IDB WORKING PAPER SERIES Nº IDB-WP-926

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The Role of Penalties and Detection

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September 2018

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Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

Lopez-Luzuriaga, Andrea.
Compliance spillovers across taxes: the role of penalties and detection / Andrea Lopez-Luzuriaga, Carlos Scartascini.
p. cm. — (IDB Working Paper ; 926)
Includes bibliographic references.
1. Taxpayer compliance-Econometric models. 2. Tax evasion-Econometric models. 3. Property tax-Econometric models. 4. Sales tax-Econometric models. I. Scartascini, Carlos G., 1971- II. Inter-American Development Bank. Vice Presidency for Sectors and Knowledge. III. Title. IV. Series.
IDB-WP-926

http://www.iadb.org

Abstract[1]

What happens when the tax authority increases enforcement in one tax with the level of compliance in other taxes? The very little evidence available is not conclusive. This paper presents a very simple analytical model that shows the conditions under which spillovers could be positive or negative in the context of uncorrelated taxes for the same individual (a property tax and a self-declaration tax—in this instance, a gross-sale tax). In the model, the sign of the spillover depends on how taxpayers update their beliefs on penalty and detection probabilities for one tax after watching the deterrence actions the tax agency took on another tax. Results from a randomized field experiment show no evidence of negative spillovers: increasing the salience of fines and enforcement probabilities for those who do not comply with the payment of the property tax does not decrease how much the same individual declares on the gross-sale tax. The result has ample implications for researchers bringing interventions to the field and for governments' enforcement strategies. Given that most taxpayers are liable for more than one tax, neither researchers nor authorities can continue to neglect potential spillovers of interventions. Instead, they should design enforcement strategies that maximize compliance across the portfolio of taxes. Importantly, this work indicates that penalties and detection may not be perfect substitutes when the portfolio is taken into consideration.

JEL classifications: H26, C93, D03, H41
Keywords: Tax compliance, Spillovers, Evasion, Property tax, Sales tax, Randomized field experiment, Behavioral economics

[1] We would like to thank the former Mayor and staff from the Municipality of Junín for providing the data, Lucio Castro and the team at CIPPEC for helping with the original data collection and intervention, and for the financial support of the Institutional Capacity Strengthening Fund (ICSF) of the Inter-American Development Bank, funded by the Government of the People's Republic of China. We would also like to thank the discussant and participants at the 2018 IRS-TPC Conference for their comments and suggestions. The opinions presented herein are those of the authors and thus do not necessarily represent the official position of their respective institutions. Authors' emails: López-Luzuriaga, aflopez@gwu.edu; Scartascini, carlossc@iadb.org.

1 Introduction

Does enforcement of one tax create positive spillovers (higher compliance in other taxes) or do taxpayers compensate across taxes to keep their total bill unchanged? While empirical studies of the direct effect of enforcement on particular taxes have blossomed in the last few years, to the best of our knowledge this is the first study that looks in detail at the spillover effect of messages for one tax on the tax declaration of other unconnected taxes in an experimental setting. More specifically, we evaluate the effect of increasing the salience of deterrence (penalties and enforcement) in the property tax on the tax declaration of the gross-sales tax.

The little evidence that exists is mixed. Carrillo, Pomeranz, and Singhal (2017) and Slemrod et al. (2017) show a negative spillover effect across margins of the same tax: taxpayers who are compelled to increase their declared sales tend to compensate for it by increasing their reported costs; thus keeping their total tax bill constant. Ortega and Scartascini (2015) show instead a positive spillover but in the context of tax delinquencies. Taxpayers who receive a claim from the tax authority about unpaid taxes are more likely to pay what they owe in the claimed tax and in other taxes as well.

Could these results be compatible? We explore the answer to this question by using a very simple analytical model á la Allingham–Sandmo that approximates the setting in which we work. Taxpayers face sequential decisions on whether to pay a tax that has neither reporting nor informational asymmetries (property tax), and then how much sales to declare in a self-reporting tax where there are informational asymmetries (gross-sales tax). In that simple setting, the comparative statics are straightforward. If there was an increase in penalties (or perceived penalties), which tend to be uniform across taxes, the spillover would be positive. The effect of an increase in the perceived probability of detection in one tax on the declaration of other taxes depends largely on assumptions regarding how taxpayers update their beliefs about overall enforcement. That is, if taxpayers extrapolated higher enforcement in one tax as applying to the other taxes they owe, spillovers would be positive. However, taxpayers could also assume that given limited resources for the tax administration, higher enforcement in one tax might imply lower enforcement in other

taxes, which could generate negative spillovers (Maciejovsky, Kirchler, and Schwarzenberger, 2007; DeBacker et al., 2015; Advani, Elming, and Shaw, 2017). Adding cash constraints or an overall budget constraint for each taxpayer would reinforce these negative effects.

In our empirical exercise, using data that combines a randomized field experiment for the property tax with administrative data on gross-sales tax declarations, we do not find evidence suggesting the existence of a negative spillover. If anything we find weakly significant evidence that taxpayers who receive a message explaining the consequences of not paying the property tax decide to declare more and pay a higher gross-sales tax. The group that received the deterrence message with their property tax bill increased their gross-sales tax payment, on average, by 2 percentage points more than the control group (which translates into an increase of about 3.4 percentage points in their declared sales). The results are consistent with the model, particularly because the deterrence message in the treatment was focused on increasing the salience of the penalty, which is the same across taxes in this city.

Our suggestive evidence of a spillover effect has several important implications. First, the results and the analytical argument seem to indicate that penalties and detection may not be perfect substitutes once we consider the full tax portfolio. Second, researchers should consider the spillover effect when designing an intervention. Otherwise, they risk losing from other taxes what they may gain from the tax under treatment. This puts an additional burden on the design stage of the intervention, particularly given that it might not have the same effect depending on whether they decide to increase the subjective perception of the penalty or the level of detection. Additionally, when manipulating enforcement, the intervention should explicitly consider how people would update detection probabilities across taxes. Third, given that there are spillover effects, tax authorities should design deterrence strategies taking into account the full portfolio for any given taxpayer. Therefore, the most efficient strategy is not the one that maximizes the direct payoff but the one that maximizes tax collection across the full portfolio.

The rest of the paper is organized as follows. Section 2 presents a literature review. Section 3 presents the model. Section 4 presents an overview of the original intervention and describes the property and gross-sales taxes. Sections 5 and 6 present our empirical strategy and results. Section

7 concludes.

2 Literature Review

There is now ample empirical literature showing that taxpayers who receive a deterrence message from the tax authority tend to react by increasing tax compliance (Slemrod, Blumenthal, and Christian, 2001; Kleven et al., 2011; Fellner, Sausgruber, and Traxler, 2013; Chirico et al., 2015; Brockmeyer, Kettle, and Smith, 2016; Doerrenberg and Schmitz, 2017; Meiselman, 2018). It has also been documented that an increase in monitoring has a positive effect on compliance (LaLumia and Sallee, 2013; Naritomi, 2016; Almunia and López-Rodriguez, 2018).[2] There is also literature supporting the idea that individuals might exhibit sub-optimal behaviors when dealing with taxes (Chetty, Looney, and Kroft, 2009; Abeler and Jäger, 2015). In fact, when taxpayers have limited attention, messages that raise the salience of fines and legal action could increase compliance (Bernheim and Rangel, 2007; Bernheim and Rangel, 2009; Castro and Scartascini, 2015; Chirico et al., 2017). Hence, it is expected that if a taxpayer received a message that underlines the probability of being penalized and explains the calculation of the fine, she would increase her level of compliance.

Within this broad and rapidly expanding empirical literature, studies looking at spillovers are still scarce. We can classify the previous literature into two types of studies: those that explore the effects of tax enforcement across individuals, and those that explore the effects for the same individual but for different margins of the same tax. Only a few studies explore the presence of spillover effects of tax enforcement across individuals. Rincke and Traxler (2011) analyze the effect of licensing inspections on the payment of TV license fees. They take advantage of the fact that inspections are not directly observable for untreated households and look at the spillover effect on their compliance generated by informal communications among neighbors. They used an instrumental variable approach using the intensity of winter as an instrument, because inspectors are paid a fixed fee per visit. Pomeranz (2015) shows that deterrence letters sent to taxpayers

[2]The tax evasion literature is far too vast to be summarized in this paper. For comprehensive overviews of the theoretical literature, see Traxler (2010), Hashimzade, Myles, and Tran-Nam (2013), and Dell'Anno (2009). Luttmer and Singhal (2014) review the literature on the moral determinants of compliance. Hallsworth (2014), Mascagni (2017), and Slemrod (2016) present broad overviews of the use of field and laboratory experiments for increasing tax compliance.

have spillover effects up the value-added-tax chain by generating a paper trail of the transaction. Drago, Mengel, and Traxler (2015) show a substantial spillover effect from treated to "untreated" individuals with results from a field experiment which varied the content of mailings sent to potential evaders of TV license fees. This result has important implications for deterrence policies, given that different individuals generate different spillovers according to the network they belong to. Similarly, Boning et al. (2018a) also show network effects in enforcement that are transmitted through tax-preparer networks, geographic neighborhoods, and parent-subsidiary relationships. Finally, Carrillo, Castro, and Scartascini (2017) find evidence of spillovers across individuals in a setting of positive incentives instead of deterrents. In the context of a program that rewarded individuals who had complied by providing them the construction of a new sidewalk, they find an increase in compliance by the neighbors of the winners. Interestingly, the results are heterogeneous regarding the salience of the sidewalk. This literature provides evidence that spillovers across taxpayers could exist for both deterrents and positive incentives but the sign and size of the spillover are not independent of the design of the intervention.

There is evidence about how a taxpayer behaves across different margins of the same tax. Carrillo, Pomeranz, and Singhal (2017) and Slemrod et al. (2017) make the case that when the tax authority signals having third-party information on transactions, taxpayers tend to increase their reported revenues, but these taxpayers largely offset increased reported revenues with increased reported expenses. The same phenomenon of compensating higher taxes in one margin by decreasing their reporting in another is reported by Boning et al. (2018b). In this case, subsidiaries of treated firms remit less tax, which is consistent either with a cash-flow effect or substitution of noncompliance to a seemingly less monitored report. There are also a couple of studies that look at the effect of enforcement for the same individual and the same tax over different periods of time (Kleven et al., 2011; Advani, Elming, and Shaw, 2017). Kleven et al. (2011) select a sample of 40,000 income tax filers in Denmark, half of whom were audited. The following year, they randomly sent a threat-to-audit letter to taxpayers who had previously been audited and taxpayers who had not been. They find that the audit and the threat of an audit decrease evasion on the self-reported income. Advani, Elming, and Shaw (2017) find a similar result when studying the random audit

program in the United Kingdom over five years. They find that the audit is more effective and more lasting on sources of income that are self-reported and less volatile over time.

Our research is different from that described above, because we look at the effect of an intervention on the same individual but across different taxes. To the best of our knowledge, the only field experiment that shows some evidence regarding spillovers for an individual taxpayer across taxes is Ortega and Scartascini (2015). Taxpayers who received a notice from the tax authority regarding owed taxes for the income tax, wealth tax or VAT tended to show a higher probability of canceling debts in other taxes too. Our study differs from theirs for two reasons: their focus is on tax delinquency and on taxes that are mechanically connected through sources of income and wealth. Instead, we analyze compliance more broadly and among taxes that are entirely independent of each other. Our work also contributes to the literature on risk perception. Bérgolo et al. (2018) present evidence that taxpayers overestimate the probability of being audited on an income tax. We present a possible explanation for this phenomenon.

In summary, the evidence for an individual taxpayer so far indicates that: i) deterrence messages that increase the salience of penalties and the stringency of enforcement in one tax increase compliance with that tax and ii) spillovers could be positive or negative. Under some conditions, there seem to be positive spillover effects. In other cases, taxpayers try to maintain the overall tax bill constant and adjust their tax declarations accordingly. How much they compensate seems to be correlated to their evaluation of the ability of the tax agency to enforce other taxes or other margins of the same tax (e.g., to evaluate sales and costs at the same time, or to audit the parent firm and its subsidiaries).

3 A Simple Analytical Model

We analyze the effect on the gross-sales tax of an intervention designed to test the determinants of compliance with the property tax. In order to understand the conditions under which spillovers could be positive or negative, we develop a very simple analytical model á la Allingham–Sandmo that approximates the setting in which we work. Within our model, taxpayers face sequential decisions

about whether to pay a tax that has no reporting and informational asymmetry (property tax) and then how much sales to declare in a self-reporting tax where there are informational asymmetries (gross-sales tax).

Before receiving the message on the property tax bill, the individual has prior beliefs regarding both the probability that the penalties for not paying the tax will be enforced, and the amount of the penalty. Upon receiving the property tax bill, the taxpayer updates either or both of those beliefs. A few days after receiving the bill, the taxpayer decides whether or not to pay, and a few weeks later, she decides how much sales to declare and whether or not to pay the gross-sale tax.

We analyze the decisions of the individual in the same sequence in which she faces them. In the first stage, she decides whether or not to pay the property tax, and in the second, how much sales to declare. We are assuming a risk-averse individual who is not credit-constrained. She has some wealth and enough money to pay both taxes, and her business is producing some profit. If the individual is credit-constrained, there will be an additional channel that allows the enforcement of one tax to affect the other.

First Stage: The individual decides whether or not to pay the property tax. She has an initial level of wealth W and has to pay a tax of amount T. The utility when paying the tax is $U(W-T)$. If she decides not to pay, her expected utility is $P_r(E)\,U(W-\theta T)+(1-P_r(E))\,U(W)$, where $P_r(E)$ is the (perceived) probability that the city government enforces the penalties of not paying the property tax, and is a function of the overall perception regarding the enforcement capacity of the tax authority (E). For instance, if the government increased its personnel or receives more funding for tax control, E would increase and so would P_r ($\frac{\partial P_r}{\partial E}>0$).

If the government enforces the payment of the fine, the individual has to pay a fine θ in addition to the billed tax. The solution can easily be interpreted according to Figure 1. She pays the tax as long as the expected utility of paying is higher than the expected utility of not paying. For a perceived fine of size θ, the taxpayer will pay the tax if she assumes that the probability of enforcement is equal to or higher than P_r. An increase in the perceived amount of the fine will make the option of paying more attractive. Notice that for a higher fine ($\theta<\hat{\theta}$) the utility of not paying is lower for any probability of enforcement. Now, if the perceived fine goes up to $\hat{\theta}$

$(>\theta)$, then taxpayers with perceived probabilities between $\hat{P}_r$ and P_r will also decide to pay the tax. Therefore, if the tax authority is able to affect the perceived fines or the perceived probability of enforcement, it can increase tax compliance.

Second Stage: In the second stage we use a traditional Allingham-Sandmo (A-S) model with a risk-averse individual with an increasing concave utility function. The individual maximizes the expected utility by choosing how much income to report. For simplicity we assume that the only cost for the business is the tax. The individual's true sales are y and the reported sales are $\tilde{y}$. The reported sales are taxed at a rate t. The probability of being caught under-reporting sales is P_s, which is a function of the overall perception of the city government's enforcement capacity (E), and a function of the enforcement in other taxes (P_r in this case). The reason is quite simple. Resources are limited, so a higher enforcement in one tax might imply lower enforcement in another. Assuming fixed overall resources is relatively standard (see Ortega and Scartascini, 2015 for a discussion). If caught cheating, the taxpayer has to pay the tax t plus a penalty θ. The individual maximization problem can be written as:

$$\underset{\tilde{y}}{max} : (1 - P_s(E, P_r(E)))\, U(y - t\tilde{y}) + P_s(E, P_r(E))\, U(y - t\tilde{y} - \theta t(y - \tilde{y}))$$

For notation convenience $X = y - t\tilde{y}$ and $\hat{X} = y - t\tilde{y} - \theta t(y - \tilde{y})$. The first order conditions can be written as:

$$-t(1 - P_s(E, P_r(E)))\, U'(X) \quad + \quad tP_s(E, P_r(E))\, U'(\hat{X})(\theta - 1) \quad = \quad 0$$

Since the utility function is concave the second order conditions are satisfied:

$$D = t^2(1 - P_s(E, P_r(E)))\, U''(X) \quad + \quad t^2 P_s(E, P_r(E))\, U''(\hat{X})(\theta - 1)^2 \quad \leq \quad 0$$

In this simple setting, comparative statics are straightforward: if there is an increase in penalties (or of perceived penalties), which tend to be uniform across taxes, then the spillover is positive. Differentiating the first order conditions with respect to θ and solving for $\frac{\partial \tilde{y}}{\partial \theta}$:

$$\frac{\partial \tilde{y}}{\partial \theta} = \frac{-t\left[P_s(E,P_r(E))U'(\hat{X})\right]}{D}$$

$$\text{sign}\left[\frac{\partial \tilde{y}}{\partial \theta}\right] = \text{sign}\left[tP_s\left(E,P_r\left(E\right)\right)U'(\hat{X})\right]$$

$$\frac{\partial \tilde{y}}{\partial \theta} > 0$$

The effect of an increase in the perceived probability of detection in one tax upon other taxes strongly depends on the assumptions about how taxpayers update their beliefs regarding overall enforcement. Differentiating the first order conditions with respect to $P_r\left(E\right)$ and solving for $\frac{\partial \tilde{y}}{\partial P_r(E)}$:

$$\frac{\partial \tilde{y}}{\partial P_r(E)} = \frac{-1}{D}\left[tU'(X) + tU'(\hat{X})\left(\theta - 1\right)\right]\frac{\partial P_s(E,P_r(E))}{\partial P_r(E)}$$

$$\text{sign}\left[\frac{\partial \tilde{y}}{\partial P_r(E)}\right] = \text{sign}\left[\frac{\partial P_s(E,P_r(E))}{\partial P_r(E)}\right]$$

If the individual assumes that $P_r\left(E\right)$ and $P_s\left(E,P_r\left(E\right)\right)$ are uncorrelated, then $\frac{\partial P_s(E,P_r(E))}{\partial P_r(E)} = \frac{\partial \tilde{y}}{\partial P_r(E)} = 0$.

If he assumes that the city government is monitoring him as an individual and not with regards to a particular tax $P_r = P_s$, then $\frac{\partial P_s(E,P_r(E))}{\partial P_r(E)} = 1$ and $\frac{\partial \tilde{y}}{\partial P_r(E)} > 0$. That is, if taxpayers extrapolate the higher enforcement of one tax to the other taxes they owe, spillovers would be positive.

However, taxpayers could also assume that given limited resources for the tax administration, higher enforcement of one tax might imply lower enforcement of other taxes, which could generate negative spillovers (Mittone, 2006; Maciejovsky, Kirchler, and Schwarzenberger, 2007; DeBacker et al., 2015). In particular, $\frac{\partial P_s(E,P_r(E))}{\partial P_r(E)} = -1$ so $\frac{\partial \tilde{y}}{\partial P_r(E)} < 0$. Adding cash constraints or an overall budget constraint would reinforce these negative effects.

Alternatively, our situation could also be analyzed in a mental accounting framework. The taxpayer could have decided her total expenditure on taxes ex-ante, so an increase in the payment of one tax could translate to a decrease in the payment of another. There is some literature that describes compliance decisions on a single tax in this loss-aversion framework. Engström et al. (2015) and Rees-Jones (2018) both analyze the manipulation decision of the income tax declaration

and show that taxpayers are more likely to manipulate their tax declaration when they face a loss (tax due) than when facing a gain (refund). In our context, consider an individual who calculates her total balance due to all taxes and decides to which evasion opportunities to take. If she decided initially not to pay the property tax and after a government action changed her mind, she would be in a loss region with respect to the original balance due. Once in the loss region, the taxpayer might be more willing to evade the sales tax than she was before paying the property tax. However, all the evasion opportunities available to the taxpayer carry a cost that not only includes the cost of evasion (e.g., cost of disclose income, hiring an accountant that would do so and so on), but also includes the fines and penalties the taxpayer would have to pay if the tax authority penalized the evasion. Again, if the government enforces the fines for evading one tax, the effect of that intervention on the other tax would depend on the assumptions the taxpayer makes about the control process across taxes and how those would affect the relative cost of each evasion opportunity.

4 Background and Data

Castro and Scartascini (2015) conducted a large field experiment designed to test the determinants of compliance with the property tax in the Municipality of Junìn in Argentina. The property tax, formally called the "Public Space Conservation Tax" (Tasa de Conservación de la Vía Pública, or CVP henceforth), is a tax levied on homes, farms, business premises, and most other real estate. The tax is calculated by the city government and is billed every two months to the property owner. The tax is computed according to the size of the property's frontage and the services the city provides, such as public lighting, trash collection, and street cleaning. Because the tax is billed by the city, there is no reporting and there are no informational asymmetries between the government and the taxpayer. The taxpayers' only choice is whether or not to pay the billed amount, which becomes known to the city government after the due date. Taxpayers have approximately 10 days to pay from the moment they receive the bill. A cumulative compound monthly interest rate of 2 percent is applied to any outstanding liabilities. By August 2011, there were around 26,000 individual taxpayers registered to pay the CVP, equivalent to a third of the population of Junín, according to

the last Argentine census, in 2010. The Municipality allows taxpayers to pay on a yearly or monthly basis. However, only around 12 percent of taxpayers choose either of these options; the rest pay every other month by default. For the experiment, the authors included only individual taxpayers in the sample and dropped firms and corporations. This is exactly the framework of the first stage of our model.

A group of individual taxpayers who pay the property tax are also liable for a gross-sales tax that is administered by the same municipality. The gross-sales tax is paid by all retail, wholesale, service and industrial businesses in the city. The gross-sales tax is formally called the "Safety and Hygiene Inspection Tax" ("Tasa por Inspección e Higiene", or SEH henceforth). The tax is calculated based on the gross monthly sales, the number of employees and the size of the establishment where the economic activity is undertaken (a description of these variables can be found in the Appendix). The tax rate depends on the economic activity (see the Appendix for the specific rate). Each taxpayer must report their sales once a month, and the number of employees and the size of the establishment once a year. Hence, within a calendar year, the tax has both a fixed and a variable component. Although the municipality allows taxpayers to pay monthly, only 11 percent of taxpayers do so; the rest pays every two months. In this tax, there are informational asymmetries: sales are only known to the taxpayer; hence, misreporting is possible. If a business owner fails to fill in the monthly form, it is assumed that the sales were the same as the previous month and taxpayers are fined a penalty of AR$250 (equivalent to 7 percent of the monthly minimum wage) for not filling in the form on time. If a tax form is filled afterwards and the reported sales are higher than those of the previous month, the difference must be paid plus a penalty of 2 percent compound monthly interest. In contrast, if the reported sales are lower than the sales of the previous month, the taxpayer does not receive a tax credit or a refund for the extra tax that was paid. As such, while there could be incentives for misreporting the actual sales, there are little incentives for not filing the sales declaration form. In this tax, the relevant evasion margin for taxpayers is how much sales to declare, which is not known by the tax authority. In contrast, the municipality knows whether taxpayers file the form and pay the assessed tax on time, making it easily enforceable.[3] By August 2011, there were around

[3]In this case; only sales matter, therefore, taxpayers can not offset their liability by increasing costs or claiming any deductions.

2,500 individual taxpayers registered to pay the SEH; most taxpayers owning only one business, and just 3 percent owning more than one business. The median payment was AR$98 (equivalent to 2.7 percent of the monthly minimum wage).

The payment scheme is very similar for both taxes. Most taxpayers pay every two months, and there are two due dates for each tax. The first due date is usually in the second week of the month and the second due date takes place the following week. Taxpayers are supposed to pay by the first due date, but if they pay by the second due date, no late fees are charged. The CVP is paid in the first month of each calendar bimester, and the SEH is paid in the second month of the bimester. For instance, in the fifth bimester of the year (September and October), the CVP is due in September, and the SEH is due in October.

In Castro and Scartascini (2015), approximately 23,000 taxpayers were randomly divided into four groups: three treatment groups and one control group. A message was included on the property tax bill of each treatment group. The messages were designed to test the main determinants of tax compliance: deterrence (beliefs about enforcement and fines), peer effects (beliefs about other taxpayers' behavior), and reciprocity (beliefs about the use of resources by the government). Private companies, social organizations, and taxpayers who paid their dues annually were excluded from the sample. A stratified randomization strategy based on the geographic location was made to select the taxpayers for each treatment. Within each block, one taxpayer was assigned to the control group for each taxpayer randomly assigned to a treatment, so that 60 percent of taxpayers were randomly assigned to the control group, and the remainder were equally distributed to each of the treatment groups. More details about the randomization can be found in Castro and Scartascini (2015). The results in Castro and Scartascini (2015) show that the deterrence message increased compliance with the property tax by almost five percentage points, which represents an increase in compliance rates of approximately 12 percent.

In this paper, we combine the deterrence message sent to property owners with data from the SEH. We end up with a small subsample of taxpayers who own property and are sole proprietors of a business. We have 608 sole proprietors in the control group and 115 in the treatment group.[4]

[4]We only look at those who received the deterrence treatment for several reasons. First, while we have an analytical framework we can use as a benchmark for the deterrence message, we have no predictions for the other two messages.

This subgroup of taxpayers was not the focus of the original experiment, yet the randomization was successful in balancing this subgroup of taxpayers between treatment and control (Table 3). On average, the annual sales of these businesses in 2010 was $226,380 Argentinean pesos and in the billing period before the treatment period (July - August 2011) they paid on average $ 111 Argentinean pesos.[5]

The deterrence message sent in the property tax bill had two components. First, one component that tried to increase the salience of the penalty and reduce the computational cost: *"Did you know that if you do not pay the CVP on time for a debt of AR$1,000 you will have to disburse AR$268 in arrears at the end of the year?"* The objective of including the example of the cost of noncompliance was to reduce the computational costs derived from the calculation of arrears on unpaid tax liabilities using a compounded interest rate. According to the literature, such a message should increase the salience of the penalty (Chetty, Looney, and Kroft, 2009; Congdon, Kling, and Mullainathan, 2011; Luttmer and Singhal, 2014).[6] The second component highlighted the additional consequences that the individual might face for not paying: *"and the Municipality can take administrative and legal action."* This message was accompanied with an image of a gavel, which intensified the idea of the penalty (see Table 1 for the message included in the tax bill and Figure 3 for an example of a tax bill).

Regarding data, we have access to the declared SEH tax for each taxpayer for each two-month period in 2011, and the information from the 2010 annual tax return which includes total annual sales for 2010, the number of employees in 2010 and the size of the building in meters.

Second, we only have a few people in the other two treatment groups. Finally, the samples for those groups are not balanced.

[5]We reproduce the analysis of Castro and Scartascini (2015) in the subgroup of sole proprietors (Table A3). Given that the differences in sample size are substantial (23,000 to 700) it is expected that results might differ across exercises. Power calculations for the small sample require large differences between control and treatment. The differences in Castro and Scartascini (2015) were around 5 percentage points. The differences in "paid" are between 2 and 6 percentage points –which are similar to the results in the original paper but are not significant. The results at the deadlines are a bit different. There is a decrease in the compliance rate by the first due date of around 9 percentage points, and an increase of similar magnitude by the second due date. This result is not entirely surprising. This subgroup of taxpayers faces a different budget constraint by having to pay two municipal taxes.

[6]We have anecdotal evidence from focus groups showing that taxpayers' reactions to the information that they have to pay a monthly compound interest of 2 percent and this alternative way of presenting the same information is quite different. While taxpayers dismiss the 2 percent figure, they become highly concerned when presented with the example.

5 Empirical Strategy

As we described in the previous section, we can exploit the assignment to treatment in Castro and Scartascini (2015) to compare the effect of receiving a deterrence message printed on the bill of the property tax (CVP) on the declaration of the gross-sales tax (SEH). It is important to notice that several factors affect the precision of our estimation. First, we have a relatively small treatment group, because the intersection of individuals owning property and having to pay the gross-sales tax is relatively small. Second, we cannot observe reported sales directly, but only the declared tax. The gross-sales tax (T_{gs}) is computed by adding a tax rate (t_s) times the declared sales ($\tilde{y}$), a tax rate (t_e) determined by the number of employees (declared the previous year) times the municipal wage, and a tax rate (t_m) determined by the square meters of the establishment (declared the previous year) times a price-per-meter, determined annually by the city government. The first element (declared sales) is the only one that varies within a fiscal year. Consequently, while we cannot observe our variable of interest directly, we can safely assume that a change in the reported tax in any specific period within a calendar year reflects a change in the reported sales. Because declared sales affect only a fraction of the estimated tax, it scales the overall effect down. For the average taxpayer, a 10 percent change in declared sales implies a 6 percent change in declared tax. Finally, there is a minimum tax that applies to all taxpayers whose sales are below a certain threshold; that is ($T_{gs} = max\left\{T_{gs}^{min}, T_{gs}\left(\tilde{y}, ...\right)\right\}$). This minimum tax is binding for a large fraction of taxpayers. Therefore, the actual distribution of the tax looks truncated compared to what it would have been absent the minimum. As such, because we can not observe declared sales directly, we can not observe the treatment effects on declared sales in the lower part of the distribution (see Figure 2). Still, we can observe and measure well the effect of the intervention on actual tax revenues given the tax code.[7] The minimum tax was updated according to inflation every four months. From January to April the minimum tax was AR$89.25, from May to August was AR$92.82, and from September was AR$96.56. All of these factors should work against finding positive results.

We calculate the minimal detectable effect with our sample size and data structure for a signif-

[7]Given that we are observing and using declared tax as the dependent variable, it is still appropriate to estimate the model using OLS. A Tobit estimation would overestimate the effect of the intervention on declared taxes. However, if we had declared sales, a Tobit model would be more appropriate.

icance level of 5 percent and a power of 0.8. The minimal detectable effect for an OLS estimation with $ln\,(tax)$ as outcome is 20 percentage points, which is much higher than any result found in the literature. As such, it would be very difficult to find any significant result in such a setting. The MDE becomes more reasonable if we consider instead the first difference of the outcome variable, which becomes our estimation of choice. The power calculations are included in the Appendix (Equation 2 and Table A2).

To address the challenges generated by the data limitations, including the fact that the original randomization was done in a different and larger sample of taxpayers, our main specification is a difference-in-difference estimator. The difference-in-difference design allows us to compare the treatment group over time by controlling the time trend and taking advantage of the panel nature of our data. We estimate the following equation

$$y_{it} = \alpha_0 + \alpha_1 T_d + \gamma t_{Sep/Oct} + \delta D_{it} + X'_{it}\beta + \varepsilon_{it} \tag{1}$$

where the variables are defined as follows. y_{it} is the variable of interest, the log of the gross-sales reported tax. T_d is one if the taxpayer received the deterrence letter for the property tax. $t_{Sep/Oct}$ is the time fix effect equal to one for the fifth bimester (Sep/Oct) and zero from the fourth bimester (Jul/Aug). D_{it} is the difference-in-difference estimator (interaction of T_d and $t_{Sep/Oct}$). X'_{it} is a vector of controls that include characteristics of the business such as: the annual sales of the previous year, economic sector, dummies for the number of employees and size of the store that correspond to the categories that are used to calculate the tax, the age of the firm and the gender of the owner. Following Castro and Scartascini (2015), because compliance is highly geographically clustered, we also include the blocks fix effect and we cluster the standard errors by the same blocks. As also discussed in the original paper, compliance display high persistence, so we include a lagged outcome variable.[8]

[8]In Castro and Scartascini (2015), the probability of paying in period t given that the taxpayer had paid in t_1 is close to 100 percent. Similarly, Dwenger et al. (2016) find that those who evaded in 2010 were 87 times more likely than others to evade in 2011. Adding a lagged outcome in a panel could bias the estimator, but it is not the case in our estimation because the treatment assignment was random, so it is uncorrelated with the outcome of the previous period. Including lagged dependent variables can generate a bias estimator because the residual is correlated with the lagged dependent variable. However, the treatment was randomly assigned, so the β estimator is consistent because the $cov\,(y_{it-1}, D_{it}) = 0$. A discussion of the problem of fixed effect estimators and lagged variables can be found

6 Results

The main question of this paper is whether enforcement in one tax creates positive or negative spillovers in other taxes. The evidence coming from the intervention we evaluate seems to reject the hypothesis that taxpayers reduce compliance with other taxes. If anything, the evidence seems to be suggestive (although tentatively) of a positive spillover. In particular, we find in a difference-in-difference estimation, that the treatment group increases its reported tax on average by 2 percentage points more than the control group (Table 4). The coefficient is stable across specifications with different control variables. In addition to running several different specifications to check the stability of results, we also run a placebo regression for the period before the intervention took place, and we find no effect (Table A4).[9]

While in the gross-sales tax the relevant margin of decision is how much sales to declare, for completeness we also look at the probability of paying the tax by the due date. In no case do we find a negative effect. If anything, there is a slightly significant effect on payment by the second due date in the difference-in-difference estimation (Table 5). The magnitude is between 6 and 9 percentage points depending on the estimation. This effect is equivalent to an increase of 35 percent in compliance. Again, we find no significant effect for the cross-section analysis.

In summary, given the tentative evidence that we have presented, we can conclude that it is highly unlikely that there was a negative spillover effect. If anything, it would seem that those taxpayers who received the treatment in the property tax declared more and were more likely to pay their gross-sales taxes than those in the control group. Again, it is important to notice that our results may well be underestimating the true results for several reasons. First, the tax is computed according to the declared sales over a two-month period. Most of those in the treatment group could have received the message after the first month's declaration. Thus, the change in declaration

in Bertrand, Duflo, and Mullainathan (2004), Imbens and Wooldridge (2008), and Angrist and Pischke (2008). The derivation of the formula for β is in the Appendix.

[9] If we only compare the treatment and control group only in the post-treatment period (Table A5), the coefficient is again quite stable and positive across specifications, and of similar magnitude—to compare the OLS specification with the difference-in-difference specification, the coefficients of the treatment and the treatment times the period should be added—but in the cross-section estimation, the difference is not statistically significant, probably due to our small sample size as expected (see the power calculation in Table A2). Again, no results exist in a placebo exercise (Table A6).

might be one half of what it could otherwise have been. Second, the declared tax—the variable we observe—is only partially affected by the level of declared sales, which also reduces the size of the estimates. Third, many of the taxpayers pay the minimum tax; if there is any effect in this group, we may be unable to observe their response.

7 Conclusion

The empirical literature on tax compliance has grown exponentially in the last few years. Greater access to administrative data, a better predisposition of authorities toward impact evaluations, and the relatively low cost of behavioral interventions have made this possible. However, most of this literature has focused almost exclusively on the direct effect of the interventions. However, as we indicate in this paper, spillovers are possible. Under some conditions, spillovers could be negative, which may reduce or completely compensate for the impact of the intervention. Spillovers could also be positive, which would enhance the impact of the intervention.

In this paper, we find no negative spillover when focusing on the effect of deterrence on the property tax on the declaration of sales in the gross-sales tax. If anything, even though we have some important limitations with the data, there seems to be a weak positive spillover effect. This result is in line with a simple analytical model that predicts that the size and sign of the spillover depends on: (i) the effect of the deterrence message on the salience of the penalty, and (ii) the effect of the deterrence message on how people evaluate the ability of the government to enforce several taxes at the same time. If taxpayers think that enforcement in one tax implies higher enforcement in all taxes, spillovers will most likely be positive. If taxpayers think that higher enforcement in one tax implies lower enforcement in other taxes because resources are limited, then spillovers should be zero or negative. Cash or financial constraints could exacerbate the negative spillover. In this way, we can explain the differences in the results found in Ortega and Scartascini (2015), and Carrillo, Pomeranz, and Singhal (2017) and Slemrod et al. (2017).

Given that most taxpayers are liable for more than one tax, tax authorities should design their control strategies taking into account the possible spillover effect across taxes as well as the fact

that penalties and detection may not be perfect substitutes. Moreover, tax authorities should be mindful of the signal that their enforcement strategy sends. If taxpayers evaluate that the resources of the tax authority are limited, then increasing detection in one tax may lead to reductions in compliance with others. In any case, ignoring the interconnections of compliance across taxes is inadvisable.

This study is also a cautionary tale for optimal tax policy design. Taxpayers who are liable for several taxes might be different from taxpayers who are not, regarding risk perception and budget constraints. In order to get a full picture of the effects of any intervention, it is important to analyze the taxes that are not the main target of the intervention as well. Researchers should also be well aware of this when designing the intervention to make sure that if spillovers are possible, they do not have a negative effect.

Finally, it is important to note that this paper raises several important points regarding the analytical determinants of spillovers and the impact they can have in actual interventions. Still, this is not the last word but only a foundation on which future studies should build. More sophisticated models that take into account other taxes and strategies as well as empirical papers that have fewer data constraints than this one are encouraged.

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8 Figures

Figure 1: Whether or not to Pay the Property Tax

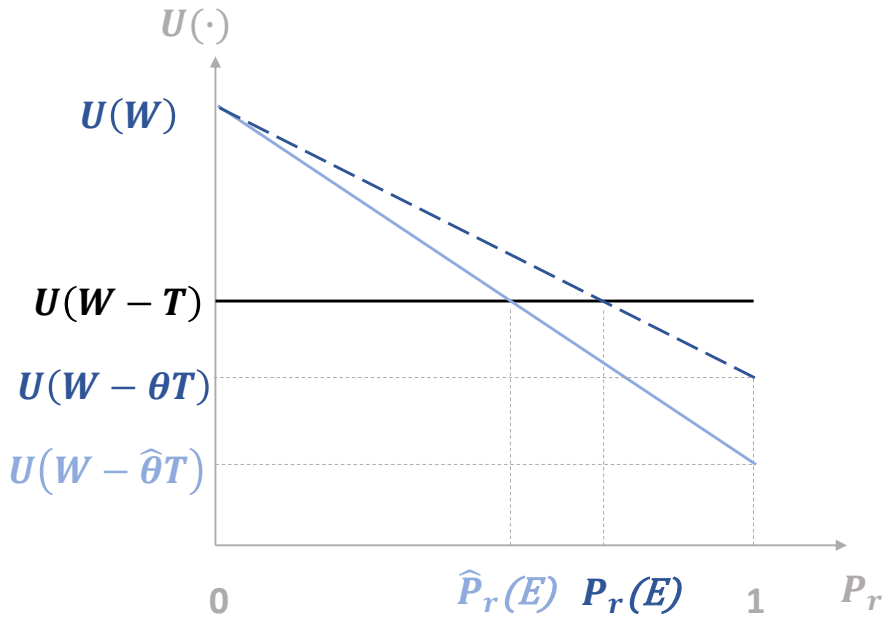
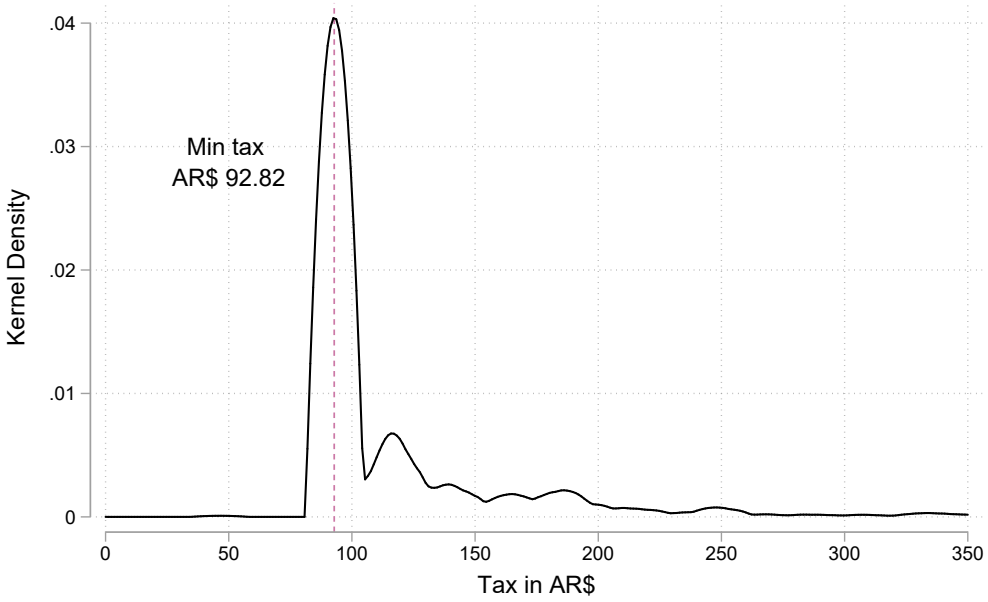


Figure 2: Distribution of the Tax Before the Treatment Period (Aug-Jul Bim 4)



The top 5% of the observations are excluded from the graph.

Figure 3: Sample Tax Bills with Treatment Messages (in Spanish)

9 Tables

Table 1: Message Included in the Property Tax Bill

Message / Group	**Text**	**Image**
Deterrence	Did you know that if you do not pay the CVP on time for a debt of AR$ 1,000 you will have to disburse AR$ 268 in arrears at the end of the year and the Municipality can take administrative and legal action?	
Control	No message	No image

Table 2: Descriptive Statistics of Sole Proprietors Pre-Treatment Period (Jul/Aug)

	Retail Sector		Other Sectors		Total	
Mean annual sales 2010 in AR$1,000	274.01	(542.67)	145.59	(290.33)	226.36	(469.16)
Mean number of employees 2010	0.55	(1.10)	0.61	(1.43)	0.57	(1.23)
Mean indoor space in square meters 2010	71.63	(97.99)	131.65	(164.47)	93.90	(129.95)
Mean SEH tax AR$	110.67	(41.58)	113.55	(39.44)	111.74	(40.79)
Percent paid SEH by 1st due date	0.27	(0.45)	0.32	(0.47)	0.29	(0.45)
Percent paid SEH by 2nd due date	0.16	(0.37)	0.13	(0.33)	0.15	(0.36)
Percent Paid SEH in Full	0.70	(0.46)	0.65	(0.48)	0.68	(0.47)
Percent of owners who are men	0.66	(0.47)	0.83	(0.38)	0.72	(0.45)
Mean number of years of the firm	13.12	(10.97)	17.08	(11.17)	14.59	(11.20)
N	417		246		663	

Monetary amounts are in Argentine Pesos (AR$). Standard errors are in parentheses.

Table 3: Balance Test Pre-Treatment Period (Jul/Aug)

	Difference: Deterrence		Control Group		N
Ln Tax SEH	0.106	(0.089)	4.817***	(0.026)	723
Ln Tax SEH excluding outliers (1%)	0.036	(0.035)	4.706***	(0.015)	694
1 if retail sector	0.014	(0.051)	0.638***	(0.024)	723
1 if industry	-0.036***	(0.009)	0.044***	(0.007)	723
Annual sales 2010 in AR$1,000	36.292	(53.967)	220.454***	(25.439)	669
Num. of employees 2010	0.278	(0.165)	0.532***	(0.058)	669
Num. of proprietors working 2010	0.036	(0.024)	1.002***	(0.004)	669
Indoor space m2	22.520	(13.920)	91.085***	(6.762)	669
Outdoor space m2	3.010	(3.551)	4.666***	(1.189)	669
Paid SEH by 1st date	0.034	(0.042)	0.288***	(0.027)	723
Paid SEH by 2nd date	-0.012	(0.029)	0.151***	(0.009)	723
Paid SEH in Full	0.028	(0.034)	0.680***	(0.022)	717
Paid CVP by 1st date	0.014	(0.042)	0.334***	(0.032)	723
Paid CVP by 2nd date	-0.019	(0.031)	0.150***	(0.018)	723
Paid CVP in Full	0.055	(0.054)	0.597***	(0.039)	723
Num. lights	0.018	(0.153)	2.955***	(0.101)	723
Manual Sweeping	-0.014	(0.059)	0.414***	(0.077)	723
Mechanical Sweeping	-0.008	(0.066)	0.408***	(0.066)	723
Ln front to street	0.007	(0.067)	2.555***	(0.038)	723
1 if paid CVP monthly	-0.005	(0.003)	0.005	(0.003)	723

Each row shows a regression of the variable on the treatment. Monetary amounts are in Argentine Pesos (AR$). Standard errors are clustered at the block level and in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effect of the Deterrence Letter on the Reported Tax

Dependent Variable: Ln of SEH Tax

	(1)	(2)	(3)	(4)	(5)	(6)
T: Deterrence	-0.016	-0.014	-0.012	-0.013	-0.014	-0.012
	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)
After (t: Sep/Oct)	0.028***	0.031***	0.031***	0.031***	0.031***	0.031***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
T: Deterrence x after	0.022	0.021*	0.020*	0.022	0.021*	0.020*
	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)
Ln Tax SEH t-1	0.990***	0.897***	0.880***	0.931***	0.901***	0.884***
	(0.002)	(0.032)	(0.038)	(0.027)	(0.034)	(0.041)
1 if paid the min tax t-1				-0.005	0.009*	0.010*
				(0.005)	(0.004)	(0.004)
Annual sales 2010 100,000 AR$				0.001	0.001	0.001
				(0.001)	(0.001)	(0.001)
1 if owner is male				0.004	0.004	0.003
				(0.004)	(0.005)	(0.004)
Age of firm Jan 2012 in years				-0.000	-0.000	-0.000
				(0.000)	(0.000)	(0.000)
Constant	0.049***	0.444**	0.509**	0.322*	0.416**	0.480**
	(0.011)	(0.135)	(0.159)	(0.129)	(0.143)	(0.168)
N	1,433	1,326	1,326	1,326	1,326	1,326
Blocks FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Dummies	No	Yes	Yes	No	Yes	Yes
Sector Dummies	No	No	Yes	No	No	Yes

Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. In specifications from three onwards, we include dummies for the economic sector, and from four to six we include dummies for the bins of the tables of the number of employees and the size of the store in square meters. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of the Deterrence Letter on the Probability of Paying Each Tax

Tax: In the Title of each Column

	(1)	(2)	(3)	(4)
	Property	Sales	Property	Sales
T: Deterrence	0.056	0.023	0.003	0.002
	(0.048)	(0.031)	(0.021)	(0.018)
After (t: Sep/Oct)	0.006	0.001	0.021	-0.003
	(0.009)	(0.008)	(0.014)	(0.013)
T: Deterrence x after	-0.001	0.013	0.011	0.013
	(0.016)	(0.025)	(0.026)	(0.028)
N	1,445	1,435	1,433	1,312
Blocks FE	Yes	Yes	Yes	Yes
Paid t-1	No	No	Yes	Yes
Controls	No	No	Yes	Yes
Period	J/A-S/O	J/A-S/O	J/A-S/O	J/A-S/O

The dependent variable takes the value 1 only if the taxpayer had paid in full the total tax liabilities for the period of the experiment. The tax is identified in the header. Controls are dummies for the sector, indicators for having paid the minimum tax in the previous period, variables from the annual declaration of 2010 (annual sales, dummies for the bins of the tables of the number of employees and the size of the store in square meters), age of the firm in years and gender of the proprietor. Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

A Appendix

SEH Tax Definitions and Tables

The SEH tax has three components that correspond based on the gross-sales, on the number of employees and on the building size.

$$T_{SEH} = T_{Bim}^{sale} + T_{y-1}^{employees} + T_{y-1}^{buldingsize}$$

T_{Bim}^{sale} is calculated by multiplying the total sales of the two-month period by the tax rate. $T_{y-1}^{employees}$ is the result of the product the tax rate determined by number of employees (paid or unpaid) who worked last year for the businesses, times the city government administrative wage. $T_{y-1}^{bulding\,size}$ is the tax rate determined by the indoor space and half of the outdoor space in square meters reported last year, times the cost of a meter of construction. The tax rates according to economic activity and size are described in table A1. The city government determines the administrative wage and the cost of a meter of construction in the city by January of each year.

Table A1: Brackets for the Components of the Gross-Sales Tax

Volume of Sales							
Range	0 to $6,000	$6,001 to $10,000	$10,001 to $18,000	$18,001 to $30,000	$30,001 to $80,000	$80,001 to $150,000	Higher than to $150,001
Industry							
Food	0.136%	0.190%	0.285%	0.456%	0.798%	1.556%	3.423%
Goods	0.114%	0.160%	0.240%	0.384%	0.672%	1.310%	2.882%
Other	0.125%	0.175%	0.263%	0.421%	0.737%	1.437%	3.161%
Whole commerce							
Food	0.125%	0.175%	0.263%	0.421%	0.737%	1.437%	3.161%
Goods	0.105%	0.147%	0.221%	0.354%	0.620%	1.209%	2.660%
Other	0.115%	0.161%	0.242%	0.387%	0.677%	1.320%	2.904%
Retail							
Food	0.109%	0.153%	0.230%	0.368%	0.644%	1.256%	2.763%
Goods	0.091%	0.127%	0.191%	0.306%	0.536%	1.045%	2.299%
Other	0.100%	0.140%	0.210%	0.336%	0.588%	1.147%	2.523%
Services							
Personal	0.100%	0.140%	0.210%	0.336%	0.588%	1.147%	2.523%
Others	0.091%	0.127%	0.191%	0.306%	0.536%	1.045%	2.299%
Number of employees							
Range	1	2 to 3	4 to 7	8 to 15	16 to 30	31 to 100	More than 101
Industry							
Food	5.924%	7.109%	9.597%	14.396%	23.034%	39.158%	72.442%
Goods	5.129%	6.155%	8.309%	12.464%	19.942%	33.901%	62.717%
Other	5.386%	6.463%	8.725%	13.088%	20.941%	35.600%	65.860%
Whole commerce							
Food	4.937%	5.924%	7.997%	11.996%	19.194%	32.630%	60.366%
Goods	4.274%	5.129%	6.924%	10.386%	16.618%	28.251%	52.264%
Other	4.488%	5.386%	7.271%	10.907%	17.451%	29.667%	54.884%
Retail							
Food	4.114%	4.937%	6.665%	9.998%	15.997%	27.195%	50.311%
Goods	3.562%	4.274%	5.770%	8.655%	13.848%	23.542%	43.553%
Other	3.740%	4.488%	6.059%	9.089%	14.542%	24.721%	45.734%
Services							
Personal	3.740%	4.488%	6.059%	9.089%	14.542%	24.721%	45.734%
Others	3.400%	4.080%	5.508%	8.262%	13.219%	22.472%	41.573%
Surface in Square Meters							
Range	0 to 40	41 to 60	61 to 90	91 to 120	81 to 120	501 to 1,500	More than 1,501
Industry							
Food	5.032%	6.038%	8.151%	12.227%	19.664%	33.429%	61.844%
Goods	4.375%	5.250%	7.088%	10.632%	17.011%	28.919%	53.500%
Other	4.594%	5.513%	7.443%	11.163%	17.864%	30.369%	56.183%
Whole commerce							
Food	4.193%	5.032%	6.793%	10.190%	16.304%	27.717%	51.276%
Goods	3.646%	4.375%	5.906%	8.859%	14.174%	24.096%	44.578%
Other	3.828%	4.594%	6.202%	9.303%	14.885%	25.305%	46.814%
Retail							
Food	3.494%	4.193%	5.661%	8.492%	13.587%	23.098%	42.731%
Goods	3.038%	3.646%	4.922%	7.383%	11.813%	20.082%	37.152%
Other	3.190%	3.828%	5.168%	7.752%	12.403%	21.085%	39.007%
Services							
Personal	3.190%	3.828%	5.168%	7.752%	12.403%	21.085%	39.007%
Others	2.900%	3.480%	4.698%	7.047%	11.275%	19.168%	65.461%

Power Calculation

$$\hat{\beta}_{MDE} = \left(t_{\frac{\alpha}{2}} + t_{1-\kappa}\right)\sqrt{\left(\frac{1}{p_T\left(1-p_T\right)}\right)\frac{\sigma^2\left(y\right)}{N}} \tag{2}$$

$$a = \sqrt{1 + \left(\frac{N}{N_c} - 1\right)\rho} \tag{3}$$

Table A2: Power Calculation

	ln tax	**Δ ln tax:**	**$ tax**	**Δ $ tax:**
	$y=\alpha+\beta T+u$	$\Delta y=\alpha+\beta T+u$	$y=\alpha+\beta T+u$	$\Delta y=\alpha+\beta T+u$
$\sigma^2\left(y\right)$	0.449	0.003	2284.31	93.38
ρ	0.006	0.008	0.001	0.008
$\hat{\beta}_{MDE}$	0.191	0.015	13.607	2.751
a	1.078	1.106	1.007	1.11
$\hat{\beta}_{MDE} \times a$	0.206	0.017	13.707	3.054
$t_{\frac{\alpha}{2}}$	1.96		p_T	0.16
$t_{1-\kappa}$	0.84		p_C	0.84
N	723		$N_{cluster}$	25

Difference-in-difference estimator

$$y_{it} = \alpha_0 + \alpha_1 T_d + \gamma t_{bim5} + \delta D_{it} + \theta y_{it-1} + X'_{it}\beta + \varepsilon_{it}$$

$$\hat{\beta} = \frac{var\left(y_{it-1}\right) cov\left(y_{it}, D_{it}\right) - cov\left(y_{it-1}, D_{it}\right) cov\left(y_{it}, y_{it-1}\right)}{var\left(D_{it}\right) var\left(y_{it-1}\right) - cov\left(y_{it-1}, D_{it}\right)^2}$$

Notice that $cov\left(y_{it-1}, D_{it}\right) = 0$ because the treatment was random. So, $\hat{\beta}$ becomes

$$\begin{aligned} \hat{\beta} &= \frac{var(y_{it-1})cov(y_{it}, D_{it})}{var(D_{it})var(y_{it-1})} \\ \hat{\beta} &= \frac{cov(y_{it}, D_{it})}{var(D_{it})} \\ plim\hat{\beta} &= \beta \end{aligned}$$

A.1 Tables

Table A3: Effect of the Deterrence Letter on the Probability of Paying Each Tax According to the Estimation by Castro and Scartascini (2015)

Tax: In the Title of each Column

	(1)	(2)	(3)	(4)
	Property	Sales	Property	Sales
T: Deterrence	0.060	0.035	0.019	0.021
	(0.048)	(0.035)	(0.016)	(0.022)
N	722	718	718	658
Blocks FE	Yes	Yes	Yes	Yes
Lagged output	No	No	Yes	Yes
Controls	No	No	Yes	Yes
Period	Sep/Oct	Sep/Oct	Sep/Oct	Sep/Oct

The dependent variable takes the value 1 only if the taxpayer had paid in full the total tax liabilities for the period of the experiment. The tax is identified in the header. Controls are dummies for the sector, indicators for having paid the minimum tax in the previous period, variables from the annual declaration of 2010 (annual sales, dummies for the bins of the tables of the number of employees and the size of the store in square meters), age of the firm in years and gender of the proprietor. Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Effect of the Deterrence Letter on the Reported Tax – Placebo Test

Dependent Variable: Ln of SEH Tax

	(1)	(2)	(3)	(4)	(5)	(6)
T: Deterrence	-0.010	-0.001	0.000	-0.000	-0.000	0.001
	(0.015)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
After placebo (t: Jul/Aug)	-0.042***	-0.028***	-0.028***	-0.029***	-0.029***	-0.028***
	(0.006)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
T: Deterrence x after placebo	-0.006	-0.014	-0.014	-0.014	-0.014	-0.014
	(0.018)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)
Ln Tax SEH t-1	1.007***	0.915***	0.905***	0.949***	0.921***	0.910***
	(0.009)	(0.029)	(0.035)	(0.026)	(0.033)	(0.039)
1 if paid the min tax t-1				0.002	0.012*	0.013*
				(0.006)	(0.005)	(0.005)
Annual sales 2010 100,000 AR$				0.001	0.001	0.001
				(0.001)	(0.001)	(0.001)
1 if owner is male				0.004	0.003	0.003
				(0.004)	(0.004)	(0.004)
Age of firm Jan 2012 in years				-0.000	-0.000	-0.000
				(0.000)	(0.000)	(0.000)
Constant	0.008	0.395**	0.438**	0.266*	0.355*	0.400*
	(0.040)	(0.122)	(0.146)	(0.122)	(0.138)	(0.163)
N	1,431	1,322	1,322	1,322	1,322	1,322
Blocks FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Dummies	No	Yes	Yes	No	Yes	Yes
Sector Dummies	No	No	Yes	No	No	Yes

Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. In specifications from three onwards, we include dummies for the economic sector, and from four to six we include dummies for the bins of the tables of the number of employees and the size of the store in square meters. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Effect of the Deterrence Letter on the Reported Tax - OLS Estimation

Dependent Variable: Ln of SEH Tax

	(1)	(2)	(3)	(4)	(5)	(6)
T: Deterrence	0.006	0.007	0.008	0.008	0.007	0.009
	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)
Ln Tax SEH t-1	0.984***	0.900***	0.883***	0.937***	0.913***	0.896***
	(0.003)	(0.030)	(0.038)	(0.025)	(0.032)	(0.039)
1 if paid the min tax t-1				0.002	0.015*	0.017*
				(0.006)	(0.007)	(0.007)
Annual sales 2010 100,000 AR$				0.000	0.000	0.000
				(0.001)	(0.001)	(0.001)
1 if owner is male				0.002	0.001	0.001
				(0.004)	(0.005)	(0.005)
Age of firm Jan 2012 in years				0.000	0.000	0.000
				(0.000)	(0.000)	(0.000)
Constant	0.105***	0.466**	0.533**	0.321*	0.392**	0.458**
	(0.013)	(0.127)	(0.154)	(0.118)	(0.137)	(0.162)
N	718	665	665	665	665	665
Blocks FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Dummies	No	Yes	Yes	No	Yes	Yes
Sector Dummies	No	No	Yes	No	No	Yes
Period	Sep/Oct	Sep/Oct	Sep/Oct	Sep/Oct	Sep/Oct	Sep/Oct

Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. In specifications from three onwards, we include dummies for the economic sector, and from four to six we include dummies for the bins of the tables of the number of employees and the size of the store in square meters. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Effect of the Deterrence Letter on the Reported Tax - OLS Placebo Test

Dependent Variable: Ln of SEH Tax

	(1)	(2)	(3)	(4)	(5)	(6)
T: Deterrence	-0.016	-0.014	-0.012	-0.013	-0.013	-0.012
	(0.010)	(0.009)	(0.007)	(0.009)	(0.009)	(0.007)
Ln Tax SEH t-1	0.996***	0.895***	0.880***	0.926***	0.890***	0.875***
	(0.003)	(0.058)	(0.069)	(0.045)	(0.061)	(0.072)
1 if paid the min tax t-1				-0.013	0.001	0.003
				(0.008)	(0.006)	(0.007)
Annual sales 2010 100,000 AR$				0.002	0.002	0.002
				(0.002)	(0.002)	(0.002)
1 if owner is male				0.007	0.006	0.006
				(0.008)	(0.007)	(0.006)
Age of firm Jan 2012 in years				-0.000	-0.000	-0.000
				(0.000)	(0.000)	(0.000)
Constant	0.021	0.447	0.509	0.354	0.465	0.528
	(0.015)	(0.241)	(0.291)	(0.213)	(0.253)	(0.300)
N	715	661	661	661	661	661
Blocks FE	Yes	Yes	Yes	Yes	Yes	Yes
Size Dummies	No	Yes	Yes	No	Yes	Yes
Sector Dummies	No	No	Yes	No	No	Yes
Period	Jul/Aug	Jul/Aug	Jul/Aug	Jul/Aug	Jul/Aug	Jul/Aug

Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. In specifications from three onwards, we include dummies for the economic sector, and from four to six we include dummies for the bins of the tables of the number of employees and the size of the store in square meters. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Effect of the Deterrence Letter on the Probability of Paying Each Tax - Placebo Test
Tax: In the Title of each Column

	(1)	(2)	(3)	(4)
	Property	Sales	Property	Sales
T: Deterrence	0.061	0.030	-0.016	-0.019
	(0.053)	(0.025)	(0.017)	(0.022)
After placebo (t: Jul/Aug)	-0.018	-0.000	-0.024	-0.005
	(0.009)	(0.009)	(0.012)	(0.013)
T: Deterrence x after placebo	-0.008	-0.007	0.014	0.021
	(0.022)	(0.016)	(0.027)	(0.033)
N	1,445	1,433	1,431	1,307
Blocks FE	Yes	Yes	Yes	Yes
Paid t-1	No	No	Yes	Yes
Controls	No	No	Yes	Yes
Period	M/J-J/A	M/J-J/A	M/J-J/A	M/J-J/A

The dependent variable takes the value 1 only if the taxpayer had paid in full the total tax liabilities for the period of the experiment. The tax is identified in the header. Controls are dummies for the sector, indicators for having paid the minimum tax in the previous period, variables from the annual declaration of 2010 (annual sales, dummies for the bins of the tables of the number of employees and the size of the store in square meters), age of the firm in years and gender of the proprietor. Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Effect of the Deterrence Letter on the Probability of Paying Each Tax According to the Estimation by Castro and Scartascini (2015) - Placebo Test

Tax: In the Title of each Column

	(1) Property	(2) Sales	(3) Property	(4) Sales
T: Deterrence	0.052	0.023	-0.002	-0.005
	(0.048)	(0.031)	(0.020)	(0.019)
N	723	717	715	654
Blocks FE	Yes	Yes	Yes	Yes
Lagged output	No	No	Yes	Yes
Controls	No	No	Yes	Yes
Period	Jul/Aug	Jul/Aug	Jul/Aug	Jul/Aug

The dependent variable takes the value 1 only if the taxpayer had paid in full the total tax liabilities for the period of the experiment. The tax is identified in the header. Controls are dummies for the sector, indicators for having paid the minimum tax in the previous period, variables from the annual declaration of 2010 (annual sales, dummies for the bins of the tables of the number of employees and the size of the store in square meters), age of the firm in years and gender of the proprietor. Monetary amounts are in Argentine Pesos (AR$). Standard errors clustered by block are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$