Casting a Wider Tax Net: Experimental Evidence from Costa Rica

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Abstract

The majority of firms in developing countries are informal, yet even among registered firms, tax filing rates are low. We argue that nonfiling 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 increased the tax payment rate (amount) by 3.4 p.p. (US$ 15) among previously nonfiling firms. Emails that highlight third-party reports of a firm's transactions further increased compliance. The effect persisted over two years, and treated firms became more likely to report transactions with other firms, facilitating future tax enforcement.

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

JEL codes: H26, H32, O17.

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Low-income countries are characterized by low tax compliance (Gordon and Li 2009; Besley and Persson 2013), one key aspect of which is nonfiling. As Figure 1 shows, tax nonfiling rates are above 25 percent in half of the countries for which data is available, and are significantly higher in lower-income countries, reaching above 90 percent in some countries. This relationship holds across four major taxes. As a reflection of the importance of tax nonfiling, the International Monetary Fund's and World Bank's tax projects usually include activities to increase filing and monitor it as a key performance indicator (IMF 2015, Junquera-Varela et al. 2017). In Costa Rica, 25 percent of tax-registered firms and over 60 percent of firms that are unregistered but known to the tax authority through third-party reports do not file their income tax declaration.

Firms that do not file taxes generate several important negative externalities. First, nonfiling leads to a loss of government revenue. We estimate that the tax authority in Costa Rica loses hundreds of thousands of dollars annually due to income tax nonfiling. Second, selective nonfiling generates horizontal inequities between firms, which can distort resource allocation (Hsieh and Klenow 2009; Restuccia and Rogerson 2008) and affect tax morale (Luttmer and Singhal 2014). Third, firms that do not file deprive tax authorities of third-party information on other firms' activities, which reduces the government's ability to enforce taxes and to design economic policy more generally. Yet, despite its empirical importance, nonfiling has received little attention in the literature, which has focused on tax registration of fully informal firms[1] (e.g., De Andrade, Bruhn and McKenzie 2014) and misreporting among tax filers (e.g., Carrillo, Pomeranz and Singhal 2016).

This paper argues that simple and highly cost-effective policies can increase tax-filing compliance. We evaluate a nationwide randomized trial conducted by

[1] We use the term "firm" to refer to both incorporated and unincorporated firms, the latter group encompassing small businesses or self-employed professionals.

Costa Rican tax authority, which contacted 33,279 nonfiling firms by email and requested that they submit their income tax declaration for 2014. In addition to drawing on behavioral insights (BIT 2014), the messages contained various threats to deter non-compliers, mentioning the possibility of an audit, shop closure, or online publication of a list of nonfilers (similar to Slemrod, Blumenthal and Christian (2001), Kleven et al. (2011), Pomeranz (2015) and Perez-Truglia and Troiano (2016)). These deterrents are strong but credible in our context; as discussed below, the tax authority implemented a personalized follow-up with phone calls and visits to a set of taxpayers that did not respond to the treatment.

The messages also leveraged third-party reports about firms' business activities, where available, including information that the tax authority collects through informative declarations presented by other firms, state institutions, and credit card companies. Within this subset of firms, the emails either highlighted the existence of third-party information about the taxpayer in question 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. These examples make the presence of third-party reports credible and salient without communicating total amounts, which could generate perverse responses (Carrillo, Pomeranz and Singhal 2016).

Using administrative data on filing and tax payments, we present four sets of results. First, the emails sent to nonfilers tripled their rate of income tax filing and more than doubled their rate and amount of payment, relative to a control group that received no message. Among firms with third-party information, these effects correspond to a 21 percentage point (p.p.) increase in the filing rate, a 3.4 p.p. increase in the payment rate, and a US$ 15 increase in the payment amount. Listing specific examples of third-party reports about the firm had an additional positive effect on all outcomes. The payment return on the strongest email was US$ 18.[2]

[2]Figures are calculated using an exchange rate of 574 Costa Rican *colones* (CRC) per U.S. dollar.

Second, we show that the intervention had largely positive effects on other dimensions of compliance. We find no reduction in sales tax filing rates or payments, and only a small increase in the deregistration rate (by 1-2 p.p.). Instead, we find that the intervention increased the rate of income tax filing and payment, both for the fiscal years prior to the intervention and in the two years following the intervention. For firms with third-party information, the effect persisted even for three years. In addition, the intervention induced firms to file 800 additional informative declarations, an effect that also persists in the year following the intervention. These new informative declarations improve future tax enforcement, as about 30 percent of them cover nonfilers or firms underreporting their sales.

Third, we study the mechanisms driving the increase in tax compliance. While part of this effect is driven by taxpayers updating their beliefs about enforcement, as evidenced by the medium-term persistence of the effects, part is also driven by a salience or reminder mechanism, which can be used repeatedly. Firms in our experiment, regardless of their treatment group and 2014 filing status, respond to repeated enforcement messages sent to nonfilers in 2016 and 2017. We observe a trend break in filing rates at the exact calendar date on which messages are sent, even among firms contacted for the fourth or fifth time. Together, the medium-term and dynamic effects suggest that frequently contacting non-compliant firms can be a sustainable policy.

Finally, we show that the treatment leads to an improvement in reporting behavior conditional on filing among the self-employed, a group with notoriously low tax compliance rates in almost all settings. Self-employed individuals contacted by the authorities report higher sales and tax liabilities and make higher tax payments, as compared with self-employed filers who were not contacted. Bunching evidence suggests that this is due to the mentioning of third-party information and a deduction rule which discourages offsetting an increase in self-reported sales with

an increase in self-reported costs, showing that a clever policy design can avert evasion shifting.

We calculate an overall cost-benefit ratio of 1:4, which leads us to conclude that the email intervention is a cost-effective way to "cast a wider tax net": by increasing compliance in current, future, and even past periods, and by broadening the tax authority's information set for future tax enforcement. We discuss under what conditions the intervention is welfare-improving in the presence of filing costs, and review alternative policies that would avoid filing costs.

This paper contributes to three strands of literature. First, this experiment adds to the literature on taxpayer communication designed to increase tax compliance, including Dwenger et al. (2016), Hallsworth et al. (2015), and other studies reviewed in Hallsworth (2014). Most importantly, 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 and on filing of informative declarations, a persistence of the main treatment effects in the medium term, and continued responsiveness to repeated interventions. We use a cost effective delivery method—emails[3]—and maximize message impact by combining different message elements that proved successful in other contexts, generating large effects compared with most other studies. In addition, our study is one of few in this literature to focus on firms as opposed to individuals , and the first (with Kettle et al. (2016)) to focus on tax filing.[4] Indeed, most studies in this literature have focused on correct reporting of liabilities or on the payment of already assessed liabilities.

Second, we contribute to the literature on taxation and development, as sur-

[3] See Ortega and Scartascini (2015) on the larger impact of emails compared to letters.

[4] Kettle et al. (2016) find much smaller treatment effects than this study. Hallsworth (2014) refers to three other papers that considered filing behavior, but all of them focused on individual taxpayers.

veyed in Besley and Persson (2013), which analyzes how tax capacity grows along the development path. While most recent contributions to this literature focus on misreporting on the intensive margin (Best et al. 2015; Pomeranz 2015; Naritomi 2015, Carrillo, Pomeranz and Singhal 2016), our study highlights the importance of compliance gaps on the extensive margin. The role of third-party information in enhancing tax compliance features prominently in the literature (Kleven et al. 2011; Kleven, Kreiner and Saez 2016), and this information enhances tax compliance on the intensive margin for the value-added tax (Pomeranz 2015; Naritomi 2015), but results for the corporate income tax are less clear-cut. 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, but Brockmeyer and Hernandez (2018) document sharp increases in reported tax liabilities when firms in Costa Rica become subject to third-party reporting for the first time.[5] 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 simultaneously a tool to identify nonfiling firms, a tool to strengthen enforcement interventions, and a by-product of better filing compliance.

Finally, this paper contributes to the literature on firm formalization, as reviewed in Bruhn and McKenzie (2014) and De Andrade, Bruhn and McKenzie (2014). Previous studies find that providing information, reducing registration costs, or simplifying regulation is relatively ineffective in encouraging formalization. Only enforcement or large payments (e.g., one month of profits) 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

[5] Brockmeyer and Hernandez (2018) also study the impact of tax withholding on business sales, finding that a doubling of the withholding rate increased sales tax payment by treated firms by 39 percent.

wider tax net. As nonfilers are firms that chose to register for taxes, their perceived benefit from tax filing likely exceeds that of fully informal firms that chose not to register. In addition, the tax authority already has these firms' contact information, and (in some cases) also third-party reports about business activities that can be leveraged to estimate outstanding tax liabilities.

The rest of this paper is organized as follows. Section I describes the context and data; section II presents the experimental design; sections III, IV, and V present the baseline results, medium-term results and mechanisms, and a cost-benefit analysis; and section VI concludes.

I Context and Data

A Income and Sales Tax

Total tax revenues in Costa Rica represented 15.3 percent of GDP in 2016, of which 34 percent were due to the sales tax and 34 percent to the income tax (CR Ministry of Finance 2016). The income tax is levied according to different schedules for corporations and unincorporated firms, i.e., self-employed individuals. Corporations face an average tax rate on profits of 10 percent, 20 percent, or 30 percent, depending on their revenue level. The self-employed face a marginal tax rate on profits between 0 percent and 25 percent, 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. All firms in our sample are required to file their income tax declaration by December 15.[6]

[6]The fiscal year is October 1 to September 30. Firms have to make three quarterly advance tax payments for the 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 advance payments.

Firms selling manufactured goods and certain service providers, such as hotels and restaurants, are liable for the monthly sales tax. This tax is effectively a narrowly-based VAT, providing credits for taxed inputs. Approximately 20 percent of income tax-liable firms are liable for the sales tax.

Instead of paying the regular income tax and sales tax, retailers in certain sectors and below certain size thresholds[7] can opt into a simplified regime, in which firms file quarterly and pay tax on inputs at sector-specific rates that vary from 3 percent to 9.8 percent. 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 switch form, or deregister by submitting a D141 deregistration form.

Figure 1 shows the share of tax-registered firms that did not file a tax declaration in Costa Rica in 2014 (square markers). For the income tax, this share was 25 percent of corporations and 19 percent of the self-employed.[8] It was lower but still substantial at 17 percent of firms liable for the sales tax. 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 some inactive firms. However, even among firms covered by third-party information, i.e., shown to be economically active, 12 percent did not file income tax for 2014. Appendix Tables A1 and A2 compare the characteristics and compliance behavior of filers and nonfilers. While nonfilers are smaller than the average filer, they are actually larger than filers with below-median sales, and most nonfilers have filed income tax in the past.

[7] In particular, those with annual purchases below 150 base salaries, net assets below 350 base salaries, or with fewer than six employees.

[8] The corresponding figures in Brockmeyer and Hernandez (2018) are comparable, albeit lower, as they use an algorithm based on tax declarations and registration records to construct the tax register for different years but do not directly observe the tax register. The figures in this paper are the Costa Rican tax authority's own estimates based on all available data.

Thus, encouraging the compliance of this valuable segment of taxpayers appears worthwhile from a revenue and horizontal equity perspective.

B Third-Party Information

To enforce taxes, the Costa Rican tax authority makes use of third-party information from firms, state institutions, and credit/debit card companies. 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).[9]

Firms have to report transactions with clients and suppliers (form D151) if the total annual amount of transactions with a single transaction partner reaches CRC 2.5 million (US$ 4,400). 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 (US$ 90). These transactions must be reported by both the seller and the buyer.

State institutions report all purchases from private firms (form D150). They also withhold tax at a rate of 2 percent of the transaction amount, which is remitted to the tax authority as an advance payment on the income tax. Credit or debit card companies report all card sales by affiliated businesses (form D153) and withhold tax at a firm-specific rate between 0 percent and 6 percent on card sales. The withheld amount is remitted to the tax authority as advance payment on the sales tax.[10]

State institutions and card companies are considered to be highly compliant with their reporting obligation, while firms are only partially compliant with their

[9] 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.

[10] 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. See Brockmeyer and Hernandez (2018) for further details on the withholding schemes.

obligation to report transactions. The tax authority uses all informative declarations and customs information to cross-check income tax returns. nonfilers are thus identified and taxpayers with strong discrepancies between third-party information and self-assessed tax declarations are selected for (desk) audits.

C Data

We use income and sales tax returns of corporations and the self-employed in Costa Rica during 2010-2016. 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, estimate the effect of the experiment on filing for fiscal year 2014, and estimate the medium-term impact on filing in 2015 and 2016. The data includes all line items from the tax declarations and tax 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 card companies. About 72 percent of firms that filed an income tax return in 2014 were reported by at least one client or supplier, 9 percent were reported by a card company, and 6 percent by a state institution.

II Experiment Design

Our study relies on a randomized communication experiment implemented by the tax authority in Costa Rica. Appendix Table A3 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 nonfilers, the experiment targeted 49,757 firms that had an email address on file.[11] The experiment was

[11] While our results do not have full external validity for nonfilers without an email address on file, they are nonetheless relevant for this sample. Firms without email addresses were only slightly

divided into two sub-experiments, targeting firms that were covered by at least one third-party report ($N = 12,515$), i.e., definitely economically active, and those that were not covered by any third-party report ($N = 37,242$).

A Treatments

Firms covered by third-party reports were randomly assigned to one of three groups: a control group and two treatment groups. The control group received no email message. The treatment groups received an email from the tax authority (shown in Appendix Figure A2) requesting them to file an income tax declaration for 2014. The message content was similar to past communication campaigns. It emphasized that nonfiling is a serious offense, that offenders could be audited or subject to business closure as stipulated by law, and that the names of nonfilers might be published online. Additionally, the message integrated findings from behavioral economics to strengthen the message impact.[12] The content of the message was fact-based and credible, given the enforcement context discussed below.

As the tax authority routinely uses third-party information in its enforcement activities and mentions this to taxpayers, the baseline treatment message (T1) also stated that "We have third-party information confirming that you or your client performed activities in 2014 which require you to pay taxes." The information treatment (T2) further strengthened this statement by providing firms with specific examples of third-party reports:

less likely to have filed in 2013 or to file in 2014, compared to our experimental control group, and exhibit no differences in reporting or payment behavior conditional on filing.

[12]The message used simplified text (BIT 2014; Dwenger et al. 2016), featured a clear call to action in red capital letters—"Please file your income tax return in the next 10 days" (Gabaix and Laibson 2005), provided a web link to the tax form (BIT 2014), and was personalized (Haynes et al. 2013). The message also presented the social norm—"8 out of 10 [tax-registered] Costa Ricans have filed their 2014 income tax return" (Wenzel 2005; Hallsworth et al. 2015; Del Carpio 2014).

Information Treatment (T2), firms with third-party information:

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), we listed the transaction amount and trading partner from the largest reported transaction. This 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 firms with small third-party reports without giving away how much or how little the tax authority knows. Finally, the design of the treatment allows us to test whether firms over- or underestimate the amount of information that the tax authority holds about them.

Firms without third-party information were also assigned randomly to either a no-message control group or one of two treatment groups. Treated firms received an email that was nearly identical to the email in the first sub-experiment (Appendix Figure A3). The only difference is the paragraph on third-party information, which was either omitted (T1), or replaced by a weaker but true statement (T2):

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

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.

B Timing and Follow-Up

Emails were sent to firms with third-party information starting March 4, 2015, and to firms without third-party information starting April 7, 2015. The list of nonfilers was extracted on February 15, 2015, and it was not possible to update it afterwards. Some firms in the experiment sample had thus already filed by the time the emails were sent, which allows us to demonstrate parallel filing rate trends in the treatment and control groups prior to the experiment.

To ensure the credibility of the enforcement threat in our intervention, the tax authority subjected a significant share of the taxpayers that did not file by July 15, 2015, to follow-up activities. These started with a phone call by a tax officer and proceeded (in the case of continued non-compliance) according to a specified sequence of steps that could lead to an audit. Cases were assigned to individual tax officers and dealt with between July 2015 and July 2016. Unlike the experimental emails, these follow-up activities were not randomly assigned, but instead targeted remaining nonfilers (and thus disproportionately the control group) and large firms with third-party information: 15 percent of remaining nonfilers with third-party information were contacted, 50 percent of which were in the control group (which represents only 33 percent of the sample).[13] The vast majority of firms subject to follow-up took action upon contact by the tax officer. Among the remaining non-compliers, at least 20 firms were subjected to an official enforcement procedure, five were temporarily closed, and four were audited.

The threat of online publication was also credible, as the authorities published

[13] Among remaining nonfilers without third-party information, 85 were contacted.

the names of 6,788 sales tax nonfilers in August 2015, 266 of whom were also income tax nonfilers and part of our experiment; and published the names of 13,025 income tax nonfilers for the period 2011-2015 on March 10, 2016, 1,509 of whom were in our experiment.[14]

The claim that third-party information is used in enforcement was supported in 2015 by (i) desk audits targeted at filers exhibiting discrepancies between self-reported and third-party reported sales, 130 of which targeted firms in our experiment sample, and (ii) desk audits for firms with a net liability of zero, 1,289 of which targeted firms in our sample.

C Enforcement Context

The credibility of our intervention is further supported by the general enforcement environment, in particular, the fact that the tax authority conducts a variety of enforcement interventions between the extremes of an audit and a simple email. Examples of these interventions for Costa Rica during 2008-2015 are listed in Appendix Table A4. Every year, the tax authority conducts approximately 700 audits, targeting taxpayers with large estimated tax liabilities or suspicious behavior, including nonfilers (less than 10 percent of audits are for large taxpayers), and forces several hundred temporary firm closures, about one-third of which target nonfilers. The threat of an audit or shop closure is routinely used in communications with taxpayers. Although the likelihood of an audit or shop closure for any particular firm is low, the monetary and reputational costs can be substantial.[15]

Less invasive enforcement activities are usually conducted on a larger scale and

[14] We examine the impact of online publication in footnote 44 and Appendix Figure A14.

[15] Focus group discussions suggest that audits generate reputational costs to a firm if the firm's neighbors, suppliers, or clients learn about the audit and interpret it as a sign of illegal behavior. The likelihood of this is larger for shop closures, which are advertised on a shop's entrance through an irremovable sign posted by the tax authority. There is abundant empirical evidence on the impact of alleged misconduct (and publication thereof) on firm performance via a reputational channel (Hanlon and Slemrod 2009; Karpoff and John R. Lott 1993; Bo, Slemrod and Thoresen 2015).

can cover several thousand taxpayers per year. These activities include desk audits, invitations of taxpayers for interviews in the tax office, controls of the provision of receipts, follow-up on complaints about taxpayers, and phone calls and messaging campaigns to reduce nonfiling for the income tax, sales tax, and informative declarations. The list of taxpayers in arrears is usually published online. There is thus a continuum of enforcement activities, and the specific type of activities and criteria for taxpayer selection change from year to year in a way that is not predictable for taxpayers. While interventions usually target particular compliance dimensions (e.g., nonfiling for a specific tax and fiscal period), they are also used to verify other compliance margins and can thus be viewed as general enforcement interventions.

Consistent with this, in a 2014 tax compliance cost survey (UN 2014), 9 percent of taxpayers in Costa Rica reported having been subject to some kind of control activity by the tax authority in the previous 12 months.[16] Importantly, this is not specific to Costa Rica. Other tax administrations in Latin America conduct a similar array of enforcement activities, and in a sample of 18 lower-income countries around the world, 28 percent of taxpayers on average reported having been subject to some kind of tax control activity in the last year.[17] We thus believe that our experimental treatment accurately represents the general enforcement probability, which is the structural parameter underpinning the treatment effect. By providing several examples of enforcement activities without stating precise probabilities, the email conveys the idea that the tax authority conducts an array of enforcement activities and that the likelihood of being subject to one of these is non-negligible for

[16] Of these taxpayers, 19 percent took more than two hours to comply with the control activity, suggesting that these activities often represent more than just a phone call.

[17] The minimum, maximum and median values are 4.4 percent, 60.0 percent and 27.5 percent, respectively. We focussed on small firms where possible and note that the mean was even higher among informal firms. The data is from tax compliance cost surveys conducted by the World Bank in Albania, Armenia, Bangladesh, Bosnia and Herzegovina, Colombia, Ethiopia, Georgia, India (Rajasthan), Kenya, Kyrgyz Republic, Nepal, Peru, Tajikistan, Uganda, Ukraine, Uzbekistan and Yemen (Coolidge 2012).

a taxpayer.

D Balance of Randomization

Given the random assignment of treatment status, the treatment and control groups do not significantly differ in terms of their characteristics and compliance outcomes measured at baseline (Appendix Tables A5 and A6). When jointly testing the significance of the difference between the three experiment groups, we reject a significant difference between the groups at the 10 percent level for 20 out of 22 firm characteristics and for 18 out of 20 compliance outcomes.

Among firms with (without) third-party information, 14 percent (26 percent) are corporations, 61 percent (66 percent) use a legal representative, and about 50 percent are located in the capital city. The larger share of self-employed among the nonfilers is consistent with the general view that the self-employed are particularly prone to tax evasion. As for compliance in the pre-experiment year, 33 percent (17 percent) filed income tax, 5 percent (1 percent) made a payment, and 19 percent (5 percent) filed a third-party report. These firms are thus truly at the margin of compliance. Regarding compliance at baseline, we find that 6 percent (1 percent) filed an income tax declaration and 1 percent (0.04 percent) made a payment after the extraction of the nonfiler list and before the start of the experiment. As would be expected given the short window between the extraction of the nonfilers list and the experiment start date, hardly any firms deregistered.

E Estimation

In all main specifications reported in this paper, we use ordinary least squares (OLS) to estimate the treatment effect on outcome y_i for firm i as

$$y_i = \alpha + \beta_1 \cdot T1_i + \beta_2 \cdot T2_i + \gamma X_i + \varepsilon_i, \tag{1}$$

where $T1_i$ and $T2_i$ indicate treatment 1 and 2, respectively, for firm i; X_i is a vector of covariates; and ε_i is an error term.[18] Outcomes are measured at 15 weeks after the experimental interventions, unless stated otherwise. 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. To increase power, we control for a set of firm characteristics measured prior to the experiment.[19]

III Baseline Results

This section presents the baseline results of our empirical analysis, considering the treatment impact on income tax compliance and on other compliance outcomes, as well as heterogeneity in the treatment effect.

A 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 remained approximately constant during the next ten weeks. This confirms that the emails generated additional tax payments rather than just bring forward payments that firms would have made anyway.[20] By 15 weeks after the start of the

[18] We compute (Huber-White) standard errors that are robust to within-cluster correlation, as randomization was conducted by clusters of the primary email address. The results are also robust to conducting the estimation using the sample of one-firm clusters only.

[19] Appendix Tables A7 to A12 report the control coefficients in our main specifications, the results from regressions without controls, as well as probit estimations and Poisson pseudo-maximum likelihood (PPML) estimations for robustness.

[20] The absence of a trend break in the control group suggests treatment spillovers were minor or absent.

intervention, the filing rate for information-covered firms reached 32.5 percent for those sent the baseline email, and 34.2 percent for those sent the email with examples of third-party information, relative to 11.5 percent for the control group.[21] The difference between the two treatment effects is statistically significant at the 10 percent level. The payment rate was 4.7 percent for those sent the baseline email, 5.3 percent for those sent the email with examples of third-party information, and 1.7 percent 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 thus did not enhance compliance among firms not covered by an information trail. This suggests that either most of these firms did not expect the tax authority to possess any third-party reports about them, or their response to the potential use of third-party information is small compared to the large response to the message itself. By 15 weeks after the intervention, the proportion of treated firms filing an income tax declaration reached 19 percent, relative to 3.9 percent for the control group. The rate of payment was below 1 percent, as most firms without third-party information declare a net liability of zero, but the payment rate was still significantly higher for the treatment group.[22]

To consider a larger number of outcomes and control for covariates, we report OLS estimates of the treatment effect at 15 weeks in Table 1. Panel A presents estimates for firms with third-party information. The first three columns report extensive-margin responses in income tax filing, in reporting a positive net liability,

[21] The fact that over 60 percent of the treatment group did not file in response to the email 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.

[22] In panels B2 and B3, it appears that the treatment and control groups are not equal in the pre-period, but this is a result of the small scale of the vertical axis. There are no statistically significant differences between the groups prior to the intervention (see Appendix Table A6).

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.).[23] The fourth and fifth columns show that the emails also increased the average payment amount, which is partly driven by the greater number of payers, and partly by larger payments conditional on making a payment.[24] The baseline email increased income tax payments by CRC 8,353 (US$ 15) per taxpayer, on average, while the information email increased payments by CRC 10,441 (US$ 18).[25] 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.[26]

Panel B shows the analogous results for firms without third-party information. 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.). The treatment increased the payment amount by CRC 217 on average (US$ 0.38).[27]

In sum, we find that the emails significantly improved income tax compliance

[23] 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 underpay or pay with delay.

[24] The dependent variable in columns 4 and 5 is 0 for firms who do not file and for filers who do not make any payment. We consider only payments made by the taxpayer with her annual declaration, and ignore the quarterly advance payments during the year. If a nonfiler has outstanding advance payments, those would be made with the annual declaration, and are thus considered in our estimation.

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

[26] Filing is measured accurately if a filer's tax declaration appears in our database, while line items on the declaration are measured accurately only if the declaration appears in our database and if the taxpayer made no mistakes when filling it out.

[27] Appendix Table A19 shows that the treatment effects for all payment outcomes are significantly higher when restricting the sample to firms that are more likely to be required to make a tax payment (corporations and firms with a positive tax liability and a positive income tax payment in 2013).

by nonfilers. 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, Sausgruber and Traxler 2013; Kettle et al. 2016),[28] 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.

Appendix Figure A4 examines the impact of follow-up activities, which were non-randomly targeted at remaining nonfilers starting in July 2015. These activities, combined with the continued gradual increase in the filing rate between July and December 2015, lead the filing rate among firms with third-party information to reach over 40 percent in December 2015, as shown in Panel A. Panel B provides evidence of a clear trend break after July 15 in the filing rate of the targeted priority group firms, with no trend break occurring among non-targeted firms. Consistent with the personalized and sequential nature of this campaign, the trend break is less sharp than in the baseline intervention. Importantly, the follow-up targeted the control group disproportionately, and thus lead to a slight narrowing of the filing gap between the treatment and control group, as evidenced in Panel C.

B Other Compliance Outcomes

As the treatment had a strong impact on the targeted compliance outcomes, notably income tax filing and payment, it is appropriate to consider spillovers to other compliance outcomes. Indeed, taxpayers might perceive the intervention as a general increase in enforcement, and hence improve compliance with other tax filing obligations. Alternatively, they might perceive the intervention as pertaining to a

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

specific tax only, and increase compliance with the targeted tax but compensate for lost income by reducing compliance elsewhere. 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 nonfilers for 2014), as a non-negligible share of firms file or pay their taxes with substantial delay (documented in Brockmeyer and Hernandez (2018)). This is to our knowledge the first study to examine the impact of enforcement on such a large variety of compliance measures.

Tables 2 and 3 show the treatment impact on the above-mentioned outcomes, for firms with and without third-party information in Panels A and B respectively. Contrary to the possibility of a crowd-out effect, columns 1-3 in Table 2 find no evidence that the treatment has an impact on compliance with the sales tax, the most important tax paid by firms. The treatment affects neither the number of months in which a firm files sales tax, nor the number of months it pays sales tax, nor the total sales tax amount. We also confirm that there is no evidence of a crowd-out effect in the sample of firms liable for the sales tax (i.e., having filed sales tax at some point prior to our experiment, Appendix Table A20).

Columns 5-6 in Table 2 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[29]), 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 percent to about

[29]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.

2 percent, and large amounts of third-party information were reassuringly negatively correlated with deregistration. For firms not covered by third-party information, treatment increased the deregistration rate from 1.2 percent to about 3 percent. These firms were also marginally more likely to deregister only from the sales tax or switch to the simplified tax regime, but this concerns less than 0.5 percent of the sample. The smaller effect on information-covered firms is consistent with these firms' reported economic activity in the fiscal year in question.

When considering the firms that deregistered and those that did not, pooling the two experiments, it appears that the deregistrants were firms striving to be compliant on paper, without transferring any revenue to the tax authority. Among deregistrants, 61 percent filed an income tax declaration for 2014 and 52 percent did so for 2013, versus 21 percent and 26 percent, respectively, for firms that remained in the tax register. Yet the mean reported liability was orders of magnitude lower for deregistrants compared to firms remaining in the tax register: CRC 40,101 (US$ 70) versus CRC 656,409 (US$ 1,144) in 2013. While some deregistrants continued their business activities informally (103 firms were third-party reported in 2015), they would have been unlikely to pay more taxes. It thus appears that the deregistrations reduced the number of taxpayers to be managed by the administration without affecting tax collection.[30]

Continuing to Table 3, columns 1-2 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 percent. The impact on firms not covered

[30]To prevent deregistrations of active firms, in 2016 the tax authority introduced a new rule according to which all nonfilers that request deregistration have to be visited by a tax official.

by third-party information was even larger—about 5 p.p. compared to the control group's average of 18.9 percent. In both samples, treated firms were also slightly more likely to make a payment for the income tax in 2013.[31]

Finally and importantly, columns 3-5 consider the treatment impact of filing third-party reports. Panel A shows that treated firms were marginally more likely to file an information report about their supplier, and significantly more likely to file a report about their client. 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 generated a response twice as large as the baseline treatment (a 5.1 p.p. versus a 2.3 p.p. increase in the reporting rate). 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 that 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.

Table 4 examines the importance of the additional third-party reports in more detail, showing regressions of the number of reports filed on a treatment indicator and control variables. Columns 1 and 2 suggest that the treatment lead to an increase

[31]The treatment effect on income tax payment for 2013 is significant when focussing on the sample most susceptible to respond, the self-employed and firms that had filed income tax at some point before (Appendix Table A20). More generally, Table A20 shows that the results discussed in this section are qualitatively identical, with larger point estimates, in the sub-samples most likely to respond.

of 0.1 in the number of third-party reports filed, or approximately 820 new reports in total, 70 percent of which are reports about clients. Columns 3-5 show that slightly more than half of the new reports are unmatched by a corresponding report in the other direction, i.e., covering a transaction link previously unobserved by the tax authority; about 4 percent of the new reports cover nonfilers; and 30 percent of them cover underreporters, i.e., firms that underreport their sales compared to third-party reports. As the tax authority uses third-party reports to identify and follow-up on underreporters (e.g., through desk audits which were found to increase underreporters' reported tax liability and tax payment by 15 percent on average (Brockmeyer and Hernandez 2018), this additional third-party information could be of substantial value to tax enforcement.[32]

In sum, our analysis of a diverse set of compliance outcomes 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 very low reported liabilities.

C Heterogeneity

Since communication campaigns such as the one analyzed here generate costs to both the tax authority and to the taxpayer (which we examine in section V), they might need to be targeted. This section therefore studies heterogeneity in taxpayer responsiveness to the intervention. Table 1 already showed that firms with third-party information display larger p.p. increases in filing and payment rates than firms without third-party information. Table A23 considers heterogeneity by firm type, location in the capital city, and past compliance record, separately for firms with third-party information in Panel A and for firms without such information in

[32] Appendix Section 2 proposes a back-of-the-envelope estimation of the value of the additional third-party reports, suggesting that they could increase the tax revenue return of the intervention by up to 20 percent.

Panel B. Corporations exhibit a weaker filing rate response but a stronger payment response compared to the self-employed (columns 1 and 6). There is less heterogeneity along this dimension among firms without third-party information. 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, is strongly associated with a larger treatment effect (with the exception of sales tax compliance among firms with third-party information).

We then zoom in on the sample of firms with third-party information and examine treatment effects by firm size. Figure 3 plots the main outcomes, for the treatment and control group, by percentiles of third-party reported sales. We use deciles for most outcomes and quintiles for deregistration.[33] The treatment effect on filing is driven by smaller firms, as larger firms are more likely to declare even when in the control group (Panel A). The effect on payment rate and amount, on the other hand, is driven by larger firms (Panels B-C). This is consistent with heterogeneity by firm type discussed above, as larger firms are more likely to be corporations, and with differences in the two tax schedules, as the self-employed can file without making a payment if below the exemption threshold.[34]

When targeting a communication intervention, it is thus advisable for the tax authority to take into account a firm's third-party reported sales, its firm type, and its past compliance record.

[33]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. Table A24 confirms the heterogeneity results in regression form, interacting the treatment dummies with different indicators for high third-party information.

[34]The deregistration response is driven by smaller firms, as would be expected, while the effect on submitting an informative declaration does not differ by firm size (Panels D-E).

IV Medium-Term Effects and Mechanisms

This section examines the medium-term persistence of treatment effects, the impact of repeated compliance interventions, and the treatment effect on reporting behavior conditional on filing.

A Persistence of Treatment Effects

Having shown that the treatment had a large impact on contemporaneous income tax compliance and other compliance outcomes, we now examine whether these effects persisted over time. In Table 5 we pool the two treatments, as the coefficients do not differ significantly by treatment, and control for whether or a not a firm was subject to follow-up.[35] For firms with (without) third-party information, the email messages increased the income tax filing rate one year later by 8 p.p. (7.2 p.p.), over a control group average of 30 percent (15 percent).[36] The emails also generated a statistically significant increase in the payment rates one year later by 1.3 p.p. (0.2 p.p.). Finally, treated firms were more likely to file informative declarations in 2015. The emails generated a 2.3 p.p. (0.4 p.p.) increase in the third-party reporting rate, thus further expanding the tax authority's enforcement capacity. While the medium-term effects on third-party reporting dissipate after two years, a significant impact on income tax filing and payment persists for three years. In 2016, treated firms with (without) third-party information were 2.6 p.p. (1.5 p.p.) more likely to file their income tax declaration, and 1.6 p.p. more likely to make a payment than firms in the control group for 2014.[37] In 2017, treated

[35] We report very similar estimates from regressions without those controls in Appendix Table A16. The tax authority also conducts other communication campaigns for the filing of sales tax and informative declarations, and the targeting of those would likely have been either orthogonal to our treatment assignment or disproportionately focused on the control group.

[36] The fact that the medium-term impact on filing is smaller than the short-term impact for 2014 is due to the higher control group average in the following years, which is due to a small degree of mean reversion and the follow-up activities started by the tax authority in July 2015, examined in Appendix Figure A4 and section A.

[37] Appendix Figures A8 and A9 show the medium-term effects graphically.

firms with third-party information still exhibited a 2.0 p.p. higher filing rate and a 1.0 p.p. higher payment rate than 2014 control firms. The persistence of treatment effects for several years is unparalleled in enforcement-letter interventions. As the 2017 effects for firms with third-party information are only slightly smaller than the 2016 effects, these treatment effects might even persist for longer than three years.

In contrast to the positive impact on firms' medium-term tax compliance and third-party reporting behavior, we do not detect any effect of the emails on firms' propensity to be subject to third-party reporting in a later year. Table 6 displays the treatment effect on firms' likelihood of being reported by state institutions, private sector clients or suppliers, or card processing companies respectively in 2015 and 2016. Treated firms were no more or less likely to be subject to these reports, even if they received the stronger information email, which provided them with examples of third-party information held by the tax authority.[38] Thus, the treatment does not seem to have distorted firms' trade networks or coverage by third-party reports.[39]

Overall, these findings suggest that one-time enforcement messages can have a positive impact on compliance in the medium term, including compliance with information reporting requirements. The persistence of the main effects shows that the messages lead some firms to update their beliefs regarding the tax authority's enforcement capacity, i.e., the capacity to identify and follow up on nonfilers, and that the update was persistent over time, hence confirming the messages' credibility.[40] Consistent with this interpretation, Appendix Table A21 shows that the

[38]The result holds when considering the two treatments separately and among sub-samples of firms more susceptible to respond to the treatment (Appendix Table A22). The result is consistent with the absence of a behavioral response (missing mass) above the mandatory third-party reporting threshold, and the fact that a large share of transactions below the threshold are voluntarily declared (Appendix Figure A10). This strengthens the power of third-party information as a compliance tool.

[39]We examined potential spillovers effects on firms' trading partners, as identified by the third-party reports, but did not find any spillovers.

[40]It is possible that the treatment allowed firms to gain new information about the tax filing procedure, which would have reduced the cost of future tax filing, but this is unlikely as tax filing in Costa Rica is very simple and all the necessary information is available online.

medium-term impacts are larger among firms that were less compliant prior to the intervention, and hence more likely to update their enforcement perceptions through the treatment.

The persistence of the treatment effect also sheds some light on the size of potential real effects. By reducing (compliant) firms' after-tax profits, the treatment might have lowered investment and firm growth. This would reduce firms' future profits, and hence their likelihood of complying with taxes, if tax compliance is partly a function of firm profits. The fact that this potential negative effect on medium-term compliance is overcome through the experimental treatment and updating of enforcement perceptions suggests that either the real effects were small, or profitability is only weakly related to tax filing propensity.[41]

B Response to Repeated Interventions

A potential concern with our intervention, as with any enforcement threat, is that it may work differently as a one-off intervention than as a frequently used policy tool. On the one hand, firms that revised upward their beliefs about enforcement in response to the treatment message may be less likely to respond to additional messages once their beliefs are closer to the true (or suggested) enforcement probability. On the other hand, treated taxpayers which did not file and were not punished for their non-compliance might revise downward their beliefs about enforcement. These changes in beliefs should be stronger for firms in the treatment group, but could also affect control firms that interact with and learn from treated firms or from the media. Both channels would lower and potentially even eliminate the impact of repeated interventions.[42]

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

[42] While taxpayers may update their beliefs about enforcement probabilities, it is unclear whether beliefs should ultimately be in equilibrium. As discussed in section C, the enforcement strategy is multi-dimensional and changes in arbitrary ways over time, so firms would need high-frequency observations on all enforcement parameters to form accurate beliefs. This is unlikely, as tax en-

While pinning down the exact process through which taxpayers update enforcement beliefs after being treated is beyond the scope of this paper, we provide evidence that taxpayers repeatedly respond to enforcement messages. Our analysis relies on the fact that the tax authority conducted repeated (non-randomized) interventions following up on nonfilers during 2015-2017. In particular, the tax authority emailed income tax nonfilers for fiscal year 2015 on April 5, 2016; and income tax nonfilers for fiscal year 2016 on January 18, 2017.[43] The message content differed slightly across interventions, but was similar to our experimental treatment. Figure 4 examines the filing response to these interventions. As in Figure 2, we plot the filing rate among contacted nonfilers around the time of the intervention, distinguishing the experimental treatment group (solid line) and control group (dotted line). Given the two aforementioned channels through which firms may update their beliefs, we further distinguish firms that remained income tax nonfilers for 2014 (Panels A1 and B1) and firms that ultimately filed income tax for 2014 (Panels A2 and B2).

The key finding from these figures is that all groups still respond to the new interventions in 2016 and 2017, sharply at the time the messages were sent.[44] There

forcement is not widely discussed online or in the media. Thus, while beliefs may correspond to the true enforcement probability on average, individual taxpayers' beliefs are likely to be widely dispersed around this average. Beyond that, it is not necessarily the case that the average taxpayer belief equal the true enforcement probability, even in equilibrium. There is ample evidence that humans overestimate small probabilities (Kahnemann and Tversky 1979), and specific survey evidence showing that taxpayers consistently overestimate the audit probability (Erard and Feinstein 1994, Scholz and Pinney 1995, Harris and Inc. 1988). Behavioral models, such as the availability heuristic, can explain this.

[43] Additional interventions conducted for the sales tax in 2015 and 2016 are presented in Appendix Figures A11 and A12 . Our analysis focuses on filing responses, but payment responses behave similarly.

[44] The August 2015 sales tax intervention also provides direct evidence that merely publishing nonfilers' names online, which was done for firms with no email address or cell-phone number on file, increases the filing rate. On August 10, 2015, the tax authority published the names of 6,788 sales tax nonfilers, 1,366 of which were also income tax nonfilers for 2014. As Appendix Figure A14 shows, the filing rates for both taxes were constant before the intervention and sharply increased once the names were published.

is some evidence of decreasing marginal returns for the interventions, as the filing-rate increases achieved from the 2016 and especially from the 2017 intervention are not as high as those achieved from our earlier experimental intervention. Comparing the magnitudes of the response across groups is complicated by the fact that the treatment is no longer randomly assigned within the target sample for these new interventions. For instance, the lower overall filing rate among firms treated in our experiment might be due to the fact that treated firms which nonetheless become nonfilers again are particularly persistent nonfilers. However, two qualitative observations can be made.

First, the sharpness of the response at the time of the intervention is actually stronger among firms that remained nonfilers for 2014 and might thus have revised their enforcement beliefs downwards (panels A1 and B1). Yet the filing rate in this sample remains below 15 percent in all cases, regardless of experimental treatment status. This suggests that a concern that the experiment might have lowered these firms' responsiveness to future enforcement interventions is of second order importance, as these firms are very unlikely to file anyways. Second, among 2014 filers, the average response rate is similar to the response rate in our experiment (pooling firms with and without third-party information), with filing rates reaching up to 30 percent among previously treated firms and up to 40 percent among previous control firms. Both the sharpness of the response, and the proportional effect size, compared to the pre-intervention filing rate, are larger among the experimental treatment group. There is thus no evidence that our treatment reduced firms' responsiveness to future enforcement interventions.[45]

Panels C1 and C2 compare firms that were targeted by the email campaigns

[45] Appendix Table A25 shows regression estimates of firms' responsiveness to the 2016 income tax intervention, controlling for 2015 treatment and filing status and the follow-up activities started in July 2015. For firms that were subject to follow-up activities, the results rule out any potential backfiring of the treatment (i.e., reduced responsiveness to future interventions).

in 2016 and 2017 with firms that had also been identified by the tax authority as nonfilers for the relevant fiscal period, but which did not receive an email (likely due to administrative capacity constraints). We detect no change in filing rates among this group at the time of the intervention, confirming that the filing rate increase observed among targeted firms is an effect of the email.

In conclusion, while some firms update their beliefs about enforcement permanently as discussed in section A, for other firms (those that respond repeatedly to enforcement messages) the treatment effect seems to work through a salience or reminder channel, which can be activated repeatedly. The effectiveness of this channel is still evident even among firms that have been contacted four or five times for nonfiling behavior, and which still respond to enforcement messages, as Figure A13 shows.[46]

C Reporting Behavior

In addition to raising the filing and hence payment rate, the treatment was designed to improve reporting behavior and hence tax payment amounts conditional on filing, by highlighting the use of third-party information in enforcement. To analyze changes in reporting behavior, we focus on the sample of filers with third-party information and examine to what extent firms match self-reported sales to third-party reported sales, and self-reported costs to self-reported sales. Figure 5 summarizes the results.[47] Panels A1 and B1 show, for each firm type and treatment group, the fraction of filers that underreport, overreport or match self-reported sales to third-party reported sales. There is no difference in filing behavior between the

[46] It is also possible that periodic messaging or contact with the tax authority is necessary for firms to maintain constant enforcement beliefs. In fact, given the numerous and repeated nonfiling interventions that the tax authority conducts, one might consider the response to our experiment as an equilibrium response. Besides, the mere fact that the tax authority conducts repeated interventions to follow up on nonfilers could be considered *prima facie* evidence that these interventions have an impact.

[47] Appendix Figures A6 and A7 present more finely-grained bunching graphs, similar to those found in Slemrod et al. 2015.

baseline treatment and the control group. However, firms filing in response to the information treatment were significantly less likely to underreport sales and significantly more likely to match third-party reports than other firms. This effect is found among both corporations and the self-employed. As the effect is not present in the baseline treatment group, it must be driven by the specific nature of the treatment, i.e., the salient mentioning of third-party information, rather than the selection effect (the fact that filers in the treatment group have different characteristics than filers in the control group). Panels A2 and B2 confirm that the increase in reported sales in the information treatment group was not offset by an increase in reported costs. Among corporations, there are hardly any significant differences between treatment groups. Among the self-employed, firms in the information treatment were in fact less likely than control firms to match sales to costs and more likely to report sales higher than costs.

This result is surprising, given that previous studies found firms to nearly fully offset reported sales increases with cost increases (Carrillo, Pomeranz and Singhal, 2016 and Slemrod et al. 2015). It can be explained by the presence of an enforcement notch. The self-employed in Costa Rica are allowed to deduct 25 percent of their sales as costs without proof of receipts, but are required to maintain auditable receipts of all costs if they deduct costs in excess of 25 percent of sales.[48] Consequently, the self-employed bunch either at a costs-to-sales ratio of 0.25 or 1, whereas corporations bunch at a costs-to-sales ratio of 1. The enforcement threat lead self-employed in the information treatment to reduce bunching at a costs-to-sales ratio of 1 and increase bunching at 0.25, to use the unenforced 25 percent deduction, as shown in Appendix Figure A7. The treatment effectively pushed

[48]This is akin to an alternative minimum tax or presumptive tax (Best et al. 2015), schemes which are common around the world and used to enhance tax compliance and reduce compliance costs. Spain has a rule similar to Costa Rica's.

firms to trade cost deductions for reduced enforcement. Our results shows that a clever design of deduction rules combined with credible enforcement can prevent firms from offsetting increases in reported sales.

Together, these results confirm that the treatment not only increased government revenue due to increased filing rates, but also improved reporting behavior among the self-employed, who declare larger tax bases and make larger payments in response to the mentioning of third-party information (Appendix Table A26). The presence of an enforcement-free minimum deduction prevents firms from offsetting sales increases with cost increases.

V Cost-Benefit Analysis

To examine under what conditions governments should conduct interventions like the one presented in this paper, we analyze in this section the intervention's direct costs and benefits, its welfare implications, and indirect benefits such as deterrence. 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$ 64) a day. We assume that each officer spent an additional five days answering taxpayers' inquiries. In addition, we account for the cost of the follow-up activities, considering that a tax officer deals with about four cases per day, and that 876 treated firms with third-party information and 47 firms without third-party information were subject to follow-up. This means the cost per email was on average 695.6 CRC (US$ 1.20), and for firms without third-party information it was one-fourth of the cost for firms with third-party information, due to the higher number of follow-up cases in the latter group.[49]

[49]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. When we attribute the total cost to the group

The benefits are measured by the increase in tax payment from treated firms. Among firms with third-party information, the baseline email increased firms' income tax payments by CRC 8,353 (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 10,441 (US$ 18). For firms without third-party information the email increased payment by CRC 217 (US$ 0.38). With this conservative approach, we find that the intervention was highly cost-effective, with an overall cost-benefit ratio of about 1:4. As the cost-benefit ratio increases to 1:6 among firms with third-party information, a tax authority with limited resources might consider targeting the intervention at these firms.

The benefit of focusing on firms with third-party information is reinforced when taking into account firms' filing costs, as we do in the appendix with a simple tax filing model based on Keen and Slemrod (2017). Ignoring filing costs and penalty costs, our intervention is welfare improving if the marginal value of public funds is larger than 1.33. With filing costs, our intervention is welfare improving if the filing cost is below 5 percent of additional tax payments. The likelihood of this being true is highest for firms that have an employee responsible for tax accounting, so that the marginal cost of filing a tax form is close to zero, assuming the employee receives a fixed wage and is not working at full capacity.

Although the aggregate revenue gains generated by this intervention are small, as in most other communication experiments, the intervention serves the broader purpose of sustaining voluntary compliance, as evidenced by the fact that treated firms improve their tax filing and payment behavior also in future tax periods. In addition, the intervention and follow-up activities allowed the tax authority to up-

of firms with and without third-party information respectively, we take into account the number of messages sent for each group, the number of filers that should be proportional to the number of inquiries, and the number of follow-up cases.

date taxpayer contact information and collect other missing tax declarations. Potentially most importantly, the intervention enhanced compliance with information reporting requirements, which facilitates future tax enforcement. Beyond revenue considerations, the intervention improved 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 production efficiency, tax morale, and the perception of fairness of the tax system. 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.

VI Conclusion

This paper has argued that nonfiling among tax registered firms constitutes an important and underresearched compliance gap in lower-income countries that can be addressed cost-effectively. We evaluate a randomized enforcement campaign in which the tax authority in Costa Rica requested nonfiling firms by email to file their income tax declaration. We find that the enforcement emails increase the tax payment rate by 3.4 p.p. and the payment amount by US$ 15 among firms covered by third-party information — treatment effects that further increase to US$ 18 when emails specifically mention examples of third-party reports. The treatment also increases firms' propensity to provide information reports about other firms, thus facilitating future tax enforcement. The main treatment effects persist for at least two years, and for three years among firms with third-party information. In addition, firms continue to be highly responsive to similar enforcement interventions by the tax authority in future fiscal years.

The intervention is highly cost-effective, and potentially welfare enhancing if taxpayers' filing costs are sufficiently small. From the tax authority side, the costs of the intervention could be further reduced by automating the identification of

nonfilers and the personalized delivery of messages with the taxpayer's name and third-party information. To avoid taxpayer filing costs, tax authorities may consider alternative policies such as tax withholding at source (Brockmeyer and Hernandez 2018), registration fees or flat-rate presumptive taxes, or automatically filled tax returns that merely need to be approved by taxpayers.

The intervention examined in this paper could likely be implemented in other developing countries, including poorer countries. Indeed, Figure A15 shows that the vast majority of countries have access to third-party information that allows them to identify nonfiling firms and target communication campaigns (bars 1-4). Yet, while the information is available, most countries do not make optimal use of it in their enforcement activities (bars 5-8). Studying what impedes the use of available third-party information is an important avenue for future research, which could focus on administrative capacity as well as political economy constraints.

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Table 1: Impact on Income Tax Compliance

	OLS				
	(1) Filed (percent)	(2) Positive net liability (percent)	(3) Positive payment (percent)	(4) Log payment	(5) Payment (CRC)
A: Firms with Third-Party Information					
T1: Baseline email	21.12	4.44	3.07	0.3408	8352.9
	(0.87)	(0.48)	(0.38)	(0.0428)	(2449.8)
T2: Information email	22.98	5.19	3.73	0.4088	10441.2
	(0.89)	(0.49)	(0.40)	(0.0444)	(2823.7)
Other controls	Yes	Yes	Yes	Yes	Yes
Control group avg.	11.48	3.39	1.72	0.1940	5015.8
T1=T2 p-value (Wald test)	0.074	0.189	0.159	0.195	0.514
Observations	12,515	12,515	12,515	12,515	12,515
B: Firms without Third-Party Information					
T1: Baseline email	15.05	0.65	0.47	0.0496	265.1
	(0.41)	(0.09)	(0.08)	(0.0083)	(59.4)
T2: Information email	14.93	0.59	0.43	0.0411	168.9
	(0.40)	(0.09)	(0.07)	(0.0075)	(51.5)
Other controls	Yes	Yes	Yes	Yes	Yes
Control group avg.	3.93	0.18	0.13	0.0142	96.2
T1=T2 p-value (Wald test)	0.809	0.565	0.649	0.397	0.161
Observations	37,242	37,242	37,242	37,242	37,242

Note: Each column in this table reports estimates from an OLS regression run at the taxpayer level. Robust standard errors clustered by email address (to account for the fact that a few taxpayers share the same email address) are in parentheses. Panel A is for the sample of firms for which the tax authority has third-party information on their economic activities. Panel B is for the sample of firms without third-party information. The dependent variables, as indicated by the column headings, are indicator variables for whether the taxpayer filed income tax for 2014, reported a positive net liability, and paid income tax (considering only final payments made with the declaration and not advance payments that may have been made earlier) for 2014 (columns 1-3 respectively), log tax payment (column 4) and tax payment in Costa Rican colones (CRC) (column 5). The dependent variable in columns 4 and 5 is zero for firms who do not file and for filers who do not make any payment. All outcomes are measured 15 weeks after the start of the experiment. Payment amounts are winsorized at the top 0.1 percent to reduce the influence of outliers. All regressions include as independent variables two indicator variables for the baseline email treatment (T1) and for the information email treatment (T2), which were randomly assigned within the samples, and the following other indicator variables (which are balanced across treatment groups): whether the taxpayer is a corporation, has a legal representative, has a second email address, is located in the capital city, has third-party information above 2.5 million CRC, has third-party information above 6 million CRC (the administratively used thresholds), made advance tax payments for the income tax in 2014, filed income tax in 2013, reported a positive net liability for the income tax in 2013, made an income tax payment in 2013, and was a third-party informant in 2013. The bottom rows in each panel report the control group average of the outcome variable and a Wald test evaluating whether the coefficients on the two treatment variables are statistically significantly different from each other. Tables A7-A12 display robstuness tests and the full regression results, including the coefficients on the control variables.

Table 2: Impact on Other Compliance Outcomes (1/2)

	OLS					
	(1) No. months decl. sales tax	(2) No. months paid sales tax	(3) Sales tax payment (CRC)	(4) Deregistered (sales tax) (percent)	(5) Deregistered (percent)	(6) Switched to simplified regime (percent)
	A: Firms with Third-Party Information					
T1: Baseline email	-0.0393	-0.0231	-9751.5	0.02	1.15	0.05
	(0.0504)	(0.0205)	(7965.0)	(0.10)	(0.27)	(0.05)
T2: Information email	-0.0418	-0.0231	-5568.2	-0.05	1.11	0.05
	(0.0493)	(0.0203)	(8331.9)	(0.10)	(0.27)	(0.05)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group avg.	0.6371	0.1330	29095.0	0.22	0.92	0.02
T1=T2 p-value (Wald test)	0.960	0.999	0.563	0.481	0.885	0.982
Observations	12,515	12,515	12,515	12,515	12,515	12,515
	B: Firms without Third-Party Information					
T: Any email	0.0004	0.0062	26.9	0.07	2.03	0.04
	(0.0162)	(0.0035)	(76.9)	(0.06)	(0.15)	(0.02)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group avg.	0.2547	0.0159	357.5	0.32	1.22	0.02
Observations	37,242	37,242	37,242	37,242	37,242	37,242

Note: This table is identical to Table 1, except for the outcome variables, which are indicated by the column headings. Table A14 displays the full regression results including the coefficients on the control variables. For Panel B, the regressions include only a single treatment indicator (combining the baseline and the information treatment groups), in addition to the control variables. The dependent variables, as indicated by the column headings, are the number of months in 2015 during which the taxpayer declarated sales tax (column 1), the number of months in 2015 for which the taxpayer paid sales tax (2), the total sales tax payment for 2015 in CRC (3), and indicator variables for whether the taxpayer deregistered from the sales tax (4), deregistered completely with the tax authority (5), switched to the simplified tax regime available to small firms (6). All outcome variables are measured 15 weeks after the start of the experiment and refer to compliance for fiscal year 2014, unless otherwise noted. Payment amounts are zero for taxpayers who did not file or did not make any payment. Sales tax payment amounts are winsorized at the top 0.1 percent to reduce the influence of outliers.

Table 3: Impact on Other Compliance Outcomes (2/2)

	OLS				
	(1) Filed income tax for 2013 (percent)	(2) Paid income tax for 2013 (percent)	(3) Presented informative declaration (percent)	(4) Reported supplier (percent)	(5) Reported client (percent)
	A: Firms with Third-Party Information				
T1: Baseline email	3.21	0.19	2.76	0.36	2.64
	(0.41)	(0.15)	(0.78)	(0.59)	(0.71)
T2: Information email	2.28	0.23	5.14	0.97	4.44
	(0.39)	(0.16)	(0.83)	(0.60)	(0.76)
Other controls	Yes	Yes	Yes	Yes	Yes
Control group avg.	35.03	5.71	17.66	9.28	12.79
T1=T2 p-value (Wald test)	0.048	0.837	0.006	0.305	0.024
Observations	12,515	12,515	12,515	12,515	12,515
	B: Firms without Third-Party Information				
T: Any email	4.94	0.09	0.20	0.02	0.19
	(0.20)	(0.03)	(0.13)	(0.11)	(0.08)
Other controls	Yes	Yes	Yes	Yes	Yes
Control group avg.	18.92	1.09	1.29	0.94	0.44
Observations	37,242	37,242	37,242	37,242	37,242

Note: This table is the continuation of Table 2. Everything is identical, expcept the outcome variables, which here are indicator variables for whether the tax payer filed income tax for 2013 (1), paid income tax for 2013 (2), presented any informative declaration (third-party report) about a client or supplier for 2014 (3), presented an informative declaration about a supplier for 2014 (4), and presented an informative declaration about a client in 2014 (5).

Table 4: Impact on Informative Declarations

	(1) No. of third-party reports filed	(2) No. of reports about clients	(3) No. of reports about suppliers	(4) No. of unmatched reports	(5) No. of reports about nonfilers	(6) No. of reports about under-reporters
T: Any email	0.098	0.070	0.027	0.051	0.004	0.030
	(0.024)	(0.012)	(0.017)	(0.016)	(0.002)	(0.008)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group avg.	0.101	0.046	0.055	0.038	0.003	0.028
Observations	12,515	12,515	12,515	12,515	12,515	12,515

Note: Each column in this table reports estimates from an OLS regression run at the taxpayer level. Robust standard errors clustered by email address (to account for the fact that a few taxpayers share the same email address) are in parentheses. The dependent variables, indicated by the column headings, are the number of informative declarations (third-party reports) a taxpayer filed (column 1), the number of reports filed about clients (2), the number of reports filed about suppliers (3), the number of unmatched reports (4), the number of reports about nonfilers (5), and the number of reports about underreporters (6). The table considers third-party reports for fiscal year 2014, filed after experimental messages were sent (4 March 2015). The results reported here are more conservative than results that consider all reports filed (before and after the experiment) or winsorizing the number of reports to account for outliers. Unmatched third-party reports (column 4) are reports for which there is not a corresponding report in the other direction for the same client-supplier pair and transaction amount (rounding to the nearest 5,000 CRC (about US$ 10)—the results are robust to rounding to the nearest 10,000 CRC or not rounding at all). Reports about nonfilers (column 5) are reports filed before the subject filed an income tax declaration. Reports about underreporters (column 6) are reports about subjects with total amounts of third-party reported sales (taking into account the latest report) that exceed the amount of self-reported sales from their income tax declarations. The independent variable is an indicator variable for whether the taxpayer was in one of our two experimental treatment groups. The control variables are the same as in Table 1 (coefficients not reported for simplicity).

Table 5: Medium-Term Impact on Compliance Outcomes, 2015–2017

	By filing deadline					
	(1) Filed income tax (percent)	(2) Paid income tax (percent)	(3) Reported client (percent)	(4) Reported supplier (percent)	(5) No. of clients reported	(6) No. of suppliers reported
	A: Firms with Third-Party Information					
T effect in 2015: Any 2014 email	8.02	1.26	2.34	0.71	0.0209	0.0039
	(0.92)	(0.42)	(0.63)	(0.52)	(0.0056)	(0.0044)
Control group avg. 2015	30.83	4.67	11.20	7.89	0.0930	0.0613
T effect in 2016: Any 2014 email	2.59	1.58	1.08	0.30	0.0102	0.0002
	(0.96)	(0.50)	(0.70)	(0.56)	(0.0063)	(0.0047)
Control group avg. 2016	35.07	6.27	13.78	9.08	0.1165	0.0702
T effect in 2017: Any 2014 email	2.03	0.97				
	(0.95)	(0.53)				
Control group avg. 2017	34.20	7.73				
Observations	12,515	12,515	12,515	12,515	12,515	12,515
	B: Firms without Third-Party Information					
T effect in 2015: Any 2014 email	7.20	0.25	0.40	0.31	0.0034	0.0022
	(0.42)	(0.12)	(0.17)	(0.15)	(0.0016)	(0.0013)
Control group avg. 2015	15.34	1.14	2.34	1.69	0.0209	0.0150
T effect in 2016: Any 2014 email	1.52	0.30	0.15	0.26	0.0006	0.0015
	(0.44)	(0.15)	(0.21)	(0.18)	(0.0020)	(0.0016)
Control group avg. 2016	19.08	1.89	3.94	2.60	0.0347	0.0214
T effect in 2017: Any 2014 email	0.15	0.14				
	(0.45)	(0.19)				
Control group avg. 2017	19.50	3.00				
Observations	37,242	37,242	37,242	37,242	37,242	37,242

Note: Each cell in this table reports estimates from an OLS regression run at the taxpayer level. Robust standard errors clustered by email address (to account for the fact that a few taxpayers share the same email address) are in parentheses. Panel A is for the sample of firms for which the tax authority has third-party information on their economic activities. Panel B is for the sample of firms without third-party information. The dependent variables, as indicated by the column headings, are indicator variables for whether the taxpayer filed income tax for a particular year (column 1), paid income tax (2), presented an informative declaration about a client (3), and presented an informative declaration about a supplier (4), the number of clients reported (5), and the number of suppliers reported (6). Within each panel, these outcomes are measured either in fiscal year 2015 (top cells), 2016 (middle) or 2017 (bottom). All regressions include as an independent variable an indicator for whether the firm was in one of the two treatment groups of our experimental intervention for nonfilers in 2014. Each cell reports the coefficient on this treatment variable and its standard error. The bottom row in each cell reports the control group average of the outcome variable. All regressions include the same control variables as in Table 1. The regressions for Panel A additionally control for whether the taxpayer was subject to the follow-up campaign in 2015, and an interaction between this follow-up indicator variable and the treatment indicator. The regressions for Panel B additionally include the follow-up indicator. The number of treated firms subject to follow-up for Panel B is small, which is why we do not control for treatment and follow-up interaction. The control group average represents the average for firms in the control group not subject to follow up. (The average for follow-up firms is higher.) The control coefficients are not reported for simplicity.

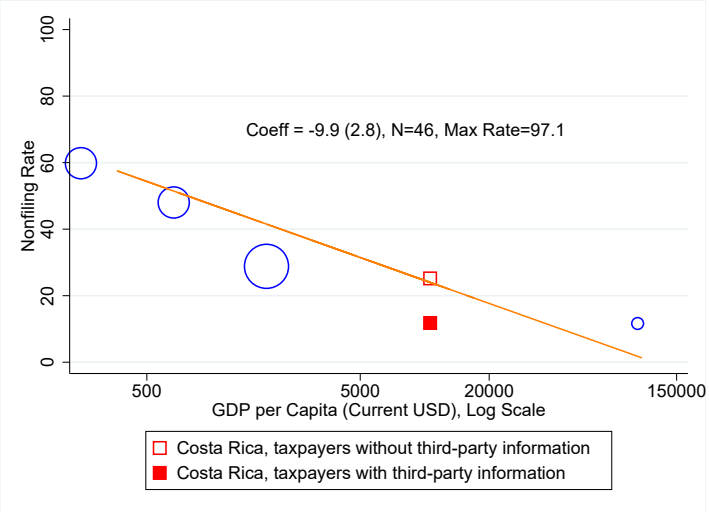
Table 6: Medium-Term Impact on Third-Party Reports, 2015–2017

	(1) Reported by state institution (percent)	(2) Reported by private client or supplier (percent)	(3) Reported by card company (percent)
T effect in 2015: Any 2014 email	-0.13	-0.42	0.23
	(0.36)	(0.94)	(0.56)
Control group avg. 2015	3.36	67.50	8.44
T effect in 2016: Any 2014 email		-1.31	
		(1.00)	
Control group avg. 2016		59.56	
Observations	12,515	12,515	12,515

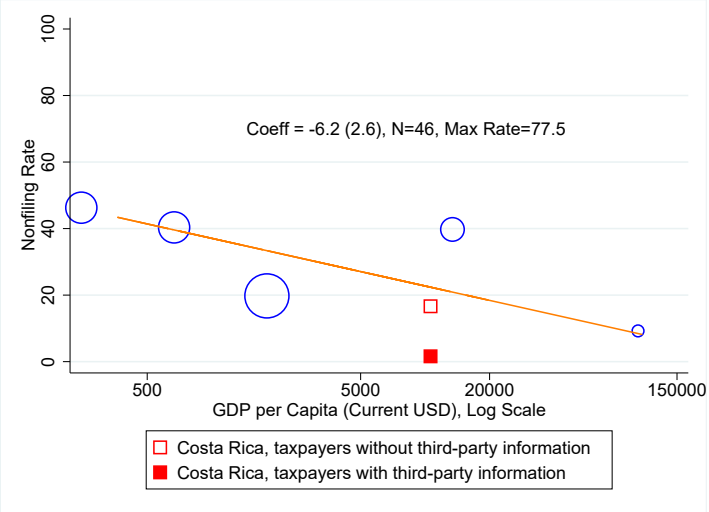
Note: Each cell in this table reports estimates from an OLS regression run at the taxpayer level. Robust standard errors clustered by email address (to account for the fact that a few taxpayers share the same email address) are in parentheses. This table covers only firms for which the tax authority has third-party information on their economic activities. The dependent variables, as indicated by the column headings, are indicator variables for whether the taxpayer was reported through an informative declaration by a state institution (1), by a private client or supplier (2) or by a credit/debit card company (3). These outcomes are measured either in fiscal year 2015 (top cells) or 2016 (bottom cells). All regressions include as an independent variable an indicator for whether the firm was in one of the two treatment groups of our experimental intervention for nonfilers in 2014. Each cell reports the coefficient on this treatment variable and its standard error. The bottom row in each cell reports the control group average of the outcome variable. All regressions include the same control variables as in Table 1, and additionally control for whether the taxpayer was subject to the follow-up campaign in 2015, and an interaction between this follow-up indicator variable and the treatment indicator. The control group average represents the average for firms in the control group not subject to follow up. (The average for follow-up firms is higher.) The control coefficients are not reported for simplicity.

Figure 1: Tax nonfiling Rates Across Countries

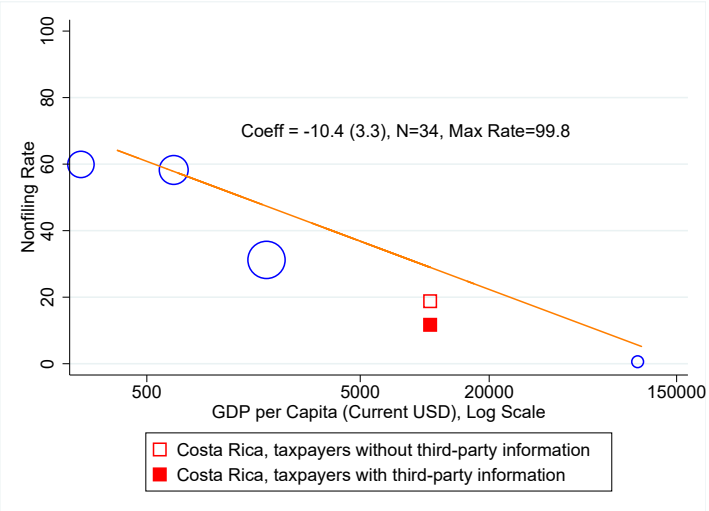
A: Corporate Income Tax



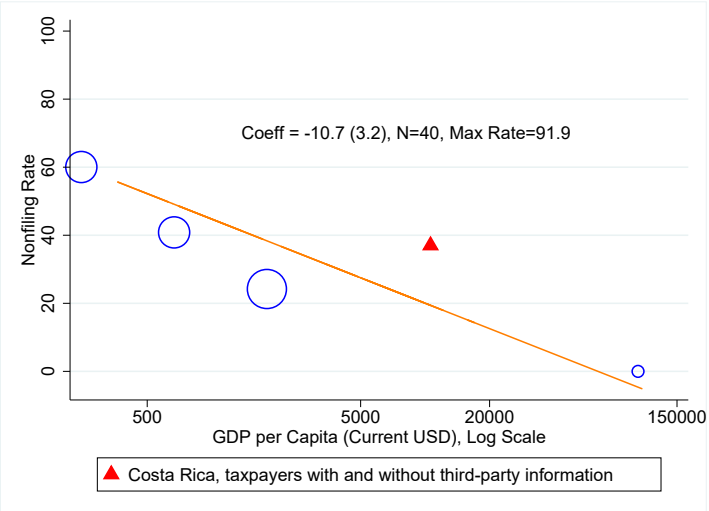
B: Value-Added Tax



C: Personal Income Tax, Self-Employed



D: Personal Income Tax, Employer Report

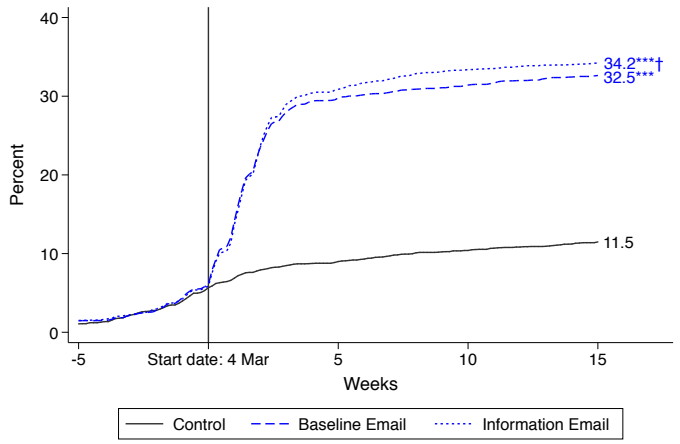


Note: These figures show the correlation between the nonfiling rate and log GDP per capita across countries, for different taxes. To avoid disclosing country-specific nonfiling rates, the data are binned by log GDP per capita (bin size is .5 log points). The marker size reflects the relative number of observations per bin. The red square/triangle markers represent Costa Rica, where we measure nonfiling rates directly using the tax authority's list of nonfilers in 2014 (based on the tax register and the income and sales tax declarations submitted). The round markers represent other countries. The regression coefficients (standard errors in parentheses) are based on the raw data. The filing rate data is from the Tax Administration Diagnostic Assessment Tool (TADAT), and from the 2016 CIAT Report "Revenue Administration in Latin America and the Caribbean": https://ciatorg-public.sharepoint.com/biblioteca/DocumentosTecnicos/Ingles/2016_State_AT_ALC_2011-2013.pdf (appendix tables 5.2-5.6). TADAT is an assessment tool that implemented globally by development partners and technical assistance providers including the IMF and World Bank. The GDP data is from the World Development Indicators. The nonfiling rate is defined as 1 - the share of returns filed on time in the TADAT data, and as the share returns not submitted in the CIAT data. The negative correlation is also present when considering only nonfiling or only 1- on time filing. Figure A1 shows that filing rates are also increasing over time.

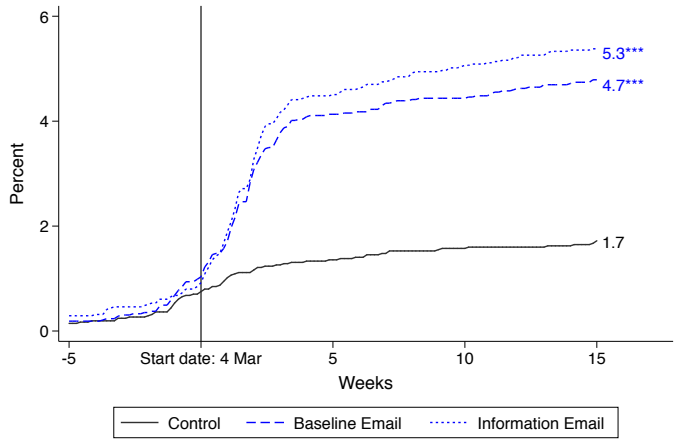
Figure 2: Compliance Over Time by Information Coverage and Treatment Group

A: Firms with Third-Party Information

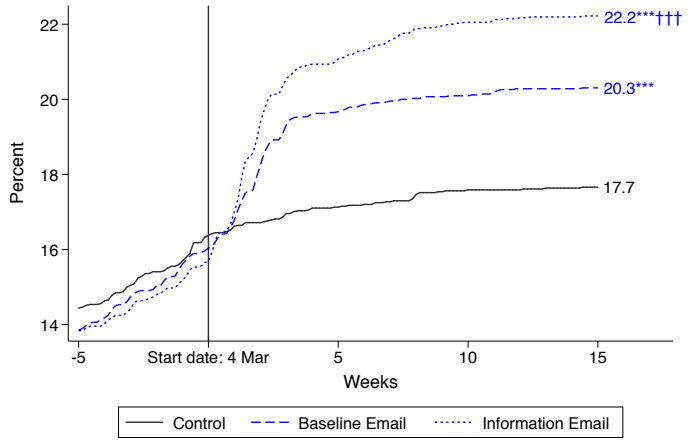
A1: Filing Rate



A2: Payment Rate

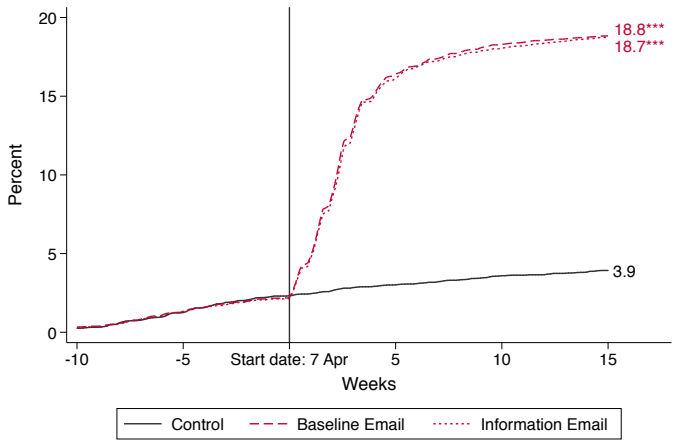


A3: Third-Party Informant

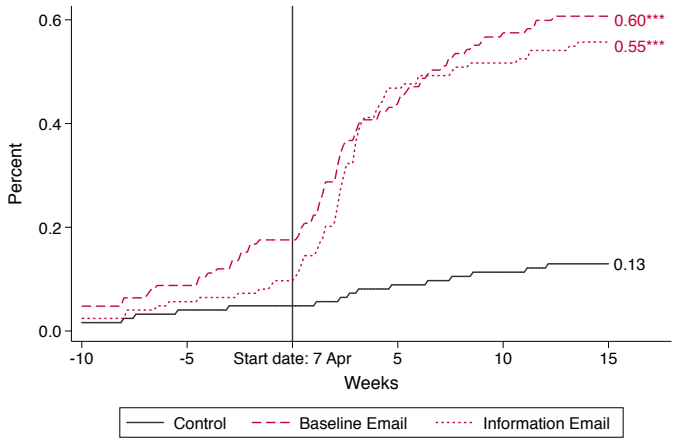


B: Firms without Third-Party Information

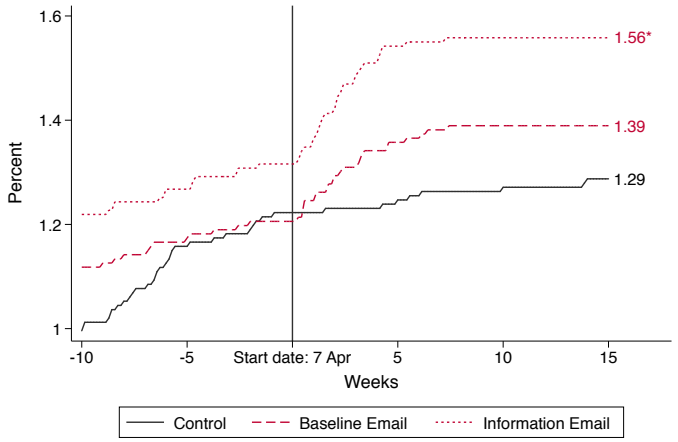
B1: Filing Rate



B2: Payment Rate

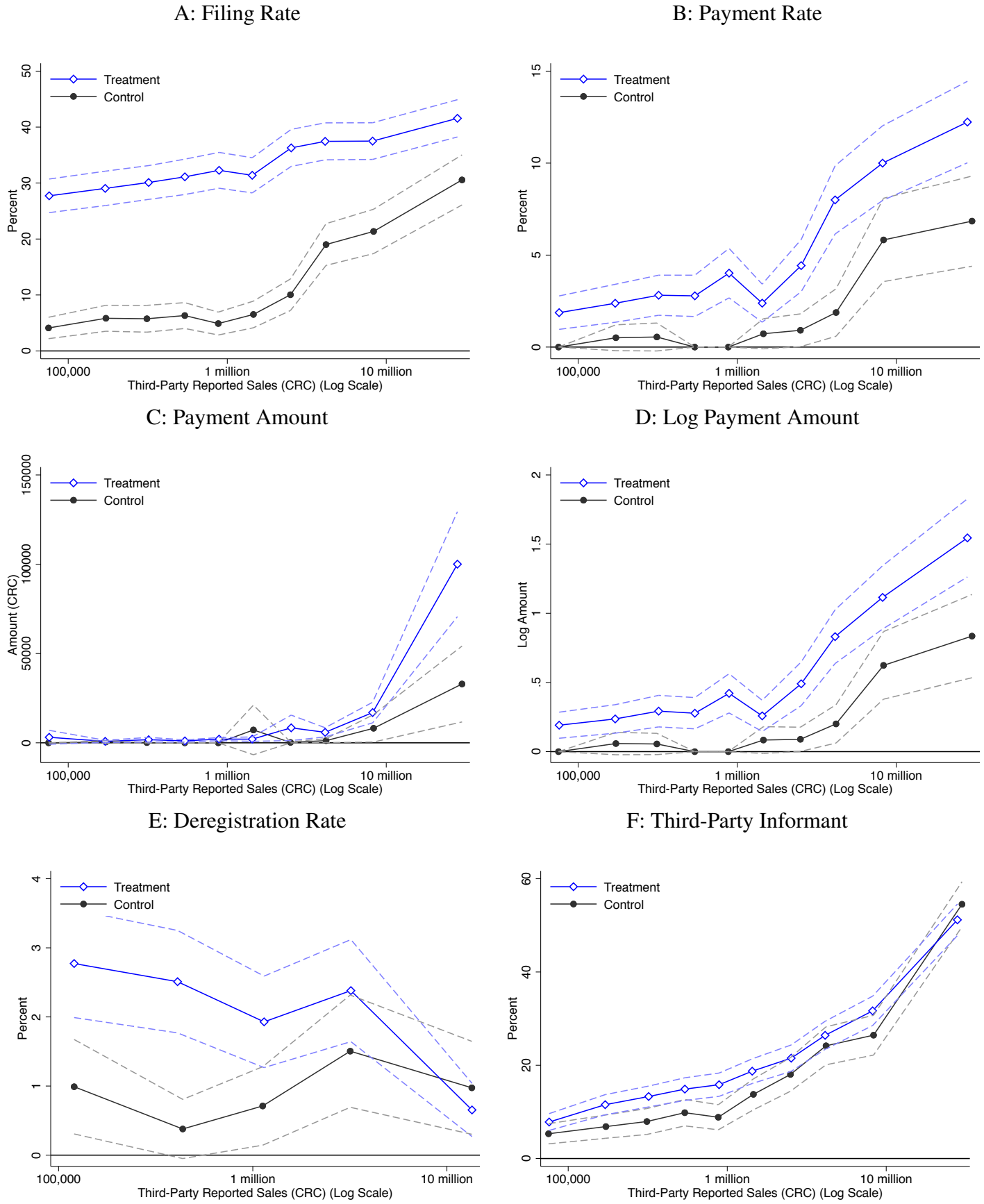


B3: Third-Party Informant



Note: These figures show the share 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 vertical 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. 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 with the control group and come from regressions that include controls (as in Table 1). Significance levels are noted as per convention: * p<.10, ** p<.05, *** p<.01. Daggers indicate significant differences between the two treatments.

Figure 3: Heterogeneity by Third-Party Information

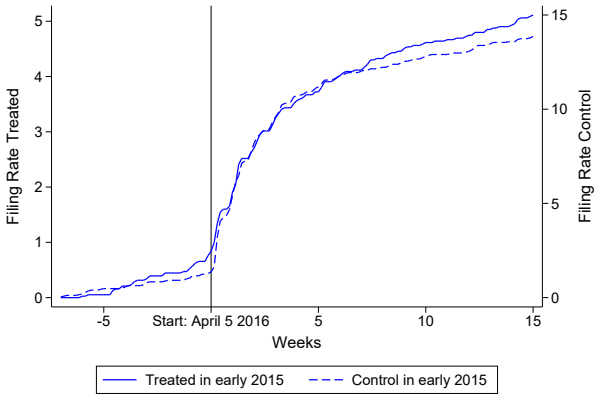


Note: This figure shows the share of nonfilers that filed an income tax declaration (Panel A), the share that made an income tax payment (Panel B), payment amounts (Panels C and D) for 2014, the share that deregistered (Panel E), and the share that filed an informative declaration (Panel F) at 15 weeks after the start of the experiment, among firms with third-party information (experiment 1). Payment amounts are winsorized at the top 0.1percent to reduce the influence of outliers. The values are displayed by deciles/quintile log third-party reported sales. The blue hollow markers are for the pooled treatment group and the black solid markers are for the control group. 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.

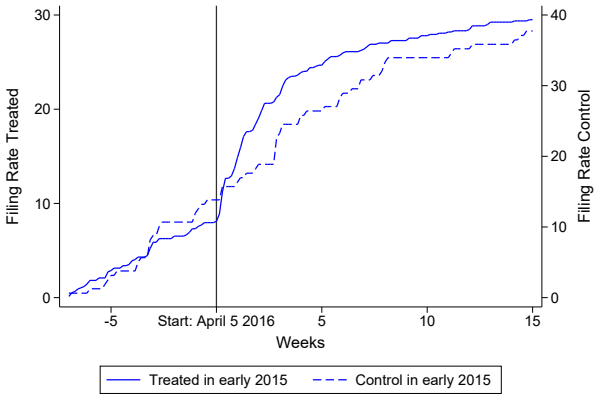
Figure 4: Filing Response to Repeated Interventions, 2016-2017

A: Response to 2016 Income Tax Intervention

A1: Taxpayers Remaining nonfilers for 2014

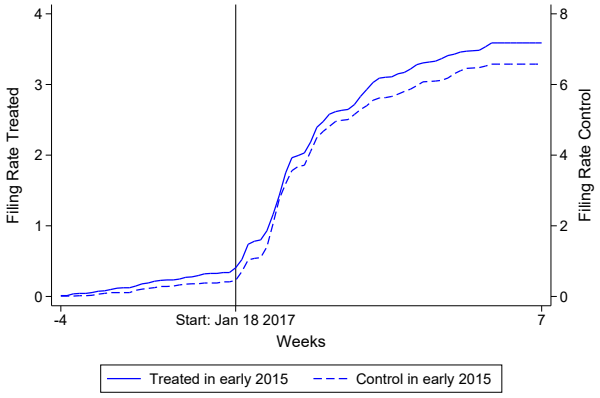


A2: Taxpayers that Filed for 2014

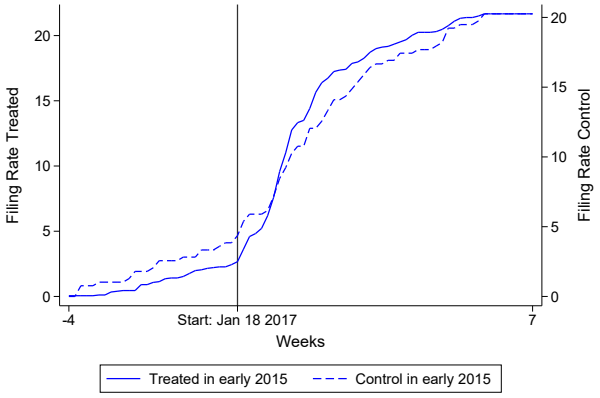


B: Response to 2017 Income Tax Intervention

B1: Taxpayers Remaining nonfilers for 2014

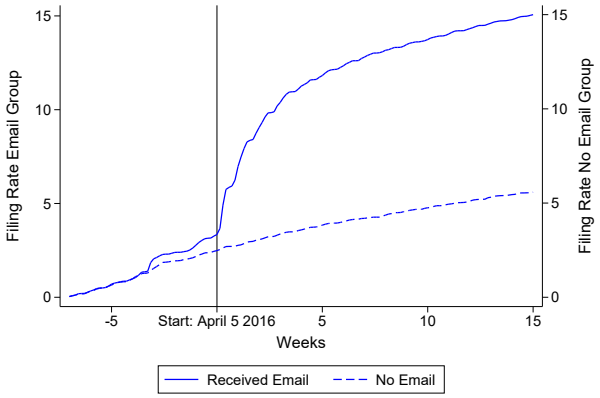


B2: Taxpayers that Filed for 2014

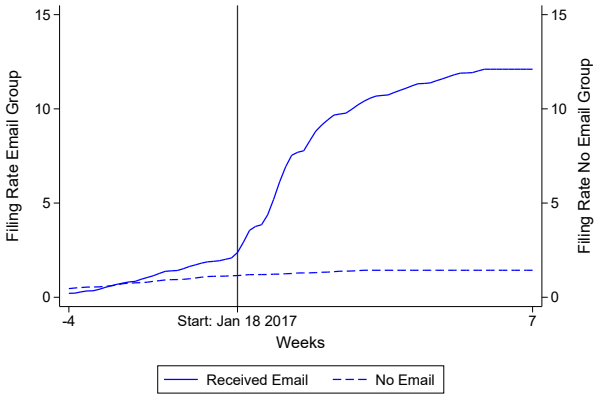


C: Comparing Firms That Did And Did Not Receive an Email

C1: 2016 Income Tax Intervention



C2: 2017 Income Tax Intervention

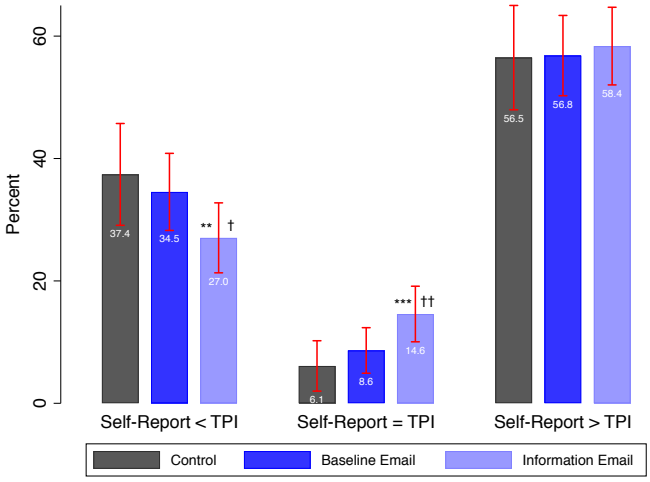


Note: These figures show the tax filing rate around two enforcement messaging campaigns, as indicated by the row titles, which used enforcement emails similar to the ones used in our experiment.Panels A and B focus only on firms in our experimental sample, distinguishing firms that remained nonfilers for the income tax in 2014 (left side), and firms that ultimately did file income tax for 2014 (right side). The black vertical line in each graph marks the date on which messages were sent. The last two panels look at the full sample of nonfilers considered for the 2016 and 2017 interventions, comparing firms that did receive an email and firms that ultimately did not receive in email.

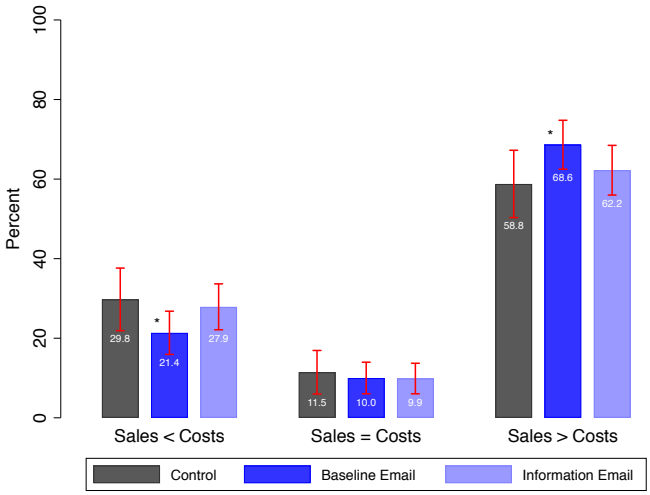
Figure 5: Reporting Behavior of Tax Filers

A: Corporations

A1: Self-Reported Versus Third-Party-Reported Sales

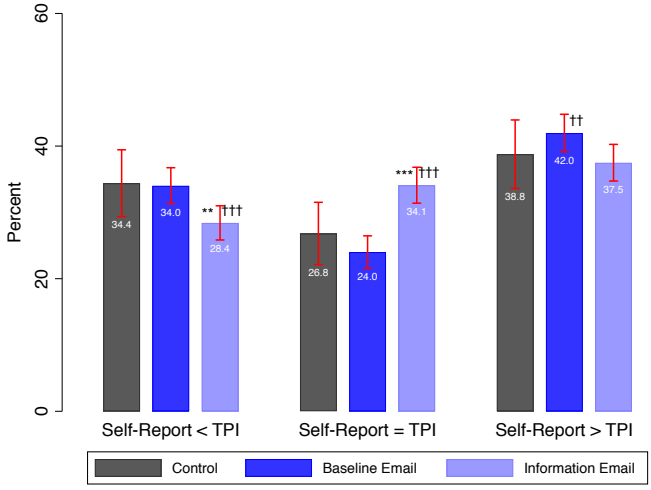


A2: Self-Reported Sales Versus Self-Reported Costs

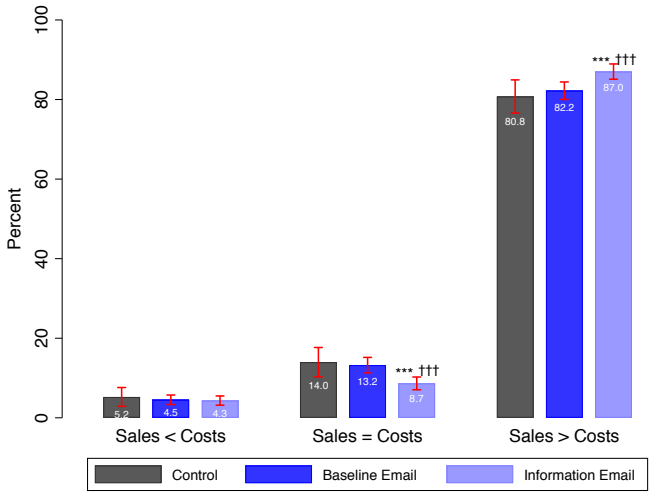


B: Self-Employed

B1: Self-Reported Versus Third-Party-Reported Sales



B2: Self-Reported Sales Versus Self-Reported Costs



Note: These figures show the share of firms with self-reported sales (recorded on firms' income tax declarations) less than, equal to, or greater than third-party reported sales (row 1) and self-reported costs (row 2). The figures are only for firms with third-party information that filed an income tax declaration within fifteen weeks of the experiment start. Error bars indicate 95percent confidence intervals. Stars indicate a significant difference from the control group (at the .10, .05, and .01 levels) and daggers (†) indicate a significant difference between the two treatments.