, there is a probability of p% that the tax returns you filed for this year will be audited in at least one of the coming three years. If, pursuant to that auditing, it is determined that tax evasion has occurred, you will be required to pay not only the amount previously unpaid, but also a fee of approximately *θ%* of that amount.

The aim of the DGI, and the primary challenge it faces, is to ensure the collection of revenue to sustain life in society. Additionally, its task is to generate a framework of fair and transparent competition where the failure of some to meet their obligations does not have an unfavorable impact on honest taxpayers. In order to meet these goals, inspections are performed in a routine fashion.

Your micro, small, or medium-sized business has been randomly selected to receive this information. *It is solely for your information and its receipt does not require you to present any documentation to the DGI offices.*

We ask you to comply with your tax obligations for the sake of the country we all want, a more and more developed Uruguay with greater and greater social cohesion.

Sincerely,

El Director General de Rentas

Lic. Joaquín Serra

Collection and Controls Division
Internal Revenues Services

A.3 Sample Letter: *Audit-Threat* Letter

DIRECCIÓN GENERAL IMPOSITIVA

Montevideo, August 20th 2015

Mr../Ms. Taxpayer:

The DGI has the authority to perform inspections (see Art. 68 of the tax code) and routine audits of taxpayers on the basis of crosschecks and assessment of data compiled to detect oversights and inconsistency on tax returns as well as pending tax debts.

We would like to inform you that the business you represent is one of a group of firms pre-selected for auditing in 2016. A p% of the firms in that group will then be randomly selected for auditing.

The aim of the DGI, and the primary challenge it faces, is to ensure the collection of revenue to sustain life in society. Additionally, its task is to generate a framework of fair and transparent competition where the failure of some to meet their obligations does not have an unfavorable impact on honest taxpayers. In order to meet these goals, inspections are performed in a routine fashion.

Your micro, small, or medium-sized business has been randomly selected to receive this information. *It is solely for your information and its receipt does not require you to present any documentation to the DGI offices.*

We ask you to comply with your tax obligations for the sake of the country we all want, a more and more developed Uruguay with greater and greater social cohesion.

Sincerely,

El Director General de Rentas

Lic. Joaquín Serra

Collection and Controls Division
Internal Revenues Services

A.4 Sample Letter: *Audit-Endogeneity* Letter

DGI DIRECCIÓN GENERAL IMPOSITIVA

Montevideo, August 20th 2015

Mr../Ms. Taxpayer:

The DGI has the authority to perform inspections (see Art. 68 of the tax code) and routine audits of taxpayers on the basis of crosschecks and assessment of data compiled to detect oversights and inconsistency on tax returns as well as pending tax debts.

The DGI uses data on thousands of taxpayers to detect firms that may be evading taxes; most of its audits are aimed at those firms. Evading taxes, then, doubles your chances of being audited.

The aim of the DGI, and the primary challenge it faces, is to ensure the collection of revenue to sustain life in society. Additionally, its task is to generate a framework of fair and transparent competition where the failure of some to meet their obligations does not have an unfavorable impact on honest taxpayers. In order to meet these goals, inspections are performed in a routine fashion.

Your micro, small, or medium-sized business has been randomly selected to receive this information. *It is solely for your information and its receipt does not require you to present any documentation to the DGI offices.*

We ask you to comply with your tax obligations for the sake of the country we all want, a more and more developed Uruguay with greater and greater social cohesion.

Sincerely,

El Director General de Rentas

Lic. Joaquín Serra

Collection and Controls Division
Internal Revenues Services

A.5 Sample Letter: *Public-Goods* Letter

DGI Dirección General Impositiva

Montevideo, August 20th 2015

Mr../Ms. Taxpayer:

The DGI has the authority to perform inspections (see Art. 68 of the tax code) and routine audits of taxpayers on the basis of crosschecks and assessment of data compiled to detect oversights and inconsistency on tax returns as well as pending tax debts.

If those who currently evade their tax obligations were to evade 10% less, the additional revenue collected would enable all of the following: to supply 42,000 portable computers to school children; to build 4 high schools, 9 elementary schools, and 2 technical schools; to acquire 80 patrol cars and to hire 500 police officers; to add 87,000 hours of medical attention by doctors at public hospitals; to hire 660 teachers; to build 1,000 public housing units (50m2 per unit). There would be resources left over to reduce the fiscal burden. The tax behavior of each of us has direct effects on the lives of us all.

The aim of the DGI, and the primary challenge it faces, is to ensure the collection of revenue to sustain life in society. Additionally, its task is to generate a framework of fair and transparent competition where the failure of some to meet their obligations does not have an unfavorable impact on honest taxpayers. In order to meet these goals, inspections are performed in a routine fashion.

Your micro, small, or medium-sized business has been randomly selected to receive this information. *It is solely for your information and its receipt does not require you to present any documentation to the DGI offices.*

We ask you to comply with your tax obligations for the sake of the country we all want, a more and more developed Uruguay with greater and greater social cohesion.

Sincerely,

El Director General de Rentas

Lic. Joaquín Serra

Collection and Controls Division
Internal Revenues Services

A.6 Sample Letter: Invitation to the Online Survey

Dear Taxpayer:

The DGI's strategic objectives for this period include improving taxpayer services. In 2013, the first Survey on the Costs of Tax Compliance for Small and Medium-Sized Businesses was administered with the support of the Inter-American Center of Tax Administrations (CIAT) and the United Nations (UN). The DGI, in conjunction with a group of academics, has designed a new version of the survey (for more information, visit www.dgi.gub.uy). You can give us your answers on the website where you will find instructions on how to fill out the simple questionnaire; the entire process should take no more than fifteen minutes.

Respond to survey

To address these concerns, a random sample of taxpayers will receive a survey to be answered anonymously.

You are one of the randomly selected taxpayers, which is why you have received this communication. We are grateful for the time and effort you dedicate to assessing this questionnaire and to responding to it as precisely as possible.

Let me assure you that the survey is completely anonymous and the selection of recipients entirely random. The success of this project lies in the precision of your responses. It is on the basis of those responses and the real information they provide that the DGI will be able to hone the design, in the present and in the future, of its strategies to reduce the costs of compliance.

If you have any questions about this questionnaire, please send an e-mail to encuestas@cedlas.org.

We would like to thank you once again for your contribution to this project, which we are sure will benefit all taxpayers.

Sincerely,

Joaquín Serra
Director of the Income Tax Department

PS: If the "Respond to survey" link doesn't open, copy the following address in your browser:https://URL.

A.7 Survey Questionnaire

Introductory Text:

We would like you to respond to a survey about the costs of paying taxes. We hope you have the ten minutes that responding to the questionnaire will require. We are interested in your opinion and hope you will be frank in your responses, which are anonymous and used only for statistical purposes. We would like to thank you for your participation.

Questions Included in Main Module:

Q1) Have you been subject to a DGI audit (inspection or monitoring) at any point in the last three years?

□Yes.

□ No.

Q2)In your opinion,what is the probability that the tax returns filed by a company like yours be audited at least once in the next three years (from 0% to 100%)?

%

Q3) How sure are you of your response?

□ Not at all sure.

□ A little sure.

□Somewhat sure.

□ Very sure.

Q4)Let's imagine that a company like yours is audited and that tax evasion is detected. What, in your opinion, is the penalty (in %) as determined by law that the firm must pay in addition to the originally unpaid amount? For example, a fee of X% means that, for each $100 not paid, the firm would have to pay those original $100 plus $X in fees.

%

Q5) How sure are you of your response?

□ Not at all sure.

□ A little sure.

□Somewhat sure.

□ Very sure.

Q6) In your opinion, if a firm that evades taxes doubles the amount it is evading, what is the effect on its probability of being audited?

□ It would increase significantly.

□It would increase slightly.

□It would not change.

□It would diminish slightly.

□It would diminish significantly.

B Additional Results, Specifications and Robustness Checks

B.1 Descriptive Statistics

Table B.1 reports the firm characteristics including VAT payments made in the three months before we sent the letters, the number of years the firm was registered with the IRS, the number of employees, and other basic variables. Column (2) provides these statistics for all firms in the main experimental sample. On average, firms in our sample had 4.8 employees and had been registered with the IRS for 15.2 years. 10% of the firms had been audited at least once over the previous three years. For comparison, column (1) of Table B.1 shows the same statistics for the universe of all registered firms. By design, the firms in our experimental sample are smaller, both in terms of the number of employees and the level of VAT payments. Column (3) of Table B.1 provides statistics about the secondary experimental sample (i.e., the *audit-threat* treatment arm). Despite some statistically significant differences between the two groups, the firms are broadly comparable in size. The main difference between firms in the two experimental samples is that the audit rates were 4 percentage points higher in the *audit-threat* sample. This difference is set up by design because the IRS selected firms classified as high-risk for the *audit-threat* treatment arm, and such firms were more likely to have been targeted for audits in the past.

Column (4) reports the same descriptive statistics for the sub-sample of firms that were invited to answer the survey and column (5) reports the p-value of the mean difference between firms that were invited and firms that were not. While the share of firms that paid VAT and the amount of VAT paid were not statistically different between the two groups of firms (p-values of 0.875 and 0.993 respectively), there were some statistically significant differences in other characteristics. Firms invited to the survey had been registered with the IRS for 10 months fewer and were 2 percentage points less likely to have been audited in the last three years relative to firms in the main experimental sample on average (p-values of 0.001 and <0.001 respectively). Firms in the survey sample also had 1.6 more employees and were 11.5 percentage points less likely to have filed a comprehensive tax return in 2013 on average (p-value<0.001 for both). There are also some statistically signficant differences in terms of the sectors of operation that the firms belonged to. In particular, firms in the services sector were overrepresented within the group of firms invited to the survey (58.8% vs 49.6%, p-value<0.001) relative to firms in the retail goods sector. Overall, while there are some statistically significant differences in some characteristics, the differences do not seem to be economically significant.

B.2 Delivery Status

One potential threat to the validity of our experimental results could be an imbalance in the delivery rate of the letters across treatment arms. Since we used the certified delivery service offered by the post office, we have credible and complete information about the delivery status of each letter. To rule out the possibility of such an imblanace, Table B.2 reports the delivery status of the letters by treatment arm. Columns (1) through (4) report the distribution of delivery statuses by treatment arm for firms in the main sample, while column (5) reports the p-value of the joint equality test. In general, there are no differences by type of letter between the three treatment arms and the *baseline* letter. In particular, only 19.9% of the letters sent to firms in the main sample were returned. This rate is very similar across treatment arms (p-value=0.290). Firms that show up in the dataset provided by the post office with a delivery status of "Returned" are excluded from the analysis since we know for sure that they did not receive the treatment.

The only statistically significant difference between the treatment arms is the percentage of missing letters, which is smaller for firms that were selected for the *public-goods* message and the *audit-endogeneity* message relative to firms that were selected for the the *audit-statistics* message and the *baseline* message. However, though statistically significant at a 5% level, these differences are not economically significant (6.7% and 6.2% for *public-goods* and *audit-endogeneity* respectively vs. 7.9% and 7.5% for *audit-statistics* and *baseline*).

Columns (6) through (8) provide an analogous breakdown for the secondary sample. In this case, each delivery status is balanced across treatment arms. Compared to the main sample, the percentage of letters returned to the sender was higher (11.9% vs. 19.9%), which may be because this sample was specifically selected by the IRS. However, there are no differences between the two treatment arms within the secondary sample.

B.3 Summary Statistics of Tax Payments

Table B.3 reports some descriptive statistics covering the pre- and post-treatment periods for the outcome variables used throughout the paper for firms that received the *baseline* letter. The pre-treatment period covers the year immediately before the treatment (October 2014 – September 2015) and the post-treatment period covers the twelve months immediately after the treatment (October 2015 – September 2016). On average, the amount of VAT paid by a firm that received a *baseline* letter during the pre-treatment period was USD 7,770 while the median was around USD 4,830, with a standard deviation of USD 8,210. In the subsequent year, the average amount of VAT paid was USD 6,470, the median USD 3,740, and the standard deviation USD 7,770. This represents a reduction of 16.7% in the average VAT payments when comparing pre- and post-treatment periods. Since the group of firms which we analyze mainly includes small- and medium-sized firms, this could be explained by a high turnover rate.

Total VAT payments are mostly comprised of concurrent VAT payments (about 95% of the total). Retroactive VAT payments represent only about 5% of total VAT payments made by the firms that received the *baseline* letter. The small share of retroactive VAT payments made by the firms shows that most taxpayers do not make this type of payment. Indeed, the 75th percentile of this distribution is 0. The average amount of retroactive payments made by these firms during the pre-treatment period was USD 400, while during the post-treatment period it was about USD 300. The trends for both concurrent and retroactive payments are consistent with the trend observed for overall VAT payments. This is further confirmed when considering payments of other taxes and amounts reported in VAT tax returns. Firms paid an average of USD 4,050 of other taxes in the pre-treatment period, and USD 3,300 in the post-treatment period. These amounts include other sales taxes and corporate income taxes, among other types of tax. The standard deviation was USD 8,540 in the pre-treatment period and USD 5,430 in the post-treatment period.

VAT tax returns provide additional information that firms report to the IRS regarding total VAT liabilities. The final VAT liability is computed as the difference between VAT debits and credits. The average final VAT liability for the *baseline* group is USD 7,790 in the pre-treatment period and USD 7,190 in the post-treatment period, showing a similar evolution to the sum of monthly VAT payments. The median VAT liability is USD 5,200 in the pre-treatment period and USD 3,840 in the post-treatment period and the standard deviation is USD 7,910 and 9,930 respectively. There is no differential pattern in the evolution of VAT debits and credits.

In addition to variables that capture the magnitude of VAT payments and are more associated with intensive margin responses, Table B.3 reports some descriptive statistics about whether firms actually make payments and the number of payments that they make. By construction, all of the firms in the sample made at least one VAT payment in the pre-treatment period. 98% of firms made more than three payments and 89% made more than six payments. In the post-treatment period, 96% of the firms made at least one payment, 91% more than three and 71% more than six. This pattern is also consistent with the evolution of the amount of payments and also suggests a high turnover rate for the firms in our sample.

This descriptive analysis indicates the importance of VAT to the Uruguayan tax structure. VAT payments are almost twice as high as payments of other taxes, representing more than 60% of total payments made by firms in our sample.

B.4 Robustness Checks and Additional Results from Online Survey

B.4.1 Survey Results: Selection into the Survey

In this section we report a series of additional results and robustness tests for the analysis conducted with the survey data.

One potential concern about the survey data is selective responsiveness. This could happen if the treatment itself induced differential response rates to the survey, or if particular groups of firms were more likely to participate in the survey. We present three pieces of evidence that contribute to our interpretation of the results.

First, we focus on the response rates to the survey and to the two specific questions about the audit probability and penalty rate that are relevant for our analysis. Table B.4 reports a series of statistics that shows the selection process from the experimental sample to the final sub-sample of firms used in the survey analysis, categorized by treatment arm. Invitations with the link to the survey were sent to all firms in our main and secondary samples who had reported their email address to the IRS. The total number of firms invited to the survey was 3,867, or about 23% of our main experimental sample.[75] The share of firms that started the survey (i.e., that answered at least the first two questions of the survey) was 24.5% and these responses were balanced across treatments. In general, the vast majority of the answers corresponded to individuals who either identified themselves as owners or did not reply to the question about self-identification (about 76.5%). However, accessing the survey does not guarantee that the individual will reach the relevant part of the questionnaire. Although we included the relevant questions for this study at the beginning of the survey, 22.3% of the owners who started the survey did not answer the questions that collected information about prior beliefs of audit probability and penalty rate, which is comparable to the overall non-response rate for the survey. Furthermore, conditional on reaching that point in the survey (i.e., having reported a non-missing value in the previous question) we find that 6.6% and 8.6% skipped the audit probability and penalty questions respectively, which is comparable to the average rate at which they skipped other questions in the survey (6.1%). These are the answers that we use in our analysis, and in all cases the response rates, however measured, are balanced across treatment arms. We provide a more direct test of selective responsiveness by testing the effect of the treatment on the response rates to the two key questions of our analysis (regarding the audit probability and penalty rate). To do this, we define a dummy variable that takes the value of 1 if the individual answered

[75] Note that while we sent some invitations to firms in the secondary sample, these responses are not used in our analysis because there were too few firms that satisfied both criteria.

the audit probability or the penalty rate questions and 0 if they did not. We regress this dummy on an *audit-statistics* treatment indicator using our pooled control group as the reference group (*baseline* or *public-goods* letter recipients). Table B.5 reports the results of these estimates. The *audit-statistics* treatment does not affect the response rate to the audit probability question or the penalty rate question. To address possible differential drop-out rates, we also report the results restricting the analysis to individuals that answered all the survey questions before audit probability and penalty rate questions. The results remain the same. Both pieces of evidence suggest that there is no sign of differential behavior in answering the survey by treatment status.

Table B.6 complements this analysis by providing a balance test for some of the characteristics of the individual survey respondents and their firms, conditional on having answered the audit probability and penalty rate questions. Column (1) presents information about the age, gender and city of the respondent as well as some firm characteristics such as the size, sector of operation, number of years registered with the IRS, number of locations and number of employees for firms that received the *audit-statistics* letter. Column (2) does the same for firms that received the *public-goods* letter, column (3) for firms that received the *audit-endogeneity* letter and column (4) for firms that received the *baseline* letter. Column (5) reports the p-value of the mean test for the four groups. All self-reported characteristics are balanced across treatment arms except for the age of the respondent, the number of years that the firm was registered with the IRS and the percentage of firms with one employee. For those characteristics, the differences are economically irrelevant, though statistically significant at a 5% level.

Third, Figure B.1 reports a series of placebo tests where we replicate our estimation strategy that estimates the effect of the *audit-statistics* message on questions that we do not expect to be affected by our treatment. These questions were reported in other survey modules and were aimed to collect information about tax compliance costs as explained in Section 2. In particular, we report the effects of the *audit-statistics* message on the answers to five placebo questions: 1) "On a scale from 1 to 5 where 1 is "Strongly disagree" and 5 is "Strongly agree", to what extent do you agree with the following statement: *Tax compliance generates non-pecuniary costs*"; 2) "On a scale from 1 to 5 where 1 is "Not stressful at all" and 5 is "Very stressful", please rate the level of stress created by all the steps required to fulfill your tax obligations"; 3) "What is the amount of time that you dedicate to informing yourself about tax your tax obligations?"; 4) "What is the amount of time that you dedicate to registering all the transactions made by your firm?"; and 5) "What is the estimated monthly cost of all the inputs that you used in activities related to tax compliance?". The estimation strategy is exactly the same as the one used for Figure 5 where the comparison group is formed by firms that received the *baseline* letter and firms that received the *public-goods* letter. Figure B.2 contains a summary of the placebo tests compared to the results corresponding to the key variables of interest for our analysis (i.e. the ones reported in Figure 5), where all estimates are expressed in standard deviations of the outcome variable for the control group for comparison purposes. Figures B.1 and B.2 show that, as expected, the *audit-statistics* message had no effect on the responses to these placebo questions.

B.4.2 Survey Results: Robustness Checks

Our analysis of the survey relied on pooling respondents from the *baseline* and the *public-goods* treatments to form a sufficiently large comparison group. The rationale was that neither of the two messages included information about audit probabilities or tax evasion penalty rates. In this appendix, we assess the robustness of the survey results in Section 5.1 to alternative definitions of the sample and of the comparison group.

Panels (a) and (b) of Figure B.3 replicate the results in panels (a) and (b) of Figure 5. The shaded gray bars show the distribution of perceptions for the 69 survey respondents that received the *baseline* letter only (Figure 5 relied on the 137 observations from the pooled *baseline* and *public-goods* groups). The red dashed curve corresponds to the distribution of signals sent to the firms in the *audit-statistics* letters. Although slightly smaller than in the pooled control group, the average perceived audit probability of the *baseline* letter group (37.7%, in panel (a)) is still substantially larger than the 11.7% that results from our data for the overall sample, and this difference is statistically significant. The results are also consistent with the main results when we look at the perceived penalty rate in panel (b). There are no statistically significant differences between the perceived penalty rate by the firms that received the *baseline* letter and our estimates from the overall data.

Second, we present an additional robustness test of the results in Figure 5. To increase the likelihood that survey respondents were the ones who received our experimental messages, we impose an additional restriction. We replicate the analysis, this time restricting our sample to survey respondents who self-identified as firm owners in the survey. The results with this restricted sample (which reduced the treatment group from 365 to 341 observations, and the pooled control group from 137 to 125), presented in panels (c) and (d) of Figure B.3, are very similar to those reported in the body of the paper. Our *audit-statistics* treatment significantly reduced the perceived probability of audits on average, although it did not affect the average perception of penalty rates. Finally, panels (e) and (f) of Figure B.3 report the results of a similar analysis, but using all answers regardless of the respondent's self-reported occupation at the firm. The respondents in this specification thus include not only owners, but also managers, internal accountants, external tax advisors and other employees. This modification increases the size of the treatment group from 365 to 465 and the size of the control group from 137 to 179. The results in terms of magnitude, direction and statistical significance are the same.

An additional concern is that respondents may not have taken the survey seriously. One way of testing this is to analyze the robustness of our results by excluding respondents who indicate 50% as their perceived audit probability or penalty rate (Bruine de Bruin et al., 2002; Bruine de Bruin and Carman, 2012).[76] To address this concern we proceed in two steps. First, we test whether the degree of certainty is different for individuals who indicate a 50% perceived audit probability or penalty rate relative to those who provide non-50% responses.[77] We present the results of this test in Figure B.4. Panel (a) of Figure B.4 reports the distributions of the degree of certainty in the answer provided to the audit probability question for 50% responses and non-50% responses. The average certainty for people with non-50% answers was 2.37 compared to a mean of 2.03 for people who answered 50% (measured on a scale from 1 to 4, where 1 corresponds to an answer of "Not sure at all" and 4 corresponds to an answer of "Very sure"). This difference is statistically significant (p-value<0.001), and suggests that 50% answers indeed corresponded to individuals who did not feel sure about the answer they provided. Panel (b) of Figure B.4 reports the results for certainty about the reported perceived penalty rate, and the results are similar. Individuals with a 50% response feel less certain of their answers relative to individuals with non-50% responses. It is worth noting that these results do not necessarily mean that individuals who provided 50% responses did not take the survey seriously, but simply reflect that they felt less certain about their answers. To rule out the possibility that our results could be driven by this type of response, Figure B.5 replicates our analysis reported in Figure 5 excluding individuals who provided 50% responses. Our findings hold when we exclude such individuals, although our estimates are less precise due to the reduction in the number of answers included in the analysis.

If firms were rational, all these results would imply that firms would have paid less taxes as a consequence of updating their beliefs. However, this is not what we observe. Hence, this evidence supports the hypothesis that the results are being driven by the fear channel rather than a rational re-optimization.

[76] For simplicity, we refer to such cases as '50% answers' or '50% responses' in the remainder of this section.

[77] We directly elicited the degree of certainty about the perceived audit probability (in Q3) and penalty rate (in Q5) in the survey. These questions are reported in the survey questionnaire in Appendix A.7.

B.4.3 Survey Results: Beliefs About Audit Endogeneity

As in the case of the *audit-statistics* treatment arm, we conducted a survey of letter recipients in which we included a specific question to assess whether the information provided in the letter had an impact on beliefs about the endogeneity of audits:

> Perceived Audit Endogeneity: "In your opinion, if a firm that evades taxes doubles the amount it is evading, what is the effect on its probability of being audited?" The possible answers were: It would increase significantly; It would increase slightly; It would not change; It would diminish slightly; It would diminish significantly.

The distribution of responses to this question about the perceived endogeneity of audits is depicted in Figure B.6. The distribution of perceptions in the pooled control group (comprised of firms that received the *baseline* and *public goods* letters) suggests that firms were already aware of this endogeneity. There are no statistically significant differences in the distribution of perceptions for the *audit-endogeneity* group relative to the pooled control group group. On a scale from 1 to 5, where 1 indicates that more evasion significantly increases the probability of being audited and 5 indicates that more evasion significantly diminishes the probability of being audited, the average belief was 1.45 in the pooled control group and 1.41 in the *audit-endogeneity* group (p-value of the difference=0.67).

B.4.4 Survey Results: Relation Between Signal and Self-Reported Perception

Figure B.7 shows the raw relation between the signal included in the letter and the perceptions of the audit probability and penalty rate reported in the survey. The x-axis depicts the signal included in the letter and the y-axis depicts the self-reported parameter. Panels (a) and (b) of Figure B.7 report the raw scatterplots for the audit probability and the penalty rate respectively, where each dot represents a pair of values and the size of the dot is proportional to the number of individuals with a given signal and self-reported perception. While there is a positive relation between the signal and the reported perception for the audit-probability parameter (depicted in panel (a)), it is clear that individuals overestimate the chances of being audited even after receiving the letter. Panel (b) also reports a positive relation between the signal sent in the letter and the self-reported perception for the penalty rate, but in contrast to the audit probability, perceptions of the penalty rate seem to be less disperse and closer to the actual value (about 30%). Panels (c) and (d) of Figure B.7 provide binned scatterplots that depict the results of regressing the self-reported perception of the parameter onto the signal included in the letter for the audit probability and penalty rate respectively. For the audit probability (depicted in panel (c)), individuals seem to have an adjustment rate of 40%. This means that for each additional percentage point increase in the signal, individuals reported 0.4 additional percentage points in their perceived audit probability. The p-value associated with this coefficient does not allow us to reject the null hypothesis of no updating (p-value=0.169). For the penalty rate, the results depicted in panel (d) suggest that the information contained in the letter did not affect individuals' perceptions at all. In particular, for each additional percentage point increase in the penalty rate as reported in the letters, individuals adjusted their perceived penalty rate by less than 10%. With a standard error of 0.20, the null hypothesis of the signal having no effect on perceptions cannot be rejected (p-value=0.965).

The information included in the letter may also have altered the degree of certainty that individuals have about their perceptions of audit probability and penalty rates. Figure B.8 shows the effect of the *audit-statistics* letter on answers to questions Q3 and Q5, which ask directly how certain individuals are about their responses to the questions about the perceived audit probability and penalty rate.[78] Each question has four possible answers: "Not sure at all", "A bit sure", "Sure" and "Very sure". Figure B.8 suggests that if anything, the information provided in the letter made firms more insecure about their perceptions of p but did not affect their confidence about their perceived θ (p-values of 0.0688 and 0.8120 respectively).

B.5 Dynamics of the Effect-Raw Comparison Treatment vs Control

Figure B.9 plots the evolution of total VAT payments by treatment status, grouped by pairs of months. This raw visualization of the data allows for a very simple comparison between the treatment and comparison groups at each pair of months depicted. We group VAT payments by pairs of months because many firms in our sample are only required to make VAT payments bi-monthly. Period 0, represented by the vertical dashed line, is defined as August-September 2015, which is the period when the letters were delivered by the post office. In all panels, the lighter solid line represents the evolution of VAT payments for the comparison group, while the darker dashed line represents the evolution of VAT payments for the treated group. Panels (a) through (c) of Figure B.9 depict the treatment-control comparison in the main sample (i.e., *audit-statistics* vs. *baseline*, *audit-endogeneity* vs. *baseline* and *public-goods* vs. *baseline* respectively). Panel (d) of Figure B.9 provides a comparison between the two treatment groups in the secondary sample: 50% audit probability threat vs. 25% audit

[78] These questions are reported in the survey questionnaire in Appendix A.7.

probability threat. In all cases, we exclude firms whose letters were marked as "Returned" by the post office.

The results in Panels (a) through (c) of Figure B.9 indicate that there is no difference in VAT payments between firms that received any of the treatment messages and firms that received the *baseline* letter before the letters were sent (this is formally contrasted by means of the balance tests reported in Table 1 and the falsification tests reported in Table 2). However, immediately after receiving a letter, a wedge in VAT payments shows up between the two groups, and firms that were treated with any of the messages start to pay larger amounts of VAT compared to firms that received the *baseline* letter. These differences seem to be larger in the first 12 months after the experiment, although a smaller gap remains after the first post-treatment year. These patterns are consistent with the ones that can be derived from the difference-in-differences specification reported in Table 2. Panel (d) of Figure B.9 focuses on the secondary sample. The figure suggests that the evolution of VAT payments by treatment arm in this special sample is more noisy than in the main sample and that no clear pattern can be observed immediately after the letters were sent or in subsequent periods. This is consistent with the results reported in Table 3, where the results clearly indicate that the *audit-threat* letter did not have a differential effect on the group of firms that received the higher signal about the probability of being audited.

B.6 Robustness Checks: Regression Analysis

B.6.1 Robustness Checks of Main Specification

To assess the robustness of the results from the main specification in Section 4, Table B.7 presents alternative estimates for the effects of the *audit-statistics*, *audit-endogeneity* and *public-goods* treatments based on different specifications. The first column presents estimates of the treatment effects based only on the extensive margin of VAT payments: i.e., the outcome is coded as 1 if the firm made at least one payment in the post-treatment period, and 0 otherwise. There is not much variation in the extensive margin: 96% of firms in the sample made positive payments in the post-treatment period. This is a direct byproduct of the selection of the subject pool: we excluded all firms who did not make at least three payments in the 12 months before the treatment assignment. To complement these results, columns (2) and (3) present the average treatment effects on alternative outcomes related to the number of payments made by the firms. Column (2) reports the average treatment effect on the probability of making at least three payments and column (3) does the same for the probability of making at least six payments in the same period.[79] The effects of the three different messages on the extensive margin and on the number of payments are close to zero and statistically insignificant.

The specifications in columns (4), (5) and (6) of Table B.7 use the amount of VAT payments as the dependent variable. Column (4) corresponds to our main Poisson specification. Column (5) presents estimates based on OLS regressions and column (6) presents estimates based on Tobit regressions. The Poisson model has a key advantage in this context: it deals naturally with the bunching of payments at exactly zero while still allowing for the effects to be proportional. By contrast, the OLS specification does not deal with the bunching at zero and does not allow for the effects on amounts to be proportional. Because of the nature of our sample, many firms are required to make VAT payments bi-monthly. The Tobit specification is more appropriate than OLS since it takes into account the censored nature of the data at zero, but it does not allow for the effects to be proportional either.

Columns (4), (5) and (6) of Table B.7 present estimates based on these specifications and show that the results are identical in terms of sign and statistical significance of the coefficients, indicating that they are robust to these three alternative specifications. If anything, the effects are more statistically significant when using the OLS and Tobit models. Even though the results from the Poisson, OLS and Tobit models are not directly comparable in terms of magnitudes, they are roughly consistent. For instance, the Tobit model suggests that the *audit-statistics* message has an effect of USD 451 (p-value=0.003). Since the average outcome is USD 6,470, this Tobit coefficient amounts to an effect of about 6.9%, which is in the same order of magnitude as the Poisson model, which indicates an effect of 7.0% for the *audit-statistics* message (p-value=0.001).

Finally, column (7) of Table B.7 reports the results of estimating our model on the final VAT liability calculated from an alternative administrative data source: the annual tax returns. This outcome overcomes the possible concern about tax delinquency as a driver of our results, specifically on the effect of retroactive payments. The time frame for this outcome is completely different to that of the monthly VAT payments used for our main specification, and therefore the amounts (and thus the results) do not necessarily match. However, the results in column (7) of panel (a) of Table B.7 indicate that the effect of the *audit-statistics* message is 5.6% with this alternative measure of the outcome variable, which is indeed similar in sign and magnitude to our main result. This effect is precisely estimated and statistically significant at the 5% level. The effect of the treatment in the falsification

[79] We do not include an outcome variable that reflects the treatment effect on the probability of making twelve payments during the first post-treatment year because many firms in our sample are required to make VAT payments just bi-monthly and therefore only 25% of the sample makes payments every single month. See Table B.3 for more details about the distribution of monthly payments.

test is indistinguishable from zero.

The results in panel (b) of Table B.7 indicate that the effect of the *audit-endogeneity* message on the VAT reported in the annual tax return is also similar to its effect on the monthly VAT payments. The point estimate of the coefficient is 5.9%, which is smaller than the result for our main specification. However, in this case the coefficient is less precisely estimated and statistically indistinguishable from zero at conventional levels. Finally, the results in panel (c) of Table B.7 indicate that the effect of the *public-goods* message on the annual VAT liability reported in the tax return is close to zero, which is consistent with the small effects observed immediately after receiving the letter that fade a few months later.

B.6.2 Alternative Specifications for the Effects of Signals about Audit Probabilities and Penalty Rates

We also assess the robustness of the estimated effects of the signals of audit probabilities and penalty rates on post-treatment payments in two different ways.

First, panel (a) in Table B.8 replicates the analysis in Table 3 for the additional specifications that we also used in Table B.7. The results are essentially the same as the ones reported in Section 5: i.e., the specific information on audit probabilities and penalty rates included in the *audit-statistics* message had no effect on compliance. Both at the extensive margin and the intensive margin, regardless of the specification used or the source of the dependent variable, all coefficients associated with the treatment variable are statistically insignificant. This is also true for the effects of the *audit-threat* message (panel (b) of Table B.8). Conditional on being treated, the information reported in the letter does not affect firms' compliance behavior.

Second, Table B.9 presents the results for an alternative specification of the elasticity estimation in Table 3. Instead of estimating the elasticities with respect to p and θ separately, we estimate the elasticity with respect to the product (expected penalty) $p * \theta$ in a regression of the form:

$$Y_i = \alpha + \tau_{p \cdot \theta} \cdot p_i \cdot \theta_i + \delta \cdot Post_t + \gamma_{p \cdot \theta} \left(p_i \cdot \theta_i \cdot Post_t\right) + \sum_{g=1}^{5} I_{\{i \in g\}} \left(\pi_g + \kappa_g \cdot Post_t\right) + \epsilon_i \quad \text{(B.1)}$$

Similar to the model where p and θ were included separately, the elasticity computed with this alternative specification is statistically and economically insignificant.

Panel (a) of Figure B.10 provides additional evidence of the effect of the *audit-statistics* message (relative to the *baseline* letter). In the spirit of Figure 4, Figure B.10 summarizes the raw data, this time dividing the sample into two groups: firms that received letters with low signals of p ($p \leq$11.7%) and firms that received high signals of p ($p >$11.7%). Note that the evolution of the outcome variable is extremely similar for the two groups.

Panel (b) of Figure B.10 presents the equivalent analysis for firms that received messages with low and high values of θ (below and above the mean penalty rate in our sample, 30.6%). Again, the effects are very similar for the two groups.

The results in Table B.8, Table B.9 and Figure B.10 provide further evidence supporting the fear channel rather than a rational re-optimization as a consequence of being exposed to some information about the tax enforcement mechanisms.

B.7 Additional Test: Decomposition of Other Taxes

In this section we report additional and more detailed evidence about the treatment effects by analyzing separately the effects of the letters on different types of payments that firms made regularly to the IRS. Regular payments by the firms to the IRS are comprised of VAT payments, corporate income tax payments, property tax payments and personal income tax withholding on behalf of employees. Table B.10 replicates the difference-in-differences estimates of the average effect of the different treatment arms presented in Table 2 but for different aggregations of payments for the first year after the experiment. The table reports both the treatment effect and the estimates from the falsification test that compares two pre-treatment periods. Table B.11 does the same but for the results presented in Table 3. Column (1) reports the average effects on the total amount of tax paid by the firms which is the sum of VAT, corporate income tax, property tax and personal income tax withholding payments. Columns (2) and (3) separate total payments into VAT and other taxes, while columns (4), (5) and (6) decompose other taxes into their three components.

The results provided in Table B.10 show that the treatment effects caused by the *audit-statistics*, *audit-endogeneity* and *public-goods* letters are not restricted to VAT payments, since payments of other taxes also increased by a slightly larger magnitude. However, when separating the 'other taxes' variable into each of its components, the effects are too imprecise to identify statistically significant effects on each of them separately. Overall, all the coefficients are positive and of the right magnitude with no pre-treatment imbalances, but only the effect of the *audit-statistics* message on the corporate income tax is statistically significant. The interpretation of the results reported in Table B.11 is similar, showing that the information contained in the letters did not seem to have an effect on tax payments except for personal income tax withholding, which is statistically significant in some specifications, but with no clear and consistent pattern across specifications.

B.8 Additional Test: Heterogeneity with Respect to Firm's Characteristics

To provide a more detailed description of how the messages affected different types of firms, in this Appendix we report the treatment effects disaggregated by different firm characteristics. It is important to note that the administrative records only contain a few observable firm characteristics. For this reason, the analysis is based on four characteristics: the number of employees, the number of years that the firms was registered with the IRS, the type of tax return filed and the activity sector the firm operates in. For the number of employees and the years registered with the IRS, we split the sample in two depending on whether the observed value falls above or below the median (2.5 and 12.7 respectively). The IRS allows different VAT schedules depending on the size of the firm and this is reflected in the type of tax return that firms have to file. Even within the small- and medium-sized firms considered in this experiment, there are different types of tax returns. For the heterogeneity analysis we divide firms into two groups: firms that have to file a more comprehensive tax form (associated with larger firms) and firms that have to file a simplified tax form (this includes special VAT regimes such as fixed VAT and professional independent VAT that have a separate form). As reported in Table 1, about 68% of firms filed a comprehensive VAT tax return in 2013. Finally, we also have information about the sector which the firm operates in. In this case we split the sample into three groups: firms that sell goods but are non-retail (about 29%), firms that sell retail goods (about 22%) and firms that provide services (49%).

Figure B.11 depicts the average effects of the letters on VAT payments in the first year after the experiment (and the associated 95% confidence intervals) for each of the groups defined in the previous paragraph (similar to the "Post-Treatment" coefficient in column (1) of Table 2). Figure B.12 reports the same estimates but for the falsification test that compares treated and control firms, but in two pre-treatment periods (similar to the "Pre-Treatment" coefficient in column (1) of Table 2). Figure B.13 reports the estimated elasticities with respect to the audit probability and penalty rate included in the letter for the same groups of firms. This is analogous to the results reported in Table 3. Figure B.14 reports the falsification test for the estimated elasticities. Figures B.11 and B.12 are based on four regressions. Each regression is an augmented version of equation (1) where we include an additional interaction between the coefficient of interest and a dummy for the group of interest and all the corresponding interaction terms. Figures B.13 and B.14 are similar but augment equation (2) instead. This is our baseline specification for the elasticities of VAT payments to the parameters provided in the letters. Panel (a) of Figure B.11 compares the *audit-statistics* message with the *baseline* letter, while results in panels (b) and (c) replicate the comparison for *audit-endogeneity* and *public-goods* messages respectively. The notes in the figure report the p-value of the test that compares whether the treatment effects are different across groups defined within a category.

The coefficients depicted in Figure B.11 show that while the letters have a positive effect on VAT payments overall, it seems to be very homogeneous across types of firms. In particular, out of fifteen tests of equality of coefficients, only one is rejected at a 10% level (firms filing a more comprehensive tax return seem to respond slightly more than firms that do not file comprehensive tax returns). Figure B.13 is also consistent with homogeneous responses across groups of firms. Estimated elasticities are close to zero on average (as our main specification suggests) and there are no differences between groups based on any of the variables considered except for firms in the service sector where the test of equality of coefficients can be rejected at a 1% level (p-value=0.013). Falsification tests reported in Figures B.12 and B.14 are reassuring since they show that there are no differences in the "fake" treatment effect between firms in the pre-treatment period.[80] The general conclusion seems to be that all firms respond in a fairly similar way, although we do not have enough power to capture these differences.

B.9 Additional Test: Heterogeneity with Respect to Prior Beliefs

B.9.1 Measuring Prior Beliefs

In the *A&S* framework, firms with different prior beliefs about the probability of being audited should react differently to signals and information about this probability. To test this hypothesis, we need a measure for the prior beliefs of a particular firm. We construct a proxy of prior beliefs based on the firm's own audit history.

The intuition behind this approach is that, since there is little publicly available information about audit probabilities, firms may form their beliefs based on their own audit experience. For instance, when a firm registers with the tax authority, its initial belief may follow the beta distribution with parameters $\{\alpha_0, \beta_0\}$. Assume that firm i has been registered for T_i years before our mailing campaign, and during this period it has experienced $N_i \leq T_i$ audits. If firm i is Bayesian, its belief about annual probability of being audited should follow a beta distribution with parameters $\{\alpha_1 = \alpha_0 + N_i,\ \beta_1 = \beta_0 + T_i - N_i\}$. The mean of that belief should be $\frac{\alpha_0+N_i}{\alpha_0+\beta_0+T_i}$. In turn, this implies a belief about the probability of

[80] It is worth noting that while in general the results from the falsification tests are reassuring and centered around 0, there is a common pattern of statistically significant "false" effects when splitting the sample by type of tax return filed. This is due to an anomalous change in VAT payments in the *baseline* group for firms that did not file a comprehensive tax return. In particular, VAT payments for the *baseline* group that did not file a tax return fell significantly between the two years considered for the falsification test, which is why we observe a positive "false" treatment effect. This is observed across panels because the baseline group is used as the comparison group in panels (a) through (c).

being audited at least once in the following three years of $\hat{p}_i = 1 - \left(1 - \frac{\alpha_0 + N_i}{\alpha_0 + \beta_0 + T_i}\right)^3$. In our main specification, we generate these proxies by setting $\{\alpha_0 = 0.13, \beta_0 = 1\}$. This baseline calibration generates an average belief that matches the actual average probability in our administrative data, and we offer robustness tests using alternative calibrations.[81]

We can use the survey data to validate this proxy for the prior belief. Among the 145 responses from the pooled control group, 10.3% of firms reported that they had been audited in the past, and these firms reported a higher average perceived probability of being audited at least once in the following three years (63.9%) relative to firms that had not recently been audited (38.1%), a large and statistically significant difference (p-value<0.001).[82] This evidence suggests that, consistent with our proxy, firms are using their own audit history to form beliefs about the probability of being audited in the future.

B.9.2 Results

In the *A&S* framework, the effect of the *audit-statistics* letter on tax compliance should be larger for firms with relatively low priors for the audit probability ($\hat{p}$) compared to those with relatively high values of $\hat{p}$. More specifically, the *audit-statistics* message conveyed a signal of $\hat{p} = 11.7\%$ on average. The effect of this message on compliance should thus have been positive for firms with $\hat{p} < 11.7\%$, since the signal should increase their perceived audit probability on average. On the contrary, the effect of the *audit-statistics* message should be negative for firms with $\hat{p}>11.7\%$: on average, the signal should reduce their perceived probability of being audited.

Figure B.15 presents the results from this exercise. Panel (a) of Figure B.15 presents a binned scatterplot of the treatment effect of the *audit-statistics* letter for the four quartiles of $\hat{p}$.[83] This figure includes a vertical dashed line at 11.7%, the average audit probability conveyed in our letters. In contrast to the predictions from *A&S*, we fail to find a negative relationship between the effect of the *audit-statistics* message and the value of the prior belief: the slope is negative (-0.0006), but economically small and statistically insignificant (p-value of 0.525). Panel (b) of Figure B.15 presents a more direct test by combining the heterogeneity

[81] Since we only have information about audits for firms in our sample for the previous 15 years, we set the maximum number of years registered with the IRS at 15 to compute these priors.

[82] As reported in a follow-up paper (Bergolo et al., 2018), the indicator of recent audits is the single most important predictor of perceived audit probabilities among a host of different factors. The recent audit experience also has a positive effect on the perceived penalty rate, but this effect is less significant: respondents from firms that were audited recently report an average perceived penalty rate of 40.0%, compared to 29.4% for respondents from firms that were not audited recently, although this difference is statistically insignificant (p-value=0.201).

[83] We base our analysis on quartiles of the probabilities because there is substantial bunching at 0, thus forcing us to divide the sample in four to get even-sized groups.

in prior beliefs with the heterogeneity in signals. Instead of grouping firms by their prior beliefs, we group them by the difference between their prior and the specific signal sent to each firm in its personalized letter. The intuition is that the difference between the prior and the signal is the "surprise" conveyed by our information treatment. The figure includes a vertical dashed line at 0, which is the point were firms receive signals that are equal to their priors. The effect of the *audit-statistics* letter on compliance should be decreasing in $\hat{p} - p_{signal}$, positive for the group with $\hat{p} - p_{signal} < 0$ (i.e., those for whom the signal was higher than their prior) and negative for the group with $\hat{p} - p_{signal} > 0$ (i.e., those receiving a signal indicating that they were overestimating the audit probability). The results in panel (b) are consistent with the results from panel (a): the slope of the relationship (indicated by the dashed red line) is zero (p-value=0.986).

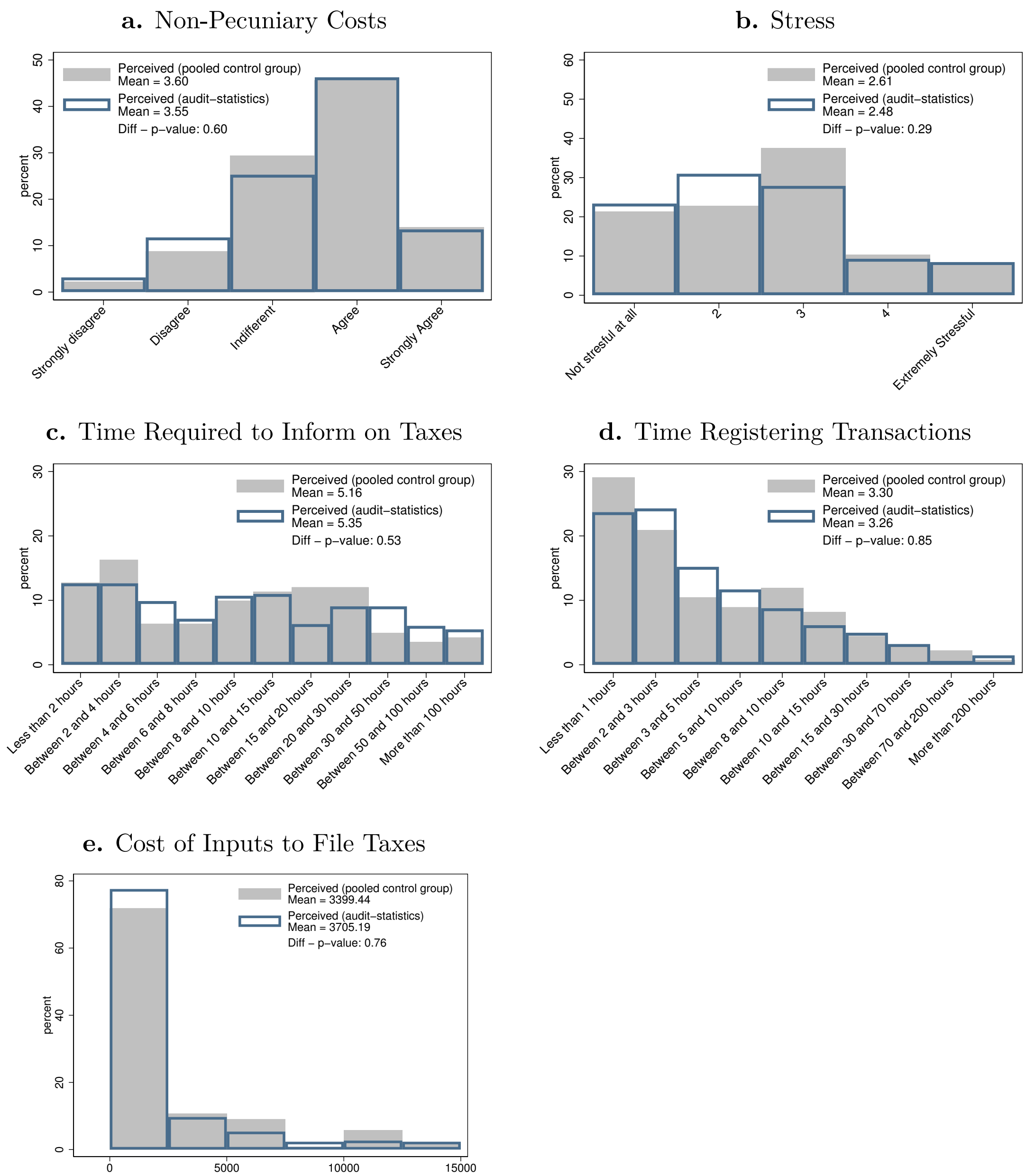
As a robustness check, we provide an alternative calibration of the Bayesian model. In the above results, we selected values for the parameters α_0 and β_0 to "center" beliefs around the true audit probability in our sample (11.7%). We present the results from an alternative calibration, which centers the perceived probability around the average value we obtain from the control group in our post-treatment survey – an average perceived audit probability of 40.5% for the comparison group. The results are presented in Figure B.16. If anything, this alternative calibration makes *A&S* even less plausible: as shown in panel (b), we would expect that almost everyone should update their perceived probability downwards, and thus reduce their tax payments, which is the opposite of what we find.

A more structured analysis is presented in Table B.12. For reference, column (1) of Table B.12 shows the baseline specification: i.e., in the whole sample the *audit-statistics* message increased tax payments by 7.0%. Next, we divide the sample into two groups: firms with prior beliefs about the probability of being audited that were lower than the probability reported in the letter they received, and firms with prior beliefs about the probability of being audited that were equal to or higher than the probability reported in the letter they received. Since we divide the groups according to a characteristic of firms that received the *audit-statistics* message, the outcomes for both groups are compared to those of the firms that received the *baseline* letter. According to *A&S*, increasing taxpayers' perceived probability of being audited should result in higher tax payments (and vice versa for reducing their perceived probability of being audited). Therefore, the expected effect of the *audit-statistics* message is positive on firms with $\hat{p} < p$, and negative for firms with $\hat{p} \geq p$. Column (2) in Table B.12 shows that the average effect of the *audit-statistics* message on VAT payments in the first year after receiving the letter is 6.1% for taxpayers with relatively low priors. Column (3) reports that the average effect for taxpayers with relatively high priors is 8.6%. The effect on firms with relatively low priors is positive but lower in magnitude than the effect on firms with relatively high priors, and the effect on the latter has the opposite sign of what would be predicted by *A&S*. Furthermore, the differences in magnitude are statistically and economically insignificant (p-value=0.347). These results, if anything, provide evidence against the *A&S* predictions.

We can reproduce a similar exercise without the need for a Bayesian learning model. We compare the responses between firms with and without prior audit experience. In columns (4) and (5) of Table B.12, we compare the effects of the *audit-statistics* message between firms that were audited in the recent past (up to 15 years before the treatment, which comprises 23.7% of the sample) and firms that were not audited during that period (the remaining 76.3% of the sample).[84] The intuition is that firms that were audited in the recent past should have a higher perception of the probability of being audited. The null hypothesis is thus that the effect of our *audit-statistics* messages should be stronger for firms that were never audited, since such firms would have had lower prior beliefs about audit probabilities and thus should have increased their perceived probabilities as a result of the *audit-statistics* messages. Moreover, we could even expect to see negative effects on compliance for firms that were audited in the past, because these firms were likely to have had high priors about audit probabilities and our information treatment should have reduced their perceptions. The results in Table B.12 indicate that there are no heterogeneous effects with respect to recent audit experience. The difference in treatment effects for the two groups is small and statistically insignificant: the effect is 8.2% for the group of firms that was previously audited, and 6.6% for those with no prior audit experience (p-value of difference=0.729).

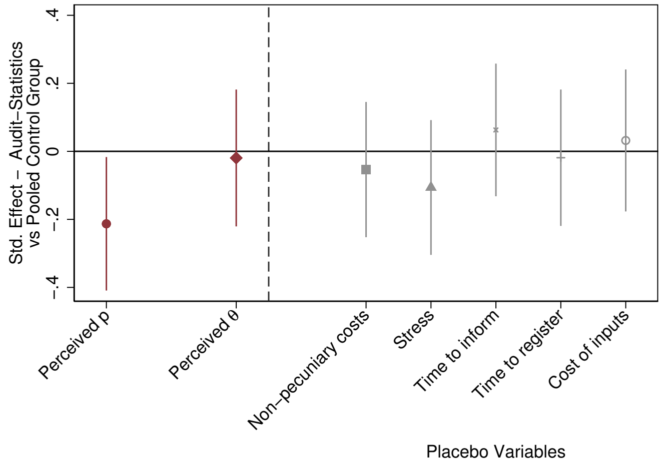
[84] The time frame is specified as the previous 15 years because that is how far back the available IRS administrative records reach.

Figure B.1: Survey Results: Placebo Variables by Treatment Group



Notes: Placebo tests where we estimate the effect of receiving the *audit-statistics* letter on questions that we do not expect to be affected by our treatment. These questions were reported in other modules of the survey and were aimed to collect information about tax compliance costs. Panel (a) reports the effects on the answers to a question that asked: "On a scale from 1 to 5 where 1 is "Strongly disagree" and 5 is "Strongly agree", to what extent do you agree with the following statement: *Tax compliance generates non-pecuniary costs*?". Panel (b) presents the responses to the following question: "On a scale from 1 to 5 where 1 is "Not stressful at all" and 5 is "Very stressful", please rate the level of stress created by all the steps required to fulfill your tax obligations". Panel (c) presents the responses to the following question: "What is the amount of time that you dedicate to inform yourself about tax your tax obligations?". Panel (d) presents the responses to the following question: "What is the amount of time that you dedicate to registering all the transactions made by your firm?". Panel (e) presents the responses to the following question: "What is the estimated monthly cost of all the inputs that you used in activities related to tax compliance?". The estimation strategy is exactly the same as the one used for Figure 5 (see corresponding notes for more detail). 'Perceived (pooled control group)' refers to to survey respondents who received the *baseline* letter or the *public-goods* letter during the experimental stage. 'Perceived (*audit-statistics*)' refers to respondents who received *audit-statistics* letters. We report the mean responses and the p-value of the difference between the two groups. Analysis is restricted to individuals who are owners (or who did not answer the ownership question) and who answered the questions on perceived audit probability and penalty rate.

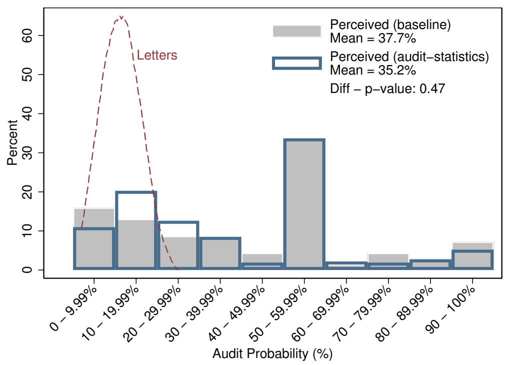
Figure B.2: Survey Results: Placebo Variables, *Audit-Endogeneity* vs. *Baseline* and *Public-Goods*



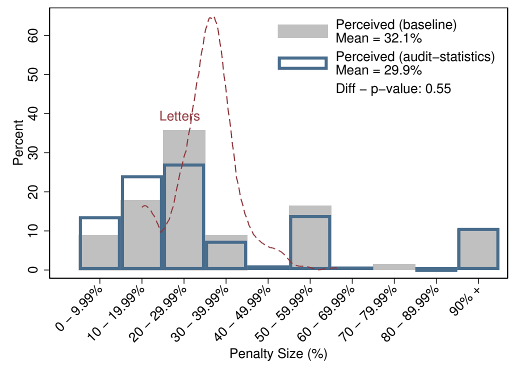
Notes: In this figure we compare the treatment effects of the *audit-statistics* letter on perceptions of the audit probability and penalty rate (coefficients reported to the left of the vertical dashed line) to the placebo outcomes reported in Figure B.1 (see corresponding notes for more details about the placebo outcomes). For all of the outcomes reported in the figure, we used the same estimation strategy as in Figure 5 (see corresponding notes for more detail). The comparison group is comprised of respondents who received the *baseline* letter or the *public-goods* letter during the experimental stage. The treatment group is comprised of respondents who received *audit-statistics* letters. For comparison purposes, all outcome variables are expressed in terms of the standard deviations of the control group. In all cases, the analysis is restricted to individuals who are owners (or who did not answer the ownership question) and who answered the two questions that were relevant to our analysis (about the perceived audit probability and penalty rate).

Figure B.3: Survey Results: Perceived p and θ, Alternative Samples and Comparison Group

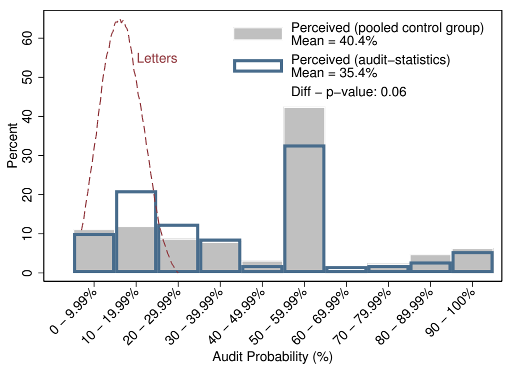
a. *p: Audit-Statistics* vs. *Baseline*



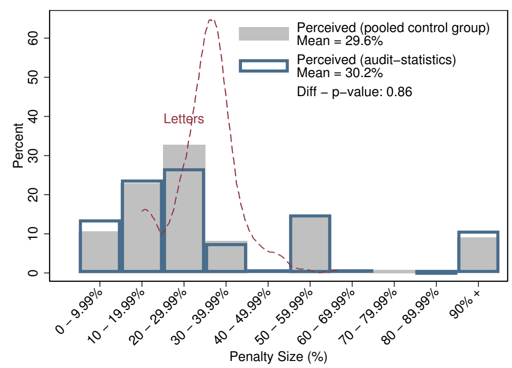
b. θ: *Audit-Statistics* vs. *Baseline*



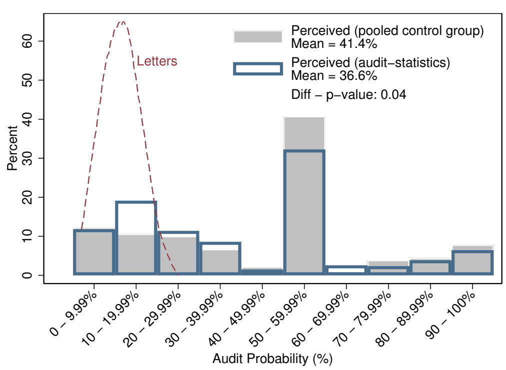
c. *p: Audit-Statistics* vs. *Baseline* and *Public-Goods*, Owners Only



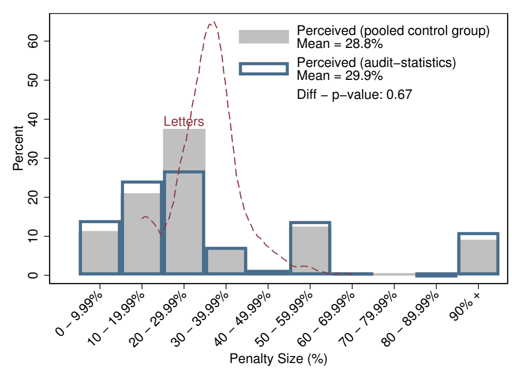
d. θ: *Audit-Statistics* vs. *Baseline* and *Public-Goods*, Owners Only



e. *p: Audit-Statistics* vs. *Baseline* and *Public-Goods*, Full Sample



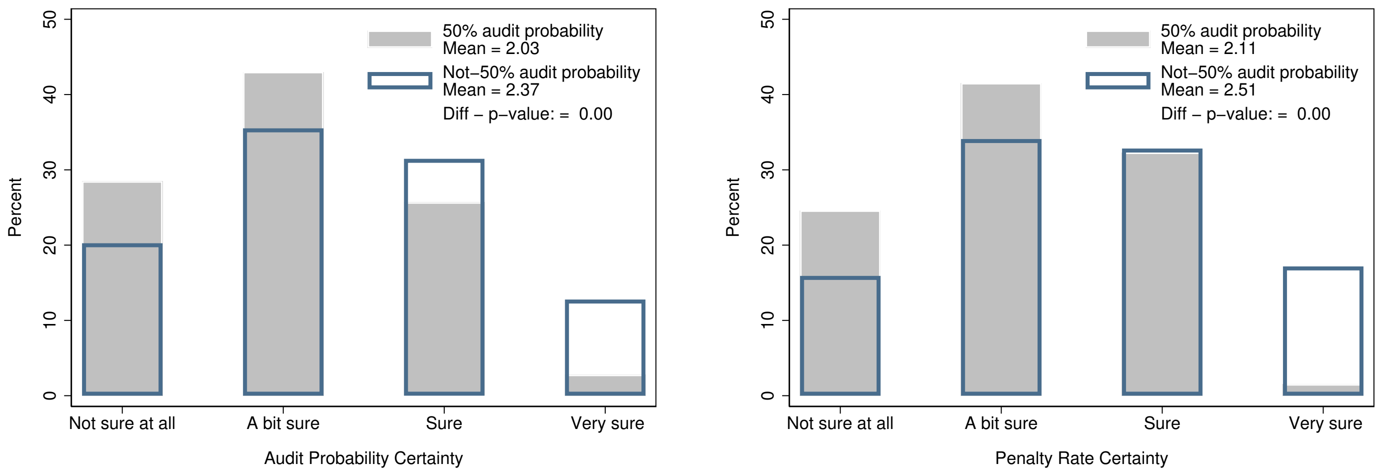
f. θ: *Audit-Statistics* vs. *Baseline* and *Public-Goods*, Full Sample



Notes: In panels (a) and (b), 'Perceived (*baseline*)' (N=69) refers to survey respondents who received the *baseline* letter during the experimental stage while 'Perceived (*audit-statistics*)' (N=365) refers to respondents who received the *audit-statistics* letter. In panels (c) and (d), we use a pooled control group comprised of recipients of the *baseline* and *public-goods* letters, but we restrict the sample to survey respondents who self-identified as owners (N of *baseline* group = 61, N of *public-goods* group=64, N of *audit-statistics* group = 341). Panels (e) and (f) use the full sample regardless of the self-reported occupation according to the survey. This includes owners, managers, internal and external accountants and other employees (N of *baseline* group = 89, N of *public-goods* group=90, N of *audit-statistics* group = 465). We also report the mean of the perceptions for each parameter and the the p-value of the difference between the groups in each panel. The answers correspond to survey questions Q2 and Q4 (see full survey questionnaire in Appendix A.7). In panels (a), (c) and (e) the x-axis represents the probability of being audited; in panels (b), (d) and (f) it represents the average penalty rate. The red line represents the density function of the information displayed in the *audit-statistics* letters, measured in the y-axis on the right (hidden for the sake clarity). In all cases, the analysis is restricted to letters that were not returned by the postal service.

Figure B.4: Certainty in Perceptions Reported by Having Answered 50%

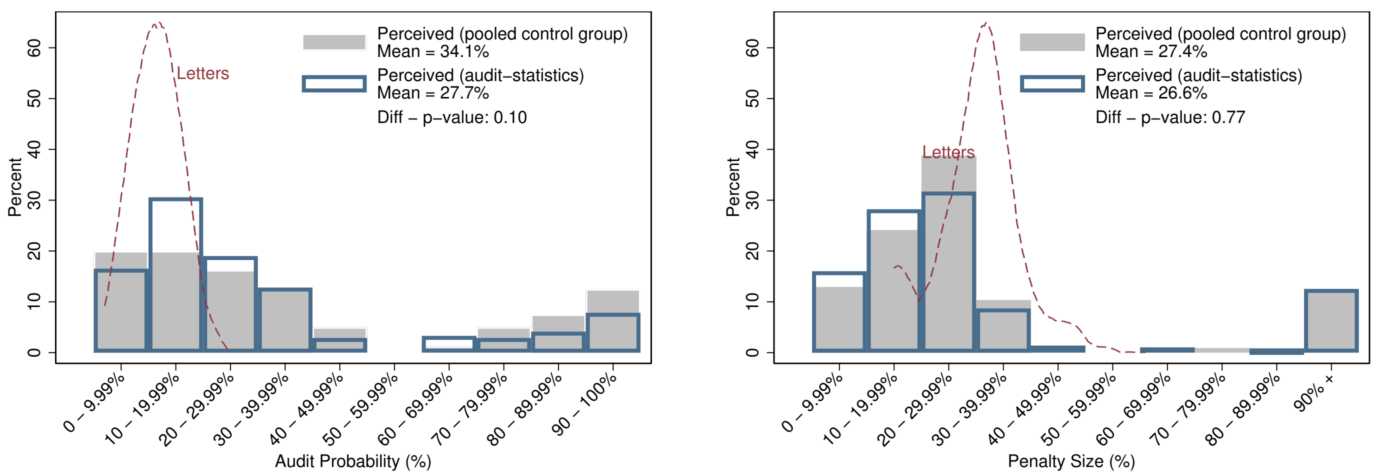
a. Audit Probability (p) **b.** Penalty Rate (θ)



Notes: The histograms present the distributions of certainty about the perceived audit probability and penalty rate as elicited in the survey. Panel (a) reports the distribution of answers to survey question Q3 (see full survey questionnaire in Appendix A.7), splitting the sample between individuals who reported a perceived audit probability of 50% (N=180) and individuals who reported a different audit probability (N=322). Panel (b) presents a similar breakdown for survey question Q5, likewise splitting the sample between individuals who reported a perceived penalty rate of 50% (N=65) and individuals who reported a different perceived penalty rate (N=397). In both panels, the sample is restricted to individuals who belong to our *audit-statistics* treatment arm or to the pooled control group (i.e., those who received either the *baseline* or the *public-goods* letter). We report the mean responses for the various groups, measured on a four-point scale where 1 corresponds to "Not sure at all" and 4 corresponds to "Very sure." We also report the p-value of the difference between the two groups.

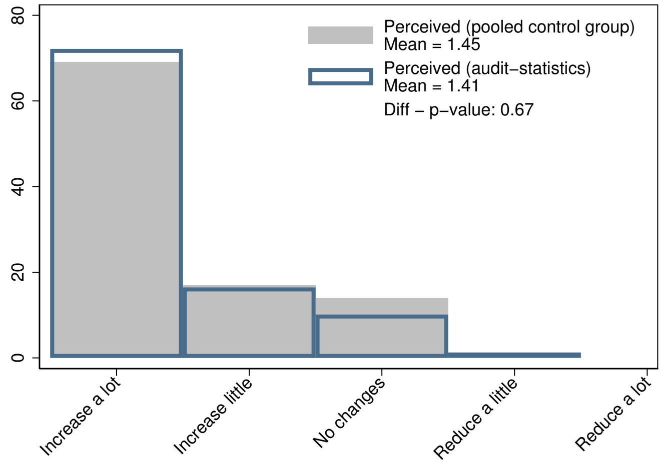
Figure B.5: Survey Results: Perception of Audit Probabilities and of Tax Evasion Penalty Rates by Treatment Group - Excluding 50% Responses

a. Audit Probability (p) **b.** Penalty Rate (θ)



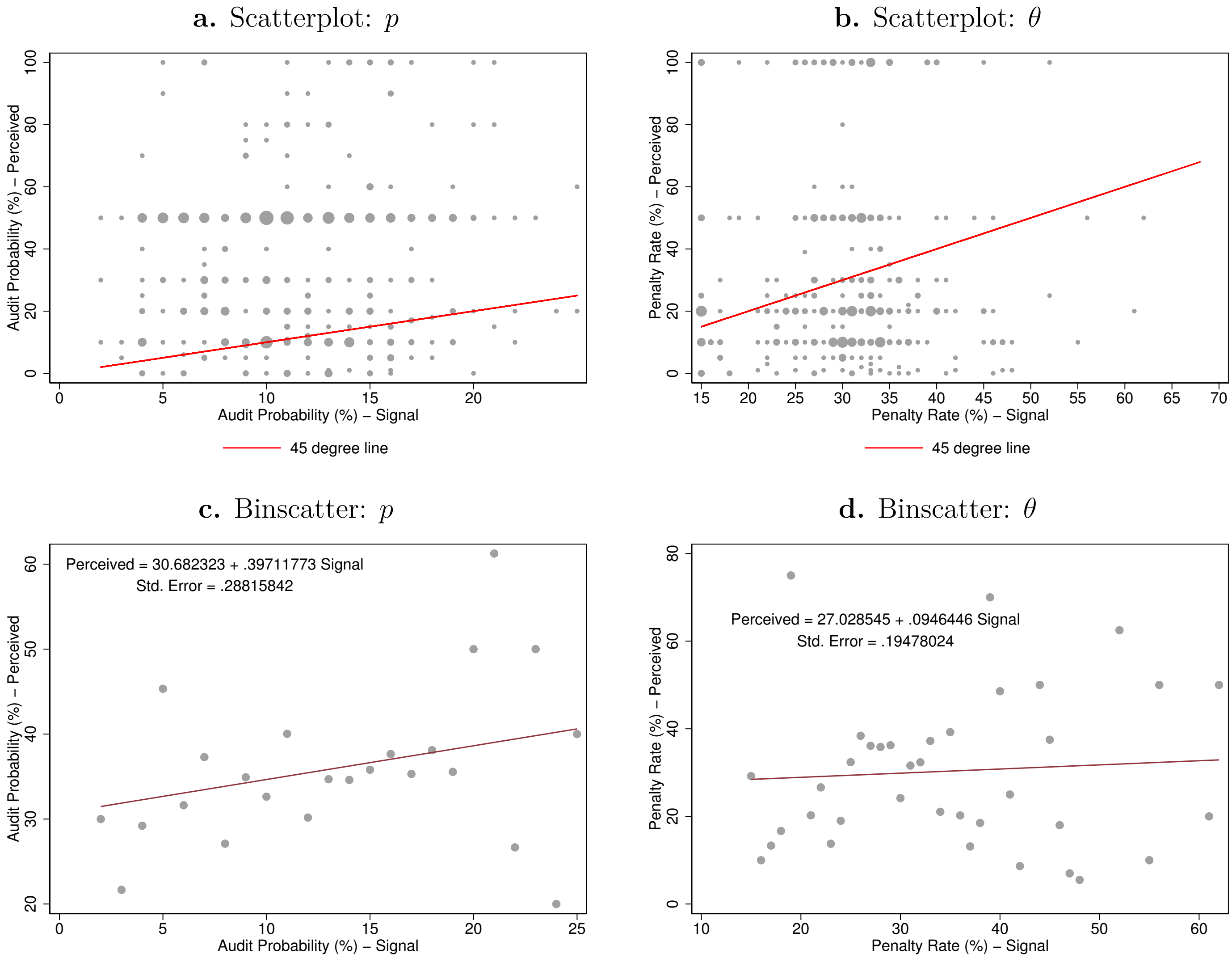
Notes: The histograms are based on the survey responses of those who self-identified as owners (or who did not answer the question regarding their occupation in the firm) in the post-treatment survey and who did not answer 50% to the audit probability and penalty rate questions. 'Perceived (pooled control group)' (N=80) refers to to survey respondents who received either the *baseline* (N=46) or the *public-goods* (N=34) letter during the experimental stage (neither of the two letters contained any information about audit probabilities or penalty rates). 'Perceived (*audit–statistics*)' refers to respondents who received *audit-statistics* letters (N=242). In panel (a) the x-axis represents the probability of being audited; in panel (b) it represents the average penalty rate. We report the mean responses and the p-value of the difference between the two groups. The answers correspond to questions Q2 and Q4 of the survey (see full survey questionnaire in Appendix A.7). The red line represents the density function of the information displayed in the *audit-statistics* letters, measured in the y-axis on the right (hidden for the sake clarity).

Figure B.6: Perception of Endogeneity of Audits: *Audit-Endogeneity* vs. *Baseline* and *Public-Goods*



Notes: 'Perceived (pooled control group)' (N=137) refers to respondents who received either the *baseline* or the *public-goods* letter during the experimental stage, while 'Perceived Endogeneity' (N=79) refers to respondents who received *audit-endogeneity* letters. These answers correspond to question Q6 of the survey (see the full questionnaire in Appendix A.7). The x-axis represents the different categories presented as survey options. We report the mean responses for the two groups, measured on a five-point scale where 1 corresponds to "Increase a lot" and 5 corresponds to "Reduce a lot." We also report the p-value of the difference between the two groups.

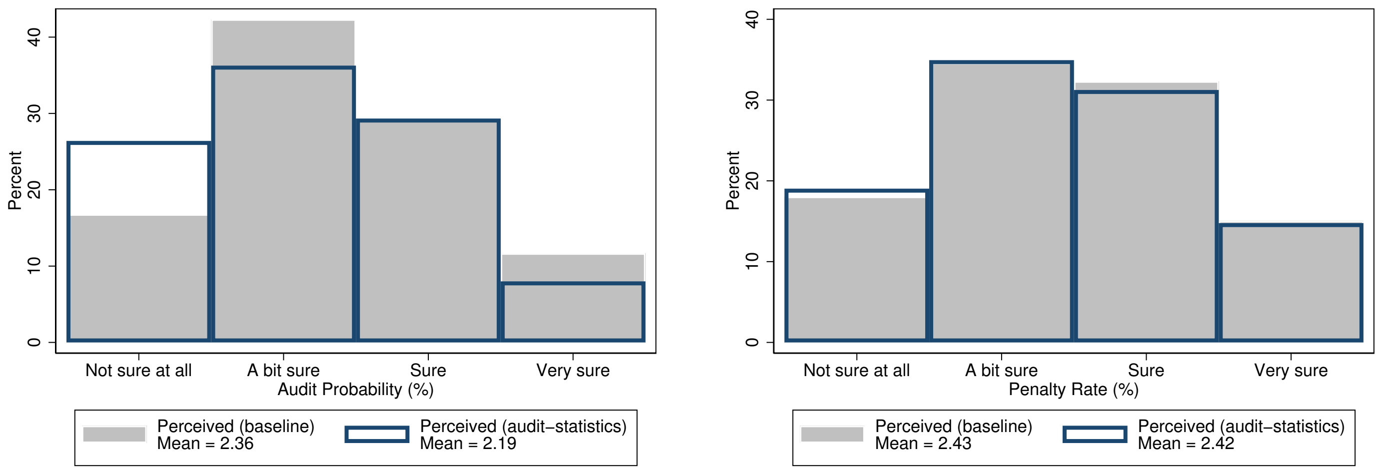
Figure B.7: Relation Between Signal Received and Self-Reported Perception



Notes: Figure B.7 shows the raw relations between the signal included in the letter and the perceptions of the audit probability and penalty rate reported in the survey for firms that received the *audit-statistics* letter (N=365). The x-axis depicts the signal included in the letter and the y-axis the self-reported parameter. Panels (a) and (b) report the raw scatterplot where each dot represents a pair of values (signal, perception) and the size of the dot is proportional to the number of individuals. A solid 45 degree line is included in each panel. Panels (c) and (d) report the binned scatterplots of the same variable, and include a line corresponding to the adjusted regression of perceptions over signals. The full equation predicted by the regression and the standard error of the coefficient on the signal are also included. In all cases, the analysis is restricted to letters that were not returned by the postal service.

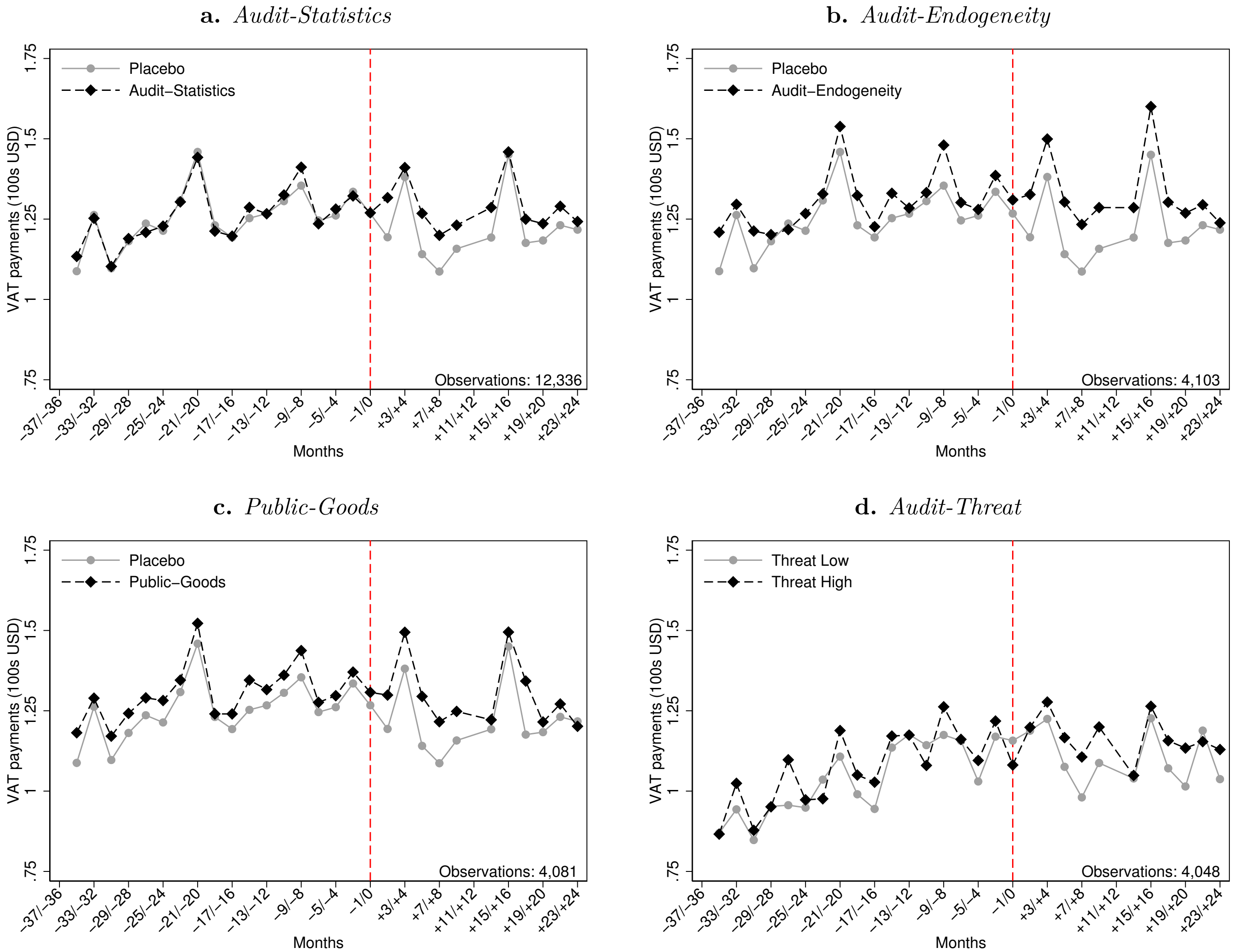
Figure B.8: Relation Between Signal Received and Self-Reported Certainty About Perception

a. Certainty about Self-Reported p **b.** Certainty about Self-Reported θ



Notes: The histograms are based on survey responses to questions Q3 and Q5 of respondents who self-identified as owners in the post-treatment survey, or who did not answer the question regarding their occupation in the firm (see full survey questionnaire in Appendix A.7). 'Perceived (*baseline)*' (N=137) refers to the survey respondents who received the *baseline* letter (N=68) or the *public-goods* letter (N=69) during the experimental stage (neither of the two letters contained any information about audit probabilities or penalty rates). 'Perceived (*audit–statistics*)' refers to respondents who received *audit-statistics* letters (N=364). In both panels the x-axis represents a four point scale that captures the degree of certainty that individuals have with respect to the perceived audit probability (panel (a)) and the perceived penalty rate (panel (b)). We also report the mean responses and the p-value of the difference between the two groups, calculated on a scale from 1 (corresponding to the "Not Sure at All" response) to 4 (corresponding to the "Very Sure" response).

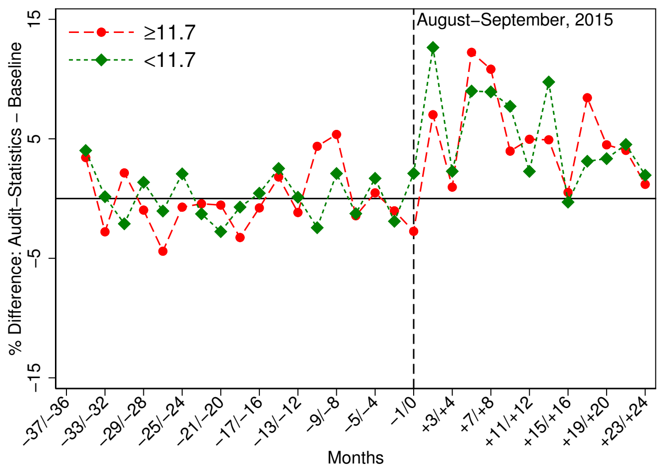
Figure B.9: Bi-Monthly VAT Payments, by Letter Type



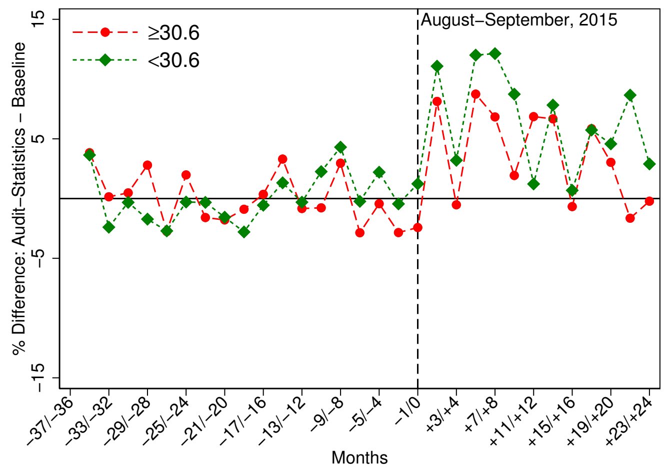
Notes: These figures plot the bi-monthly total VAT payments separated by treatment and comparison groups. Similar figures for the difference between these two groups are reported in Figure 4. The data used for this figure corresponds to the period from October, 2012 to September, 2017. The months of August and September 2015 - when most of the letters were delivered - are defined as the reference bi-monthly period (and marked with the dashed vertical line). Each figure plots the time series of total VAT payments for the treatment arm and the comparison group separately. Panel (a) (N=12,336) presents the evolution of the main outcome variable for firms that received the *audit-statistics* message and the *baseline* message. Panel (b) (N=4,103) provides the results for the *audit-endogeneity* message, and panel (c) (N=4,081) for the *public-goods* message. Panel (d) (N=4,048) presents the two treatment groups selected from the secondary sample. In this case, the two lines depict the two *audit-threat* messages that contain different probabilities of being audited: 50% vs 25%. In all cases, the analysis is restricted to letters that were not returned by the postal service. For each pair of months, VAT payments are top-coded at the 99.99th percentile to avoid contamination of the results by outliers. In all panels we omit the +11/+12 pair of months (August, 2016 - September, 2016) because the information provided by the IRS for this pair of months is incomplete.

Figure B.10: Effects of *Audit-Statistics* by Level of the Signal

a. Effect of *audit-statistics* vs. *baseline*, by level of p

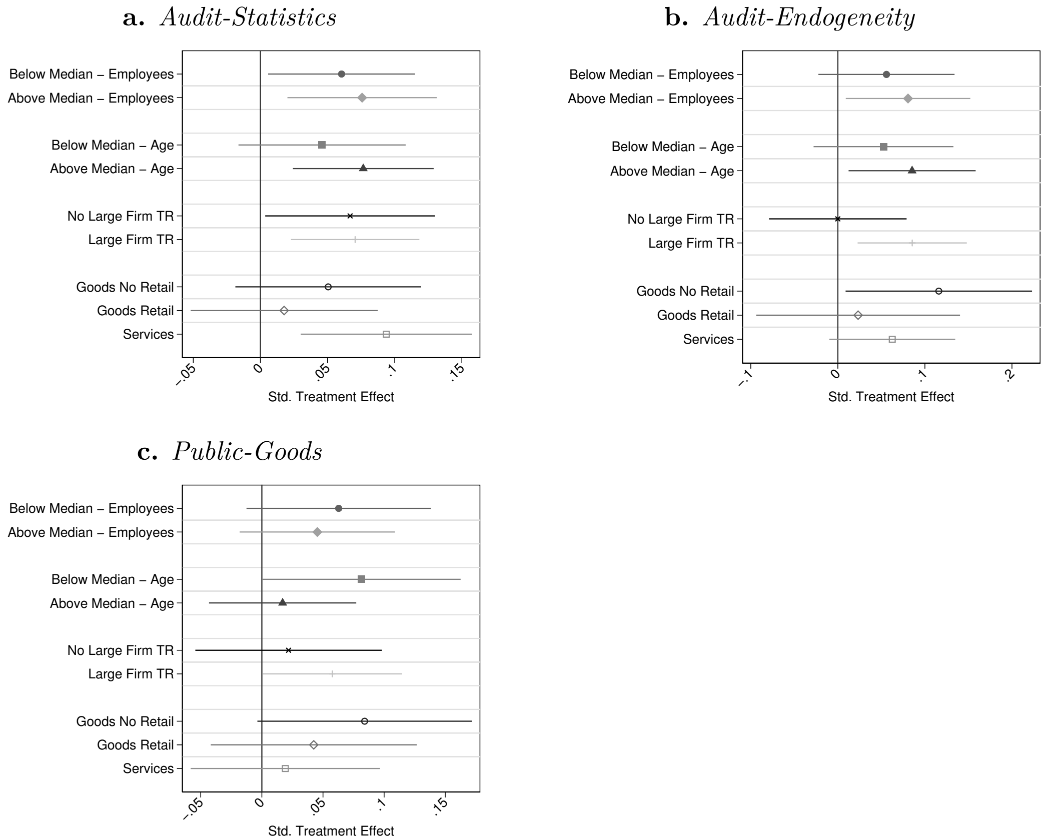


b. Effect of *audit-statistics* vs. *baseline*, by level of θ



Notes: These figures plot the percentage difference in bi-monthly total VAT payments between treatment and control groups, normalized by the average pre-treatment percentage difference (i.e. between months -35 and 0) for the same outcome, dividing the sample into firms which fall above and below the mean value of the corresponding signal. The red dots in panels (a) and (b) represent the effect for *audit-statistics* letter recipients with signals about p or θ above the mean respectively. Green dots represent the same effect but for those with signals about p and θ below the mean respectively. The data used in the figure covers the period October, 2012 - September, 2017. The period of August, 2015 - September, 2015 – when most of the letters were delivered – is defined as the reference pair of months (indicated with the dashed vertical line). Each figure plots the difference between each treatment arm and the *baseline* letter (N=12,336). In all cases, the analysis is restricted to letters that were not returned by the postal service. For each pair of months, VAT payments are top-coded at the 99.99th percentile to avoid the contamination of the results by outliers.

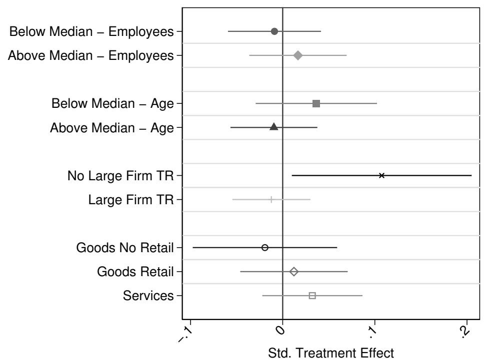
Figure B.11: Average Effects of *Audit-Statistics*, *Audit-Endogeneity* and *Public-Goods* Letters on VAT Payments by Type of Firm



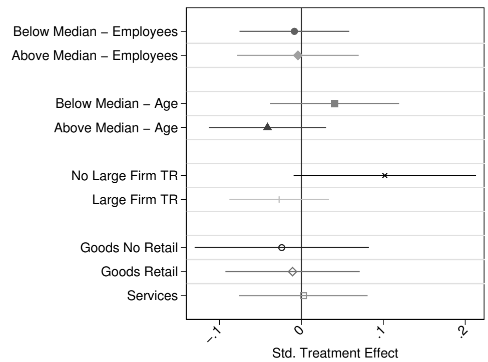
Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors are clustered at the firm level. Treatment effects are estimated for the first year post-treatment (October 2015 – September 2016 vs. October 2014 - September 2015), and are based on an augmented version of the difference-in-differences specification reported in equation (1) which compares treated firms to control firms and pre-treatment to post-treatment periods using yearly aggregated variables. To capture the differential effects by group, we include a dummy variable for the interaction between the group of interest and the coefficient that captures the treatment effect. We also include all additional corresponding interactions with the time and treatment variables. More details about the baseline specification can be found in Table 2 and Section 4. More details about the augmented model can be found in Section B.8. Panel (a) compares the *audit-statistics* message with the *baseline* letter, while panels (b) and (c) replicate the comparison for the *audit-endogeneity* and *public-goods* messages respectively. The heterogeneity analysis is based on four characteristics: the number of employees, the age of the firm (i.e., the number of years registered with the IRS), the type of tax return filed by the company and the sector of operation. The median number of employees in the experimental sample is 2.5, while the median firm age is 12.7 years. 68% of firms filed a comprehensive VAT tax return in 2013 while the remaining 32% filed a simplified VAT tax return. Regarding the distribution of the sector of operation, 29% of firms sell goods but are non-retail firms, 22% sell goods and are retail firms, and 49% provide services. In all cases, the analysis is restricted to letters that were not returned by the postal service.

Figure B.12: Average Effects of *Audit-Statistics*, *Audit-Endogeneity* and *Public-Goods* Letters on VAT Payments by Type of Firm, Pre-Treatment Falsification Test

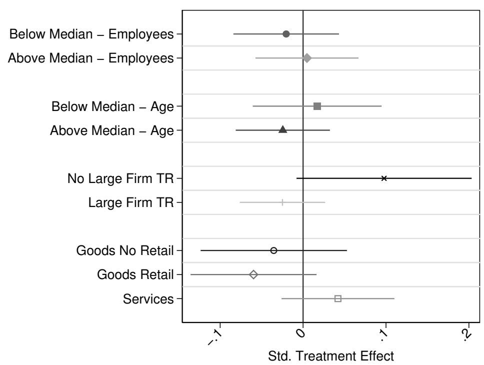
a. *Audit-Statistics*



b. *Audit-Endogeneity*



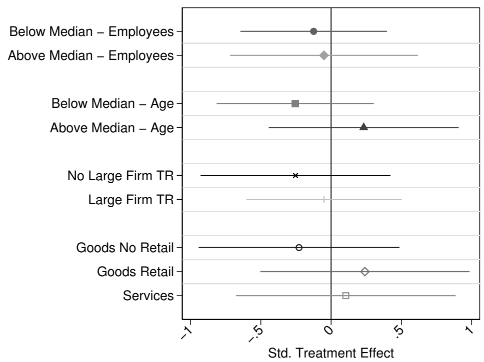
c. *Public-Goods*



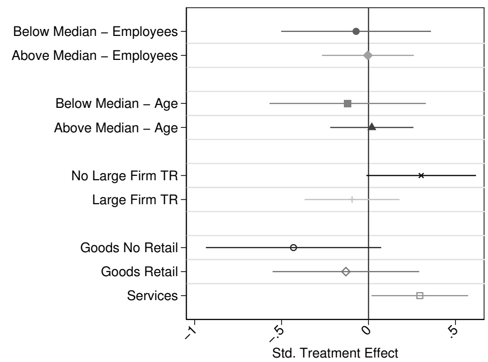
Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors are clustered at the firm level. Estimates presented in the figure correspond to falsification tests that compare two pre-treatment periods (October 2014 – September 2015 vs. October 2013 - September 2014) and are based on an augmented version of the difference-in-differences specification reported in equation (1) which compares treated firms to control firms using yearly aggregated variables. To capture the differential effects by group, we include a dummy variable for the interaction between the group of interest and the coefficient that captures the treatment effect. We also include all additional corresponding interactions with the time and treatment variables. More details about the baseline specification can be found in Table 2 and Section 4. More details about the augmented model can be found in Section B.8. Panel (a) compares the *audit-statistics* message with the *baseline* letter, while panels (b) and (c) replicate the comparison for the *audit-endogeneity* and *public-goods* messages respectively. The heterogeneity analysis is based on four characteristics: the number of employees, the age of the firm (i.e., the number of years registered with the IRS), the type of tax return filed by the company and the sector of operation. The median number of employees in the experimental sample is 2.5, while the median age of the firms is 12.7 years. 68% of firms filed a comprehensive VAT tax return in 2013 while the remaining 32% filed a simplified VAT tax return. Regarding the distribution of the sector of operation, 29% of firms sell goods but are non-retail firms, 22% sell goods and are retail firms, and 49% provide services. In all cases, the analysis is restricted to letters that were not returned by the postal service.

Figure B.13: Elasticities of Tax Payments with Respect to Audit Probability and Penalty Rate in *Audit-Statistics* and *Audit-Threat* Sub-Treatments by Type of Firm

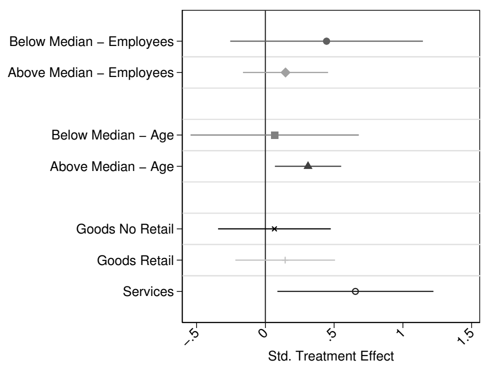
a. *Audit Probability*



b. *Penalty Rate*



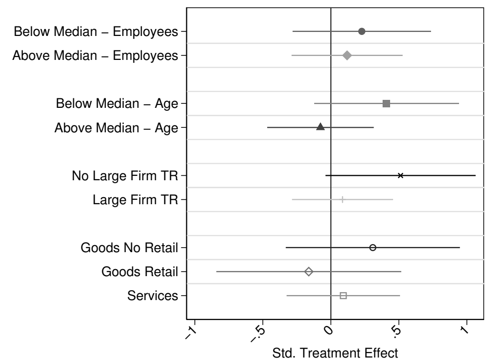
c. *Audit-Threat*



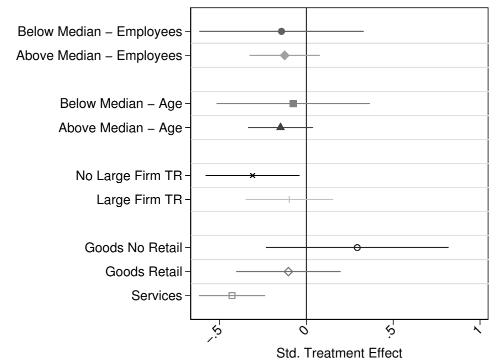
Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors are clustered at the firm level. Treatment effects are estimated for the first year post-treatment (October 2015 – September 2016 vs. October 2014 - September 2015) and are based on an augmented version of the difference-in-differences specification reported in equation (2) which compares treated firms which received different signals about p and θ. In all cases, we include an additional set of dummies for quintiles of the pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p and θ, and the corresponding interactions with the time variable. The results are based on Poisson regressions with variables expressed in percentage terms, so the coefficients can be interpreted directly as elasticities. To capture the differential effects by group we include a dummy variable for the interaction between the group of interest and the coefficient that captures the treatment effect. We also include all additional corresponding interactions with the quintile, time and treatment variables. More details about the baseline specification can be found in Table 3 and Section 5.2.1. More details about the augmented model can be found in Section B.8. Panel (a) presents the effect of providing different information regarding p in the *audit-statistics* message and panel (b) does the same for θ. Panel (c) compares the two *audit-threat* messages, i.e. the 50% threat of audit vs. the 25% threat of audit. Estimates should be interpreted as the effect of an additional percentage point of p and θ (respectively) in the information included in the letters on post-treatment VAT payments. The heterogeneity analysis is based on four characteristics: the number of employees, the age of the firm (i.e., the number of years registered with the IRS), the type of tax return filed by the company and the sector of operation. The median number of employees in the experimental sample is 2.5, while the median age of the firms is 12.7 years. 68% of firms filed a comprehensive VAT tax return in 2013 while the remaining 32% filed a simplified tax return. Regarding the distribution of the sector of operation, 29% of firms sell goods but are non-retail firms, 22% sell goods and are retail firms, and 49% provide services. In all cases, the analysis is restricted to letters that were not returned by the postal service.

Figure B.14: Elasticities of Tax Payments with Respect to Audit Probability and Penalty Rate in *Audit-Statistics* and *Audit-Threat* Sub-Treatments by Type of Firm, Pre-Treatment Falsification Test

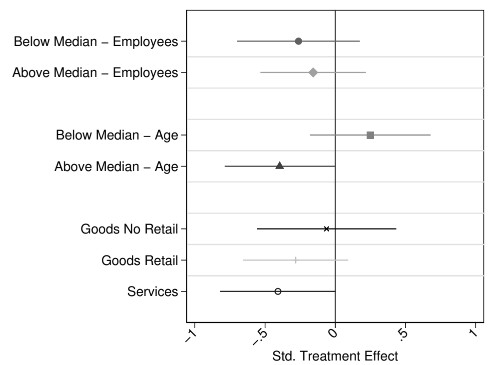
a. *Audit Probability*



b. *Penalty Rate*



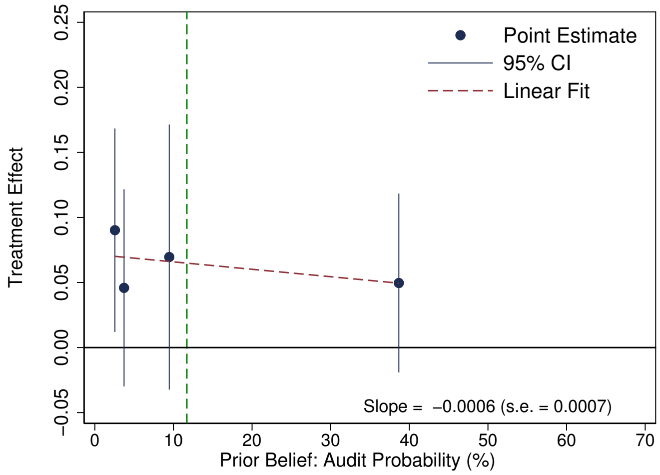
c. *Audit-Threat*



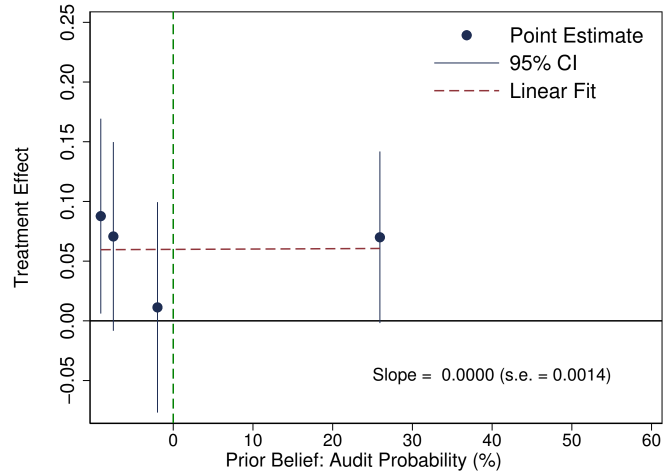
Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors are clustered at the firm level. Estimates presented in the figure correspond to falsification tests that compare two pre-treatment periods (October 2014 – September 2015 vs. October 2013 - September 2014) and are based on an augmented version of the difference-in-differences specification reported in equation (2) which compares treated firms which received different signals about p and θ. In all cases, we include an additional set of dummies for quintiles of the pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p and θ, and the corresponding interactions with the time variable. The results are based on Poisson regressions with variables expressed in percentage terms, so the coefficients can be interpreted directly as elasticities. To capture the differential effects by group we include a dummy variable for the interaction between the group of interest and the coefficient that captures the treatment effect. We also include all additional corresponding interactions with the quintile, time and treatment variables. More details about the baseline specification can be found in Table 3 and Section 5.2.1. More details about the augmented model can be found in Section B.8. Panel (a) presents the effect of providing different information regarding p in the *audit-statistics* message and panel (b) does the same for θ. Panel (c) compares the two *audit-threat* messages, i.e. the 50% threat of audit vs. the 25% threat of audit. Estimates should be interpreted as the effect of an additional percentage point of p and θ (respectively) in the information included in the letters on post-treatment VAT payments. The heterogeneity analysis is based on four characteristics: the number of employees, the age of the firm (i.e., the number of years registered with the IRS), the type of tax return filed by the company and the sector of operation. The median number of employees in the experimental sample is 2.5, while the median age of the firms is 12.7 years. 68% of firms filed a comprehensive VAT tax return in 2013 while the remaining 32% filed a simplified VAT tax return. Regarding the distribution of the sector of operation, 29% of firms sell goods but are non-retail firms, 22% of firms sell goods and are retail firms, and 49% of firms provide services. In all cases, the analysis is restricted to letters that were not returned by the postal service.

Figure B.15: Effect of *Audit-Statistics* vs. *Baseline* by Prior Beliefs

a. Audit probability - *prior (calibrated to 11.7%)*

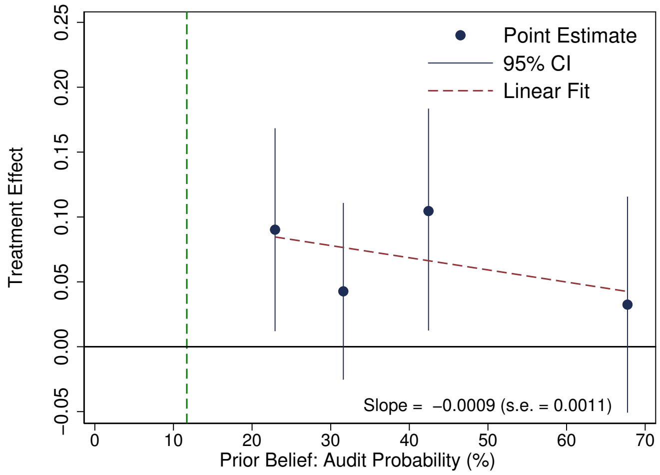


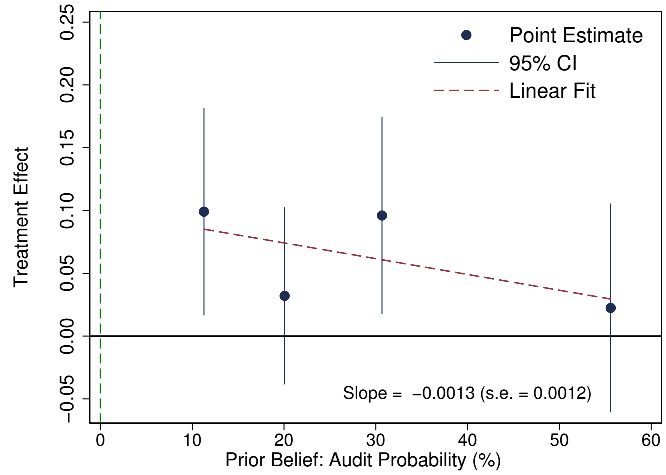
b. Audit probability - *prior* - *signal (calibrated to 11.7%)*



Notes: Panel (a) plots the first-year effect (October 2015 – September 2016 vs October 2014 - September 2015) of the *audit-statistics* letter on total VAT payments by quartiles of prior beliefs, while panel (b) reports the same results by quartiles of the difference between the prior belief and the signal sent in the *audit-statistics* message (N=11,989). The prior belief (before the experiment) is computed as $\hat{p}_i = 1 - \left(1 - \frac{\alpha_0 + N_i}{\alpha_0 + \beta_0 + T_i}\right)^3$ with parameters $\alpha_0 = 0.13$ and $\beta_0 = 1$ such that the mean prior belief about the probability of being audited at least once in the following three years matches the actual average probability observed in our sample. In panel (b) the signal for the placebo group was randomly assigned using the same strategy as for the *audit-statistics* group. The red dashed line represents the linear fit corresponding to the four estimates. In panel (a), the dashed vertical line represents the average actual probability of being audited as provided in the *audit-statistics* letters (11.7%). In panel (b), it represents the point at which the prior belief and the signal provided are equal. In both panels, each dot represents the estimated treatment effect for each quartile of the variable considered. Regressions are estimated using the baseline difference-in-differences specification reported in equation (1), but including additional interaction terms for each quartile. More details are reported in the notes included in Table 2 and Section 4. All effects are depicted with 95% confidence intervals. The results are based on Poisson regressions, so the coefficients can be interpreted directly as semi-elasticities. Confidence intervals are computed with standard errors clustered at the firm level.

Figure B.16: Effect of *Audit-Statistics* vs. *baseline* by Prior Beliefs





Notes: Panel (a) plots the first-year effect (October 2015 – September 2016 vs October 2014 - September 2015) of the *audit-statistics* letter on total VAT payments by quartiles of prior beliefs while panel (b) reports the same results by quartiles of the difference between the prior belief and the signal sent in the *audit-statistics* message (N=11,989). The prior belief is computed as $\hat{p}_i = 1 - \left(1 - \frac{\alpha_0 + N_i}{\alpha_0 + \beta_0 + T_i}\right)^3$ where $\alpha_0 = 1.36$ and $\beta_0 = 1$ such that the mean prior belief about the probability of being audited at least once in the following three years matches the average probability perceived by firms in the *baseline* and *public-goods* groups according to the survey answers (40.5%). In panel (b) the signal for the placebo group was randomly assigned using the same strategy as for the *audit-statistics* group. The red dashed line represents the linear fit corresponding to the estimates for the four quartiles. In panel (a), the green dashed line represents the average perceived probability (40.5%) for the comparison group, and in panel (b) it represents the point at which the prior belief and the signal provided are equal. In both panels, each dot represents the estimated treatment effect for each quartile of the variable considered. Regressions are estimated using the baseline difference-in-differences specification reported in equation (1), but including additional interactions terms for each quartile. More details are reported in the notes included in Table 2 and Section 4. All effects are depicted with 95% confidence intervals. The results are based on Poisson regressions, so the coefficients can be interpreted directly as semi-elasticities. Confidence intervals are computed with standard errors clustered at the firm level.

Table B.1: Comparison of Firm Characteristics for Different Groups

	All firms (1)	Experimental Sample: Main (2)	Experimental Sample: Secondary (3)	Invited to the survey (4)	p-value test Survey. vs Non-Survey (5)
Share paid VAT (3 months pre-mailing)	0.778	0.927	0.894	0.926	0.875
	(0.001)	(0.002)	(0.005)	(0.004)	
Amount of VAT paid (3 months pre-mailing)	3.717	1.894	1.744	1.894	0.993
	(0.033)	(0.022)	(0.067)	(0.048)	
Years registered in tax agency	14.208	15.233	19.437	14.445	0.001
	(0.039)	(0.132)	(0.202)	(0.159)	
Share audited between 2013-2015	0.065	0.102	0.141	0.079	<0.001
	(0.001)	(0.003)	(0.007)	(0.006)	
Number of employees	12.653	4.838	5.600	6.431	0.000
	(0.657)	(0.208)	(0.093)	(0.867)	
Share file comprehensive tax return in 2013	0.447	0.684	0.999	0.569	<0.001
	(0.001)	(0.004)	(0.000)	(0.008)	
Share no retail goods sector	0.246	0.288	0.432	0.263	<0.001
	(0.001)	(0.003)	(0.008)	(0.007)	
Share retail goods sector	0.132	0.216	0.328	0.150	<0.001
	(0.001)	(0.003)	(0.007)	(0.006)	
Share services sector	0.622	0.496	0.240	0.588	<0.001
	(0.001)	(0.004)	(0.007)	(0.008)	
N	120,142	16,392	4,048	3,845	

Notes: This table presents the average characteristics for different subsamples of the universe of firms registered with the tax agency (standard deviations in parentheses). Column (1) includes all firms that submitted at least one VAT payment in 2014 or 2015. Column (2) includes the subset of firms selected for the experimental sample according to the criteria described in section 3.2. Column (3) represents a group of high-risk firms which were selected from a special sample defined by the IRS and which received the *audit-threat* letter. Column (4) corresponds to firms in the main sample with valid e-mail addresses on file with the IRS. These are the firms that were selected to participate in the online survey conducted after the experiment. Column (5) reports the p-value of the means test between firms in the experimental sample that were invited to the survey and firms that were not. All data is based on administrative tax records (monthly payments, annual tax returns and auditing registers) and Columns (2) to (4) are restricted to firms that did not return the letter, which is the group of firms used in the empirical analysis. Robust standard errors are provided in parentheses.

Table B.2: Delivery Status by Treatment Arm

	Main Sample					Secondary Sample		
	Audit Statistics (1)	Public Goods (2)	Audit Endogeneity (3)	Baseline (4)	p-value test (5)	Audit Threat (25%) (6)	Audit Threat (50%) (7)	p-value test (8)
Delivered (%)	72.215	72.094	73.004	72.869	0.781	77.360	78.086	0.555
	(0.396)	(0.888)	(0.876)	(0.879)		(0.875)	(0.861)	
In Process (%)	0.219	0.117	0.234	0.274	0.660	0.131	0.303	0.211
	(0.041)	(0.068)	(0.095)	(0.103)		(0.076)	(0.114)	
Returned (%)	19.694	21.057	20.569	19.312	0.290	11.932	11.953	0.982
	(0.352)	(0.807)	(0.798)	(0.781)		(0.678)	(0.675)	
Missing (%)	7.873	6.732	6.194	7.545	0.011	10.577	9.658	0.302
	(0.238)	(0.496)	(0.476)	(0.522)		(0.643)	(0.615)	
N	12,791	2,555	2,567	2,558		2,288	2,309	

Notes: This table reports the delivery status of the letters by treatment arm. Columns (1) through (4) report the distribution of delivery statuses by treatment arm for firms in the main sample, while column (5) reports the p-value of the joint equality test. Columns (6) through (8) provide an analogous breakdown for the secondary sample. Information about delivery status is provided by Uruguayan Post Office using the service of certified delivery. "Delivered" letters correspond to letters that were delivered by the post office and signed for by the recipient. "In Process" letters correspond to letters that were in the process of being delivered when the post office provided us with the delivery report, but had not been delivered yet. "Returned" letters are letters that the post office was not able to deliver and that were returned to the sender. Finally, there are some letters for which the post office did not provide any information. These are coded as "Missing."

Table B.3: Tax Payments: Summary Statistics

	Mean (1)	SD (2)	10th (3)	25th (4)	50th (5)	75th (6)	90th (7)
VAT Amounts							
Post-treatment	6.47	7.77	0.44	1.30	3.74	8.48	16.55
Pre-Treatment	7.77	8.21	0.95	1.97	4.83	10.93	19.49
Retroactive VAT Amounts							
Post-treatment	0.30	1.40	0.00	0.00	0.00	0.00	0.62
Pre-Treatment	0.40	1.85	0.00	0.00	0.00	0.00	0.77
Concurrent VAT Amounts							
Post-treatment	6.16	7.51	0.33	1.16	3.52	8.07	15.84
Pre-Treatment	7.37	7.85	0.86	1.83	4.47	10.28	18.72
Other Taxes Amounts							
Post-treatment	3.30	5.43	0.00	0.95	1.81	3.52	7.42
Pre-Treatment	4.05	8.54	0.04	1.44	2.13	4.36	8.58
Total Taxes Amounts							
Post-treatment	9.77	11.31	1.04	2.68	6.14	12.39	23.33
Pre-Treatment	11.82	13.61	1.84	3.64	7.45	15.83	27.20
Some Payment							
Post-treatment	0.96	0.20	1.00	1.00	1.00	1.00	1.00
Pre-Treatment	1.00	0.06	1.00	1.00	1.00	1.00	1.00
More Than 6 payments							
Post-treatment	0.71	0.45	0.00	0.00	1.00	1.00	1.00
Pre-Treatment	0.89	0.32	0.00	1.00	1.00	1.00	1.00
More Than 3 payments							
Post-treatment	0.91	0.29	1.00	1.00	1.00	1.00	1.00
Pre-Treatment	0.98	0.15	1.00	1.00	1.00	1.00	1.00
VAT in Tax Return							
Post-treatment	7.19	9.93	0.00	0.49	3.84	9.69	18.97
Pre-Treatment	7.79	7.91	0.14	1.88	5.20	11.57	18.91

Notes: The statistics in this table correspond to firms that received the *baseline* letter (N=2,064). The pre-treatment period ranges from October 1, 2014 to September 30, 2015, and the post-treatment period ranges from October 1, 2015 to September 30, 2016. "VAT Amounts" correspond to total VAT payments and withholding (the sum of retroactive and concurrent VAT amounts). "Retroactive VAT Amounts" correspond to VAT payments and withholding submitted for liabilities incurred two or more months ago (e.g., VAT payments made in March 2016 corresponding to September 2015). "Concurrent VAT Amounts" include VAT payments made on behalf of the current or previous month. "Other Taxes Amounts" include payments for the corporate income tax, the wealth tax and other taxes specific to business activity. "Total Taxes Amounts" is the sum of "VAT Amounts" and "Other Taxes Amounts". "VAT in Tax Return" is the final tax liability calculated in the yearly tax return that firms submit to the Uruguayan IRS.

Table B.4: Response Rates by Treatment Arm

	Audit Statistics (1)	Public Goods (2)	Audit Endogeneity (3)	Baseline (4)	Audit Threat (5)
a. Total firms by treatment arm					
Total	10,272	2,017	2,039	2,064	4,048
b. Invited to the survey					
Total	2,408	490	480	467	22
%	23.44	24.29	23.54	22.63	0.54
c. Started the survey					
Total	584	117	135	107	5
%	24.25	23.88	28.13	22.91	22.73
d. Owners that responded the survey					
Total	449	87	105	82	2
%	76.88	74.36	77.78	76.64	40.00
e. Non-Skip Rate - audit probability					
Total	351	64	82	65	1
%	92.61	92.75	95.35	95.59	100.00
f. Non-Skip Rate - penalty size					
Total	320	62	76	64	1
%	88.89	95.38	91.57	98.46	100.00

Notes: This table describes the composition of the online survey participants by treatment arm. Panel (a) reports the total number of firms by treatment arm. Panel (b) reports the raw number of invitations sent to firms in the experimental sample. The percentage reported in this panel uses the number of firms in each treatment arm reported in panel (a) as the denominator.. Panel (c) reports the number of firms that started the survey (i.e. answered at least the first two questions). The percentage reported in this panel is calculated using the totals reported in panel (b) as the denominator. Panel (d) reports the number of individuals who identified themselves as owners or who did not answer the question regarding their occupation in the firm, conditional on having started the survey (i.e., using the totals reported in panel (c) in the denominator). Finally, panels (e) and (f) report the non-skip rates - i.e., the response rate conditional on having answered the previous question - by treatment arm for the two key questions in the survey that ask about self-reported perceptions of the audit probability and penalty rate.

Table B.5: Effect of Treatment Status on Probability of Answering the Relevant Questions

	Answer: Perceived p		Answer: Perceived θ	
	Unconditional (1)	Conditional (2)	Unconditional (3)	Unconditional (4)
Treated	0.019	0.009	-0.010	-0.052
	(0.033)	(0.030)	(0.033)	(0.040)
Observations	1,105	267	1,105	267

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Robust standard errors reported in parentheses. This table reports the effect of receiving the *audit-statistics* letter on the probability of answering the two key questions of our survey analysis. The treatment variable is defined as 1 if the firm received the *audit-statistics* letter and 0 if the firm received the *baseline* or the *public-goods* letter (our pooled control group). Column (1) reports the effect of receiving the *audit-statistics* letter on the probability of answering the audit probability question. Column (3) does the same for the penalty rate question. To address the fact that the treatment could have induced differential drop-out rates, columns (2) and (4) report the same results but conditioning the sample to individuals who answered every single question before the audit probability and penalty rates questions. We use OLS regressions to estimate the treatment effects. In all cases, the analysis is restricted to individuals who are owners (or who did not answer the question regarding their occupation in the firm).

Table B.6: Balance Test for Survey Responses Conditional on Having Answered Perceptions Questions

	Main Sample				
	Audit Statistics (1)	Public Goods (2)	Audit Endogeneity (3)	Baseline (4)	p-value test (5)
Age	27.752	24.441	25.892	29.765	0.012
	(0.561)	(1.260)	(1.052)	(1.269)	
Female (%)	48.864	48.485	50.617	52.308	0.955
	(2.668)	(6.199)	(5.590)	(6.243)	
Capital city (%)	66.860	68.254	62.821	56.923	0.419
	(2.542)	(5.912)	(5.508)	(6.190)	
Very small firms (%)	11.966	9.231	14.634	15.152	0.685
	(1.735)	(3.618)	(3.927)	(4.447)	
Professional services (%)	50.829	44.118	44.578	52.941	0.540
	(2.631)	(6.066)	(5.489)	(6.098)	
Years registered with tax agency	16.366	13.385	15.025	17.894	0.036
	(0.539)	(0.925)	(1.033)	(1.221)	
Operates in one location (%)	61.582	62.687	64.634	67.164	0.829
	(2.589)	(5.953)	(5.312)	(5.781)	
One employee (%)	66.376	45.098	57.407	54.762	0.027
	(3.129)	(7.037)	(6.792)	(7.773)	
Audited in the last 3 years (%)	6.849	13.235	12.048	7.246	0.190
	(1.324)	(4.140)	(3.595)	(3.144)	
N	365	68	83	69	

Notes: This table reports the averages for different self-reported characteristics in the survey data by treatment group. The last column of each sample reports the p-value of a test in which the null hypothesis is that the mean is equal for all the treatment groups. "Age", "Female" and "Capital city" refer to individual characteristics of the respondent. "Very small firms" refers to firms with a special VAT regime in which there is a fixed VAT annual payment. "Professional services" include individuals that pay VAT because they provide professional services. Observations included in the analysis are restricted to individuals who are owners (or who did not answer the question regarding their occupation in the firm) and who answered the two relevant questions for our analysis (regarding the perceived audit probability and penalty rate).

Table B.7: Average Effects of *Audit-Statistics*, *Audit-Endogeneity* and *Public-Goods* Messages: Alternative Specifications and Data Sources

	Made VAT Payments			VAT Payments			VAT Tax Returns
	≥ 1	≥ 3	≥ 6				Balance
	Probit	Probit	Probit	Poisson	OLS	Tobit	Poisson
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
a. *Audit-Statistics* (10,272 firms [6,088]) vs *Baseline* (2,064 firms [1,270])							
Post-Treatment	0.095	-0.019	-0.045	0.070***	0.459***	0.451***	0.056*
	(0.132)	(0.064)	(0.040)	(0.021)	(0.147)	(0.153)	(0.029)
Pre-Treatment				0.009	0.069	0.052	0.007
				(0.020)	(0.154)	(0.155)	(0.032)
b. *Audit-Endogeneity* (2,039 firms [1,233]) vs *Baseline* (2,064 firms [1,270])							
Post-Treatment	0.109	-0.021	-0.039	0.071***	0.444**	0.452**	0.059
	(0.161)	(0.084)	(0.052)	(0.028)	(0.199)	(0.206)	(0.041)
Pre-Treatment				-0.005	-0.034	-0.048	0.011
				(0.028)	(0.219)	(0.220)	(0.040)
c. *Public-Goods* (2,017 firms [1,240]) vs *Baseline* (2,064 firms [1,270])							
Post-Treatment	-0.099	-0.140	-0.116**	0.051**	0.301*	0.332*	-0.021
	(0.200)	(0.091)	(0.053)	(0.025)	(0.177)	(0.184)	(0.038)
Pre-Treatment				-0.003	-0.019	-0.015	0.015
				(0.024)	(0.186)	(0.187)	(0.039)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (1) which compares treated firms to control firms and pre-treatment to post-treatment periods using yearly aggregated variables. In the first row of each panel ("Post-Treatment"), the coefficient corresponds to a comparison between a post-treatment period and a pre-treatment period (October 2015 – September 2016 vs October 2014 - September 2015). The second row ("Pre-Treatment") presents a falsification test where two pre-treatment periods are compared (October 2014 – September 2015 vs October 2013 - September 2014). Additional details about the model are reported in the notes of Table 2 and Section 4. Panel (a) compares the *audit-statistics* message with the *baseline* letter, while panels (b) and (c) replicate the comparison for the *audit-endogeneity* and *public-goods* messages respectively. Column (1) presents the treatment effect on the probability of making at least one VAT payment in the post treatment period using a Probit model. Columns (2) and (3) are identical to column (1), except that the dependent variable is defined as making at least three or six payments in the post-treatment period respectively. Columns (4), (5) and (6) present different estimation strategies for the intensive margin, i.e. the total amount of VAT paid. Column (4) corresponds to the baseline specification (a Poisson model), while column (5) uses an OLS regression and column (6) uses a Tobit regression. Column (7) reports the result of estimating a Poisson model on the final VAT liability calculated from annual tax returns. The number of observations corresponding to specifications reported in column (7) is reported in square brackets.

Table B.8: Effects of *Audit-Statistics* and *Audit-Threat* Sub-Treatments: Alternative Specifications

	Made VAT Payments			VAT Payments			VAT Tax Returns
	≥ 1	≥ 3	≥ 6				Balance
	Probit (1)	Probit (2)	Probit (3)	Poisson (4)	OLS (5)	Tobit (6)	Poisson (7)
a. *Audit-Statistics* (N=10,272 firms [6,088])							
Audit Probability (%)							
Post-Treatment	0.205	0.481	0.121	-0.063	-0.517	-0.128	-0.218
	(1.333)	(0.669)	(0.380)	(0.242)	(1.783)	(1.851)	(0.257)
Pre-Treatment				0.141	1.141	1.190	-0.038
				(0.164)	(1.336)	(1.343)	(0.230)
Penalty Size (%)							
Post-Treatment	0.717	-0.353	-0.005	-0.033	-0.233	-0.152	-0.223
	(0.575)	(0.333)	(0.202)	(0.118)	(0.976)	(0.995)	(0.138)
Pre-Treatment				-0.128	-1.130	-1.148	-0.092
				(0.108)	(0.964)	(0.970)	(0.114)
b. *Audit-Threat* (N=4,048 firms [3,236])							
Post-Treatment	0.044	0.229	0.007	0.217	1.375	1.298	0.354**
	(0.435)	(0.240)	(0.170)	(0.142)	(0.901)	(0.997)	(0.178)
Pre-Treatment				-0.185	-1.215	-1.246	-0.249
				(0.157)	(1.072)	(1.117)	(0.167)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (2) which compares treated firms that received different signals about p and θ. In all cases, we include an additional set of dummies for quintiles of the pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p and θ, and the corresponding interactions with the time variable. The results are based on Poisson regressions with variables expressed in percentage terms, so the coefficients can be interpreted directly as elasticities. Panel (a) presents the effect of providing different information regarding p and θ in the *audit-statistics* message. Panel (b) compares the two *audit-threat* messages, i.e. the 50% threat of audit vs. the 25% threat of audit. In the "Post-Treatment" rows, the coefficient reported corresponds to a comparison between a post-treatment period and a pre-treatment period (October 2015 – September 2016 vs October 2014 - September 2015). In the "Pre-Treatment" rows we present a falsification test where two pre-treatment periods are compared (October 2014 – September 2015 vs October 2013 - September 2014). Columns (1) and (2) show the effect on the extensive margin using two alternative strategies. Column (1) presents the treatment effect on the probability of making at least one VAT payment in the post-treatment period using a Probit model. Columns (2) and (3) are identical to column (1), except that the dependent variable is defined as making at least three or six payments in the post-treatment period respectively. Columns (4), (5) and (6) present different estimation strategies for the intensive margin, i.e. the total amount of VAT paid. Column (4) corresponds to the baseline specification (a Poisson model), while column (5) uses an OLS regression and column (6) uses a Tobit regression. Column (7) reports the result of estimating a Poisson model on the final VAT liability calculated from annual tax returns. The number of observations corresponding to specifications reported in column (7) is reported in square brackets.

Table B.9: Effects of *Audit-Statistics* Sub-Treatments: Alternative Specification of $p \cdot \theta$

	By Time Horizon		By Payment Timing		By Tax Type	
	First Year (1)	Second Year (2)	Retroactive (3)	Concurrent (4)	Non-VAT (5)	VAT + Non-VAT (6)
$p \cdot \theta$ (%)						
Post-Treatment	-0.292	-0.516	2.503	-0.406	0.398	0.017
	(0.529)	(0.560)	(3.095)	(0.532)	(0.531)	(0.439)
Pre-Treatment	-0.011	-0.011	-3.193	0.132	-0.173	-0.094
	(0.415)	(0.415)	(2.701)	(0.412)	(0.528)	(0.355)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (B.1) which compares treated firms that received different signals about $p \times \theta$. In all cases, we include an additional set of dummies for quintiles of the pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p and θ, and the corresponding interactions with the time variable. This table presents the effect of providing different information regarding $p \times \theta$ in the *audit-statistics* message. In the "Post-Treatment" row, the coefficient reported corresponds to a comparison between a post-treatment period and a pre-treatment period. In the "Pre-Treatment" row we present a falsification test where two pre-treatment periods are compared. Columns (1) and (2) report the effect of treatment by time horizon. The post-treatment effect reported in column (1) corresponds to the difference-in-differences estimate that compares October 2015 – September 2016 vs. October 2014 - September 2015. The post-treatment effect reported in column (2) is analogous but uses the second year after the treatment as the post-treatment period (i.e., October 2016 – September 2017). For the falsification tests, column (1) is based on a comparison between October 2014 – September 2015 and October 2013 - September 2014 while column (2) compares October 2014 – September 2015 to October 2012 - September 2013. Columns (3) and (4) present the first year effect of treatment on retroactive (3) and concurrent (4) VAT payments. Columns (5) and (6) report the first year results by type of tax. Column (5) presents the effect of the treatment on other (non-VAT) tax payments, while column (6) reports the effect on the total amount of taxes paid by the firms during the same period. In all cases, we restrict the analysis to firms that effectively received the letter as reported by the postal service.

Table B.10: Average Effects of *Audit-Statistics*, *Audit-Endogeneity* and *Public-Goods* Messages: Decomposition of Taxes

	Total Taxes	VAT	Other	Other Taxes		
				Corporate	Wealth	PIT Withholdings
	(1)	(2)	(3)	(4)	(5)	(6)
a. *Audit-Statitstics* (10,272 firms) vs *Baseline* (2,064 firms)						
Post-Treatment	0.073***	0.070***	0.086**	0.058*	0.198	0.078
	(0.020)	(0.021)	(0.037)	(0.030)	(0.129)	(0.048)
Pre-Treatment	0.014	0.009	0.008	0.024	-0.057	0.030
	(0.021)	(0.020)	(0.043)	(0.030)	(0.166)	(0.046)
b. *Audit-Endogeneity* (2,039 firms) vs *Baseline* (2,064 firms)						
Post-Treatment	0.078***	0.071***	0.090*	0.069	0.204	-0.003
	(0.028)	(0.028)	(0.054)	(0.054)	(0.133)	(0.077)
Pre-Treatment	0.017	-0.005	0.056	0.063	0.019	0.072
	(0.028)	(0.028)	(0.055)	(0.049)	(0.169)	(0.080)
c. *Public-Goods* (2,017 firms) vs *Baseline* (2,064 firms)						
Post-Treatment	0.056**	0.051**	0.067	0.058	0.135	0.005
	(0.024)	(0.025)	(0.043)	(0.043)	(0.129)	(0.059)
Pre-Treatment	-0.015	-0.003	-0.038	-0.045	-0.005	-0.008
	(0.026)	(0.024)	(0.054)	(0.053)	(0.162)	(0.065)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (1) which compares treated firms to control firms and pre-treatment to post-treatment periods, using yearly aggregated variables. The results are based on Poisson regressions, so the coefficients can be interpreted directly as semi-elasticities. Panel (a) compares the *audit-statistics* message with the *baseline* message, while panels (b) and (c) replicate the comparison for the *audit-endogeneity* and *public-goods* messages respectively. In the first row of each panel ("Post-Treatment"), the coefficient reported corresponds to a comparison between a post-treatment period (October 2015 - September 2016) and a pre-treatment period (October 2014 - September 2015). The second row ("Pre-Treatment") presents a falsification test where two pre-treatment periods are compared (October 2014 – September 2015 and October 2013 - September 2014). Column (1) reports the treatment effects on total tax payments. Columns (2) and (3) decompose total tax payments into VAT and other tax payments. Columns (4), (5) and (6) decompose other taxes into corporate income taxes, property taxes and personal income tax withholding respectively. In all cases, we restrict the analysis to firms that effectively received the letter as reported by the postal service.

Table B.11: Elasticities of Tax Payments with Respect to Audit Probability and Penalty Rate, *Audit-Statistics* and *Audit-Threat* Sub-Treatments: Decomposition of Taxes

				Other Taxes		
	Total Taxes (1)	VAT (2)	Other (3)	Corporate (4)	Wealth (5)	PIT Withholdings (6)
a. *Audit-Statitstics* (10,272 firms)						
Audit Probability (%)						
Post-Treatment	0.038	-0.063	0.109	0.316	-0.125	-0.288
	(0.208)	(0.242)	(0.240)	(0.293)	(0.358)	(0.573)
Pre-Treatment	0.063	0.141	-0.035	0.002	-0.609	0.517
	(0.147)	(0.164)	(0.230)	(0.257)	(0.652)	(0.509)
Penalty Size (%)						
Post-Treatment	-0.001	-0.033	0.061	0.001	0.021	0.608**
	(0.092)	(0.118)	(0.103)	(0.123)	(0.184)	(0.299)
Pre-Treatment	-0.078	-0.128	0.018	-0.066	0.450	-0.129
	(0.087)	(0.108)	(0.119)	(0.132)	(0.283)	(0.281)
b. *Audit-Threat* (4,048 firms)						
Audit Probability (%)						
Post-Treatment	0.233**	0.217	0.002	-0.060	0.146	2.567***
	(0.111)	(0.142)	(0.176)	(0.210)	(0.146)	(0.989)
Pre-Treatment	-0.257	-0.185	-0.067	-0.047	-0.100	-1.480
	(0.164)	(0.157)	(0.148)	(0.181)	(0.194)	(1.013)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (2) which compares treated firms that received different signals about p and θ. In all cases, we include an additional set of dummies for quintiles of the pre-treatment VAT payments, which are the groups from which we drew the sample of "similar firms" to calculate p and θ, and the corresponding interactions with the time variable. The results are based on Poisson regressions with variables expressed in percentage terms, so the coefficients can be interpreted directly as elasticities. Panel (a) presents the effect of providing different information regarding p and θ in the *audit-statistics* message. Panel (b) compares the two *audit-threat* messages, i.e. the 50% threat of audit vs. the 25% threat of audit. In the first row of each panel ("Post-Treatment"), the coefficient reported corresponds to a comparison between a post-treatment period (October 2015 - September 2016) and a pre-treatment period (October 2014 - September 2015). The second row ("Pre-Treatment") presents a falsification test where two pre-treatment periods are compared (October 2014 – September 2015 and October 2013 - September 2014). Column (1) reports the treatment effects on total tax payments. Columns (2) and (3) decompose total tax payments into VAT and other tax payments. Columns (4), (5) and (6) decompose other taxes into corporate income taxes, property taxes and personal income tax withholding respectively. In all cases, we restrict the analysis to firms that effectively received the letter as reported by the postal service.

Table B.12: Effects of *Audit-Statistics*: By Prior Beliefs ($\hat{p}$) and Information Treatment's Audit Probability (p), and by Previous Audit Experience

		By $\hat{p}$		Audited in 2001-2015	
	All	$\hat{p} < p$	$\hat{p} \geq p$	Yes	No
	(1)	(2)	(3)	(4)	(5)
a. *Audit-Statitstics* (10,272 firms) vs *Baseline* (2,064 firms)					
Post-Treatment	0.070***	0.061***	0.086***	0.082**	0.066***
	(0.021)	(0.023)	(0.028)	(0.039)	(0.025)
Pre-Treatment	0.009	0.011	0.004	-0.028	0.023
	(0.020)	(0.020)	(0.024)	(0.043)	(0.022)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors reported in parentheses are clustered at the firm level. Treatment effects are estimated using the difference-in-differences specification reported in equation (1) which compares treated firms to control firms and pre-treatment to post-treatment periods, using yearly aggregated variables. In the first row ("Post-Treatment"), the coefficient reported corresponds to a comparison between a post-treatment period and a pre-treatment period (October 2015 – September 2016 vs October 2014 - September 2015). The second row ("Pre-Treatment") presents a falsification test where two pre-treatment periods are compared (October 2014 – September 2015 vs October 2013 - September 2014). Additional details about the model are reported in the notes of Table 2 and Section 4. Column (1) shows results from the main specification, i.e. the first year post-treatment effect estimates reported in Table 2. Column (2) reports the effect of the *audit-statistics* message on firms whose prior beliefs about the probability of being audited were below the signal (p) reported in the letter that they received. Column (3) reports the results for firms whose prior beliefs were above the reported p. In both cases, the reference group is the *baseline* group. Column (4) reports estimates for firms that were audited at least once between 2001 and 2015, and column (5) for the group of firms that were not audited during that period.

C Solution to the Model

The optimal evasion is given by maximizing the expected utility:

$$\max_{E\in[0,Y]} \frac{1-p\left(\frac{E}{Y}\right)-\epsilon}{1-\sigma}\Big(Y-\alpha\tau(Y-E)\Big)^{1-\sigma}+\frac{p\left(\frac{E}{Y}\right)+\epsilon}{1-\sigma}\Big(Y-\alpha\tau(Y-E)-(1+\theta)\tau E\Big)^{1-\sigma}$$

The FOC for the interior solution are:

$$\begin{aligned}\left(1-p\left(\frac{E}{Y}\right)-\epsilon\right)\Big(Y-\alpha\tau(Y-E)\Big)^{-\sigma}\alpha\tau-\frac{\frac{\partial p\left(\frac{E}{Y}\right)}{\partial E}}{Y(1-\sigma)}\Big(Y-\alpha\tau(Y-E)\Big)^{1-\sigma}-\\ -\left(p\left(\frac{E}{Y}\right)+\epsilon\right)\Big(Y-\alpha\tau(Y-E)-(1+\theta)\tau E\Big)^{-\sigma}(1+\theta-\alpha)\tau+\\ +\frac{\frac{\partial p\left(\frac{E}{Y}\right)}{\partial E}}{Y(1-\sigma)}\Big(Y-\alpha\tau(Y-E)-(1+\theta)\tau E\Big)^{1-\sigma}=0\end{aligned}$$

$$\begin{aligned}\Big(Y-\alpha\tau(Y-E)\Big)^{-\sigma}\left((1-p\left(\frac{E}{Y}\right)-\epsilon)\alpha\tau-\frac{\frac{\partial p\left(\frac{E}{Y}\right)}{\partial E}}{Y(1-\sigma)}(Y-\alpha\tau(Y-E))\right)=\\ \Big(Y-\alpha\tau(Y-E)-(1+\theta)\tau E\Big)^{-\sigma}\left(\left(p\left(\frac{E}{Y}\right)+\epsilon\right)(1+\theta-\alpha)\tau-\frac{\frac{\partial p\left(\frac{E}{Y}\right)}{\partial E}}{Y(1-\sigma)}(Y-\alpha\tau(Y-E)-(1+\theta)\tau E)\right)\end{aligned}$$

In the traditional *A&S* specification, with $\alpha=1$, $p_1=0$ and $\epsilon=0$, we can obtain a closed analytical form for the elasticities between the VAT payments and p and θ:

$$\frac{\partial log\left(\tau(Y-E)\right)}{\partial p}=-(1-\tau)\frac{-\frac{1}{\sigma}\left(\frac{p\theta}{1-p}\right)^{-\frac{\sigma+1}{\sigma}}\frac{\theta(1+\theta)}{(1-p)^2}}{\left(\theta\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}+1\right)^2\left(\tau-(1-\tau)\left(\frac{\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}-1}{1+\theta\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}}\right)\right)}$$

$$\frac{\partial log\left(\tau(Y-E)\right)}{\partial\theta}=-(1-\tau)\frac{-(1+\theta)\frac{1}{\sigma}\left(\frac{p\theta}{1-p}\right)^{-\frac{\sigma+1}{\sigma}}\frac{p}{1-p}-\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}(\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}-1)}{\left(\theta\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}+1\right)^2\left(\tau-(1-\tau)\left(\frac{\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}-1}{1+\theta\left(\frac{p\theta}{1-p}\right)^{-\frac{1}{\sigma}}}\right)\right)}$$

The closed-form expressions are still available for the extensions with $\alpha<1$ and $\epsilon>0$, but not for $p_1>0$ (and thus we use standard numerical methods to compute these elasticities).

D Additional Material

Figure D.1: Mass Advertising Campaign Sample: Billboard Poster, United Kingdom's Tax Authority, 2012

Notes: Advertising campaign by the United Kingdom's tax authority, HMRC (Her Majesty's Revenue and Customs), 2012. Previously hosted at http://www.gov.uk/sortmytax (no longer available, accessed through http://web.archive.org/)