Casting a Wider Tax Net:

Experimental Evidence from Costa Rica

Anne Brockmeyer, Marco Hernandez, Stewart Kettle, and Spencer Smith[*]

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Abstract

The majority of firms in developing countries are informal, and encouraging them to register for taxation is challenging. We argue that non-filing of taxes among registered firms constitutes an important intermediate form of informality, which can be tackled cost-effectively. Using a randomized experiment in Costa Rica, we show that credible enforcement emails tripled (doubled) the income tax filing (payment) rate among previously non-filing firms. Highlighting third-party reports of a firm's transactions further increased compliance. The effect persisted in the medium term, and treated firms became more likely to file information reports about other firms, facilitating future tax enforcement.

Keywords: informality, tax evasion, firms, communication experiment.

JEL codes: H26, H32, O17.

Developing countries struggle to raise sufficient tax revenues to fund public services and anti-poverty programs. Indeed, low-income countries have significantly lower tax-to-GDP ratios than high-income countries, despite similar tax rates (Gordon and Li 2009; Besley and Persson 2013). A large part of the tax revenue gap can thus be explained by low compliance with statutory tax obligations, one key aspect of which is non-filing. In Costa Rica, 25% of tax-registered firms and over 60% of firms that are unregistered but known to the tax authority through third-party reports from other firms do not file their income tax declaration. The numbers are similar in Guatemala, where 28% of registered firms do not file, and across the Latin America region, where the average non-filing rate ranges from 20% to 30%.[1] In addition to the loss of government revenue, non-filing generates horizontal inequities, i.e., competitiveness gaps, between compliant and non-compliant firms, which can distort resource allocation (Hsieh and Klenow 2009; Restuccia and Rogerson 2008) and affect the tax morale of compliant firms (Luttmer and Singhal 2014). Yet, despite its empirical importance, non-filing has received little attention in the literature, which has focused on tax registration of fully informal firms[2] (e.g., De Andrade, Bruhn and McKenzie 2014) and misreporting among tax filers (e.g., Carrillo, Pomeranz and Singhal 2016), notably finding these compliance gaps difficult to address.[3]

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[1]Kettle et al. (2016), discussions with the Inter-American Center of Tax Administrations.

[2]For the purpose of this paper, firms that are not tax registered are considered fully informal. Firms that comply only partially with their tax filing obligations are considered partially informal.

[3]Formalizing firms is challenging and costly because their (private) benefit of formalization is often lower than the cost of formalization, especially for small firms (McKenzie and Sakho 2010; de Mel, McKenzie and Woodruff 2013; Bruhn and McKenzie 2013). Interventions to reduce misreporting have shown success for the value-added tax (Pomeranz 2015; Naritomi 2015), but have been less successful for the income tax, where firms can shift evasion from the sales margin to the less verifiable cost margin (Carrillo, Pomeranz and Singhal 2016; Slemrod et al. 2015).

This paper argues that compliance at the tax filing margin can be enhanced with simple and highly cost-effective interventions. We evaluate a nation-wide randomized trial conducted by the tax authority of Costa Rica, in which 49,757 non-filing firms were requested by email to submit their income tax declaration for 2014. To design the strongest possible message, the emails combined three features tested in previous communication interventions aimed at reducing misreporting or payment arrears. Specifically, the emails contained strong deterrence content, integrated insights from behavioral economics, and, where possible, leveraged third-party information that the tax authority collected about firms' business activities.

The deterrence content included a threat of audit (as in Slemrod, Blumenthal and Christian (2001), Kleven et al. (2011), and Pomeranz (2015)) and of temporary firm closure, as well as a threat of public shaming through the publication of a list of non-filers online (similar to Perez-Truglia and Troiano (2016)). These deterrence features are strong but credible. Every year, the tax authority conducts approximately 700 audits[4], targeting firms with large estimated tax liabilities and suspicious behavior, including non-filers, and forces several hundred week-long firm closures, which cover all four administrative regions of the country.[5] The tax authority routinely uses the threat of audit and shop closure in compliance communications with taxpayers.[6] While the likelihood of an audit or shop closure for any particular firm is low, the monetary and particularly the reputational costs are substantial. Shop closures for instance, are accompanied by an official sign on the

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[4]The number of audits in 2013 and 2014 were 667 and 752, respectively. Of these, less than 10% targeted large taxpayers.

[5]In 2014, the tax authority conducted 293 firm closures, of which 94 were for non-filing.

[6]Previous communication campaigns conducted by the tax authority did not incorporate all late-filers and varied the message content and delivery method in a non-random way.

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business's door indicating closure by the tax authority. The authority also began to publish names of non-filers for the sales tax in August 2015, and routinely publishes the names of late-payers, i.e., taxpayers who filed but have outstanding liabilities to pay.

Additionally, the emails incorporated insights from behavioral economics, such as personalization of the messages (Haynes et al. 2013), the inclusion of a clear call to action (Gabaix and Laibson 2005) and of a direct web link to the income tax declaration (BIT 2014), simplified text (BIT 2014; Dwenger et al. 2016), and a social norm statement (e.g., "8 out of 10 taxpayers have already filed") (Wenzel 2005; Hallsworth et al. 2015; Del Carpio 2014).[7]

Finally, the messages leveraged third-party reported information about firms' business activities, i.e., information that the tax authority collects through informative declarations presented by other firms, state institutions and credit/debit card processing companies. In the sample of firms for which no third-party information was available, the emails either did not mention third-party information at all (treatment 1), or mentioned the general *use* of this information by the tax authority (treatment 2). In the sample of firms for which third-party information was available, the emails either highlighted the *existence* of third-party information about the taxpayer in question (treatment 1), or provided specific examples of the taxpayer's third-party reported sales, such as the name of a client firm and the reported purchase amount (treatment 2). We use examples rather than the total amount of third-party reported sales in the message, as the amounts are small for many tax-

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[7] Hallsworth (2014) notes that a social norm can backfire among relatively compliant taxpayers. This is not a concern for our study, which deals only with non-compliers, none of whom conform to the norm.

payers, and it is possible that firms over-estimate the total amount of third-party reports, in which case providing examples rather than specific amounts might lead to larger compliance gains. The experiment design allows us to test if the existence of third-party information increases firms' response to a basic deterrence message and if leveraging third-party information can further strengthen the message.

We analyze the impact of the intervention using rich administrative data on multiple taxes and filing obligations of firms, and we present three sets of results. First, the emails sent to non-filers tripled their rate of income tax filing and more than doubled the rate and amount of payment, relative to the control group that received no message. Among firms covered by third-party information, listing specific examples of third-party information about the firm had an additional positive effect on all outcomes, and significantly increased the rate of filing by two percentage points (p.p.). The filing rate reached 34% among treated firms covered by third-party information and 19% among treated firms not covered by third-party information. While the proportional treatment effect is larger in the latter group, the absolute treatment effect is larger in the former group. The return on the (strongest) email is US\$ 19 for firms with third-party information and US\$ 0.39 for firms without third-party information.[8]

Second, we advance the understanding of compliance spillover effects by examining whether the treatment had an impact on a wide variety of compliance outcomes that were not specifically targeted by the intervention. The possibility of negative spillovers on other compliance margins is indeed a key concern for compliance interventions. In our experiment, we find no negative impact on com-

[8]Figures are calculated using an exchange rate of 545 Costa Rican *colones* (CRC) per U.S. dollar.

pliance with sales tax filing or payment, and a significant but small increase in the deregistration rate by 1-2 p.p., though this latter result applies only to firms with extremely small reported tax liabilities. Instead, we find that the intervention increased the rate of income tax filing and payment, both in the fiscal years prior to the intervention—meaning that treated taxpayers were more likely to catch up with previous outstanding obligations—and also in the year following the intervention. The persistence of the treatment effect one year later without any further communication suggests that the intervention did not just act as a reminder or nudge, but led taxpayers to update their belief about enforcement. In addition, we find that the emails, and particularly those mentioning specific examples of third-party information, induced a higher share of firms to file an informative declaration, reporting transactions with clients and suppliers, an effect that also persists in the year following the intervention. This means the intervention increased the tax authority's information set for future tax enforcement.

Third, in an effort to provide guidance for targeting future communication interventions, we analyze heterogeneity in the main treatment effect on tax filing and payment. We find that the impact on filing is driven by smaller firms and the self-employed, and the impact on payment is driven by larger firms, particularly corporations and those covered by larger amounts of third-party information. This can be explained by the presence of an exemption threshold in the tax schedule for the self-employed, but not for corporations, as is the case in most countries. We also find that firms with a better past compliance record respond more strongly to the treatment. Finally, we provide some evidence suggesting that the treatment mentioning specific examples of third-party information generates an improvement in

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taxpayers' reporting behavior conditional on filing, leading firms to declare higher sales without fully offsetting this effect through a cost increase.

Given an overall cost-benefit ratio of about 1:5, we conclude that the email intervention is a cost-effective and sustainable way to "cast a wider tax net," by not only increasing present compliance but also past and future compliance among targeted taxpayers, as well as broadening the tax authority's information set for future tax enforcement. If enforcement messages could be automatically personalized with the taxpayer's name and third-party information, and sent costlessly through the tax authority's IT system, they should be part and parcel of any tax enforcement strategy.[9] While the aggregate revenue gains are small, as in most other communication experiments,[10] the intervention does level the competitive playing field between compliant and non-compliant firms, which might foster production efficiency and the tax morale of compliers.

This paper contributes to three strands of literature. First, we contribute to the literature on taxation and development, as surveyed in Besley and Persson (2013), that analyzed how tax capacity grows along the development path and which interventions are best suited to accelerate this process. While most recent contributions to this literature have focused on misreporting on the intensive margin (Best et al. 2015; Pomeranz 2015; Naritomi 2015, Carrillo, Pomeranz and Singhal 2016), our

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[9]If the intervention cannot be implemented costlessly, the tax authority should compare the elasticities of revenue to different enforcement interventions, e.g. audits or communications with non-filers, to determine the optimal portfolio of enforcement activities (Keen and Slemrod 2016).

[10]In general, revenue gains from enforcement are small because the tax revenue distribution is very skewed with most revenue coming from a very small fraction of (highly monitored) taxpayers. Hallsworth et al. (2015), for instance, report that their experiment generated an additional UK£ 9 million of revenue in the first 23 days after the communications, which represents about .3% of income tax revenue, but it remains unclear to what extent this is an advancement of payments that would be made at a later point anyway. Many other studies apply only to sub-samples of taxpayers and do not benchmark treatment effects to aggregate revenue.

study highlights the importance of compliance gaps on the extensive margin, and the cost-effectiveness of simple interventions to reduce these compliance gaps. The role of third-party information in enhancing tax compliance has featured prominently in the literature (Kleven et al. 2011; Kleven, Kreiner and Saez 2016). Third-party reporting has been shown to enhance tax compliance at the intensive margin for the value-added tax (Pomeranz 2015; Naritomi 2015), but results regarding intensive margin compliance with corporate income taxes are less clear-cut. While Carrillo, Pomeranz and Singhal 2016 and Slemrod et al. 2015 show that the use of third-party information in enforcement leads to evasion shifting to the cost margin, Brockmeyer and Hernandez (2016) document sudden and substantial increases in reported tax liabilities when firms in Costa Rica become subject to third-party reporting for the first time.[11] This paper is the first to focus on the role of third-party reporting in enhancing extensive margin compliance, showing that third-party information is both a tool for identifying non-filing firms and strengthening deterrence interventions as well as a by-product of better filing compliance.

Second, this experiment adds to other communication experiments to increase tax compliance, including Dwenger et al. (2016), Hallsworth et al. (2015), and other studies reviewed in Hallsworth (2014). Our study uses a cost effective delivery method—emails[12]—and maximizes message impact by combining different message elements that proved successful in other contexts. As expected, the observed treatment effect of our emails is large compared to most other studies.[13] Besides,

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[11]Brockmeyer and Hernandez (2016) also quasi-experimentally study the impact of tax withholding on business sales, finding that a doubling of the withholding rate increased sales tax payment among treated firms by about 33%.

[12]Also see Ortega and Scartascini (2015) on the larger impact of emails compared to letters.

[13]More precise comparisons are complicated by the fact that most other studies target misreporting or payment arrears rather than non-filing, and differ in whether the control group receives the

our study is one of few in this literature to focus on firms as opposed to individual taxpayers, and the first with Kettle et al. (2016) to focus on tax filing.[14] Indeed, most studies in this literature have focused on correct reporting of liabilities or on the payment of already assessed liabilities. Furthermore, we extend the literature by using rich administrative data to measure a variety of outcomes. Contrary to compliance crowd-out, we find positive impacts on compliance in prior tax periods, filing of informative declarations, and a persistence of the main treatment effects in the medium term.

Finally, this paper contributes to the literature on firm formalization, as reviewed in Bruhn and McKenzie (2014) and De Andrade, Bruhn and McKenzie (2014). This literature found that providing information, reducing registration costs or simplifying regulation is relatively ineffective in encouraging firms to formalize. Only enforcement, e.g., in the form of inspection visits, or payment of about one month of profits were found to significantly increase registration rates. We suggest that encouraging regular tax filing among firms that are tax-registered but do not file (regularly) is a more cost-effective way of casting a wider tax net. As non-filers are firms that chose to register for taxes, their perceived benefit from compliance (tax filing), likely exceeds that of fully informal firms that chose not to register. In addition, the tax authority already has the contact information and in some cases also third-party reports about the business activities of these firms, which can be leveraged to estimate outstanding tax liabilities and contact non-filers. In general, we extend this literature by considering an empirically important intermediate form

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baseline message or no message.

[14]Kettle et al. (2016) find much smaller treatment effects than this study, possibly due to the lower compliance context in Guatemala. Hallsworth (2014) refers to three other papers considering filing behavior, but all are focused on individual taxpayers.

of informality which has so far received little attention.

The rest of this paper is organized as follows. Section 1 describes the tax system in Costa Rica and the data we use. Section 2 presents the experimental design. We discuss the results of the experiment in Section 3 and conclude in Section 4.

1 Context and Data

1.1 Income and Sales Tax

Total tax revenues in Costa Rica represented 13.5% of GDP in 2014. Sales tax revenues constitute the largest share (36%) of total revenues, followed by income tax revenues from corporations (16%) and income tax revenues from wage earners and self-employed individuals (10%) (CR Ministry of Finance 2015).

All firms in Costa Rica are subject to the income tax, and a subset of firms is subject to the sales tax. The income tax schedule depends on whether a firm is registered as a corporation or as an unincorporated firm, i.e., a self-employed individual. There are no size requirements for either firm type. Corporations face an average tax rate on profits of 10, 20 or 30%, depending on their revenue level. The self-employed face a marginal tax rate on profits between 0 and 25%, depending on their profit level. The self-employed thus have lower tax rates on average, and the self-employed below the exemption threshold file their income tax declaration without incurring a liability. Firms in our sample are required to file the income tax declaration by December 15.[15]

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[15]The fiscal year is October 1 to September 30. A few large firms use the calendar year as fiscal year and file the annual declaration by March 15. Firms also have to make three quarterly advance tax payments for the annual income tax if their previous year's liability or their average liability over the last three years is non-zero, with the maximum of these two amounts determining the level of

Firms selling manufactured goods and certain service providers, such as hotels and restaurants, are also liable for the monthly sales tax. This tax is effectively a VAT, providing credits for taxed inputs, but it has a relatively narrow base that excludes most services. Approximately 20% of income tax-liable firms are liable for the sales tax.

Instead of paying the regular income tax (and, where applicable, the sales tax), retailers in certain sectors and below certain size thresholds[16] can opt into a simplified regime. Under the simplified regime, firms file quarterly and pay tax on inputs at sector-specific rates that vary from 3% to 9.8%. During the period we study, approximately 30,000 firms filed their taxes under the simplified regime, while 360,000 firms filed their income taxes under the regular regime. Firms can opt into or out of the simplified regime by submitting a D140 modification form, or deregister completely by submitting a D141 deregistration form.

Figure 1 shows the percentage of tax-registered firms that did not file a tax declaration in 2014 by firm and tax type. For the income tax, this share was 19% of corporations and 25% of the self-employed.[17] It is lower but still substantial for the sales tax: 19% of corporations and 14% of the self-employed. This proportion has been stable over the last five years, and it is based on a tax register that is updated on a regular basis. It is possible that the tax register contains inactive firms whose owners decided not to deregister, as deregistration requires a personal visit to the tax

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advance payments.

[16]I..e., those with annual purchases below 150 base salaries, net assets below 350 base salaries, or with less than six employees.

[17]The corresponding figures in Brockmeyer and Hernandez (2016) are comparable, albeit lower, as they use an algorithm based on tax declarations and registration/deregistration records to construct snapshots of the tax register for different fiscal years, but do not directly observe the tax register. The figures in this paper are the authority's own estimates based on all available data.

office and the payment of any outstanding tax obligations. However, even among firms covered by third-party information, i.e., shown to be economically active, 12% did not file their income tax for the 2014 tax year.

1.2 Third-Party Information

To enforce taxes, the Costa Rican tax authority makes use of third-party information from different sources. The relevant informative declarations, submitted by public or private sector agents about the economic activities of tax-liable firms and individuals, are explained below. An informant submits one informative declaration for each customer or provider, specifying their tax identification number, the transaction amount, the tax withheld (if applicable), and the income or transaction type (in general categories). Taxpayers are not provided with the informative declarations about their transactions. Given the structure of reporting requirements, firms should be aware of the existence of third-party reports about them, but firms with unsophisticated accounting systems may not be able to accurately estimate the amount of third-party reports.

- **Firms** have to report transactions with clients and suppliers (form D151) if the accumulated annual amount of transactions with a single transaction partner reaches CRC 2.5 million. The payment of rent, commissions, professional service fees, or interests must be reported if the annual transaction amount with a single transaction partner reaches CRC 50,000. These transactions must be reported by both the seller and the purchaser. As compliance with this reporting requirement is considered to be incomplete, a firm may not know whether it has been reported by a client or supplier.

- **State institutions** report all purchases from private firms (form D150). They also withhold tax at a rate of 2% on the transaction amount, which is remitted to the tax authority as an advance payment on the income tax. State institutions are considered to be highly compliant with their reporting obligation, so that a firm selling to a state entity can be relatively certain that the transaction will be reported to the tax authority.

- **Credit or debit card companies** report all card sales by affiliated businesses (form D153). They also withhold tax at a firm-specific rate between 0% and 6% on card sales.[18] The withheld amount is remitted to the tax authority as advance payment on the sales tax. Card companies are also considered to be highly compliant with their reporting obligation, so that a firm can be relatively certain that its card-machine sales will be reported to the tax authority.

The filing deadline for third-party reports by firms and state institutions is the same as the filing deadline for the income tax, December 15. Credit and debit card reports are filed monthly. The tax authority uses all informative declarations, combined with customs declarations, to automatically cross-check all income tax returns. Taxpayers with strong discrepancies between third-party information and the self-assessed tax declaration, and/or meeting other criteria[19], are selected for audits.

Third-party reports, combined with the list of tax registered firms, are also used to identify non-filers, i.e. firms that have not complied with their filing obligations for the income tax, the sales tax or third-party reports (reporting transactions with suppliers or clients). The tax authority uses different communication interventions,

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[18]See Brockmeyer and Hernandez (2016) for details on this withholding scheme.

[19]E.g., pertaining to specific sectors or reporting a much lower profit rate than the sector average.

variably relying on emails, phone calls, or personal visits, to follow up with non-filers. However, given human resource constraints, the non-filer campaigns do not systematically cover all non-filers and all filing obligations each year.

1.3 Data

The data set used in this paper includes the income and sales tax returns of corporations and the self-employed in Costa Rica during 2013-2015. This amounts to about 360,000 income tax returns per year and 68,000 sales tax returns per month. The data allows us to measure compliance prior to the experiment (2013), estimate the effect of the experiment on filing for the fiscal year 2014, and estimate the medium term impact on filing in 2015. The data includes all line items from the tax declarations and the payment receipts.. The fact that all declarations and payment receipts carry a time-stamp and indicate the corresponding fiscal period allows us to precisely capture compliance at different points in time. We match the firms' declarations with third-party reports submitted by other firms, state institutions, and credit/debit card companies. These data identify sellers and purchasers, transaction types, transaction amounts, and tax withheld where applicable. About 72% of firms that filed an income tax return in 2014 were reported by at least one client or supplier, 9% were reported by a credit or debit card company, and 6% by a state institution.

2 Experiment Design

Our study design relies on a randomized communication experiment implemented by the tax authority in Costa Rica. Table 1 summarizes the experiment design. The target population included 115,000 firms that were registered with the tax authority but had not filed their income tax declaration for 2014 by February

15, 2015, two months after the regular filing deadline. Of these non-filers, the experiment targeted 49,757 firms that had an email address on file.[20] The experiment was divided into two sub-experiments, targeting firms that were covered by at least one third-party report from a client or supplier ($N = 12,515$), i.e., the firms that were reported to be economically active, and those that were not covered by any third-party report ($N = 37,242$). To determine which firms were covered by third-party information, the tax authority considered all reports by other firms, by state institutions, and by credit/debit card companies.[21]

2.1 Firms Covered by Third-Party Information

Firms covered by third-party information were randomly assigned to one of three groups: a control group and two treatment groups. The control group received no email message. Firms in the treatment group received an email from the tax authority, requesting them to file the income tax declaration for 2014, as shown in Figure A1 in the Appendix. The message content was similar to past communication campaigns. It emphasized that not filing taxes is a serious offense, that offenders could be audited or subject to business closure as stipulated by law, and that the names of non-filers might be published online. Additionally, the message integrated findings from behavioral design to strengthen the message impact. The message used shortened and simplified text (with legal details below the main body of the email), featured a clear call to action written in red capital letters—"Please

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[20]Given the costs of sending letters as well as their slow delivery, the tax authority contacts firms by post only in the context of audits, when a written announcement is required.

[21]A small number of firms in the study shared the same primary email address, either because they had a common owner or shared a common legal representative. For this reason, firms were grouped into clusters based on their primary email address and randomization took place at the cluster level. Throughout our analysis, we use standard errors that are robust to within-cluster correlation, and the results are robust to limiting the sample to clusters with only one firm (98% of clusters). The mean number of firms per cluster is 1.04 and the median is 1.

file your income tax return in the next 10 days"—, provided a direct link the the tax form, and was personalized with the name of the addressee featuring in the address field and the contact information of the responsible tax officer in the signature. The message also presented the social norm—"8 out of 10 [tax-registered] Costa Ricans have filed their 2014 income tax return". The social norm and the deterrence content of the message are all fact-based and credible, given the usual enforcement activities the tax authority conducts through audits, firm closures, and the online publication of the list of late-filers (for the sales tax) and late-payers (for various taxes).[22]

As the tax authority routinely uses third-party information in its enforcement activities and mentions this to taxpayers, the baseline treatment message (treatment 1) also stated that "We have third-party information confirming that you or your client performed activities in 2014 which require you to pay taxes."[23] Treatment 2 further strengthened this statement by providing firms with specific examples of third-party information held by the tax authority about their activities:

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[22]As the message style and content used in past communication campaigns with non-filers varied from year to year, relied on a diverse set of communication methods (emails, SMS, phone calls, personal interviews), and messages were not systematically sent to *all* late filers, we consider that a treatment effect compared to the control group cannot simply be due to sending a *different* message.

[23]This mention of "your client" is added to make the message suitable for being sent directly to the firm or the firm's legal representative.

Treatment 2 (T2), firms with third-party information (experiment 1):

> *We have third-party information confirming that you or your client performed activities in 2014 which require you to pay taxes. From third-party reports (D150, D151 and D153), we know about your operations, for example:*
>
> - *Sales of at least XXX reported by COMPANY,*
> - *Sales from credit/debit cards of at least ZZZ reported by BANK,*
> - *Sales or contracts with state entities of at least WWW.*

For each type of third-party information held by the tax authority (reports from other firms, state institutions, and credit/debit card companies), it listed the transaction amount and partner of the largest reported transaction. Providing evidence of third-party reported transactions and transaction partners clearly demonstrates the tax authority's possession of third-party reports and thus makes the message highly credible. In addition, providing examples rather than the total amount of third-party reported transactions allows us to apply this treatment to small or marginally formal firms with very small third-party reports. Finally, the design of the treatment allows us to test whether firms over- or under-estimate the amount of information that the tax authority holds about them.

2.2 Firms Not Covered by Third-Party Information

As in the first sub-experiment, firms not covered by third-party information were also assigned randomly to either a control group, which received no email, or one of two treatment groups. The treated groups received an email that was nearly identical to the email in the first sub-experiment, as shown in Figure A2 in the Appendix. The only difference is the paragraph on third-party information, which was either omitted entirely (treatment 1), or replaced by a weaker but true statement

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(treatment 2):

Treatment 2 (T2), firms without third-party information (experiment 2):

The tax authority uses information from third-party returns (D150, D151 and D153) to identify economic activity and sources of income.

This treatment thus does not claim that the tax authority has third-party information about the taxpayer in question, but it may encourage compliance among firms who are uncertain about whether they have been reported by a supplier or client.

As the message content here differs slightly from sub-experiment 1, comparing treatment effects across sub-experiments confounds the difference in treatment and the difference in the target population. The most comparable treatments are the weaker message for firms covered by third-party information (T1) and the stronger message for firms not covered by third-party information (T2). We will return to this comparison in our analysis of how treatment effects differ between firms covered and not covered by third-party information.

2.3 Experiment Timing

The experiment took place between March and April 2015, as shown in Table 1. The list of non-filers was extracted on February 15, 2015, two-and-a-half weeks before any emails were sent to firms covered by third-party information and seven weeks before any emails were sent to firms not covered by third-party information. The emails were sent on specific dates. As emails were not sent automatically but manually by individual tax officers in the regional tax offices, the interventions were sequenced to accommodate the available human resources. Given the existing data management procedures, it was not possible to update the list of non-filers

at the start of each experiment. Some firms in the experiment sample had thus already filed by the time the emails were sent. This allows us to demonstrate parallel trends in the treatment and control groups between the extraction of the list and the start date of the experiment. Moreover, the fact that emails are delivered instantly, as opposed to letters which vary in their delivery time, allows us to test whether treatment effects emerge sharply at the start of the experiment.

2.4 Balance of Randomization

To assess the internal validity of the experimental design, we examine the balance of treatment and control groups in terms of characteristics and compliance outcomes at baseline. Table 3 presents balance in terms of firm characteristics that are relevant to compliance behavior, and of which we include a subset as controls in our estimation. The characteristics include the firm type (corporation or self-employed), whether the firm has a legal representative or a secondary email address on file[24], whether it is located in the capital city of San José, and the total transaction amount reported by third-parties. The latter is measured by indicators capturing whether the total amount of third-party reported sales for a given firm is above CRC 2.5 million or above CRC 6 million. These cutoffs correspond to the priority group designations used by the tax authority in prior communication campaigns, when firms with larger third-party reports were more likely to be contacted.

In addition, as one regional tax office deviated from the experiment design and contacted firms prior to the start of the experiment date through phone calls and emails (different from the experimental emails presented in the previous sections),

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[24]When treated firms had two email addresses on file, the tax authority sent the same email to both. We ensure that the proportion of firms with two email addresses is balanced across control and treatment groups, and we control for second email addresses when we estimate treatment effects.

we also consider whether the occurrence of such early communication is balanced across treatment groups. Finally, as we expect past tax compliance to be a predictor of future compliance, we also consider balance of indicator variables capturing whether the firm made any quarterly advance tax payments for 2014, and whether it filed income tax, had a positive net liability, made a payment, or submitted a third-party informative declaration (D151) about a client or supplier in 2013.

Table 3 covers experiment 1 in columns 1-4 and experiment 2 in columns 5-8. Column 1 (5) displays the average for the control group, columns 2 and 3 (5 and 7) show the difference between the control group and treatment groups 1 and 2, respectively, and column 4 (8) reports p-values from a test of the hypothesis that the two treatment groups are jointly equal to the control group. All joint-test p-values exceed the 10% significance cutoff and for 40 out of 42 coefficients, we fail to reject at the 10% level of significance the hypothesis that the treatment group mean equals the control group mean. We thus conclude that the control and treatment groups in both experiments are well-balanced.

In experiment 1, only 14% of firms are corporations, 61% use a legal representative, and 35% and 19% have medium or high amounts of third-party information, respectively. The experiment 2 sample features a 26% share of corporations, and a 66% share of firms using a legal representative. The larger share of self-employed among the non-filers is consistent with the general view that the self-employed are a particularly evasion-prone group of taxpayers. Only 6% of firms in experiment 1 and 5% of firms in experiment 2 have a second email address on file. In both experiments, about 50% of firms are located in the capital city. As for past compliance, only 2% of firms in experiment 1 made advance payments for the income tax for

2014, 33% filed income tax in 2013, and 5% made a payment in 2013. Past compliance rates are even lower among firms in experiment 2, as would be expected given that these firms are not covered by any third-party information. The firms that were contacted early mostly pertain to experiment 1, representing 12% of the sample, but are perfectly balanced across control and treatment groups.

Table A1 in the Appendix examines the balance of outcomes of interest—income tax compliance and other compliance outcomes—measured on the day prior to the start of the first email experiment (March 3, 2015). The structure of this table is identical to Table 3. We find that about 6% of firms in experiment 1 and 1% of firms in experiment 2 filed their income tax declaration after the extraction of the non-filer list and before the start of the experiment. The share of firms reporting a positive net liability and making a payment is below 2% for experiment 1, and close to zero for experiment 2. Despite not filing their own tax declaration, 16% of firms in experiment 1 and 1% in experiment 2 presented a third-party declaration about a client or supplier. Compliance with the sales tax is also non-negligible, with an average number of sales tax declarations over the last twelve months of .6 in experiment 1 and .24 in experiment 2, despite the fact that only about 15% of these firms are liable for the sales tax. As would be expected given the short window between extraction of the non-filers list and the experiment start date, hardly any firms deregistered from the income or sales tax or switched to the simplified tax regime in this period.

We can reject the hypothesis that the treatment group mean equals the control group mean in 38 out of 40 tests in Table A1, and the p-values of the test jointly comparing the treatment groups to the control group exceeds the 10% significance

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cutoff in all cases. For the two variables for which we detect a statistically significant difference (positive net liability and deregistration, both in experiment 2), the means are extremely close to zero, so that the result is driven by a handful of firms. We thus conclude that the control and treatment groups are well-balanced in terms of the relevant firm characteristics and outcomes measured prior to the start of the experiment.

2.5 Estimation

To estimate treatment effects on binary outcomes, such as income tax filing or making a payment, we use the probit specification:

$$\Pr(y_i = 1 | T1_i; T2_i; X_i) = \Phi(\alpha + \beta_1 \cdot T1_i + \beta_2 \cdot T2_i + \gamma X_i), \tag{1}$$

where $T1_i$ and $T2_i$ indicate treatment 1 and 2, respectively, for firm i; X_i is a vector of covariates; and Φ is the cumulative distribution function of the standard normal distribution. The parameters α, β_1, and β_2, and the parameter vector γ are estimated using maximum likelihood.[25]

To estimate treatment effects on continuous outcomes, such as the payment amount, we use two specifications. We first use ordinary least squares (OLS) to estimate the log-linear model

$$\log y_i = \alpha + \beta_1 \cdot T1_i + \beta_2 \cdot T2_i + \gamma X_i + \varepsilon_i, \tag{2}$$

where y_i is payment for firm i, ε_i is an error term, and $T1_i$, $T2_i$, and X_i are defined as before. Second, we use iterated, reweighted least-squares (maximum quasi-

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[25]Our probit estimates are very similar to ordinary least squares (OLS) estimates.

likelihood) to estimate the Poisson pseudo-maximum likelihood (PPML) model:

$$g\{E(y_i)\} = \alpha + \beta_1 \cdot T1_i + \beta_2 \cdot T2_i + \gamma X_i, \tag{3}$$

where $g(\cdot)$ is the natural log function, $y_i \sim$ Poisson, and the other parameters are defined as before. PPML has been shown to outperform OLS if the outcome variable has many zeros (Santos Silva and Tenreyro 2006). Nonetheless, our PPML estimates of the effect of treatment on payment are very similar to OLS estimates.

In all specifications, we compute (Huber-White) standard errors that are robust to within-cluster correlation, as randomization was conducted by clusters of the primary email address.[26] For the probit and PPML specifications, we report average partial effects of discrete changes from zero to one for binary independent variables, such as the treatment indicators, and compute the clustered standard errors using the delta method. In addition to testing the hypotheses that β_1 and β_2 are significantly different from zero, we report the p-value from a (Wald) test of the hypothesis that β_1 and β_2 are equal.

We condition on a set of control variables, which include firm type (corporation), whether the firm has a legal representative, firm location, amount reported by third parties, and the firm's previous compliance record (i.e., income tax filing and payment in the previous year). Each of these controls is defined as in the balance tests in Section 2.4 and measured prior to the start of the experiment.

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[26]The results are also robust to conducting the estimation on the sample of one-firm clusters only.

3   Results

This section presents the results of our empirical analysis, starting with the results on income tax compliance and other compliance outcomes. We then examine persistence of the effects in the medium term, discuss the heterogeneity of our results and potential mechanisms, and conduct a cost-benefit analysis.

3.1   Income Tax Compliance

We start by analyzing the impact of the emails non-parametrically. Figure 2 shows the rate of income tax filing and payment over time by treatment status, with the start of the experiment indicated by a vertical line. The left (right) column is for firms with (without) third-party information. While pre-intervention trends in the treatment and control groups were nearly identical for all outcomes, a positive treatment effect on filing and payment emerged sharply at the start of the experiment. This effect stabilized by about five weeks after the experiment start date, and did not decrease thereafter.[27] This confirms that the emails generated additional tax payments rather than just bring forward payments that firms would have made anyway. After 15 weeks from the start of the intervention, the filing rate for information-covered firms reached 32.5% for those sent the baseline email, and 34.2% for those sent the email with examples of third-party information, relative to 11.5% for the control group.[28] The difference between the two treatment effects is statistically significant at the 10% level. The payment rate was 4.7% for those sent the baseline email, 5.3% for those sent the email with examples of third-party information, and

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[27]This is true also when considering a longer post-intervention period.

[28]The fact that over 60% of the treatment group did not file in response to the (strong) enforcement message might partially be attributable to the fact that some taxpayers did not receive and open the email. It is unfortunately not possible to identify these taxpayers.

1.7% for the control group.

For firms not covered by third-party information, the shape of the filing and payment response relative to the control group is similar, except that there is no difference between the two treatments. Emphasizing the use of third-party information, in addition to highlighting deterrence measures, thus did not enhance compliance among firms not covered by an information trail. This suggests that these firms did not expect the tax authority to possess any third-party reports about them. After 15 weeks of the intervention, the proportion of treated firms filing an income tax declaration reached 19%, relative to 3.9% for the control group. The rate of payment among firms not covered by third-party information was below 1%, as most firms declare a net liability of zero, but the payment rate was still significantly higher for the treatment group.

To consider a larger number of outcomes and control for covariates, we report probit, OLS, and PPML estimates of the treatment effect at 15 weeks in Table 2.[29] Panel A of Table 2 presents estimates for firms covered by third-party information. The first three columns report extensive-margin responses on income tax filing, reporting a positive net liability, and making a payment. We estimate that the baseline email increased the probability of filing by 21.3 p.p.. The effect of the information email, at 23.2 p.p., is significantly greater than the baseline email. The two emails also increased the probability that a firm reported a positive net liability (by about 5 p.p.) and made a positive payment (by about 3.4 p.p.).[30] The fourth and fifth columns show that the emails also increased the average payment amount, which is

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[29]Appendix tables A2 and A3 display the full regression results including the controls.

[30]Note that the payment rate is lower than the share of firms reporting a positive liability, as some firms make quarterly advance payments or are subject to withholding at amounts that fully cover their liability, and others under-pay or pay with delay.

partly driven driven by the greater number of payers, and partly by larger payments conditional on making a payment.[31] Using PPML, we estimate that the baseline email increased income tax payments by CRC 8,168 (US\$ 15) per taxpayer, on average, while the information email increased payments by CRC 10,192 (US\$ 19).[32] The information email had a larger impact on all outcomes than the baseline email, but the difference is statistically significant at conventional levels only for filing, which is presumably the most precisely measured outcome.

Panel B of Table 2 shows the analogous results for firms not covered by third-party information. Recall that, for these firms, the baseline treatment omitted any mention of third-party information, and the information treatment mentioned the general use of third-party information by the tax authority, without making specific reference to the email recipient (see Section 2). The emails increased the probability of income tax filing by 15 p.p.; they also increased the probability of reporting a positive net liability and of making a payment, but the magnitude of the effect is small—0.6 p.p.—compared with the effect on filing. The treatment increased the payment amount by CRC 215 on average (US\$ 0.39).

In sum, we find that the emails significantly improved income tax compliance by non-filers. For firms covered by third-party information, the impact of emails that listed specific examples of information known to the tax authority was larger. All treatment effects estimated here are similar to or exceed those from other communication experiments (Castro and Scartascini 2015; Del Carpio 2014; Fellner,

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[31]Note that we consider payments made by the taxpayer with her annual declaration, and ignore the quarterly advance payments during the year. If a non-filer has outstanding advance payments, those would have to be made with the annual declaration, and are thus considered in our estimation.

[32]We winsorize payment amounts at the top 0.1% to reduce the influence of outliers.

Sausgruber and Traxler 2013; Kettle et al. 2016),[33] which suggests that combining different message features that proved to be impactful individually (deterrence, behavioral design, use of third-party information) can generate a larger impact overall.

3.2 Other Compliance Outcomes

As the treatment had a strong impact on the targeted compliance outcomes, notably income tax filing and payment, it is also appropriate to consider spillovers on other compliance outcomes. Indeed, taxpayers might perceive the intervention as a general increase in enforcement, and also improve compliance with other tax filing and payment obligations. Alternatively, they might perceive the intervention as pertaining to a specific tax only, and increase compliance with the targeted tax but compensate for lost income by reducing compliance with obligations to the government. To shed light on this, we use rich administrative data on firms' filing of informative declarations, sales tax declarations, sales tax payments, deregistration, and switches to the simplified regime, all of these representing compliance outcomes which were not directly targeted by the intervention. We also consider income tax compliance in 2013, which may be affected by the intervention (targeting non-filers for 2014), as a non-negligible share of firms file or pay their taxes with substantial delay (as documented in Brockmeyer and Hernandez (2016)), and 80% of non-filers for 2014 were also non-filers in 2013. This is to our knowledge the first study to examine the impact of enforcement on such a large variety of compliance measures.

Table 4 shows the treatment impact on the above-mentioned outcomes, for experiment 1 in Panel A and experiment 2 in Panel B. Columns 1-3 in Panel A show

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[33]In Fellner, Sausgruber and Traxler 2013, who consider compliance with TV license fees, the proportional effect is larger than ours - 7.7% of letter recipients start to pay the fee within 50 days of the experiment versus 0.8% in the no-letter control group - though the absolute effect is smaller.

that treated firms were marginally more likely to file an information report (D151) about their supplier, and significantly more likely to file an information report about their client.[34] This can be explained by the fact that, for firms filing an income tax declaration, filing an informative declaration generates only a small hassle cost, but no monetary cost, and prevents a possible fine for non-compliance with the reporting obligation. As the emails with additional third-party information made the existence of third-party reports salient and provided information about the nature of these reports, it is not surprising that this treatment had a significantly larger impact on reporting compliance than the baseline treatment. With a 4.8 p.p. increase over a control group average of 12.8%, the effect is twice as large for the information treatment as for the baseline treatment. A simple difference-in-difference analysis (details available upon request) reveals that both treatments combined generated approximately 800 additional third-party reports, 40% of which mirror existing third-party reports, and 60% of which provide new information to the tax authority on transactions that were not previously reported. The impact on information reporting of clients is also present and significant, though smaller in magnitude, among firms not covered by third-party information. For both samples, we confirm in the bottom panels of Figure 2 that the pre-intervention trends in treatment and control groups were similar, and the effect emerged sharply at the time the intervention started. A targeted (income tax) enforcement intervention thus led to an expansion of the tax authority's information set for future enforcement of the income tax and other taxes.

Columns 4-6 show that the intervention had no significant impact on sales tax

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[34]The other information reports (D150 and D153) are not filed by firms and thus considered here.

compliance, which is the most important tax paid by firms. However, given the small share of firms that are liable for the sales tax, it is difficult to detect significant changes in compliance with the sample size at hand.

Columns 7-9 show that the emails increased firms' likelihood of deregistering with the tax authority. This is consistent with the fact that deregistration generates a hassle cost (visiting the tax office[35]), and there are effectively no fines for remaining registered but economically inactive, so that firms are unlikely to voluntarily deregister when ceasing activities. For firms with third-party information, treatment increased the deregistration rate from 0.9% to about 2%, and coverage by particularly large amounts of third-party information was reassuringly negatively correlated with deregistration. For firms not covered by third-party information, treatment increased the deregistration rate from 1.2% to about 3%. Firms not covered by third-party information were also marginally more likely to deregister only from the sales tax (rather than from all tax obligations) or switch to the simplified tax regime, but this concerns less than 0.5% of the sample. The smaller effect on information-covered firms is consistent with these firms' reported economic activity in the fiscal year in question (although they could have ceased activities during the year).

When considering the firms that deregistered and those that did not, pooling the two experiments, it appears that the deregistrants were firms that strive to be compliant on paper, without transferring any revenue to the tax authority. Among deregistrants, 61% filed an income tax declaration for 2014 and 52% did so for 2013, versus 21% and 26%, respectively, for firms that remained in the tax register.

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[35]The possibility for online deregistration was abolished as firms allegedly exploited it to register with the aim of obtaining a book of official receipts, only to deregister immediately afterwards.

Yet the mean reported liability was orders of magnitude lower for deregistrants compared with firms remaining in the tax register—CRC 5,100 versus CRC 826,620 in 2014, and CRC 40,101 versus CRC 656,409 in 2013. While some deregistrants continued their business activities informally (8.2% or 103 firms were third-party reported in 2015), they would have been unlikely to pay more taxes in the current enforcement environment. It thus appears that the deregistrations reduced the number of taxpayers to be managed by the administration, but did not significantly affect tax collection.

Finally, columns 10-11 show that the emails improved compliance for the previous tax year (2013), by significantly increasing the probability of (late) filing and payment. This is true even though the emails specifically mentioned the requirement to file the 2014 income tax return. Emails to information-covered firms increased firms' likelihood of filing an income tax declaration by 2-3 p.p., compared to the control group's average of 35%. The impact on firms no covered by third-party information was even larger—about 5 p.p. compared to the control group's average of 18.9%. In both samples, treated firms were also slightly more likely to make a payment for the income tax in 2013.

In sum, our analysis of a diverse set of compliance outcomes that could potentially be impacted by the enforcement intervention detects positive treatment effects on compliance with third-party reporting and past income tax obligations, and a small increase in deregistration rates, mostly reflecting the exit of firms with especially low reported liabilities.

3.3 Persistence of Effects

Having shown that the treatment had an economically large impact on contemporaneous income tax compliance and other compliance outcomes, we now examine whether these effects persisted over time without a follow-up communication.[36] Table 5 shows the impact on compliance outcomes and third-party reports in 2015, one year after the treatment. For compliance outcomes (columns 1-3 and 7-9), we pool the two treatments for simplicity, as the coefficients do not differ significantly by treatment. For firms covered by third-party information, the email messages increased the income tax filing rate one year later by 6.5 p.p., over a control group average of 35.1%. The effect was even larger—7.3 p.p. or 46%—for firms not covered by third-party information, which received a presumably weaker message in the treatment year. The emails also generated a statistically significant increase in the payment rates one year later: 1.0 p.p. for firms with third-party information and 0.2 p.p. for firms without information. Finally, treated firms were more likely to file informative declarations in 2015. For information-covered firms, the emails generated a 2.3 p.p. increase in the third-party reporting rate, thus further expanding the tax authority's enforcement capacity.

In contrast, we do not detect any effect of the emails on firms' propensity to be the subject of third-party reports in a later year. Columns 4-5 in Table 5 display the effect of the two treatments on firms' likelihood of being reported by state institutions (D150), private sector clients or suppliers (D151), or card processing companies (D153) in 2015. Treated firms were no more or less likely to be the

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[36]The tax authority conducts other communication campaigns for the filing of sales tax and informative declarations, but the targeting would have been orthogonal to our treatment groups.

subject of these reports, even if they received the stronger information email, which provided them with examples of third-party information held by the tax authority. The result holds also when pooling the two treatments. Thus, the treatment does not seem to have distorted production networks by encouraging firms to reduce trade with state agencies or credit/debit card machine usage, or to change trading practices with other firms in an effort to avoid being covered by third-party reports.[37]

Overall, these findings suggest that one-time deterrence messages can have a significant and positive impact on compliance in the medium term, including compliance with information reporting requirements. The strong medium-term effects in our experiment suggest that the email messages lead firms to update their beliefs regarding the tax authority's enforcement capacity, i.e., the capacity to identify and follow up on non-filers, and that the update was persistent over time, hence confirming the messages' credibility.[38] The emails thus did not just act as a reminder or "nudge", nor as a temporary, yet ultimately empty, enforcement threat.

The positive treatment effect in the medium term also sheds some light on the size of potential real effects. By reducing (compliant) firms' after-tax profitability, the treatment might have lowered investment and firm growth. However, the persistent treatment effect on compliance shows that the (positive) effect on income reporting is larger than the (potentially negative) effect on true income.[39]

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[37]We also examine potential compliance spillovers of treatment on firms' trading partners, as identified by the D151 reports, but do not find any spillover effects, similar to Meiselman (2016).

[38]It is possible that the treatment and subsequent exchange with tax officers allowed firms to gain new information about the tax filing procedure, which would have reduced the cost of future tax filing. However, we consider this mechanism to be less important than the deterrence mechanism, as tax filing in Costa Rica is very simple, all the necessary information is available online, and firms in our sample have been in contact with the tax authority at some point, at least to register.

[39]It is also conceivable that the real effect is positive, for instance if better tax compliance allows firms to trade with more formalized and more productive firms.

3.4 Heterogeneity

Although the emails can be sent at a marginal cost of zero, communications campaigns such as the one analyzed here still generate non-negligible costs to the tax authority (more on this below) as emails have to be personalized manually or an algorithm needs to be constructed for this task, and the communications generate information requests from targeted taxpayers to tax officials. Officials report that responding to these inquiries constitutes the most important cost of communication campaigns. Given resource constraints, it is thus important to understand which taxpayers are most likely to respond to the treatment and target the intervention accordingly. This section considers heterogeneity in treatment effects by coverage of third-party information, and by other firm characteristics.

3.4.1 Heterogeneity by Third-Party Information

It is ex ante ambiguous whether coverage by (more) third-party information would be associated with larger treatment effects. The information could interact with the intervention to strengthen its effect. Alternatively, firms that are covered by third-party information may be more likely to comply even without a treatment, and thus less likely to respond to the intervention. We start by considering hetero-geneous treatment effects by information coverage, and then examine heterogeneity by the amount of information within the sample of information-covered firms.

To compare treatment effects across the two sub-experiments for firms with and without third-party information, we focus on the most comparable treatments. These are the baseline email for firms covered by third-party information, which emphasized the presence of third-party information about the firm in question with-out providing specific examples, and the second email for firms that are not covered,

which mentioned the general use of third-party information by the authority. The latter message was still weaker, but delivered the strongest message on third-party information that the authority can credibly send to firms in this sample. While Figure 2 and Table 2 show that the p.p. increase in the rate of income tax filing is greater for firms covered by third-party information (21 p.p.) than for firms not covered (15 p.p.), the proportional effect is greater for firms not covered by third-party information. Their treatment group filing rate is 380% greater than the control group, compared with 180% for information-covered firms. The same qualitative difference between the p.p. increase and the proportional effect holds when considering treatment impact on the rate of payment, the payment amount, and the rate of deregistration. Only the response in terms of information reporting is proportionally larger for information-covered firms. The large proportional effects among firms not covered by a paper trail can be explained by the low baseline (and control group) compliance rates.[40] Regardless, policymakers striving to increase revenue should target information-covered firms first, as their absolute payment response is an order of magnitude larger than the response among non-covered firms.

Zooming in on firms covered by third-party information, Table 6 shows that treatment effect heterogeneity with respect to the extent of information coverage is complex. The filing rate responds less strongly to the treatment for firms with larger amounts of third-party reported sales (columns 1-3), but these firms' payment rate responds more strongly, at least to the information treatment (5-7). The number

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[40]The fact that baseline compliance is higher among information-covered firms but the proportional treatment effects are larger among firms without third-party information suggests that enforcement and third-party information are *substitutes* when it comes to extensive margin tax compliance. This contrasts with Almunia and Lopez-Rodriguez (2016) who find that information trails and monitoring are complements at the intensive compliance margin (bunching below revenue thresholds).

of different third-party reports is associated with a larger treatment effect for both outcomes (columns 4 and 8).

To ascertain that the heterogeneity by third-party reported sales is not driven by a particular cutoff choice, we also report the compliance outcomes by deciles of third-party information in Figure 3. The results are similar when using deciles of the maximum of self-reported sales in year $t - 1$ (or the most recent year available) and third-party reported sales in t. The exercise is thus equivalent to examining treatment effects by firm size as measured in sales.[41] We find that the treatment effect on filing is driven by seemingly smaller firms, with larger firms being significantly more likely to declare even when in the control group, whereas the treatment effect on payment is driven by larger firms. This is consistent with the fact that smaller firms, especially the self-employed, may declare without making a payment (due to being below the exemption threshold or deducting sufficiently high advance payments or tax withheld), and large firms are more likely to incur a positive tax liability. The intervention thus enhanced compliance along the firm-size distribution but derived its revenue effect from larger firms.[42]

3.4.2 Heterogeneity by Other Firm Characteristics

Other firm characteristics along which the treatment effect may vary include firm type, as corporations and the self-employed are taxed under different tax schedules, location in the capital city, and past compliance record. Table 7 considers het-

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[41]We can also estimate firm size for firms without third-party information, taking reported sales from the most recent available income tax declaration, but this measure is available only for a subset of firms and is more noisy, as many firms have not filed for several consecutive years. This, combined with the smaller absolute treatment effects in the sample of firms without third-party information makes it difficult to examine treatment heterogeneity among these firms.

[42]With the caveat of low precision, we find that the effect of the treatment on submitting an informative declaration does not differ by firm size, and the effect on deregistration is concentrated among smaller firms.

erogeneity along these lines for firms with third-party information in Panel A and for firms without such information in Panel B. Unsurprisingly, given the lack of an exemption in their tax schedule, the filing rate of corporations compared to the self-employed responds less strongly but their payment rate responds more strongly to the treatment (columns 1 and 6). There is no heterogeneity in treatment effect along this line on the likelihood of filing by firms without third-party information (Panel A, column 1). Firm location in the capital city is associated with a marginally stronger treatment effect in only one out of the four estimations (columns 2 and 7). Past compliance in the form of filing and payment of the income tax in 2013, and filing of sales tax, strongly predicts a larger treatment effect, as would be expected. This is the case for both compliance outcomes and both sub-samples (with the exception of sales tax compliance which is negatively correlated with the treatment effect on filing among information-covered firms). When targeting their intervention, it is thus advisable for the tax authority to take into account a firm's degree of third-party information coverage, its firm type, and its past compliance record.

3.5   Mechanisms

Before concluding our study, we examine the mechanisms of the large treatment effects on tax payment for firms covered by third-party information. Treatment more than doubled the rate of payment and approximately doubled the amount of payment, with larger effects for the information treatment. These large effects contrast with other studies using third-party information to enhance compliance on the intensive margin. These studies find that firms respond to an increase in third-party reported sales by increasing both their sales and costs, thus minimizing any increase in reported profits and taxes paid (Carrillo, Pomeranz and Singhal, 2016 and Slem-

rod et al. 2015). The challenge in our study is that treatment both increased the filing rate and potentially altered reported liabilities conditional on filing.

We start by comparing third-party reported sales, self-reported sales, and self-reported costs non-parametrically, as in Carrillo, Pomeranz and Singhal (2016). Figure 4 shows that self-reported sales increase less than one-for-one with third-party reported sales (Panel A), although self-reported sales are on average 30% higher than third-party reported sales. Firms thus do not seem to under-estimate third-party reports or match reported sales to an amount lower than true third-party reports. This result supports the use of examples rather than amounts of third-party information in the information treatment. However, the average firm matches declared sales very closely with declared costs (Panel B). Controlling for covariates, Table A4 in the Appendix shows the impact of the two treatments on declared sales, costs, and profits, measuring these variables with an indicator for positive amounts, an indicator for amounts larger than third-party reports, or in logs (in absolute amounts for profits as these can be negative). The results suggest that the information treatment generated a significantly larger increase in reported sales than the baseline treatment (columns 1-3), but only a marginally larger increase in reported costs (columns 4-6). Firms receiving the information treatment thus increased reported sales by more than other treated firms, and did not fully offset this through cost increases. As a result, the likelihood of reporting a positive profit was significantly higher among firms receiving the information treatment than among firms receiving the baseline treatment (column 7). While this evidence remains suggestive, it is consistent with the idea that the information treatment generated an

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improvement in firm reporting behavior conditional on tax filing.[43]

3.6   Cost-Benefit Analysis

We conclude with a cost-benefit analysis, considering the cost of the intervention and the increase in tax revenue it generates. The primary cost to the tax authority is the human resource cost of sending the personalized emails and responding to taxpayer inquiries. The sending of the emails was executed in seven and a half days by 32 tax officers paid at about CRC 36,700 (US$ 67) a day. We assume that each officer spent at most an additional five days answering taxpayers' inquiries. This means the cost per email was on average CRC 295.4 (US$ .54) While the cost per email for firms without third-party information was almost certainly lower, as these firms were less likely to file and hence to consult their tax officer, it is unfortunately not possible to break the administrative cost down by firm type.[44]

The direct benefits are measured by the increase in tax payment from treated firms. Among firms covered by third-party information, the baseline email increased firms' income tax payments by CRC 8,168 (US$ 15), on average, 15 weeks after the start of the experiment, while an email that lists specific examples of information known to the tax authority increased payment by CRC 10,192 (US$ 19). The effect on payment at 15 weeks was smaller for firms not covered by third-party information, for whom the email increased payment by CRC 215 (US$ 0.39). Although we find also significant increases in the propensity to make income tax

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[43]Results are similar when conditioning on filing, with the obvious caveat that the comparison across treatments is then no longer experimental.

[44]To draw the most conservative conclusion possible, we take the tax officer's time cost into account, although other studies implicitly assume that the opportunity cost of time for the tax officers and hence the cost of the entire intervention is zero. We further assume that the administrative costs of filing for taxpayers are small, which is appropriate given the simplicity of the tax code and the online filing system.

payments for prior and future years, we do not include these in our calculation as the point estimates on payment amounts are imprecisely estimated.

With this conservative approach, we find that the intervention was highly cost-effective, with an overall cost-benefit ratio of about 1:5. Although the additional revenue generated constitutes less than 1% of total income tax revenue, the intervention serves the broader purpose of sustaining voluntary compliance by detecting and following up on non-compliers. Besides, the intervention had positive indirect effects in terms of enhancing compliance with information reporting requirements, which facilitates future tax enforcement, and in terms of better medium term compliance for treated firms that update their beliefs about tax enforcement.

Beyond revenue considerations, the intervention improves horizontal equity of taxation by enhancing compliance among relatively small firms, leveling the playing field between full and partial tax compliers, and could thus improve tax morale and the perception of fairness of the tax system. In addition, the new information generated through firms' self-reports and third-party reports give the government a broader view of the economy and hence a better basis for policy design.

4   Conclusion

This paper has argued that non-filing among tax registered firms constitutes an important and under-researched compliance gap in low-income countries that can be addressed cost-effectively. In Costa Rica, approximately 25% of tax registered firms do not file their income tax declaration in a given year, and this share is similar in other countries in the Latin America region. We evaluate a nation-wide communication campaign in which the tax authority in Costa Rica requested non-filing firms by email to file their income tax declaration. The emails featured strong but

credible deterrence messages, integrated behavioral insights and leveraged third-party reports about firms' business activities. We find that the emails tripled the filing rate and doubled the payment rate among previous non-filers—treatment effects that are substantially larger than those found in much of the existing literature, and that further increase when emails specifically mention examples of third-party reported sales. The return on an email was US$ 19 for firms covered by third-party information, but only US$ .39 for firms not covered by third-party information.

We extend the literature on tax compliance interventions by considering a large variety of outcomes, measured through administrative tax records. We show that the emails generated no negative spillovers on sales tax compliance. They slightly increased the deregistration rate, but they also increased the likelihood of filing third-party reports about clients or suppliers, and income tax filing in the year prior and following the intervention. This finding of a persistent treatment effect, which applies to income tax filing and payment and the filing of third-party reports, suggests that the intervention permanently raised firms' perceived enforcement probability and hence their compliance level.

Although the intervention generated only a small aggregate increase in tax revenue, it was highly cost-effective and could easily be automated to become a core part of the tax authority's enforcement strategy. The intervention is likely to generate positive compliance spillovers in the future, through medium-term effects on targeted firms and the increased availability of third-party reports on firms' business activities. Beyond revenue considerations, the intervention reduces horizontal inequities between small and partially compliant firms and their more compliant competitors. This can improve factor allocation and increase perceptions of fair-

ness, fostering tax morale.

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Figure 1: Share of Non-Filers by Firm and Tax Type, 2014
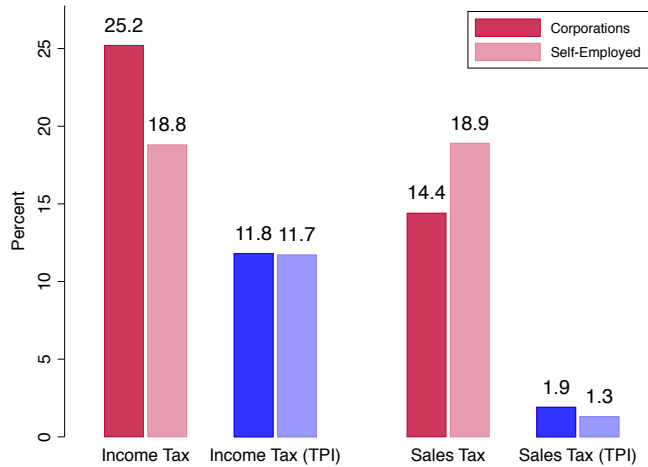
Note: This figure shows the share of non-filers for the income tax (first four bars) and the sales tax (second four bars), distinguishing corporations (dark bars) and the self-employed (light bars). The bar titles indicate whether the bar refers to the full sample (red bars), or the sub-sample of firms covered by at least one type of third party information (reports from other firms, state institutions, or credit/debit card companies) (blue bars). The estimates are for 2014 and are calculated using the tax authority's list of non-filers (based on all available data) and the database of income tax and sales tax declarations submitted for 2014.

Table 1: Experiment Timeline

Extraction of non-filers list ($N = 49,757$)	February 15
Emails to taxpayers with third-party information ($N = 12,515$)	March 4–10
No message control group	
T1: Baseline email	
T2: Information email (mentions amount of largest third-party information report)	
Emails to taxpayers without third-party information ($N = 37,242$)	April 7–23
No message control group	
T1: Baseline email	
T2: Information email (emphasizes tax authority's use of third-party information)	

Table 2: Impact on Income Tax Compliance

	Probit			OLS	PPML
	(1) Filed	(2) Positive net liability	(3) Positive payment	(4) Log payment	(5) Payment
A: Firms with Third-Party Information					
T1: Baseline email	0.213	0.046	0.031	0.340	8,167.736
	(0.009)	(0.005)	(0.004)	(0.043)	(2,394.801)
T2: Information email	0.232	0.054	0.038	0.411	10,191.998
	(0.009)	(0.005)	(0.004)	(0.044)	(2,712.887)
Control group avg.	0.115	0.0339	0.0172	0.194	5015.8
T1=T2 p-value (Wald test)	0.062	0.135	0.145	0.177	0.510
Observations	12,515	12,515	12,515	12,515	12,515
B: Firms without Third-Party Information					
T1: Baseline email	0.150	0.006	0.005	0.050	262.430
	(0.004)	(0.001)	(0.001)	(0.008)	(59.096)
T2: Information email	0.149	0.006	0.004	0.041	166.064
	(0.004)	(0.001)	(0.001)	(0.008)	(52.392)
Control group avg.	0.0393	0.00178	0.00130	0.0142	96.15
T1=T2 p-value (Wald test)	0.807	0.617	0.613	0.397	0.160
Observations	37,242	37,242	37,242	37,242	37,242

Note: This table displays estimates using probit, OLS and PPML as explained in Section 2.5, using the control variables displayed in Table 3. Appendix tables A2 and A3 display the full regression results including the controls. The table columns display the outcome variables: indicators for whether the firm filed income tax for 2014, reported a positive net liability and made a payment (considering only final payments made with the declaration and not advance payments that may have been made earlier), and the (log) payment amount. All outcomes are measured 15 weeks after the start of the experiment. Robust standard errors clustered by email address are in parentheses. Average partial effects are reported for probit and PPML. Payment amounts are winsorized at the top 0.1% to reduce the influence of outliers.

Table 3: Balance of Randomization, Firm Characteristics

	Firms with Third-Party Information (Experiment 1)				Firms without Third-Party Information (Experiment 2)			
	(1)	(2) T1: Baseline email	(3) T2: Information email	(4) T1=T2=0 p-value	(5)	(6) T1: Baseline email	(7) T2: Information email	(8) T1=T2=0 p-value
	Control				Control			
Corporation	0.142 (0.006)	-0.003 (0.008)	-0.004 (0.008)	0.893	0.262 (0.004)	0.011 (0.008)	-0.002 (0.007)	0.238
Has legal representative	0.607 (0.008)	-0.002 (0.011)	0.004 (0.012)	0.882	0.659 (0.005)	0.009 (0.007)	-0.001 (0.007)	0.289
Has second email	0.056 (0.004)	0.001 (0.005)	0.003 (0.005)	0.851	0.054 (0.002)	-0.001 (0.003)	0.002 (0.003)	0.547
Located in San José	0.490 (0.008)	-0.002 (0.011)	0.000 (0.011)	0.975	0.488 (0.005)	0.004 (0.008)	0.004 (0.007)	0.821
Was contacted early	0.117 (0.005)	0.006 (0.007)	-0.002 (0.007)	0.543	0.002 (0.000)	0.000 (0.001)	0.001 (0.001)	0.489
TPI > 2.5 million CRC	0.351 (0.008)	-0.003 (0.011)	-0.021 (0.011)	0.109				
TPI > 6 million CRC	0.185 (0.007)	0.008 (0.010)	-0.009 (0.009)	0.193				
Made advance payment in 2014	0.023 (0.002)	0.001 (0.003)	0.000 (0.003)	0.892	0.005 (0.001)	0.001 (0.001)	-0.001 (0.001)	0.188
Filed in 2013	0.329 (0.007)	0.007 (0.010)	-0.005 (0.011)	0.517	0.174 (0.004)	-0.010 (0.005)	-0.007 (0.005)	0.131
Reported net liability > 0 in 2013	0.085 (0.004)	0.000 (0.006)	0.006 (0.006)	0.561	0.016 (0.001)	0.001 (0.002)	0.000 (0.002)	0.873
Made payment in 2013	0.053 (0.004)	-0.002 (0.005)	0.000 (0.005)	0.927	0.011 (0.001)	0.000 (0.001)	-0.000 (0.001)	0.941
Was third-party informant in 2013	0.190 (0.006)	-0.007 (0.009)	-0.012 (0.009)	0.417	0.046 (0.002)	-0.000 (0.003)	-0.002 (0.003)	0.834
Observations	4,128	4,260	4,127		12,350	12,516	12,376	

Note: The table shows the balance of randomization in terms of firm characteristics, as measured before the experiment start. The rows correspond to the different variables. Columns 1-4 (5-8) correspond to the sample of firms with (without) third-party information, i.e., experiment 1 (2). Column 1 (5) displays the mean for the control group, columns 2 and 3 (5 and 7) show the mean difference between the control group and treatment groups 1 and 2 respectively, and column 4 (8) reports p-values from a test of the hypothesis that the two treatment groups are jointly equal to the control group. Robust standard errors clustered by email address are in parentheses. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

Figure 2: Compliance Over Time by Information Coverage and Treatment Group
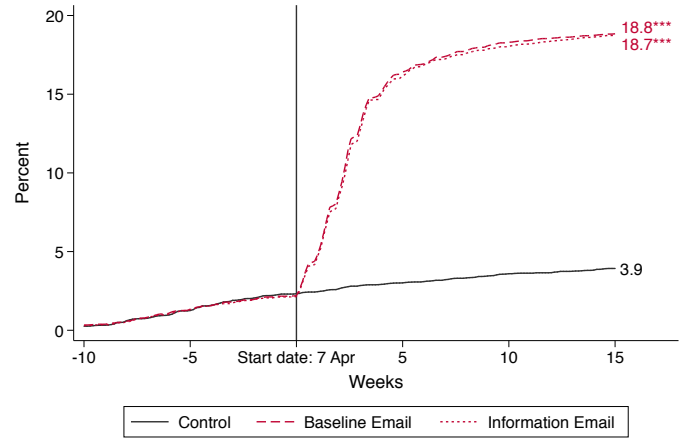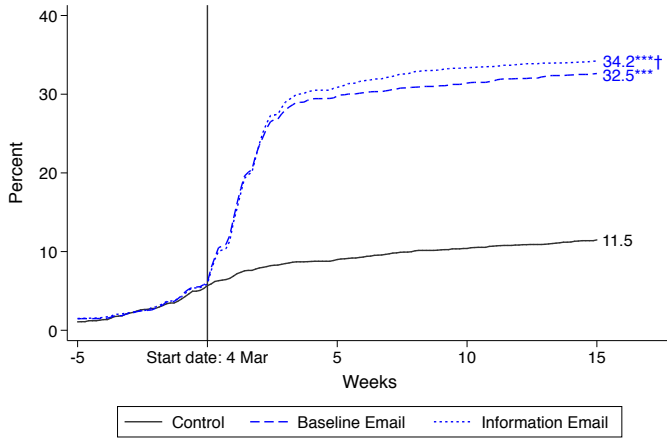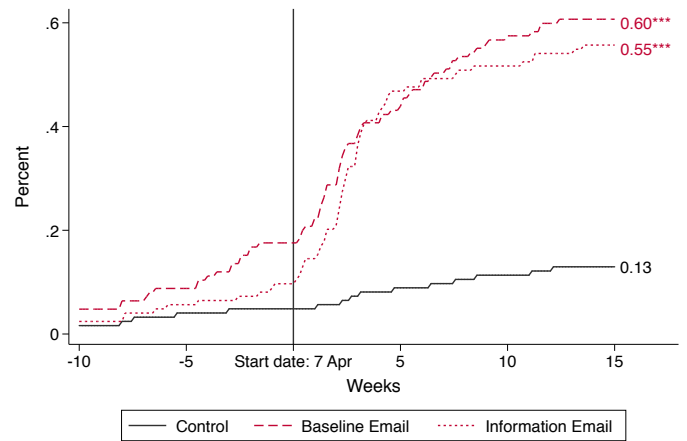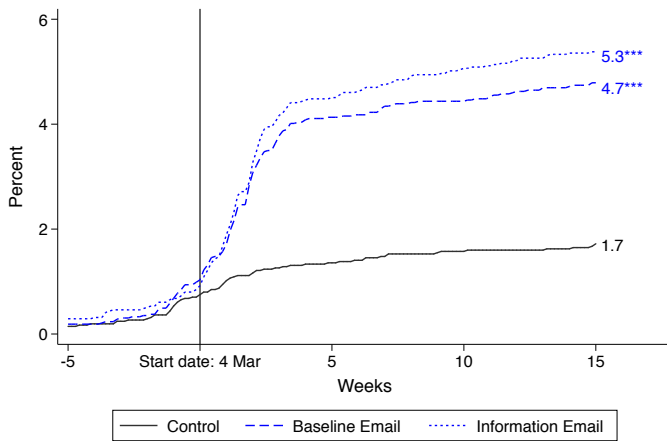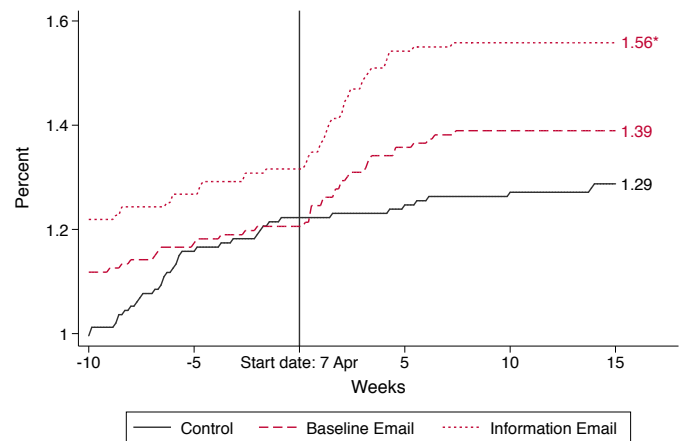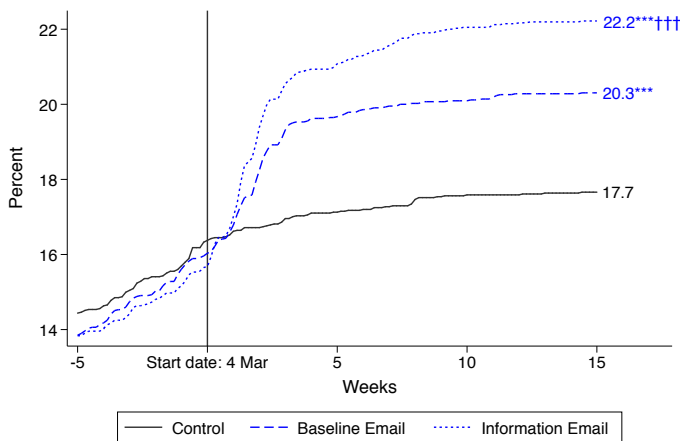
Note: These figures show the percentage of firms filing income tax (row 1), paying income tax (row 2) and filing a third-party informative declaration (D151) about a client or supplier (row 3), all for fiscal year 2014. Column A corresponds to firms with third-party information and column B corresponds to firms without third-party information. The horizontal line in each figure indicates the experiment start date. The black solid line corresponds to the control group and the blue/red dashed/dotted lines correspond to the baseline treatment and information treatment respectively for the two different subsamples, as explained in experiment design Table 1. The numbers indicate the mean for each outcome and treatment group at 15 weeks after the start of the experiment. Stars indicate a significant difference compared to the control group and come from regressions that include controls (as in Table 2 and Table 4). Significance levels are noted as per convention: * p<.10, ** p<.05, *** p<.01. Daggers indicate significant differences between the two treatments.

Table 4: Impact on Other Compliance Outcomes

	Probit			OLS		PPML		Probit			
	(1) Presented informative declaration	(2) Reported supplier	(3) Reported client	(4) No. months decl. sales tax	(5) No. months paid sales tax	(6) Sales tax payment	(7) Deregistered	(8) Deregistered (sales tax)	(9) Switched to simplified regime	(10) Filed income tax for 2013	(11) Paid income tax for 2013
					A: Firms with Third-Party Information						
T1: Baseline email	0.029	0.004	0.028	-0.038	-0.023	-9,902.181	0.011	0.000	0.000	0.031	0.002
	(0.008)	(0.006)	(0.007)	(0.050)	(0.021)	(7,588.325)	(0.003)	(0.001)	(0.000)	(0.004)	(0.001)
T2: Information email	0.055	0.010	0.048	-0.040	-0.022	-4,894.750	0.011	-0.000	0.000	0.023	0.003
	(0.008)	(0.006)	(0.008)	(0.049)	(0.020)	(8,276.246)	(0.003)	(0.001)	(0.000)	(0.004)	(0.002)
TPI > 2.5 million CRC	0.087	0.024	0.080	-0.060	-0.021	4,616.031	0.003	-0.000	0.000	0.017	-0.000
	(0.010)	(0.007)	(0.009)	(0.052)	(0.019)	(14,514.704)	(0.003)	(0.001)	(0.000)	(0.005)	(0.002)
TPI > 6 million CRC	0.067	0.061	0.072	0.551	0.166	25,176.254	-0.012	-0.002	-0.001	0.007	0.006
	(0.012)	(0.009)	(0.011)	(0.088)	(0.035)	(9,631.292)	(0.003)	(0.001)	(0.000)	(0.006)	(0.003)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control group avg.	0.177	0.0928	0.128	0.637	0.133	29095.0	0.00921	0.00218	0.000242	0.350	0.0572
T1=T2 p-value (Wald test)	0.003	0.274	0.013	0.969	0.989	0.461	0.830	0.563	0.975	0.061	0.690
Observations	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515
					B: Firms without Third-Party Information						
T: Any email	0.002	0.000	0.002	0.000	0.006	24.853	0.020	0.001	0.000	0.049	0.001
	(0.001)	(0.001)	(0.001)	(0.016)	(0.004)	(76.896)	(0.001)	(0.001)	(0.000)	(0.002)	(0.000)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control group avg.	0.0129	0.00939	0.00437	0.255	0.0159	357.5	0.0121	0.00316	0.000162	0.189	0.0109
Observations	37,242	37,242	37,242	37,242	37,242	37,242	37,242	37,242	37,242	37,242	37,242

Note: This table displays estimates using probit, OLS and PPML as explained in Section 2.5. Panel A corresponds to firms with third-party information, using the same controls as in Appendix Table A2, and Panel B corresponds to firms without third-party information, using the same controls as in Appendix Table A3. The columns display the outcome variables, which refer to compliance for fiscal year 2014 unless otherwise noted. All outcomes are measured 15 weeks after the start of the experiment. Robust standard errors clustered by email address are in parentheses. Average partial effects are reported for probit and PPML. Sales tax payment amounts are winsorized at the top 0.1% to reduce the influence of outliers. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

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Table 5: Medium-Term Impact on Compliance Outcomes and Third-Party Reports in 2015

	Firms with Third-Party Information						Firms without Third-Party Information		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Filed income tax	Paid income tax	Reported client or supplier	Reported by state institution	Reported by private client or supplier	Reported by card company	Filed income tax	Paid income tax	Reported client or supplier
T: Any email	0.0649 (0.00893)	0.00963 (0.00417)	0.0225 (0.00716)				0.0732 (0.00419)	0.00216 (0.00118)	0.00639 (0.00219)
T1: Baseline email				-0.00290 (0.00419)	-0.00927 (0.0101)	0.00615 (0.00640)			
T2: Information email				-0.00265 (0.00423)	-0.00435 (0.0103)	0.000659 (0.00634)			
TPI > 2.5 million CRC	0.102 (0.0122)	0.0444 (0.00624)	0.109 (0.0105)	0.0320 (0.00520)	0.0968 (0.0113)	-0.0155 (0.00743)			
TPI > 6 million CRC	0.00894 (0.0152)	0.0187 (0.00696)	0.0583 (0.0126)	0.0217 (0.00601)	0.0989 (0.0141)	0.0353 (0.0109)			
Control group avg.	0.351	0.0523	0.203	0.0407	0.691	0.0916	0.159	0.0113	0.0398
T1=T2 p-value (Wald test)				0.952	0.631	0.393			
Observations	12,515	12,515	12,515	12,515	12,515	12,515	37,242	37,242	37,242

Note: This table displays probit estimates , with binary outcome variables as denoted in the column titles, measured in June 2016. Panel A corresponds to firms with third-party information, using the same controls as in Appendix Table A2, and Panel B corresponds to firms without third-party information, using the same controls as in Appendix Table A3. The treatment groups are pooled into one binary treatment variable. The reported coefficients are average partial effects, and robust standard errors clustered by email address are in parentheses. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

Table 6: Heterogeneity by Third-Party Information

	Outcome: Filed Income Tax for 2014				Outcome: Paid Income Tax for 2014			
	(1) Above median TPI	(2) TPI > 2.5 million CRC	(3) TPI > 6 million CRC	(4) No. of D151 reports by clients	(5) Above median TPI	(6) TPI > 2.5 million CRC	(7) TPI > 6 million CRC	(8) No. of D151 reports by clients
T1: Baseline email	0.244 (0.011)	0.242 (0.010)	0.234 (0.009)	0.159 (0.020)	0.025 (0.004)	0.022 (0.003)	0.027 (0.003)	0.023 (0.008)
T2: Information email	0.244 (0.011)	0.251 (0.010)	0.245 (0.009)	0.134 (0.019)	0.025 (0.004)	0.026 (0.003)	0.034 (0.004)	0.013 (0.007)
Control	0.041 (0.012)	0.110 (0.014)	0.083 (0.019)	-0.019 (0.013)	-0.011 (0.005)	0.009 (0.006)	0.012 (0.010)	-0.001 (0.005)
T1 X Control	-0.065 (0.017)	-0.089 (0.020)	-0.123 (0.026)	0.070 (0.023)	0.010 (0.007)	0.025 (0.010)	0.020 (0.014)	0.005 (0.009)
T2 X Control	-0.027 (0.018)	-0.059 (0.020)	-0.082 (0.026)	0.120 (0.023)	0.025 (0.008)	0.033 (0.010)	0.020 (0.015)	0.026 (0.009)
Control variable avg.	0.500	0.343	0.184	1.451	0.500	0.343	0.184	1.451
T1 = T2 p-value (Wald test)	0.992	0.497	0.335	0.260	0.966	0.296	0.117	0.276
T1 + T1 X Control = T2 + T2 X Control	0.012	0.041	0.045	0.083	0.069	0.279	0.681	0.058
Observations	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515

Note: This table displays OLS estimates, with binary outcome variables (measured 15 weeks after the experiment start) as indicated in the panel titles, and interactions between the treatment indicators and different control variables, as indicated by the column titles. The rows display the coefficients on the two treatments, the control, and the interactions between each treatment and the control. All regressions are for the sample of firms with third-party information only, and use the same controls as in Table 2. Robust standard errors clustered by email address are in parentheses. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

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Table 7: Heterogeneity by Other Firm Characteristics

|	Outcome: Declared Income Tax for 2014					Outcome: Paid Income Tax for 2014				
	(1) Corporation	(2) HQ in San José	(3) Filed in 2013	(4) Paid in 2013	(5) Declared sales tax	(6) Corporation	(7) HQ in San José	(8) Filed in 2013	(9) Paid in 2013	(10) Declared sales tax
	A: Firms with Third-Party Information									
Any message	0.230 (0.007)	0.222 (0.010)	0.205 (0.008)	0.216 (0.007)	0.224 (0.007)	0.030 (0.003)	0.035 (0.004)	0.028 (0.003)	0.027 (0.003)	0.032 (0.003)
Control	0.036 (0.020)	0.012 (0.010)	0.073 (0.013)	0.047 (0.038)	0.039 (0.026)	-0.014 (0.010)	0.005 (0.004)	-0.010 (0.006)	0.052 (0.023)	-0.038 (0.009)
Any message X control	-0.066 (0.023)	-0.004 (0.014)	0.045 (0.016)	0.091 (0.038)	-0.060 (0.033)	0.031 (0.012)	-0.002 (0.006)	0.019 (0.007)	0.126 (0.029)	0.030 (0.014)
Control variable avg.	0.140	0.489	0.330	0.0527	0.0637	0.140	0.489	0.330	0.0527	0.0637
Observations	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515
	B: Firms without Third-Party Information									
Any message	0.147 (0.004)	0.143 (0.004)	0.129 (0.003)	0.148 (0.003)	0.148 (0.003)	0.004 (0.001)	0.005 (0.001)	0.004 (0.001)	0.004 (0.001)	0.004 (0.001)
Control	-0.020 (0.004)	-0.011 (0.004)	0.050 (0.007)	-0.062 (0.039)	0.000 (0.017)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.002)	0.030 (0.018)	0.001 (0.003)
Any message X control	0.010 (0.007)	0.015 (0.006)	0.124 (0.010)	0.156 (0.040)	0.070 (0.026)	0.003 (0.001)	-0.000 (0.001)	0.003 (0.002)	0.067 (0.025)	0.007 (0.006)
Control variable avg.	0.265	0.491	0.168	0.0107	0.0251	0.265	0.491	0.168	0.0107	0.0251
Observations	37,242	37,242	37,242	37,242	37,242	37,242	37,242	37,242	37,242	37,242

Note: This table displays OLS estimates, with binary outcome variables (measured 15 weeks after the experiment start) as indicated in the panel titles, and interactions between the treatment indicators and different control variables, as indicated by the column titles. Panel A corresponds to firms with third-party information, using the same controls as in Appendix Table A2, and Panel B corresponds to firms without third-party information, using the same controls as in Appendix Table A3. The rows display the coefficients on the treatment indicator (pooling the two treatment groups), the control, and the interactions between the treatment and the control. Robust standard errors clustered by email address are in parentheses.

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Figure 3: Heterogeneity by Third-Party Information

A: Filing
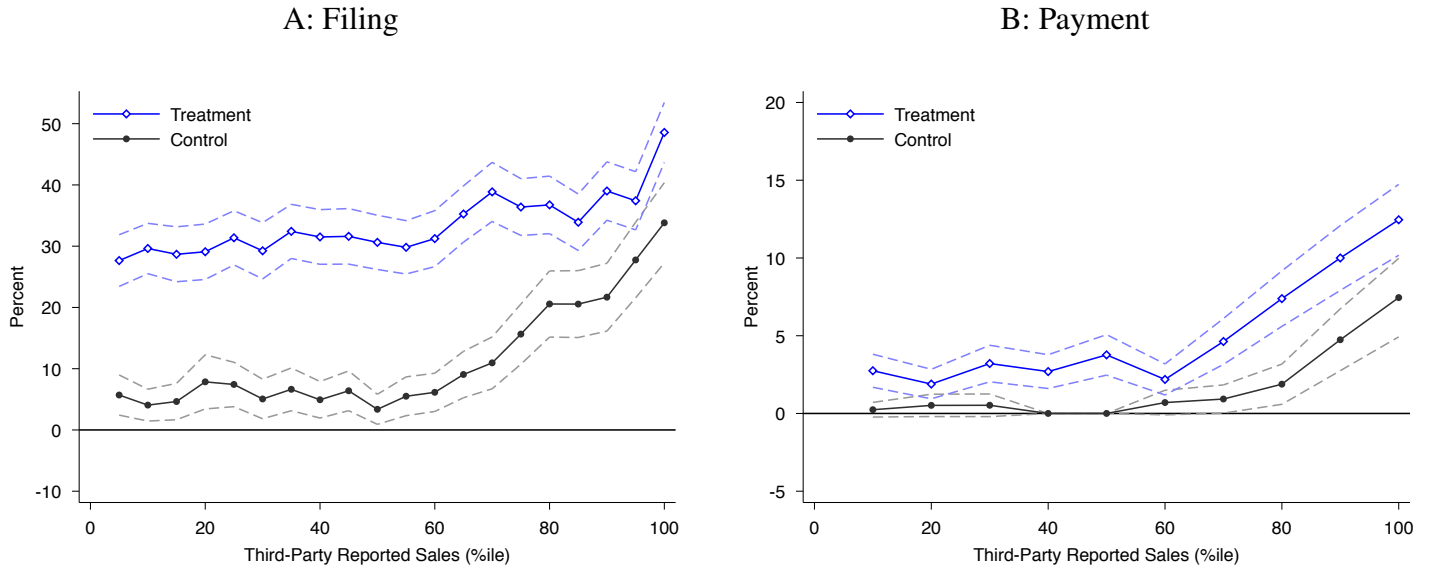
B: Payment

Note: This figure shows the share of non-filers that filed an income tax declaration (Panel A) and the share that made an income tax payment (Panel B) for 2014 at 15 weeks after the start of the experiment, for firms with third-party information (experiment 1). The shares are displayed by ventiles of third-party reported sales for Panel A and by deciles for Panel B. The blue solid line is for the pooled treatment group and the black solid line is for the control group, with dashed lines representing the 95% confidence intervals. For the 4th and 5th decile in the control group, not one observation made any payment, so the average payment rate is zero, and so is the standard deviation. Estimates are similar when calculated by bins of the maximum of self-reported sales in year $t-1$ (or the most recent year available) and third-party reported sales in t.

Figure 4: Response Mechanisms, Firms with Third-Party Information

A: Self-Reported vs. Third-Party Reported Sales

B: Self-Reported Costs vs. Self-Reported Sales
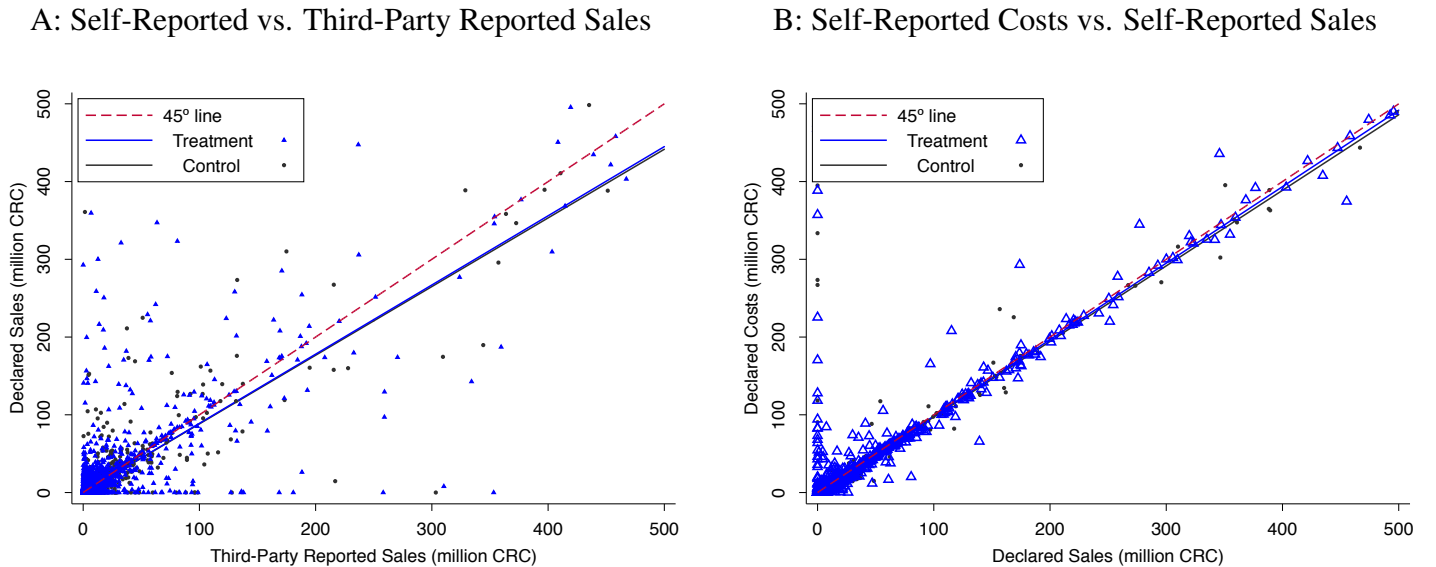
Note: This figure plots self-reported sales against third-party reported sales (Panel A) and self-reported costs against self-reported sales (Panel B). The panels are based on the income tax declarations for fiscal year 2014 filed by firms covered by third-party information. The blue dots are for the pooled treatment group and the black dots are for the control group. The corresponding solid lines represent a linear fit (based on a subsample that excludes observations outside the graph area) and the dashed red line corresponds to the 45° line. In Panel A, the control group slope is 0.88 and the treatment group slope is 0.89. In Panel B, the control group slope is 0.97 and the treatment group slope is 0.98. The slopes for the two groups are statistically indistinguishable in both panels.

Appendix

Figure A1: Email to Firms Covered by Third-Party Information

SUBJECT: Urgent: Please submit your tax return now

Attention: NAME,

PLEASE FILE YOUR INCOME TAX RETURN IN THE NEXT 10 DAYS

Declare now by visiting:
https://www.haciendadigital.go.cr/tribunet/loginDeclaraciones.jsp

According to our records, you have not filed your tax return (Form D101). 8 out of 10 Costa Ricans have already filed their 2014 income tax return. You are part of a small minority of citizens who have not.

It is a serious offense to not file your taxes.

T1: We have third-party information confirming that you or your client performed activities in 2014 which require you to pay taxes.

T2: We have third-party information confirming that you or your client performed activities in 2014 which require you to pay taxes. From third-party returns (D150, D151 and D153), we know about your operations, for example:
- **Revenues of at least XXX reported by COMPANY,**
- **Revenues from credit/debit cards of at least ZZZ reported by BANK,**
- **Sales or contracts with state entities of at least WWW.**

If you do not file, you could be audited and your business may be closed. Furthermore, your name may be published on our website as someone who did not contribute.

If you have any questions, you can contact me by PHONE or E-MAIL. I'll be checking whether you respond to this message.

Sincerely,

NAME OF OFFICIAL
POSITION

Note: Firms in the study received an email with either treatment 1 (T1) or treatment 2 (T2) shown.

Figure A2: Email to Firms Not Covered by Third-Party Information

SUBJECT: Urgent: Please submit your tax return now

Attention: NAME,

PLEASE FILE YOUR INCOME TAX RETURN IN THE NEXT 10 DAYS

Declare now by visiting:
https://www.haciendadigital.go.cr/tribunet/loginDeclaraciones.jsp

According to our records, you have not filed your tax return (Form D101). 8 out of 10 Costa Ricans have already filed their 2014 income tax return. You are part of a small minority of citizens who have not.

It is a serious offense to not file your taxes.

T1:

T2: The DGT uses information from third-party returns (D150, D151 and D153) to identify economic activity and sources of income.

If you do not file, you could be audited and your business may be closed. Furthermore, your name may be published on our website as someone who did not contribute.

If you have any questions, you can contact me by PHONE or E-MAIL. I will be checking whether you respond to this message.

Sincerely,

NAME OF OFFICIAL
POSITION

Note: Firms in the study received an email with either treatment 1 (T1) or treatment 2 (T2) shown.

Table A1: Balance of Randomization, Outcomes at Baseline

	Firms with Third-Party Information (Experiment 1)				Firms without Third-Party Information (Experiment 2)			
	(1)	(2) T1: Baseline email	(3) T2: Information email	(4) T1=T2=0 p-value	(5)	(6) T1: Baseline email	(7) T2: Information email	(8) T1=T2=0 p-value
	Control				Control			
Filed	0.057	0.004	0.002	0.742	0.013	0.001	0.000	0.934
	(0.004)	(0.005)	(0.005)		(0.001)	(0.001)	(0.002)	
Had positive net liability	0.017	0.003	0.002	0.513	0.001	0.001	0.000	0.089
	(0.002)	(0.003)	(0.003)		(0.000)	(0.000)	(0.000)	
Made payment	0.008	0.003	0.002	0.373	0.000	0.000	0.000	0.377
	(0.001)	(0.002)	(0.002)		(0.000)	(0.000)	(0.000)	
Log payment	0.086	0.034	0.018	0.348				
	(0.015)	(0.024)	(0.023)					
Was third-party informant	0.164	-0.003	-0.007	0.710	0.012	0.000	0.001	0.761
	(0.006)	(0.008)	(0.008)		(0.001)	(0.001)	(0.001)	
No. months decl. sales tax	0.619	-0.040	-0.061	0.501	0.243	-0.025	0.004	0.239
	(0.038)	(0.052)	(0.052)		(0.013)	(0.018)	(0.019)	
No. months paid sales tax	0.126	-0.023	-0.026	0.390	0.015	0.006	0.004	0.260
	(0.015)	(0.020)	(0.020)		(0.002)	(0.004)	(0.004)	
Sales tax payment	108900.860	-75006.413	-64056.208	0.489	736.743	1416.450	245.104	0.641
	(62816.239)	(64536.416)	(65884.055)		(207.981)	(1728.289)	(483.252)	
Deregistered	0.004	-0.001	0.000	0.615	0.004	-0.001	-0.001	0.182
	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	
Deregistered (sales tax)	0.001	-0.000	-0.000	0.876	0.001	-0.000	-0.000	0.258
	(0.000)	(0.001)	(0.001)		(0.000)	(0.000)	(0.000)	
Switched to simplified tax regime	0.000	-0.000	-0.000	0.317				
	(0.000)	(0.000)	(0.000)					
Observations	4,128	4,260	4,127		12,350	12,516	12,376	

Note: The table shows the balance of randomization in terms of outcomes, as measured before the experiment start. The rows correspond to the different variables for fiscal year 2014. The number of months that a taxpayer filed and paid sales tax, and the sales tax payment are calculated over July 2013 until June 2014. Columns 1-4 (5-8) correspond to the sample of firms with (without) third-party information, i.e., experiment 1 (2). Column 1 (5) displays the mean for the control group, columns 2 and 3 (5 and 7) show the mean difference between the control group and treatment groups 1 and 2 respectively, and column 4 (8) reports p-values from a test of the hypothesis that the two treatment groups are jointly equal to the control group. Robust standard errors clustered by email address are in parentheses.

	Probit			OLS	PPML
	(1) Filed	(2) Positive net liability	(3) Positive payment	(4) Log payment	(5) Payment
T1: Baseline email	0.213 (0.009)	0.046 (0.005)	0.031 (0.004)	0.340 (0.043)	8,167.736 (2,394.801)
T2: Information email	0.232 (0.009)	0.054 (0.005)	0.038 (0.004)	0.411 (0.044)	10,191.998 (2,712.887)
Corporation	-0.005 (0.015)	0.033 (0.008)	0.007 (0.006)	0.104 (0.099)	6,334.570 (3,392.022)
Has legal representative	-0.000 (0.008)	-0.002 (0.004)	0.002 (0.004)	0.042 (0.038)	3,364.378 (2,530.740)
Has second email	0.045 (0.021)	-0.000 (0.008)	0.007 (0.008)	0.169 (0.151)	2,259.939 (3,425.219)
Located in San José	0.009 (0.008)	-0.003 (0.004)	0.003 (0.003)	0.059 (0.039)	7,366.114 (2,202.358)
TPI > 2.5 million CRC	0.065 (0.011)	0.052 (0.006)	0.032 (0.005)	0.294 (0.056)	6,911.006 (2,639.431)
TPI > 6 million CRC	0.013 (0.014)	0.042 (0.008)	0.016 (0.006)	0.365 (0.093)	14,807.488 (3,628.608)
Made advance payment for 2014	0.122 (0.030)	0.051 (0.016)	0.049 (0.014)	1.292 (0.304)	6,384.847 (3,732.480)
Filed in 2013	0.105 (0.010)	0.004 (0.006)	0.005 (0.005)	0.003 (0.051)	-6,565.205 (5,190.648)
Reported net liability > 0 in 2013	-0.057 (0.019)	0.072 (0.015)	-0.008 (0.008)	-0.216 (0.126)	-9,026.498 (7,612.882)
Made payment in 2013	0.095 (0.029)	0.023 (0.012)	0.106 (0.026)	1.681 (0.219)	76,309.211 (31,830.381)
Was third-party informant in 2013	0.046 (0.012)	0.004 (0.006)	-0.007 (0.005)	-0.075 (0.075)	4,738.215 (2,804.348)
Constant				-0.143 (0.040)	
Control group avg.	0.115	0.0339	0.0172	0.194	5015.8
T1=T2 p-value (Wald test)	0.062	0.135	0.145	0.177	0.510
Observations	12,515	12,515	12,515	12,515	12,515

Note: This table displays estimates using probit, OLS and PPML as explained in Section 3.1, using the control variables noted in the table rows. The columns display the outcome variables: indicators for whether the firm filed income tax for 2014, reported a positive net liability and made a payment (considering only final payments made with the declaration and not advance payments that may have been made earlier), and the (log) payment amount. All outcomes are measured 15 weeks after the start of the experiment. Robust standard errors clustered by email address are in parentheses. Average partial effects are reported for probit and PPML. Payment amounts are winsorized at the top 0.1% to reduce the influence of outliers. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

Table A3: Impact on Income Tax Compliance, Firms without Third-Party Information

	Probit			OLS	PPML
	(1)	(2)	(3)	(4)	(5)
	Filed	Positive net liability	Positive payment	Log payment	Payment
T1: Baseline email	0.150	0.006	0.005	0.050	262.430
	(0.004)	(0.001)	(0.001)	(0.008)	(59.096)
T2: Information email	0.149	0.006	0.004	0.041	166.064
	(0.004)	(0.001)	(0.001)	(0.008)	(52.392)
Corporation	-0.013	0.002	0.002	0.017	177.133
	(0.005)	(0.001)	(0.001)	(0.010)	(73.143)
Has legal representative	-0.000	0.001	-0.000	0.003	103.848
	(0.004)	(0.001)	(0.001)	(0.007)	(49.538)
Has second email	0.027	0.001	0.002	0.033	98.140
	(0.009)	(0.002)	(0.002)	(0.025)	(105.699)
Located in San José	-0.002	-0.001	-0.001	-0.011	-60.903
	(0.004)	(0.001)	(0.001)	(0.007)	(50.039)
Made advance payment for 2014	0.075	0.014	0.003	0.153	151.766
	(0.028)	(0.007)	(0.004)	(0.156)	(171.565)
Filed in 2013	0.132	0.002	0.001	0.006	-3.763
	(0.006)	(0.001)	(0.001)	(0.013)	(92.633)
Reported net liability > 0 in 2013	-0.009	0.032	-0.003	-0.048	-315.752
	(0.020)	(0.012)	(0.004)	(0.078)	(426.717)
Made payment in 2013	0.031	0.006	0.086	0.801	13,505.357
	(0.028)	(0.005)	(0.082)	(0.176)	(21,428.248)
Was third-party informant in 2013	0.015	0.002	0.001	0.016	75.884
	(0.009)	(0.002)	(0.002)	(0.036)	(143.009)
Constant				0.001	
				(0.006)	
Control group avg.	0.0393	0.00178	0.00130	0.0142	96.15
T1=T2 p-value (Wald test)	0.807	0.617	0.613	0.397	0.160
Observations	37,242	37,242	37,242	37,242	37,242

Note: This table displays estimates using probit, OLS and PPML as explained in Section 3.1, using the control variables noted in the table rows. The notes to Table A2 apply.

Table A4: Impact on Declared Sales and Costs: Firms with Third-Party Information

	Probit	Probit	OLS	Probit	Probit	OLS	Probit	OLS
	(1) Positive sales	(2) Sales ≥ TPI	(3) Log sales	(4) Positive costs	(5) Costs ≥ TPI	(6) Log costs	(7) Positive profit	(8) Profit amount
T1: Baseline email	0.169	0.126	2.314	0.101	0.0318	1.307	0.154	179207.9
	(0.00915)	(0.00857)	(0.139)	(0.00810)	(0.0104)	(0.125)	(0.00893)	(33886.5)
T2: Information email	0.193	0.158	2.607	0.116	0.0341	1.501	0.175	175280.3
	(0.00929)	(0.00885)	(0.139)	(0.00827)	(0.0106)	(0.124)	(0.00909)	(33805.9)
TPI > 2.5 million CRC	0.183	0.0919	3.022	0.228	0.0935	3.241	0.172	662066.2
	(0.0118)	(0.0109)	(0.180)	(0.0112)	(0.0118)	(0.167)	(0.0115)	(37834.5)
TPI > 6 million CRC	0.0891	0.00642	2.262	0.104	-0.0160	3.010	0.0804	717352.7
	(0.0148)	(0.0129)	(0.253)	(0.0133)	(0.0157)	(0.247)	(0.0144)	(69226.6)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control group avg.	0.205	0.146	3.230	0.177	0.636	2.787	0.183	426815.1
T1=T2 p-value (Wald test)	0.025	0.001	0.056	0.082	0.821	0.154	0.037	0.912
Observations	12,515	12,515	12,515	12,515	12,515	12,515	12,515	12,515

Note: This table shows estimates using probit and OLS, with binary outcome variables as indicated by the statements in the column headings. Profits are winsorized at the first and 99th percentile to reduce the influence of outliers. All regressions are for the sample of firms with third-party information and include the same controls as in Table A2. Average partial effects are reported for probit estimations. Robust standard errors clustered by email address are in parentheses. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

SMS Experiment

In addition to the email experiment presented in this paper, the tax authority implemented a parallel experiment using SMS reminders. Like the email experiment, the SMS experiment targeted firms that registered for income taxes but did not file. Only firms that had a cell phone number but no email address on file were included in the SMS experiment ($N = 30,844$). Firms in the email experiments did not receive SMS messages. The SMS messages were sent between March 16–26, 2015—around the same time as the email messages. Among the SMS-eligible firms, 16% were covered by third-party information, which we control for in our analysis.

One third of SMS-eligible firms were assigned to a control group, which received no message. The other firms were divided between two treatment groups, each firm receiving one SMS message limited to 160 characters. Firms assigned to the first treatment condition received an SMS message informing them that if they do not file, their name may be published on a government website. This treatment combined public shaming, which has been shown to reduce tax delinquency (Perez-Truglia and Troiano 2016), with an implicit "call to action," specifying a consequence if the firm does not act:

Treatment 1 (T1):

```
You have not filed your 2014 income tax return.  If you
do not file, your name may be published on our website.
Questions XXXX-XXXX extXXX
```

The second treatment message stated that the tax authority uses third-party information to identify taxable income. This message did not ask firms to comply or

specify any consequence of non-compliance:

Treatment 2 (T2):

```
You have not filed your 2014 income tax return.  The
tax authority uses third-party information to identify
taxable income.  Questions XXXX-XXXX extXXX
```

When comparing the treatment effects of the two messages, one needs to keep in mind that the treatments differ along two dimensions (i.e., public shaming *plus* implicit call to action, versus mention of third-party information). For this reason, we emphasize the results of the two email experiments in the paper. While we compare the results of the SMS experiment to the email experiments, for completeness, it is important to keep in mind that the experiments differ in terms of the sample of firms targeted (sophisticated firms using email versus unsophisticated firms, or firms that have not updated their records recently), the message content, and the delivery mechanism, which has been shown to matter for compliance (Ortega and Scartascini 2015).

Table A5 in this Appendix displays the balance of randomization for the SMS experiment. Table A6 shows the balance of outcomes measured prior to the start of the experiments. These estimates are generated by regressing each row variable on a constant and two treatment indicators. Like the email experiment, the SMS experiment exhibits balance.

Table A7 displays estimates of the effect of the SMS messages on income tax compliance measured at 15 weeks after the start of the experiment. Like the emails, the SMS messages significantly increase the rate of income tax filing. However, the absolute and relative increase in the filing rate is smaller than the increase from

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an email. The SMS that threatens public shaming as a consequence of inaction increases the probability of filing more than the SMS that mentions the use of third-party information, and this difference is significant. Neither message results in an economically large or statistically significant increase in average tax payment, and the overall payment rate for this group (tax-registered firms that did not have an email address on file) is below 1%. Table A8 shows other effects of the SMS messages, which also increased prior-year filing and income tax deregistration, although the effects (which are less than 1 p.p.) are again smaller than the corresponding effects from the email experiment. The threat of public shaming increased deregistration significantly relative to the SMS that mentioned third-party information.

Table A5: Balance of Randomization, Firm Characteristics (SMS Experiment)

	(1) Control	(2) T1: Public shaming SMS	(3) T2: Information SMS	(4) T1=T2=0 p-value
Corporation	0.312	0.011	0.011	0.356
	(0.006)	(0.008)	(0.009)	
Has second phone	0.174	0.003	0.003	0.819
	(0.004)	(0.006)	(0.006)	
Located in San José	0.395	-0.004	0.003	0.618
	(0.006)	(0.008)	(0.009)	
Was contacted early	0.011	-0.001	0.005	0.509
	(0.001)	(0.001)	(0.006)	
TPI reportee	0.154	-0.001	0.003	0.697
	(0.004)	(0.005)	(0.005)	
TPI > 2.5 million CRC	0.071	-0.000	0.002	0.743
	(0.003)	(0.004)	(0.004)	
TPI > 6 million CRC	0.038	0.005	0.004	0.157
	(0.002)	(0.003)	(0.003)	
Made advance payment in 2014	0.007	-0.000	0.001	0.656
	(0.001)	(0.001)	(0.001)	
Filed in 2013	0.161	0.011	0.005	0.116
	(0.004)	(0.005)	(0.005)	
Reported net liability > 0 in 2013	0.032	-0.003	-0.000	0.486
	(0.002)	(0.002)	(0.002)	
Made payment 2013	0.018	-0.002	0.001	0.476
	(0.001)	(0.002)	(0.002)	
Was third-party informant 2013	0.056	0.003	0.003	0.586
	(0.002)	(0.003)	(0.003)	
Observations	10,227	10,195	10,420	

Note: The table shows the balance of randomization in terms of firm characteristics, as measured before the experiment start. The rows correspond to the different variables. Column 1 displays the mean for the control group, columns 2 and 3 show the mean difference between the control group and treatment groups 1 and 2 respectively, and column 4 reports p-values from a test of the hypothesis that the two treatment groups are jointly equal to the control group. Robust standard errors clustered by phone number are in parentheses. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

Table A6: Balance of Randomization, Outcomes at Baseline (SMS Experiment)

	(1) Control	(2) T1: Public shaming SMS	(3) T2: Information SMS	(4) T1=T2=0 p-value
Filed	0.013	-0.000	0.001	0.554
	(0.001)	(0.002)	(0.002)	
Had positive net liability	0.004	-0.000	0.000	0.650
	(0.001)	(0.001)	(0.001)	
Made payment	0.001	-0.000	-0.000	0.702
	(0.000)	(0.001)	(0.000)	
Log payment	0.014	-0.002	-0.004	0.691
	(0.004)	(0.005)	(0.005)	
Was third-party informant	0.027	-0.001	0.002	0.414
	(0.002)	(0.002)	(0.002)	
No. months decl. sales tax	0.269	0.037	0.004	0.224
	(0.016)	(0.023)	(0.023)	
No. months paid sales tax	0.039	0.004	0.005	0.781
	(0.006)	(0.008)	(0.008)	
Sales tax payment	1113.920	1126.130	1048.929	0.065
	(259.200)	(579.110)	(645.096)	
Deregistered	0.003	-0.001	-0.001	0.498
	(0.001)	(0.001)	(0.001)	
Deregistered (sales tax)	0.001	-0.001	-0.001	0.008
	(0.000)	(0.000)	(0.000)	
Observations	10,227	10,195	10,420	

Note: The table shows the balance of randomization in terms of outcomes, as measured before the experiment start. The rows correspond to the different variables for fiscal year 2014. The number of months that a taxpayer filed and paid sales tax, and the sales tax payment are calculated over July 2013 until June 2014. Column 1 displays the mean for the control group, columns 2 and 3 show the mean difference between the control group and treatment groups 1 and 2 respectively, and column 4 reports p-values from a test of the hypothesis that the two treatment groups are jointly equal to the control group. Robust standard errors clustered by phone number are in parentheses.

Table A7: SMS Impact on Income Tax Compliance

	Probit			OLS	PPML
	(1)	(2)	(3)	(4)	(5)
	Filed	Positive net liability	Positive payment	Log payment	Payment
T1: Public Shaming SMS	0.031	0.002	0.002	0.016	136.070
	(0.003)	(0.001)	(0.001)	(0.010)	(108.186)
T2: Information SMS	0.024	0.002	0.000	0.004	99.784
	(0.003)	(0.001)	(0.001)	(0.010)	(103.860)
Corporated	-0.018	0.001	-0.001	-0.003	100.539
	(0.003)	(0.001)	(0.001)	(0.010)	(114.204)
Has second phone	0.004	0.000	-0.000	-0.004	-45.322
	(0.004)	(0.001)	(0.001)	(0.012)	(111.178)
Located in San José	-0.011	-0.002	-0.000	-0.010	-203.961
	(0.003)	(0.001)	(0.001)	(0.009)	(84.752)
TPI reportee	0.029	0.008	0.005	0.045	472.924
	(0.005)	(0.002)	(0.002)	(0.019)	(158.965)
TPI > 2.5 million CRC	0.030	0.010	0.004	0.123	425.925
	(0.009)	(0.003)	(0.002)	(0.054)	(204.018)
TPI > 6 million CRC	0.006	0.006	0.001	0.172	238.749
	(0.007)	(0.002)	(0.001)	(0.081)	(154.156)
Made advance payment for 2014	0.061	0.012	0.011	0.831	533.082
	(0.019)	(0.005)	(0.005)	(0.245)	(271.232)
Filed in 2013	0.084	0.005	0.004	0.027	251.788
	(0.006)	(0.002)	(0.001)	(0.018)	(157.196)
Reported net liability > 0 in 2013	0.004	0.021	0.003	0.083	211.891
	(0.009)	(0.006)	(0.003)	(0.086)	(243.252)
Made payment in 2013	0.018	0.002	0.013	0.711	932.340
	(0.013)	(0.003)	(0.006)	(0.161)	(452.038)
Was third-party informant in 2013	0.003	0.000	-0.001	-0.021	-74.626
	(0.005)	(0.002)	(0.001)	(0.044)	(123.444)
Constant				0.003	
				(0.007)	
Control group avg.	0.0363	0.00743	0.00411	0.0435	339.8
T1=T2 p-value (Wald test)	0.036	0.745	0.155	0.277	0.747
Observations	30,842	30,842	30,842	30,842	30,842

Note: This table displays estimates using probit, OLS and PPML as explained in Section 3.1, using the control variables noted in the table rows. The columns display the outcome variables: indicators for whether the firm filed income tax for 2014, reported a positive net liability and made a payment (considering only final payments made with the declaration and not advance payments that may have been made earlier), and the (log) payment amount. All outcomes are measured 15 weeks after the start of the experiment. Robust standard errors clustered by phone number are in parentheses. Average partial effects are reported for probit and PPML. Payment amounts are winsorized at the top 0.1% to reduce the influence of outliers. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.

Table A8: SMS Impact on Other Compliance Outcomes

	Probit			OLS		PPML		Probit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Presented informative declaration	Reported supplier	Reported client	No. months decl. sales tax	No. months paid sales tax	Sales tax payment	Deregistered	Deregistered (sales tax)	Switched to simplified regime	Filed income tax for 2013	Paid income tax for 2013
T1: Public shaming SMS	0.001	0.000	0.001	0.027	0.003	426.698	0.007	-0.000	-0.000	0.010	0.001
	(0.002)	(0.002)	(0.002)	(0.023)	(0.008)	(280.415)	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)
T2: Information SMS	0.002	0.002	0.000	-0.006	0.002	176.151	0.003	-0.001	-0.001	0.007	0.000
	(0.002)	(0.002)	(0.002)	(0.022)	(0.008)	(258.053)	(0.002)	(0.001)	(0.000)	(0.002)	(0.000)
TPI reportee	0.049	0.022	0.031	-0.005	0.031	1,370.309	-0.002	-0.003	0.000	0.007	0.001
	(0.004)	(0.003)	(0.003)	(0.038)	(0.017)	(457.876)	(0.002)	(0.001)	(0.000)	(0.003)	(0.001)
TPI > 2.5 million CRC	0.014	-0.003	0.018	0.168	0.072	400.869	-0.005	0.012	0.001	0.021	0.001
	(0.004)	(0.003)	(0.003)	(0.090)	(0.044)	(431.705)	(0.004)	(0.012)	(0.001)	(0.008)	(0.001)
TPI > 6 million CRC	0.015	0.016	0.011	0.129	-0.046	183.805	-0.002	-0.002	-0.000	0.000	0.002
	(0.004)	(0.005)	(0.003)	(0.121)	(0.057)	(372.037)	(0.005)	(0.001)	(0.000)	(0.005)	(0.002)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control group avg.	0.0286	0.0155	0.0182	0.276	0.0416	935.9	0.0104	0.00284	0.000587	0.175	0.0184
T1=T2 p-value (Wald test)	0.575	0.382	0.684	0.153	0.892	0.399	0.040	0.271	0.076	0.124	0.238
Observations	30,842	30,842	30,842	30,842	30,842	30,842	30,842	30,842	30,842	30,842	30,842

Note: This table displays estimates using probit, OLS and PPML and the same controls as in Table A7. The columns display the outcome variables, which refer to compliance for fiscal year 2014 unless otherwise noted. All outcomes are measured 15 weeks after the start of the experiment. Robust standard errors clustered by phone number are in parentheses. Average partial effects are reported for probit and PPML. TPI stands for third-party information (third-party reported sales), meaning the sum of sales reported by clients (D151), state institutions (D150) and credit/debit card companies (D153). The cutoffs of 2.5 million and 6 million CRC correspond to the priority group designations used by the tax authority.