No Taxation without Information

Deterrence and Self-Enforcement in the Value Added Tax*

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JOB MARKET PAPER

January 2010

Abstract

Tax evasion generates billions of dollars of losses in government revenue and creates large distortions, especially in developing countries. A growing, mostly theoretical literature argues that information flows are central to understanding effective taxation. This paper analyzes the role of information for tax enforcement in the case of the Value Added Tax (VAT) through two randomized field experiments with over 445,000 Chilean firms. Claims that the VAT facilitates tax enforcement by generating a paper trail on transactions between firms have led to widespread VAT adoption worldwide, but there is surprisingly little evidence. I find that the paper trail leads to spillovers that create important multiplier effects in tax enforcement. The impact of a random audit announcement is transmitted up the VAT chain, increasing compliance by firms' suppliers. A second experiment finds that the paper trail acts as a substitute to a firm's own audit risk. A message announcing increased tax enforcement has a much smaller effect on reporting of transactions that are already covered by a paper trail. These findings confirm that when evasion is taken into account, significant differences emerge between taxes that are equivalent in standard models but generate different information on taxable transactions.

*I thank Alberto Alesina, Samuel Asher, Lorenzo Casaburi, Raj Chetty, David Cutler, Oeindrila Dube, Martin Feldstein, Edward Glaeser, Lakshmi Iyer, Kelsey Jack, Louis Kaplow, Felipe Kast, Lawrence Katz, Wojciech Kopczuk, Michael Kremer, Jeffrey Liebman, Stephan Litschig, Isaac Mbiti, Stephan Meier, Sendhil Mullainathan, Paul Niehaus, Suresh Naidu, Joel Slemrod, Matthew Weinzierl, Tristan Zajonc and participants at seminars and conferences at Harvard University, MIT, the National Tax Association, the University of Basel, the University of Michigan, and Wharton for helpful comments and discussions. I am exceedingly grateful to the Chilean Tax Authority for outstanding collaboration in the implementation and to the Swiss Study Foundation, the David Rockefeller Center for Latin American Studies, and the Project on Justice Welfare and Economics for research and travel grants that made this project possible.

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1 Introduction

Tax evasion is a fundamental challenge for developing countries, where on average, the informal sector represents about 40% of GDP, ranging up to 70% (Schneider et al., 2010). High evasion rates can severely restrict funding for basic public infrastructure and can lead to significant distortions in the economy. Even in the US, overall tax evasion is estimated to be around 16% (Internal Revenue Services, 2008), a loss similar in size to the entire corporate income tax. A key constraint is that many transactions in the economy are not readily observable by the government. A growing, mostly theoretical literature therefore argues that understanding information flows is central to effective taxation. When governments imperfectly observe transactions, important differences emerge between forms of taxation that are equivalent in standard models of taxation but differ in the information they generate for the government (Slemrod, 2008). Third-party reporting, verifiable paper trails, and whistle-blowers are thought to play an important role in facilitating tax enforcement (Kopczuk and Slemrod 2006, Kleven et al. 2009, Kleven et al. 2010), and it has been argued that differences in the prevalence of such sources of information can explain some of the key differences in tax systems between developed and developing countries (Gordon and Li, 2009).

The Value Added Tax (VAT) is a stark example of a tax believed to facilitate enforcement through a built-in incentive structure that generates a paper trail on transactions between firms (e.g. Agha and Haughton 1996, Kopczuk and Slemrod 2006). This belief has contributed to one of the most significant developments in tax policy of recent decades (Keen and Lockwood, 2010): a striking increase of VAT adoption from 47 countries in 1990 to over 140 today (Bird and Gendron, 2007). There is, however, surprisingly little evidence evaluating these self-enforcing properties of the VAT.

This paper investigates the role of third-party reporting and paper trails for tax enforcement and tests for the self-enforcing properties in the VAT through two ran-

domized field experiments with over 445,000 firms in Chile. A first experiment, the "Spillover Experiment", examines the transmission of tax enforcement through the VAT paper trail. Half of a sample of 5,600 firms suspected of tax evasion was randomly selected to receive an announcement of an upcoming audit. The whole sample was later summoned for an audit, and information about their pre-treatment trading partners was collected. The randomly administered audit announcement affects not only the treated firms, but also strongly increases VAT payments by their suppliers. In line with the asymmetric incentives between clients and suppliers in the self-enforcing mechanism of the VAT, there are no spillover effects to client firms.

A second "Deterrence Message" experiment investigates whether the paper trail acts as a substitute to firms' own audit probability, whether it affects tax enforcement nationwide, and for what type of firms it matters most. It exploits the fact that the built-in incentive structure, which generates the paper trail, breaks down at the final production stage: while it is in firms' interest to ask suppliers for receipts in order to deduct input costs from their VAT bill, consumers have no incentive to do so.

The Deterrence Message Experiment shows that the VAT paper trail acts as a substitute to a firm's own audit probability. The tax authority sent letters indicating an increased audit probability to over 100,000 randomly selected firms. While the letters generate an immediate and strong increase in VAT payments - indicating that the message has a credible deterrence effect - this effect is much weaker on transactions between two firms - where the paper trail is present - than on sales from firms to final consumers - where there is no paper trail. The VAT paper trail hence is found to act as a substitute to a firm's own audit probability. The evidence is also consistent with a model by Kleven et al. (2009) about the impact of firm size on evasion. Small firms respond more to the deterrence message, and the presence of the VAT paper trail is less relevant among larger firms, where other forms of paper trails and monitoring are likely to be present. Finally, in order to test whether the impact of the letter really

stems from deterrence, I compare its effect to a motivational letter that appeals to tax morale and social norms, and a placebo letter that contains information that is irrelevant for tax compliance.[1]

The findings of this paper provide the first evidence of spillovers in tax enforcement. They show that when choosing an optimal audit strategy, a tax authority may not only want to consider the expected deterrence effect on the audited firm, but also the multiplier effect through the firm's trading network. Taken together, the two experiments show that globally, the VAT paper trail acts as a complement to the audit probability, while for a given firm, it acts as a substitute to the firm's own audit probability. This represents the first micro-empirical evidence for the self-enforcing properties of the VAT. The existing evidence has been limited to cross-country comparisons, investigating whether countries that adopt a VAT subsequently raise more taxes (e.g., Nellor 1987, Ebrill 2001).

These findings also provide empirical evidence for a larger, mostly theoretical literature on the importance of information and third-party reporting for effective taxation, particularly in developing countries (e.g., Kopczuk and Slemrod 2006, Gordon and Li 2009, Kleven et al. 2009). Because evasion is by its nature difficult to detect, and micro-level tax data is highly confidential, there has been a dearth of micro-empirical evidence. One of the very few studies in this area has been conducted simultaneously with this paper by Kleven et al. (2010), who analyze the individual income tax in Denmark. Consistent with the notion that in highly developed countries, governments encounter fewer difficulties in gathering information about transactions in the economy, they find that evasion is generally low, except for the small fraction of income that is not subject to third-party reporting. These results confirm similar findings for the income tax on a more aggregate level by the US Tax-Payer

[1]The methodology of analyzing the impact of different letter messages on tax payments has first been developed by Slemrod et al. (2001), and has recently been employed by Fellner et al. (2009), shedding light on the impact of deterrence and motivational appeals on tax payments by individuals in developed countries.

Compliance Measurement Program (TCMP) (Internal Revenue Service, 1996, 2006).[2]

This paper extends the findings of this literature in several ways. First, it looks at tax compliance by firms, rather than by individuals. Raising and enforcing tax payments from firms strongly reduces the number of agents the tax authorities must oversee (Kopczuk and Slemrod, 2006) and may be more feasible for many developing countries. Second, it analyzes tax compliance in a developing country context. Evasion rates are much higher in poorer countries, and while there is a growing empirical literature investigating taxation in such contexts (e.g. Engel et al. 1998, Fisman and Wei 2004, and Olken and Singhal 2009), there is still very little micro-empirical evidence. Third, and particularly relevant for developing countries, I find that third-party records strongly affect tax compliance even in a context where they are not automatically accessible to the tax authority. For most transactions in Chile, as in most developing countries, records are kept in handwritten books. The tax authority can verify them during audits, but in contrast to many developed countries, these records are not available in electronic form for automatic cross-checks. Finally, this paper speaks to the interplay of information with deterrence in tax enforcement. In the sample of the Spillover Experiment, prior to the audit announcement, self-enforcement was incomplete at best. The deterrence effect from the audit announcement was necessary to trigger the effectiveness of the paper trail, showing that it is the interaction of information with deterrence that leads to effective tax enforcement.

The remainder of this paper is structured as follows: Section 2 presents a conceptual framework illustrating the mechanism of the self-enforcing properties of the VAT, Section 3 describes the context in Chile, study design, data and estimation strategy, and Section 4 presents the results of the Spillover Experiment and the Deterrence Message Experiment in turn. Section 5 concludes.

[2]Alm et al. (2006) study a related question in the lab, where they experimentally vary the portion of an individual's income that is subject to third-party information, and find that cheating increases as individuals earn larger shares of income that are not perfectly detectable.

2 Conceptual Framework and Background on the VAT

This section provides a brief description of the VAT, followed by a conceptual framework that builds on the model of Kleven et al.(2010), which extends the classical Allingham-Sandmo model of tax evasion (Allingham and Sandmo, 1972) to the case in which some transactions are subject to third-party reporting.

As its name suggests, the Value Added Tax is paid on the value added at each production stage: firms pay VAT on the difference between total sales and total input costs. The tax base is therefore equivalent to that of a retail sales tax, which is paid only at the retail stage, covering the whole value produced by the entire production chain[3]. The main difference between a VAT and a retail sales tax lies in the way it is collected and in who remits the tax to the government, a difference which is irrelevant in most standard tax models, but is thought to have significant impacts when taking tax administration and tax evasion into account (Slemrod 2008).

According to the self-enforcement hypothesis in the VAT, firms have an incentive to ask their suppliers for accurate receipts because they can deduct input costs from their VAT bill. This incentive builds the creation of paper trails directly into the tax structure. The two sides of a transaction in inter-business trade have conflicting incentives: buyers want to overstate purchase prices to inflate tax deductions, while sellers want to understate sales to reduce tax liabilities. Since the amounts are recorded in two sets of books, the risk of cross-checks is thought to deter firms from reporting differing amounts (Bird and Gendron 2007). In this way, the buyer acts as the third-party that reports a transaction, which creates a liability for the supplier.

This self-enforcing mechanism can be represented in the framework of the classical Allingham-Sandmo model of tax evasion. In this model, risk neutral tax payers choose their level of evasion, given a probability p of being detected, and a penalty θ that is

[3]This equivalence holds when the VAT has a uniform rate and no exemptions

proportional to the evaded tax, by maximizing expected net-of-tax income, i.e.,

$$u = (1 - p(y)) \cdot [\bar{y} - \tau y] + p(y) \cdot [\bar{y}(1 - \tau) - \theta\tau(\bar{y} - y)], \tag{1}$$

where $\bar{y}$ is true income, y is reported income, p is the probability of being detected, and τ is the tax rate. The probability of being detected decreases with reported income, $p = p(y)$ where $p'(y) < 0$, as higher evasion is more likely to raise suspicion and trigger an audit. In the context of the VAT, income stands for sales minus input costs.

Following Kleven et al. (2010), the probability of being detected varies depending on whether the transaction is subject to a paper trail. It is a function of the probability of being audited a for all transactions, plus the probability of cross-checks c for transactions with a paper trail. In the framework of Kleven et al., the probability of cross-checks of paper trails is assumed to be close to 1, corresponding to the context of their study, which analyzes the income tax in the highly developed economy of Denmark. In that context, systematic matching of tax returns and information reports can be done routinely on a large scale, given that agents report directly to the tax authority and most paper trails are available in electronic form. The situation of the VAT in developing countries differs from this context in two ways. First, most paper trails are kept literally on paper. Second, information about individual transactions is not reported directly to the tax authority, but kept on record with the tax payer. These two factors make systematic cross-checks of paper trails much more expensive. This can be expected to imply a probability of cross-checks significantly below 1.

Extending the standard assumption of this class of models, the VAT paper trail can be embedded in the following way. The probability of being detected is increasing in the probabilities of both audits and cross-checks, $p = p(a(y),\ c)$ where $p'(a) >$

0,and $p'(c) > 0$. Additionally, and crucially for the analysis of spillover effects, the probability of cross-checks on transactions that generate a paper trail is increasing with the audit probability of trading-partners $\tilde{a}$, since during such audits, the tax authority will gain direct access to the paper trail documents. Finally, the audit probability of trading partners also affects the likelihood that a paper trail t is created. The higher the audit probability of trading partners, the more likely they are to insist that the transactions are recorded in the books. The probability of being detected can therefore be represented as $p = p(a(y),\ c(\tilde{a}, t(\tilde{a}))\ )$. Equation 1 for the case of the VAT then becomes

$$u = (1 - p(a(y), c(\tilde{a}, t(\tilde{a}))) \cdot [\bar{y} - \tau y] + p(a(y), c(\tilde{a}, t(\tilde{a}))) \cdot [\bar{y}(1 - \tau) - \theta\tau(\bar{y} - y)]. \quad (2)$$

The first order condition provides an interior optimum for reported income y at $[p(\cdot) - \frac{\partial p}{\partial a}\frac{\partial a}{\partial y}\ (\bar{y} - y)](1 + \theta)\tau - \tau = 0$.[4]

In the Spillover Experiment, we analyze a randomly generated increase of $\tilde{a}$, the audit probability of a firm's trading partners. Differentiating with respect to $\tilde{a}$ allows discussing the corresponding comparative statics:[5]

$$\frac{\partial^2 u}{\partial y \partial \tilde{a}} = \left[\frac{\partial c}{\partial t} \cdot \frac{\partial t}{\partial \tilde{a}} + \frac{\partial c}{\partial \tilde{a}}\right] \left[\frac{\partial p}{\partial c} - \frac{\partial^2 p}{\partial a \partial c}\ \cdot \frac{\partial a}{\partial y}\ (\bar{y} - y)\right] \cdot (1 + \theta)\ \tau. \quad (3)$$

Since the second term can be expected to be positive, we can focus on the first term to determine the sign of the effect of an increase in a trading-partner's audit probability $\tilde{a}$ on declared income y.[6] The audit probability of trading partners $\tilde{a}$

[4]The second-order condition is given by $2p'(y) - p''(y)(\bar{y} - y) < 0$, which is satisfied if p(.) is convex so that $p''(y) > 0$.

[5]By Topkis's Theorem, the sign of $\frac{\partial^2 u}{\partial y \partial \tilde{a}}$ indicates the sign of $\frac{\partial y*}{\partial \tilde{a}}$.

[6]The expression in the second bracket can be expected to be positive for the following reasons. As discussed above, $\frac{\partial p}{\partial c} > 0$ and $\frac{\partial a}{\partial y} < 0$. The difference between real and declared income $(\bar{y} - y)$ represents evasion and is positive by construction. Finally, the sign of $\frac{\partial^2 p}{\partial a \partial c}$ depends on whether audits a and cross-checks c are substitutes or complements. If they are complements, the expression in the second bracket is unambiguously positive. If they are substitutes, an increase in $\tilde{a}$ will increase declared income y by less. However, this substitution effect can be expected not to be so large as

appears twice, representing the two channels through which a change in $\tilde{a}$ can affect y. These channels correspond to the two types of VAT evasion on inter-firm transactions, as described in the following (see Table 1 for an illustration of the intuition).

The first term of equation (3), $\frac{\partial c}{\partial t} \cdot \frac{\partial t}{\partial \tilde{a}}$ shows the channel of "Omission", whereby a transaction is completely omitted from the books of both the seller and the buyer firm. This type of inter-firm evasion requires collusion between the firms, and only reduces overall VAT payments if the omission is carried through all the way to the end of the production chain. In this case, no paper trail is created for the transaction. An increase in the audit probability of a trading partner may increase the likelihood that the latter will insist that the transaction be "on the books." In this case, an increase in $\tilde{a}$ generates a paper trail. As illustrated in Table 1, this can be expected to have asymmetric effects depending on whether the optimizing firm is a supplier or a client of the treated firm. The arrows in Table 1 indicate the expected direction of change for the line-item in question resulting from an increased audit probability of the treated firm. If the optimizing firm is a supplier, then the fact that the treated firm demands a receipt will increase the optimizing firm's declared sales, thereby increasing its tax liability. If the optimizing firm is the client in the transaction, the fact that the treated firm now provides a receipt will increase the declarable input costs, thereby lowering the tax liability of the optimizing firm.

The second term in the first bracket of equation (3), $\frac{\partial c}{\partial \tilde{a}}$, represents the second form of inter-firm VAT evasion. "Discrepancies" stands for the type of evasion where a paper trail is created, but there are discrepancies in the reporting of the transaction between the books of the seller and the buyer. This type of evasion is based on the hope that the tax authority will not cross-check the records. In order to reduce tax payments, the seller firm will tend to understate the value of transaction, or omit the transaction from its books, since it represents a tax liability, while the buyer will tend

to offset the effect of the increase in $\tilde{a}$ completely, so overall, the expression in the second bracket can be expected to be positive.

to inflate the cost of the transaction, since it creates a tax deduction. The expected impact of an increase in $\tilde{a}$ is symmetrical for suppliers and clients. The increased risk of cross-checks will lead all firms to revise their declared transactions in the direction of the actual value, increasing their tax liabilities.

In sum, an increase in $\tilde{a}$ can generate spillovers to the treated firm's trading partners by reducing both types of evasion. The spillovers are asymmetric in the case of omission, and not in the case of discrepancies. Taking both types of evasion together, the effect on reported tax liability of suppliers is unambiguously positive, while in the case of clients, the net effect is ambiguous, depending on which type of inter-firm evasion dominates. In the Spillover Experiment, I show that increasing the audit probability of one firm indeed increases tax payments by its trading partner, and that the effect is asymmetrically concentrated on the suppliers.

The second part of the study, the Deterrence Message Experiment, analyzes whether the paper trail t acts as a substitute to the firm's own audit probability a by testing whether the impact of an increased audit probability has less of an effect where the probability of detection through the paper trail is already higher. Taking derivatives of equation (2) with respect to a and t reveals that it is theoretically ambiguous whether a and t are substitutes or complements. Economically, both types of interactions of the paper trail with the firm's own audit probability are plausible: They may be complements if firms respond more strongly to an increased audit probability where the paper trail is present, because they expect that the audit will detect more evasions where a paper trail is present. Alternatively, they may be substitutes if evasion is already so low in cases where the paper trail is present that an additional increase in the audit probability will detect less evasion than where the paper trail is absent.

3 Background, Study Design, Data and Empirical Specification

3.1 Background on the VAT in Chile

In Chile, as in many developing countries, the VAT is the largest tax, accounting for about half of tax revenues (Servicio de Impuestos Internos, 2010a). Chile has a single 19% VAT rate across all forms of economic activity, and very few industries are exempt.[7] There is no lower threshold size at which firms become subject to the VAT, firms of all sizes are included. This context allows the analysis of response to the VAT across a large set of different types of firms, without the interference of confounding institutional factors such as industry-specific exemptions or varying taxation rates.

The Chilean tax authority has a reputation of being highly effective, and is one of the most respected institutions in the country (Adimark, 2006). This reputation is due in part to its success in reducing estimated VAT evasion by more than half since 1990, from 27% to 12% (Servicio de Impuestos Internos, 2010b).

3.2 Study Design: Spillover Experiment

Both randomized field studies analyzed in this paper were conducted in collaboration with the Chilean Tax Authority ("Servicio de Impuestos Internos"). The first experiment was designed to study the self-enforcing mechanisms in action, by focusing on the upstream part of the VAT chain, i.e., on transactions between firms. It analyzes whether, as predicted by the self-enforcement hypothesis, increased tax enforcement on one delinquent firm generates spillovers to its trading partners along the VAT chain.

[7] Only the following entities are VAT exempt: news organizations, transportation, education, public universities and hospitals, the central bank, the social security administration, the ministry of national defense, the national postal services, and the public lottery. As is usual for a VAT, exports are excluded, and exporters are reimbursed for the VAT included in the purchase price of their inputs.

In order to study the question, this experiment focuses on a sample of firms where self-enforcement is expected to be incomplete, which means there is suspicion of evasion of tax payments for upstream transactions among firms. The presence of tax evasion in these transactions, which involve two VAT-declaring parties, allows studying whether increasing tax enforcement on one party has an impact on the tax declarations by the other party, through the VAT paper trail.

The sample selected for this analysis consists of 5,600 mostly rural, micro size firms[8] with tax declarations that show patterns suggestive of evasion. While these firms submitted tax declarations regularly each month and have not been found guilty of any infractions, their sales/input-ratio is much lower than that of other firms in the same industry. The mean of their sales/input ratio is 0.67. Given that the VAT is paid on sales-inputs, a ratio of below 1 indicates that a firm is paying no tax during the corresponding period. This can happen when firms make large investments into costly inputs, but over the long-run, a firm with higher costs than sales is clearly not economically viable, so this pattern raises suspicion of under-reported sales or over-reported input costs.

All 5,600 firms in the sample were scheduled for an audit by the tax authority. Half of them were randomly selected to receive a pre-announcement for this audit. The other half did not receive any message from the tax authority. Half a year later, the tax authority began auditing all firms in the sample. During the audits, information was collected on the firms' suppliers and clients by reviewing the books and proofs of transactions of the three months prior to the mailing of the audit pre-announcements. This made it possible to identify the firms' main suppliers and clients in a period not yet affected by the treatment.

[8]In Chile, micro size firms are defined as firms with annual sales of under approximately 100,000 USD.

3.3 Study Design: Deterrence Message Experiment

The second experiment consists of three different types of letters sent to randomly selected firms in a stratified sample among most small and medium size firms in the country in order to study their impact on the firms' monthly VAT payments.

The main letter contains a message of deterrence and was sent to around 102,000 firms. It is aimed at increasing the perceived audit probability: the letter informs the firm that it has been randomly chosen for an analysis and that if any irregularities are detected, it may be audited. The intervention did not affect the actual audit probability. The content of the message was nevertheless factually true - as certified after careful consideration by the tax authority's legal department - since the tax authority conducts analysis of all firms routinely, and firms always may be audited if irregularities are detected.

A smaller sample of about 18,500 firms each received either a tax morale letter aimed at affecting perceived social norms or a "placebo" letter to test whether the simple fact of receiving mail from the tax authority was driving the impact.[9] The tax morale letter contains a message aimed at increasing the perceived social norm of tax compliance. It reads, "Did you know that Chile is one of the countries with the highest level of tax compliance in the world? 98.3% of taxes paid are paid voluntarily. This has allowed Chile to have one of the highest levels of tax compliance, according to information from the OECD."[10] Finally, the placebo letter informs the recipient about some new features on the tax authority's website. (See Appendix A1 for the full

[9]The larger sample size for the deterrence letter was chosen in order to be able to analyze differential treatment effects for different types of firms, such as firm size, discussed in this paper, but also other characteristics not reported in this paper: firm age, region, industry, etc. Analyzing what type of firms are more likely to respond allowed me to develop an instrument to optimize audit strategies for the Chilean Tax Authority, enabling this project to provide not only an academic output, but also a directly applicable policy instrument.

[10]Motivational messages have been found to increase voluntary cooperation in some instances. E.g. Dal Bó and Dal Bó (2009) find that moral suasion can increase cooperation in the lab, especially when coupled with deterrence, and Fellner et al (2009) find that social norm letters increase compliance with broadcast tax obligations for those who live in regions with generally low compliance.

text of the letters.) The impacts of the letters are measured by comparing the VAT payments of recipient firms to payments by firms who have been randomly selected not to receive any letter.

In order to study whether the effectiveness of the letters decreases when repeated, the letters were mailed in two waves. The first wave was sent in December 2008, affecting VAT payments for November 2008 and onward (November taxes are due in December). The second wave was sent 5 months later. Firms were assigned randomly to the different waves. The particular wave a firm was included in was selected randomly. The first wave included approximately 84,500 deterrence letters, 15,500 motivational letters and 15,500 placebo letters, and the remaining letters (17,500, 3,200 and 3,200 respectively) were sent in the second wave. A total of 306,600 firms form the control group that received no letter.

3.4 Data and Implementation

The universe of firms considered in this study consists of almost all small and medium size firms in Chile that were operating in June 2008 and had declared a positive amount of VAT for at least one month between July 2007 and June 2008. A sample of 5,600 firms was selected for the Spillover Experiment. Since this sample consists of small, mostly rural firms, many of which are located in remote areas, there was a substantial fraction with no valid postal addresses.[11] In cases of non-delivery, the tax authority delivered the audit announcement in person.

In the Deterrence Message Experiment, such in-person delivery was not possible, due to the large scale of the experiment. In order to minimize non-compliance with treatment assignment, I therefore excluded firms with invalid postal addresses through

[11] Undeliverable addresses can result from one of three reasons: 1) the firm indicated an incorrect or outdated address, 2) the way the firm registered its addresses diverges from the formal format that the postal service requires, or 3) the firm was situated in a very remote location, where the postal service does not deliver. If need be, the tax authority can still reach firms in category 2) and 3), and most firms in category 1), as has been shown in the implementation of the second experiment.

a special procedure with the Chilean Postal Service. This procedure allowed me to reduce non-compliance with treatment assignment (i.e., firms that were assigned to receive a letter but did not receive one) from around 26% to 6%, thus substantially increasing statistical power. The use of certified mail provided information about which firms actually received a letter and by what date. In addition, the 5,600 firms in the Spillover Experiment were excluded, leaving a sample of 445,734 firms in the Deterrence Message Experiment.

The main data used in this study consists of monthly VAT declarations by the firms. The main outcome variable is monthly declared VAT, i.e., 19% of declared sales minus declared input costs. This value can be negative, as input costs can exceed sales in a given month.[12] The tax data also contains line items such as total final sales, total intermediary sales and total input costs. I combine this data with information about the characteristics of the firm, such as the firm's size and number of employees. All data was obtained from official tax records.

Implementation and Summary Statistics at Baseline: Spillover Experiment

The letters containing the audit announcement for the Spillover Experiment were sent in early December 2008, affecting tax payments starting in November 2008. Despite delays due to remote addresses, discussed earlier, by April 2009 96% of assigned firms had received the audit announcement.[13]

In Table 2, Columns(1) and (2) present summary statistics for the 5,600 firms in the sample of the Spillover Experiment. None of the differences between treatment and control group are statistically significant at the 10%-level. As discussed, this sample was selected for having suspiciously high input costs compared to their sales.

[12]In this case, the firm does not immediately receive a refund, but instead declares a negative amount for carry-over to the next month. In the measure of VAT used in this paper, the carryover from the previous month is excluded, so that the analysis focuses on new transactions in the current month.

[13]Figure A1 shows how many firms received the audit announcement by what date.

It is therefore not surprising that their actual mean declared VAT is negative. The mean of -18,452 pesos indicates that on average, these firms declare about 37 US dollars more in input costs than in sales. This negative value of the mean results from some large negative VAT declarations, and the median tax declaration is zero.

The sample of the Spillover Experiment consists of very small, rural firms, mostly in remote areas: all are micro size firms, and they are among the smallest even within that category, with a average of 2.3 on the official firm size classification within micro size, which ranges from 2 to 4.[14] Compared to other firms in the country, the firms in the Spillover Experiment also have a smaller share of final sales (16%), and are more likely to be in the agricultural sector (54%). Finally, as discussed above, their sales/input-ratio, the characteristics by which they were selected, is suspiciously low, averaging only 0.67.

[Table 2]

Information about the business partners of the firms in the study sample was obtained during the audits. This restricts the sample size for the spillover analysis to the audited firms. Due to administrative delays and the consequences of a large earthquake, only about 27% of the 5,600 firms were actually audited, and they provided information about 2,829 trading partners.

One potential concern of this attrition rate is that it may introduce a selection bias, if attrition is different between the treatment and control groups. This could generate differences between treatment and control groups within the sample of audited firms. Columns (3) and (4) of Table 2 suggest that this does not seem to be the case. Treatment and control firms do not differ in their probability of being audited, and apart from a slightly higher agriculture share, there is no significant difference between treatment and control firms among the audited sample. Similarly, as shown

[14]Given that the official size classification is based on declared sales, and that this sample is suspected to under-declare their true sales, this is likely to be an underestimate of the true size of these firms' operations.

in Columns (5) and (6), trading partners of treated firms do not differ from trading partners of control firms, except for a slightly lower share of months in which they failed to submit a declaration.[15]

Columns (5) and (6) show the characteristics of the trading partners in the Spillover Experiment. They are substantially larger than the audited firms, both in terms of their VAT paid and their official size category, have about the same average age, a slightly higher final sales share and a much lower share of agriculture. Their sales/input-ratio is in a much less suspicious range, with an average of 1.6. About 57% of trading partners are suppliers of the audited firms, while the rest are clients.

Implementation and Summary Statistics at Baseline: Deterrence Message Experiment

Most letters in the Deterrence Message Experiment were sent in early December 2008, affecting tax declarations starting in November, which are due in the following months. A smaller, also randomly chosen second wave was mailed in April 2009. Table 3 presents summary statistics for the different treatment groups in the Deterrence Message Experiment, compared to the control group. As one would expect, given the random assignment, average characteristics in the groups assigned to different treatments look very similar: none of the differences between treatment and control groups are statistically significant at the 10%-level.

[Table 3]

Firms in the Deterrence Message Experiment pay an average of 264,000 pesos (equivalent to about 500 USD) in monthly VAT, with a median of approximately 70,000 (equivalent to about 140 USD). The large difference between the mean and

[15]Such a difference in one variable at the 5% or 10% level is in the range of what can statistically be expected.

the median indicates a very large dispersion in the distribution of tax payments. As discussed below, this dispersion has implications for the empirical specification of the analysis.

There are five official categories of firm size in Chile, based on sales in the prior tax year: micro, small, medium, large (excluded from this study), and firms with no sales in the preceding tax year.[16] Micro-sized firms are by far the largest group, comprising 74.5% of the universe. The second largest group are small firms (18.2%), followed by medium (2.8%), and firms with no sales in the preceding year (1.5%). The remaining 3% are new firms that have not been yet classified.

Importantly for the analysis of heterogeneous treatment effects for different parts of the VAT chain, firms are balanced across the treatment groups with respect to their position in the production chain: 28.8% are retailers that sell only to final consumers and 38.2% are intermediary firms that sell only to other firms. Overall, the share of sales to the final consumer is 45.6%.

3.5 Empirical Strategy

The outcome variable is monthly declared VAT, i.e., 19% of declared sales minus declared input costs. Monthly VAT can be negative in months where input costs exceed sales. As discussed above, the dispersion of monthly VAT payments is very large. Indeed, the range of monthly VAT declared reaches from -800 billion pesos (equivalent to 1.7 billion USD) to 16 billion pesos (33 million USD). Figure 1 shows a histogram of the values of monthly VAT, excluding the top and bottom 5% of values. Even excluding the most extreme values, the distribution has a large dispersion. At the same time, there is a large density at zero.

[Figure 1]

[16] Micro size firms sell less than the equivalent of 100,000 USD per year, small firms have sales between 100,000 and 1.1 million USD, medium size firms between 1.1 and 4.2 million USD, and large firms over 4.2 million USD.

Due to the larger dispersion of the data and the fat tails of the distribution, analyzing the impact of the intervention on the mean of declared VAT does not lead to significant results, since the resulting variance is extremely large. I therefore use quantile regressions, as well as linear probability models for the probability of declared VAT being larger than three key thresholds: VAT declared in the same month of the previous year, the predicted value based on the control group, and zero. I will discuss these specifications below.

Quantile regressions provide an indication of the magnitude of the impact, while being much less sensitive to extreme values than the mean. The specification used throughout is a difference-in-difference approach, comparing treated firms to control firms and pre-treatment to the post-treatment period. The specification for the quantile regressions is therefore

$$Q_\tau(VAT_{it}|Z_i, t) = \alpha_\tau + \beta_\tau(t \cdot Z_i) + \gamma_\tau Z_i + \partial_t,$$

where Z_i is the treatment dummy indicating that a firm is in the treatment group, ∂_t stands for month fixed effects and $t * Z_i$ indicates treatment, i.e., a firm in the treatment group in the time after the letters have been sent. The treatment period starts in November 2008 for firms in the first wave, and in March 2009 for firms in the second wave.

To analyze the extensive margin, I employ a linear probability model of the probability of declaring any positive amount of VAT, i.e. declared VAT $>$ zero:

$$PositiveVAT_{it} = \alpha + \beta(t \cdot Z_i) + \partial_t \ + e_{it}$$

.

However, this paper focuses not on the overall impact of the treatment and the magnitude of the effect in pesos, but the comparison of the response in different types

of firms and transactions, such as clients vs. suppliers, large vs. small firms or sales vs. input costs. Since the amount in pesos will mechanically be larger in larger firms, and the probability of declaring zero VAT will be smaller, these measures are not appropriate to analyze differences in behavior between different types of firms. For such comparisons, measures that captured relative changes are indicated.

A specification that fulfills these criteria is a linear probability model withthe binary outcome variable indicating whether declared VAT is higher in the current month compared to the same month a year earlier:

$$TaxIncrease_{it} = \alpha + \beta(t * Z_i) + \gamma Z_i + \partial_t \ + e_{it}.$$

This measure has several benefits compared to alternative measures. First, as opposed to log specifications or count data models, it is applicable to variables that include zero or negative values, such as is the case for declared VAT. In addition, it provides a relative measure indicating a change in tax declarations compared to the firm's own history, it is robust against outliers, and at the same time takes into account firms in all parts of the distribution of VAT payments.[17] This specification is therefore used to compare the treatment effect between different types of firms or different types of transactions within firms.

One possible confounding factor could be differential trends between different types of firms or transactions. Differential trends lead to differences in the probability of paying more taxes than in the previous year, and to difference in the probability of being close enough to VAT paid in the previous year that the treatment effect can pushes the outcome over that threshold value. As a robustness check, I therefore also

[17]One of the alternative specifications is quantile regression using a normalized version of monthly tax payments in the form of monthly VAT/(pre-treatment average VAT). However, this specification is very sensitive to firms with very small pre-treatment averages, which end up with very high values when dividing by their pre-treatment value. Also, since the median of some line-items is zero, median regressions for these line-items are not informative, and the choice of alternative quantiles becomes to a certain extent arbitrary.

run a specification with a linear probability model using a dummy indicating whether declared VAT is higher than the predicted value for that firm in that month.

The prediction is based on median regressions among the firms in the control group, with a separate regression for each calendar month.[18] Declared VAT is regressed on the firms' pre-treatment VAT payments and on those characteristics for which differential treatment effects are evaluated in this paper, such as size and the share of sales to the final consumer. The predicting regression is therefore:

$$Q_\tau(VAT_i|preVAT_i, X_i) = \alpha_\tau + \beta_\tau preVAT_i + X_i^{'}\gamma_\tau,$$

where $preVAT$ is the firm's average monthly VAT prior to November 2008. This then allows me to use the following linear probability model:

$$VAT_{it} > \widehat{VAT}_{it} = \alpha + \beta(t \cdot Z_i) + \gamma Z_i + \partial_t \ + e_{it}.$$

The same specifications are used when comparing the impact of the letter messages for different line-items, such as total sales or input costs. In addition, in order to hold firm characteristics constant, I analyze the differential impact of the deterrence messages on different line-items using within-firm estimation. The corresponding regressions have as the outcome variable the probability that a given line-item has increased compared to the previous year. For example, when comparing sales to input costs, there are two observations for each firm and month: one for its sales and one for its input costs. To analyze the differential impact on different line-items, an interaction term is included of the type of transaction interacted with treatment. Including the necessary controls, this gives the following regression:

$$TransactionIncreased_{itl} = \alpha + \beta(t \cdot Z_i) + c_i + \partial_t \ + \phi(t \cdot Z_i \cdot l) + \eta(t \cdot l) + \iota_l + e_{itl},$$

[18] Predicted medians are used instead of means, since due to the high variance, the predicted mean is again not very informative, and few firms end up close to their predicted mean.

where ϕ is the parameter of interest and l indicates the line-item.

Given that the random variation affects only the firms' perceived audit probabilities, holding everything else constant, following Engel et al (2001), I interpret changes in declared income in response to the randomized interventions as a changes in tax evasion. There may, however, also be a response of real economic activity to the increased perceived audit probability: As the audit risk increases and firms increase tax compliance, their effective tax rate increases. The increased tax payments makes their business less profitable, and may lead to increased prices, which may decrease demand for their products. All this may lead them to reduce production. The observed change in declared VAT may therefore be an underestimate of the reduction in evasion resulting from the letter messages, since the reduction in production will lead to a decrease in declared VAT.

4 Results

4.1 Spillover Experiment

The Spillover Experiment was designed to find direct evidence for the self-enforcing mechanism of the VAT. It therefore focuses on the upstream part of the VAT chain, i.e., transactions between firms. The following first establishes that the audit announcement had a direct effect on recipient firms' monthly declared VAT. I then show that not only the firms that received an audit announcement increased their VAT payments, but their trading partners as well. Finally, I will show that as predicted by the self-enforcing hypothesis discussed in section 2, this spillover effect on trading partners is concentrated on suppliers firms, and there is no impact on client.

Table 4 establishes that the audit announcement had a significant impact on VAT payments by the recipient firms. Panel A shows the effect on the mean and the

binary outcome variables discussed above, while Panel B provides the quantile effects. Since the median in this sample is zero, I report a series of additional quantiles. All specifications show a similar picture of an increase of declared VAT for treated firms, even though, as expected due to the high variance, there is no significant effect in the specification using mean VAT as the outcome variable.

[Table 4]

As discussed in Section 2, firms are linked to each other along the supply chain through the VAT paper trails, which are created at each transaction between firms. The self-enforcing hypothesis posits that these paper trails have enforcing properties, which can lead to spillovers of tax enforcement. The Spillover Experiment allows testing for such spillovers, by analyzing the monthly tax payments of trading partners of the audited firms. Table 5 compares declared VAT of the trading partners of firms that received an audit announcement to trading partners of firms in the control group. It shows a highly significant increase in the declared VAT in the months following the audit announcements for trading partners of firms that received the announcement.

[Table 5]

These results represent the first documentation of tax enforcement on one firm generating spillovers to other firms. However, they cannot by themselves establish that the channel of these spillovers is the VAT chain. The spillovers may also result from a generally perceived increase in the audit risk by firms that are in communication with the treated firms, resulting from information that the tax authority is announcing increased audit activities in the region.

Table 6 therefore tests for the asymmetry in the prediction of the self-enforcement hypothesis: while it clearly predicts an increase in compliance for suppliers, its prediction is ambiguous for client firms. Table 6 shows the spillover effects separately

for client and supplier firms. In line with the predictions of the self-enforcing mechanism of the VAT, there are strong increases in declared VAT for suppliers, and no significant increases for client firms.

[Table 6]

Since client and supplier firms are different from each other, and who is a client or supplier is clearly not randomly assigned, I test for robustness of this result by including a series of control variables and their interaction with treatment, treatment period, etc. in Columns (2) and (4) of Table 6. The included control variables are firm size, sales/input-ratio, share of final sales, and whether the firm's industry is categorized as hard-to-monitor. Inclusion of the control variables does not significantly affect the findings.

The results of the Spillover Experiment provide several insights. First, as predicted by the self-enforcement hypothesis, the built-in paper trail of the VAT leads to spillovers of enforcement along the production chain. Monitoring a firm increases tax payments by its upstream trading partner. Second, this indicates that when taking the whole network of firms into account, the paper trail globally acts as a complement to the audit probability: it augments the effectiveness of an increase in the audit probability for one firm, by increasing VAT payments by other firms.

Third, the mere existence of information through the paper trail - not surprisingly - is not in itself self-enforcing in an environment where the risk of cross-checks is low. Prior to the audit announcement introduced through this study, self-enforcement was incomplete at best among this sample of firms. The deterrence effect resulting from the increased audit probability was necessary to trigger the effectiveness of the paper trail.

4.2 Deterrence Message Experiment

The Deterrence Message Experiment is designed to examine how the VAT-generated paper trail interacts with tax enforcement by testing whether an increased expected audit probability has a differential effect on reporting of transactions that are subject to the VAT chain - i.e., transactions between two firms - compared to transactions that are not - i.e., sales to the final consumer. It further analyzes how the impact of the deterrence message and the importance of the paper trail vary with firm size and other firm characteristics.

The following section first establishes the effectiveness of the deterrence message, then shows that the increase in the perceived audit probability has much less of an effect on transactions where the paper trail is present, and finally provides evidence that this effect is most important for small firms.

The Effectiveness of Deterrence

Figure 2 Panel A shows the impact of the deterrence letters on declared VAT of recipient firms, compared to a control group, which received no letter. The x-axis indicates time, with monthly observations. The vertical line represents the month when the letters were mailed. The graph represents the percent difference between medians of the treated and control firms in each month: (median VAT treated - median VAT control)/ (median VAT control), normalizing pre-treatment percent difference to zero. We see a marked jump in tax payments after receipt of the deterrence letter.

[Figure 2]

In order to establish whether it is really deterrence that drives the effect, I compare the impact of the deterrence letter to that of two other letter messages: a letter that appeals to tax morale through information about the social norm of compliance and a placebo letter that simply informs firms about a new feature on the tax authority's

website. Panels B and C of Figure 2 show the impact of these letters. In contrast to the deterrence letter, no marked increase is visible in Panel B for the tax morale letter.[19] Due to the smaller sample size of tax morale letters, the variance in the percentage difference of the medians is larger in this sample.[20] Panel C indicates clearly that there is no positive effect of the placebo letter.[21]

This comparison shows that it is the content of the deterrence letter that drives this response, not simply the fact of receiving a letter from the tax authority. Table 7 shows the same result in regressions for the mean and the four outcome variables discussed above: the median VAT, the probability of paying more VAT than in the same month of the previous year, the probability of paying more than the predicted value, and the probability of declaring any positive amount.

[Table 7]

All specifications confirm the same pattern, except for the regression using mean VAT, which as expected does not provide statistically significant results. There is a highly significant impact of the deterrence message, but not of the tax morale or placebo letter. Table 8 shows the dynamics of the effect of the deterrence letter over time. The horizontal line indicates the time of the mailing, with *t1* indicating the first month affected by the letter, and the rows below showing monthly effects through one year after mailing. It confirms the same pattern shown in the graphical results

[19] The apparent first increase in tax payments happens before mailing of the letter, and the second increase almost a year later is very unlikely due to the deterrence message.

[20] For certain subsegments of the universe, I do find significant impacts of the tax morale letter, such as for new firms, and firms that declare higher input costs than sales. In the latter case, the tax morale letter may actually have a deterrence effect, in that there may be a perception by these firms that the reason they received a letter telling them that others pay their VAT is that the tax authority suspects them of tax evasion due to their low sales-input ratio.

[21] Figure A5 shows a similar pattern for the second wave of mailings, sent five months after the first wave. It indicates that even 5 months after the first wave of mailings, the deterrence message is perceived as credible. If anything, the treatment effect is stronger than in the first wave, increasing the median by up to 18%. A possible reason for this stronger response is that tax evasion is suspected to have increased in this period due to a downturn in the economy. All regression analyses include both waves of mailing.

earlier: a marked increase immediately after the mailing of the letter, and a steady decline.

[Table 8]

Interaction with the Paper Trail

Having established that the Chilean Tax Authority was able to credibly increase the expected audit probability through the letter messages, I turn to the question of whether there is a differential impact for transactions covered by the VAT paper trail.

Table 9 shows four different types of transactions among firms that had both intermediary sales to other firms and final sales to consumers in the pre-treatment period. The first two columns look separately at the two components of the VAT: sales and input costs. By definition, input costs are based on transactions between firms. In line with the self-enforcement hypothesis, Table 9 shows a significant response in sales, but not in input costs. Further disentangling the effect by distinguishing between intermediary and final sales, Columns (3) and (4) show that the effect is concentrated an sales to the final consumer, which do not generate a paper trail.[22]

[Table 9]

To rule out selection effects, I confirm these findings with within-firm estimations in Table 10. Column (1) compares the effect on declared sales and input costs, and finds that the response is clearly concentrated on sales, even when comparing transactions within the firm. Column (2) compares the impact on final and intermediary sales, and again confirms that the response is much stronger in declared final sales than in declared intermediary sales.

[22]When looking at the whole economy, including retailers that sell only to final consumers, and upstream firms that sell only to other firms, I do find that there is some response in inter-firm sales. The response is smaller than on final sales. However, since these results are based on selection, and other characteristics that affect the response to an audit may be correlated with being an upstream firm, they are not reported in the table

[Table 10]

Interaction with Size

Kleven et al. (2009) posit that large firms evade less taxes, since firms with more employees run a higher risk of whistle-blowers informing the government about evasion practices. The employees in this case function as potential third-party reporters. In the following, I look at whether small firms respond more to an increased audit risk.

Figure 3 shows the differential impact of the deterrence letter by firm size, based on three median regressions, one for each category: micro, small and medium. While the impact in pesos increases mechanically with firm size, since larger firms have larger tax payments, the effect decreases with firm size when it is expressed in percentage terms relative to the firms' total VAT payments. This finding is consistent with the claim that larger fimrs evade less taxes, and therefore may respond less to an external increase in the audit probability.

[Figure 3]

The question therefore arises whether the VAT paper trail is particularly important for tax enforcement in small firms, where other sources of monitoring that are often present in larger firms - through their employees, or through other paper trails and sources of information, such as electronic billing and accounting - may be absent. Table 11 looks at the interaction of firm size and the impact of the VAT chain. It shows that the positive association of a larger share of sales to the final consumer with a stronger response to the increased audit risk is stronger in smaller firms: Within micro-size firms, the differential effect for firms with a larger share of final sales is significantly larger than within medium-size firms.

[Table 11]

5 Conclusion

This paper investigates the effectiveness of the Value Added Tax in facilitating tax enforcement and sheds light on the role of information and third-party reporting for taxation. It provides the first micro-empirical evidence for spillovers in tax enforcement and for the self-enforcing power of the paper trail in the VAT, and shows that in line with a growing, mostly theoretical literature on taxation in countries with limited state capacity, information reporting plays a crucial role for effective taxation.

Two randomized field experiments shed light on the role of the paper trail in the VAT. The Spillover Experiment shows that as predicted by the self-enforcement hypothesis, increasing the audit probability of firms suspected of VAT evasion increases their suppliers' tax payments. This indicates that globally, the VAT paper trail acts as a complement to increased audit probability. The Deterrence Message Experiment shows that within a given firm, transactions that are subject to the VAT paper trail respond much less to an increase in perceived audit probability, indicating that the paper trail acts as a substitute to a firm's own audit probability. The Deterrence Message Experiment also establishes the relevance of this mechanism nationwide, and finds that in line with predictions of Kleven et al. (2009), it is strongest for smaller firms. This is consistent with the hypothesis that the self-enforcing mechanism of the VAT is most important in environments where fewer other forms of paper trails exist.

These results have a number of implications for public finance in developing countries and for tax policy in general. First, in line with findings from the literature on corruption and illegal capture of public funds, such as Reinikka and Svensson (2004), the findings confirm that verifiable paper trails on financial flows can provide a powerful tool, rendering misappropriation of funds more difficult.

Second, the results are informative for the choice of tax instruments. They suggest that forms of taxation such as the VAT, which leave a stronger paper trail and thereby generate more information for the tax authority, provide an advantage over

other forms of taxation, such as a retail sales tax, where this is not the case. Other mechanisms that provide information to the government, such as online billing systems or electronic receipts, as recently introduced by Brazil and Kenya, may have high returns.

Third, optimal audit strategies by the tax authority can take into account the higher response in final sales transactions and spillovers up the production chain. All else equal, increasing the audit probability at the end of the production chain is beneficial both because it yields higher direct returns and because the spillovers will transmit the effect up the production chain. More generally, when choosing which firms to audit, a tax authority may not only want to consider the expected deterrence effect on the audited firm, but also the multiplier effect through the firm's trading network. Further research is required to analyze how to optimally allocate audit probabilities to different nodes in the network.

Fourth, as Emran and Stiglitz (2005) point out, the VAT is only effective among firms in the formal sector, and a heavy reliance on the VAT can therefore increase the inter-sectoral distortions between formal and informal sectors. De Paula and Scheinkman (2007) find that where the VAT is present, formalized firms tend to trade with other formalized firms, since these can provide them with receipts that allow them to deduct the input costs from VAT payments, while informal firms tend to trade among themselves. Combined with the spillover findings, this suggests that enforcing formalization at the final stage of production might contribute to formalizing entire production chains.

Fifth, the differential enforcement through the paper trail at different production stages leads to differences in effective tax rates and potential distortions in the product market. If the evasion rate is higher for downstream firms, a flat VAT rate will result in an implicit progressive tax system between firms, where upstream firms pay a higher effective tax rate, since they evade less. This difference in the tax rate

may lead to incentives for increased vertical integration at the last production stage and can create distortions in production between intermediary goods and final goods. Further research is required to investigate whether such distortions will be economically significant and warrant a revision of the frequently postulated recommendation that, putting aside redistributive considerations, a flat nominal VAT rate is optimal (Ebrill, 2001).

Finally, the results suggest a possible explanation for the differences in tax evasion between developed and developing economies. In many developing countries, home production plays an important role, gains from trade and division of labor are relatively small, and production chains tend to be shorter. Moreover, if gains from trade are small, division of labor may not only be low, but also more elastic with respect to taxation. If division of labor leads to transactions between agents, which - in contrast to home production - are traceable by the tax authority, small taxes may be enough to discourage such division of labor and thereby erode the traceable tax base. All these factors may make it harder for developing countries to develop an effective tax system, since they reduce the number of transactions that can lead to verifiable paper trails - through the VAT or through other forms of third-party reporting.

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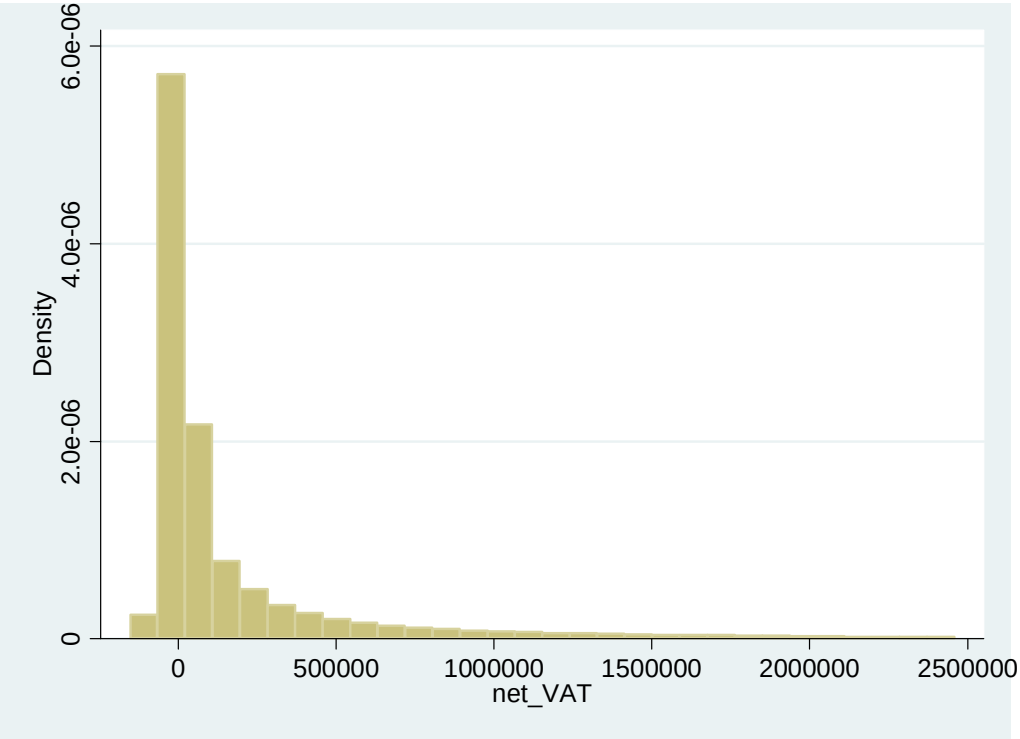
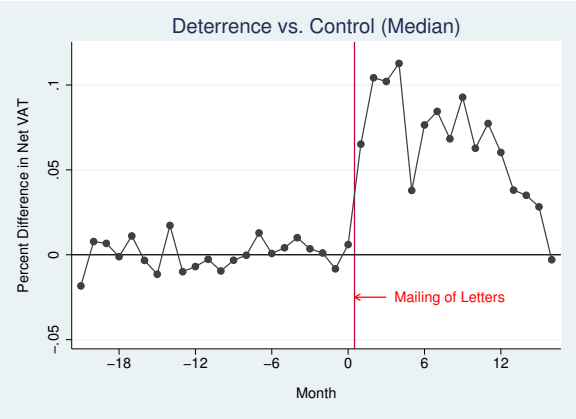
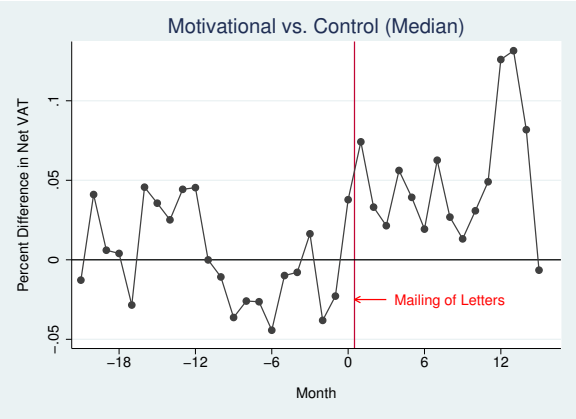


Figure 1: Distribution of monthly declared VAT excluding the top and bottom 5%

Panel A



Panel B



Panel C

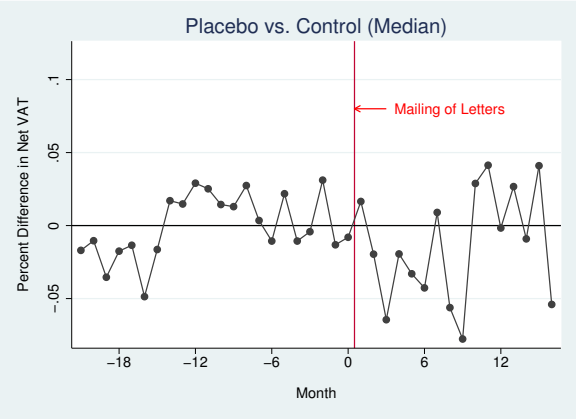


Figure 2: Impact of the three types of letters

Notes: This figure plots the monthly percent difference between the medians of the treatment and the control group of for each type of letter: (median VAT treatment group - median VAT control group) / (median VAT control group), normalizing pre-treatment percent difference to zero. The y-axis indicates time, with monthly observations, and zero indicates the last month before the mailing of the letters. The vertical line marks mailing of the letters. The figure shows the first wave of mailing. For the second (much smaller) wave of mailing, see Appendix A5.

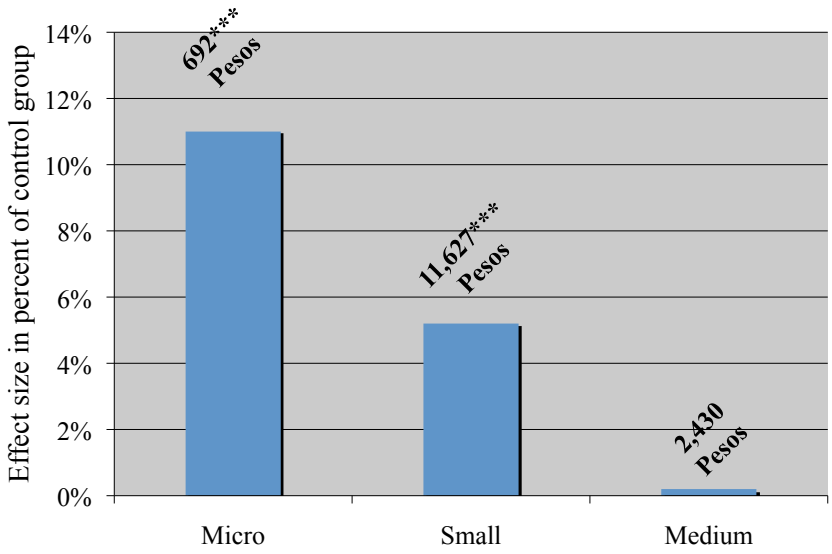


Figure 3: Impact by Firm Size (Median Regression)

Notes: Each bar represents a separate median regression for each size category. The numbers on top of the bars indicate the coefficient on being in the deterrence treatment group of a median regression of mean monthly VAT payments by firm in the first four post-treatment months on a treatment dummy. Stars indicate levels of significance. The height of the bar indicates the effect in percent relative to the control group in that size category.

Table 1: Two Forms of VAT Evasion on Inter-Firm Transactions

Position in supply chain	Omission		Discrepancies	
Supplier	Sales ↑	VAT ↑	Sales ↑	VAT ↑
Treated firm	Inputs ↑ Sales ↑	VAT (↑)	Inputs ↓ Sales ↑	VAT ↑
Client	Inputs ↑	VAT ↓	Inputs ↓	VAT ↑

Notes: "Omission" stands for the type of evasion where a transaction is completely omitted from the books of both the seller and the buyer firm. "Discrepancies" stands for the type of evasion where the books of the seller and the buyer reveal discrepancies. Buyers, for whom inputs represent a tax deduction, will tend to overstate the value of the transaction, while sellers, for whom the transaction represents a tax liability, will tend to understate its value. The arrows indicate the expected direction of change for the line-item in question resulting from an increased audit probability on the treated firm.

Table 2: Spillover Experiment: Baseline Summary Statistics and Balance of Randomization

	All Firms		Audited Firms		Trading Partners	
	(1)	(2)	(3)	(4)	(5)	(6)
	Control Group	Difference	Control Group	Difference	Trading Partners of Control Group	Difference
Monthly VAT (mean)	-18,452	-3,849	-8,024	-16,385	1,077,915	11,845
	(11,370)	(12,326)	(6,271)	(12,988)	(81,348)	(115,061)
Monthly VAT (median)	0	0	0	0	244,872	-11,454
	(0)	(0)	(0)	(0)	(16,869)	(23,470)
Firm age in months	130.93	-0.43	138.98	-3.33	126.46	1.5
	(1.19)	(1.69)	(2.18)	(3.13)	(1.77)	(2.52)
% Non-filed declaration	1.09	0.1	0.40	-0.21	1.30	-0.42*
	(0.13)	(0.18)	(0.11)	(0.13)	(0.19)	(0.23)
Size category	2.28	-0.000047	2.34	0.02	5.90	0.03
	(0.01)	(0.01)	(0.02)	(0.03)	(0.06)	(0.09)
% Retail firms	6.82	-0.54	7.27	-1.16	2.22	-0.56
	(0.48)	(0.66)	(0.94)	(1.28)	(0.48)	(0.60)
% Intermediary firms	66.32	-0.46	63.01	1.01	35.25	-1.53
	(0.89)	(1.27)	(1.76)	(2.46)	(1.50)	(2.14)
% Final sales	15.66	-0.35	19.37	-2.99	22.60	0.25
	(0.65)	(0.91)	(1.35)	(1.82)	(1.08)	(1.46)
Sales/Input	0.68	-0.01	0.73	-0.03	1.64	-0.14
	(0.01)	(0.01)	(0.02)	(0.02)	(0.10)	(0.11)
% Agriculture	54.00	0.71	45.31	5.34**	18.07	2.00
	(0.94)	(1.33)	(1.81)	(2.55)	(1.29)	(1.83)
% Audited	27.04	0.46	100	0		
	(0.84)	(1.19)	(.)	(.)		
% Suppliers					57.13	-1.32
					(1.44)	(2.10)
Number of firms	2,800	2,800	757	770	1,444	1385

Notes: This table shows summary statistics for the pre-treatment period and balance of randomization for three groups: the 5,600 firms in the sample of the Spillover Experiment, the firms that are actually audited, and the trading partners of the audited firms. Each row shows three regressions of the pre-treatment variable in question on a dummy indicating treatment assignment and a constant term: Columns (1) and (2) for the firms in the full Spillover Experiment sample, Columns (3) and (4) for the audited firms, and Columns (5) and (6) for the trading partners. Observations are monthly for 10 months prior to treatment. The constant terms capture the values for the control group. Columns (2), (4) and (6) show the difference of the treatment group to the control group. Robust standard errors in parentheses, clustered at the firm level for Columns (1) to (4) and at the audited firm's level for Columns (5) and (6). Monetary amounts in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. *** = p<0.01, ** = p<0.05, * = p<0.1

Table 3: Deterrence Message Experiment: Baseline Summary Statistics and Balance of Randomization

	(1) Control Group	(2) Difference to Deterrence	(3) Difference to Tax Morale	(4) Difference to Placebo
Monthly tax paid	264,434	1,342	9,959	-8,484
(mean)	(2,746)	(10,144)	(11,874)	(9,084)
Monthly tax paid	69,892	-779	-1,841	7
(median)	(458)	(920)	(1,959)	(1,836)
Firm age in months	108	-0.32	-0.43	-0.60
	(0.12)	(0.25)	(0.51)	(0.51)
% Non-filed declarations	4.3	-0.03	-0.07	-0.02
	(0.02)	(0.05)	(0.10)	(0.10)
% No sales year prior	1.5	-0.05	0.06	-0.01
	(0.02)	(0.04)	(0.09)	(0.09)
% Micro size	74.5	-0.04	-0.28	-0.6
	(0.08)	(0.16)	(0.33)	(0.33)
% Small size	18.2	0.028	0.102	-0.055
	(0.07)	(0.14)	(0.29)	(0.29)
% Medium size	2.8	0.01	0.02	0.07
	(0.03)	(0.06)	(0.12)	(0.13)
% Retail firms	28.7	-0.06	-0.28	-0.13
	(0.08)	(0.16)	(0.34)	(0.34)
% Intermediary firms	38.2	0.15	0.31	-0.08
	(0.09)	(0.18)	(0.37)	(0.37)
% Final sales	45.6	-0.10	-0.29	-0.07
	(0.08)	(0.17)	(0.35)	(0.35)
Number of firms	306,605	102,031	18,579	18,519

Notes: Each row shows a regression of the pre-treatment variable in question on treatment dummies and a constant term. Observations are monthly for 10 months prior to treatment. The constant term captures the value for the control group. Columns (2)-(4) show the difference of the treatment groups to the control group. Robust standard errors in parentheses, clustered at the firm level for all variables except for median tax paid, for which the table shows the result of a median regression for October 2008, the month before the tax payment. Monetary amounts in Chilean pesos, with 500 pesos approximately equivalent to 1 USD. None of the differences are statistically significant at the 10%-level.

Table 4: Spillover Experiment: Direct Effect of Audit Announcement on VAT Payments

Panel A: Mean and Binary Outcome Variables

	(1)	(2)	(3)	(4)
	Mean VAT	Percent VAT > Previous Year	Percent VAT > Predicted	Percent VAT > Zero
Audit Announcement	6,410	2.68***	1.56**	2.61***
X Post	(14,513)	(0.82)	(0.64)	(0.66)
Constant	-24,359	49.07***	32.43***	32.85***
	(8,538)	(0.66)	(0.55)	(0.74)
Month fixed effects	Yes	Yes	Yes	Yes
Audit Announcement	Yes	Yes	Yes	Yes
Number of observations	89,600	89,600	89,600	89,600
Number of firms	5,600	5,600	5,600	5,600
R-squared	0.000	0.002	0.001	0.003

Panel B: Quantile Treatment Effects

	(1)	(2)	(3)	(4)	(5)
	p10	p25	median	p75	p90
Audit Announcement	9,281*	2,467**	0	3,957**	8,384
X Post	(5,514)	(1,196)	(0)	(1,725)	(7,476)
Constant	-108,259***	-22,326***	0	11,952***	97,353***
	(6,493)	(1,458)	(0)	(1,240)	(5,145)
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Audit Announcement	Yes	Yes	Yes	Yes	Yes
Number of observations	89,600	89,600	89,600	89,600	89,600
Number of firms	5600	5600	5600	5600	5600

Notes: In Panel A, Column (1) shows the mean VAT, and Columns (2)-(4) show linear probability models of the probability of an increase in declared VAT since the previous year, the probability of declaring more than predicted and the probability of declaring any positive amount. Coefficients and standard errors of binary outcome variables multiplied by 100 to express effects in percent. Robust standard errors in parentheses, clustered at the firm level. Panel B shows quantile effects for the 10th, 25th, 50th, 75th and 90th percentile for the first six months after mailing of the audit announcements. Robust standard errors in parentheses, clustered at the firm level. Monetary amounts in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. *** = p<0.01, ** = p<0.05, * = p<0.1

Table 5: Spillover Effects on Trading Partners' VAT Payments

	(1)	(2)	(3)	(4)
	Median	Percent VAT > Previous Year	Percent VAT > Predicted	Percent VAT > Zero
Audit Announcement	31,804**	2.50**	2.08*	1.74*
X Post	(13,711)	(1.14)	(1.08)	(0.97)
Constant	267,112***	52.07***	49.06***	1.93***
	(18,505)	(0.92)	(0.91)	(0.69)
Month fixed effects	Yes	Yes	Yes	Yes
Audit Announcement	Yes	Yes	Yes	Yes
Number of observations	45,264	45,264	45,264	45,264
Number of firms	2,829	2,829	2,829	2,829
R-squared		0.002	0.000	0.002

Notes: Column (1) is a median regression of VAT payments of trading partners of the audited firms on a dummy variable indicating whether the audited firm received an audit announcement. Columns (2)-(4) show linear probability models of the probability of an increase in declared VAT since the previous year, the probability of declaring more than predicted and the probability of declaring any positive amount. Robust standard errors in parentheses, clustered at the level of the audited firm. All coefficients and standard errors multiplied by 100 to express effects in percent. Monetary amounts in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Differential Spillovers: Suppliers vs. Clients

	(1)	(2)	(3)	(4)
	Percent VAT > Previous Year	Percent VAT > Previous Year	Percent VAT > Predicted	Percent VAT > Predicted
Audit Announcement	4.61***	4.42***	4.10***	4.02***
X Supplier X Post	(1.51)	(1.49)	(1.46)	(1.48)
Audit Announcement	-0.88	-0.67	-0.83	-1.02
X Client X Post	(1.54)	(1.56)	(1.50)	(1.53)
Constant	51.27***	50.84***	46.53***	51.07***
	(1.18)	(2.20)	(1.23)	(2.34)
Controls	No	Yes	No	Yes
Controls X Audit Announcement	No	Yes	No	Yes
Controls X Audit Announcement X Post	No	Yes	No	Yes
Controls X Post	No	Yes	No	Yes
Supplier	Yes	Yes	Yes	Yes
Supplier X Post	Yes	Yes	Yes	Yes
Supplier X Audit Announcement	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes
Audit Announcement	Yes	Yes	Yes	Yes
Number of observations	45,264	44,288	45,264	44,288
Number of firms	2,829	2,790	2,829	2,790
R-squared	0.002	0.003	0.000	0.001

Notes: Linear probability models of the probability of an increase in declared VAT since the previous year and the probability of declaring more than predicted. The controls in Columns (2) and (4) are firm sales, sales/input-ratio, final sales share and industry categories as "hard-to-monitor." Time period includes 6 months after mailing of the letters. Robust standard errors in parentheses, clustered at the level of the audited firm. All coefficients and standard errors multiplied by 100 to express effects in percent. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Deterrence Message Experiment: Treatment Effects on VAT Payments by Type of Letter

	(1)	(2)	(3)	(4)	(5)
	Mean VAT	Median VAT	Percent VAT > Previous Year	Percent VAT > Predicted	Percent VAT > Zero
Deterrence	5,679	5,302***	1.40***	1.45***	0.50***
Letter X Post	(14,375)	(1,214)	(0.12)	(0.10)	(0.14)
Tax Morale	3,937	1,419	0.39	0.33	-0.01
Letter X Post	(14,227)	(2,552)	(0.25)	(0.22)	(0.30)
Placebo	16,616	1,430	-0.09	-0.15	-0.13
Letter X Post	(14,973)	(2,598)	(0.25)	(0.22)	(0.30)
Constant	260,465***	69,459***	47.49***	48.30***	67.29***
	(4,934)	(432)	(0.08)	(0.08)	(0.07)
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Treatment assignment	Yes	Yes	Yes	Yes	Yes
# of obs.	7,892,076	445,734	7,892,076	7,892,076	7,892,076
# of firms	445,734	445,734	445,734	445,734	445,734
R-squared	0.000		0.005	0.000	0.012

Notes: Column (1) shows a regression of the mean declared VAT on treatment dummies, Column (2) shows a median regression of the mean post-treatment VAT, and Columns (3)-(5) show linear probability regressions of the probability of an increase in declared VAT since the previous year, the probability of declaring more than predicted and the probability of declaring any positive amount. Standard errors in parenthesis, robust and clustered at the firm level for Columns (1) and (2)-(4). Coefficients of the linear probability regressions are multiplied by 100 to express effects in percent, for ease of reading. Monetary amounts in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Dynamics Over Time: Monthly Effects of Deterrence Letter on VAT Payments

	(1) Percent VAT > Previous Year	(2) Percent VAT > Predicted	(3) Percent VAT > Zero
t-3	0.10	-0.09	-0.05
	(0.17)	(0.17)	(0.17)
t-2	0.04	-0.10	-0.15
	(0.17)	(0.17)	(0.17)
t-1	0.17	0.15	-0.10
	(0.17)	(0.17)	(0.17)
t1	1.07***	1.37***	0.49***
	(0.17)	(0.17)	(0.17)
t2	1.74***	1.93***	0.56***
	(0.17)	(0.17)	(0.17)
t3	1.46***	1.50***	0.48***
	(0.17)	(0.18)	(0.17)
t4	1.64***	1.42***	0.46***
	(0.17)	(0.18)	(0.17)
t5	0.99***	1.03***	-0.15
	(0.17)	(0.18)	(0.17)
t6	0.94***	0.92***	0.12
	(0.17)	(0.18)	(0.17)
t7	0.88***	0.79***	0.04
	(0.17)	(0.18)	(0.17)
t8	0.92***	0.83***	0.17
	(0.19)	(0.20)	(0.20)
t9	0.85***	0.95***	0.31
	(0.19)	(0.20)	(0.20)
t10	0.87***	0.88***	0.10
	(0.19)	(0.20)	(0.20)
t11	0.73***	0.82***	0.16
	(0.19)	(0.20)	(0.20)
t12	0.77***	0.71***	0.12
	(0.19)	(0.20)	(0.20)
Constant	47.51***	48.46***	67.30***
	(0.08)	(0.08)	(0.07)
Month fixed effects	Yes	Yes	Yes
Deterrence Letter	Yes	Yes	Yes
Number of observations	8,989,992	8,989,992	8,989,992
Number of firms	408,636	408,636	408,636
R-squared	0.006	0.000	0.008

Notes: Each column shows a linear probability regression on interaction terms of being assigned to receive a deterrence letter with month dummies. Robust standard errors clustered at the firm level in parenthesis. Coefficients are multiplied by 100 to express effect as percent. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Impact of Deterrence Letter on Different Types of Transactions

	(1)	(2)	(3)	(4)
	Percent Sales > Previous Year	Percent Input Costs > Previous Year	Percent Intermediary Sales > Previous Year	Percent Final Sales > Previous Year
Deterrence Letter	1.13***	0.09	0.10	1.32***
X Post	(0.21)	(0.21)	(0.19)	(0.21)
Constant	55.41***	53.30***	38.38***	45.03***
	(0.14)	(0.14)	(0.14)	(0.15)
Month fixed effects	Yes	Yes	Yes	Yes
Deterrence Letter	Yes	Yes	Yes	Yes
Number of observations	2,392,529	2,392,529	2,392,529	2,392,529
Number of firms	133,156	133,156	133,156	133,156
R-squared	0.015	0.013	0.008	0.007

Notes: Regressions of the probability of the line-item (total sales, total input costs, intermediary sales, and final sales) being higher than in the same month the previous year, among firms that have both final and intermediary sales in the period prior to treatment. Coefficients are multiplied by 100 to be interpretable as percent, for ease of reading. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Impact of Deterrence Letter on Different Types of Transactions with Firm Fixed Effects

	(1)	(2)
	Percent VAT > Previous Year	
Deterrence Letter X Sales X Post	1.2***	
	(0.2)	
Deterrence Letter X Final Sales X Post		1.2***
		(0.3)
Deterrence Letter X Post	0.0	-0.0
	(0.2)	(0.2)
Sales	2.0***	
	(0.1)	
Final Sales		5.6***
		(0.1)
Constant	4.4***	3.1***
	(0.0)	(0.0)
Sales X Post	Yes	
Final Sales X Post		Yes
Firm fixed effects	Yes	Yes
Number of observations	4,785,058	4,785,058
Number of firms	133,156	133,156
R-squared	0.018	0.013

Notes: Regression of the probability of the line-item (total sales, total input costs, intermediary sales, and final sales) being higher than in the same month the previous year, for four months after mailing of the letters, among firms that have sales to both other firms and final consumers in the period prior to treatment. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Interaction of Firm Size and Final Sales Share

	Percent VAT > Previous Year	Percent VAT > Predicted
Micro X Final Sales Share X	2.00*	1.97**
Deterrence Letter X Post	(1.08)	(0.98)
Small X Final Sales Share X	1.31	1.15
Deterrence Letter X Post	(1.26)	(1.20)
Final Sales Share X Deterrence	0.36	-0.13
Letter X Post	(1.04)	(0.92)
Deterrence Letter X Post	0.21	-0.19
	(0.52)	(0.59)
Size Categories and Final Sales Share	Yes	Yes
Size Categories X Deterrence Letter	Yes	Yes
Size Categories X Post	Yes	Yes
Final Sales Share X Post	Yes	Yes
Size Categories X Final Sales Share X Post	Yes	Yes
Month fixed effects	Yes	Yes
Deterrence Letter	Yes	Yes
Number of observations	7,340,994	7,340,994
Number of firms	408,636	408,636
R-squared	0.005	0.000

Notes: Regression of the probability of monthly VAT declared being higher than in the same month the previous year and being higher than predicted, for four months after mailing of the letters. Robust standard errors in parentheses, clustered at the firm level. Coefficients are multiplied by 100 to be interpretable as percent, for ease of reading. *** p <0.01, ** p<0.05, * p<0.1

A Audit Announcement (Translated)

Santiago, November 19th 2008.

Mr. (s)
Business XYZ
210 Example Street
Concepción

Ref: Information about the SII future audit process.

Dear Taxpayer:

This is an informational letter and, therefore, does not require that you take any action vis-à-vis the SII.

We wish to inform you that according to control policies carried out by our institution, and under Law 18,320, YOU WILL RECEIVE A NOTIFICATION THAT YOU WILL BE AUDITED AT SOME POINT BETWEEN JUNE AND AUGUST 2009.

The purpose of this letter is to give you time to prepare your records for future audit. Later, you can expect a FORMAL NOTIFICATION LETTER, which will inform you which SII office you need to attend, what documents you need to bring and the date of the audit.

As such, YOU SHOULD NOT APPROACH THE SII OFFICES before receiving the formal notification letter.

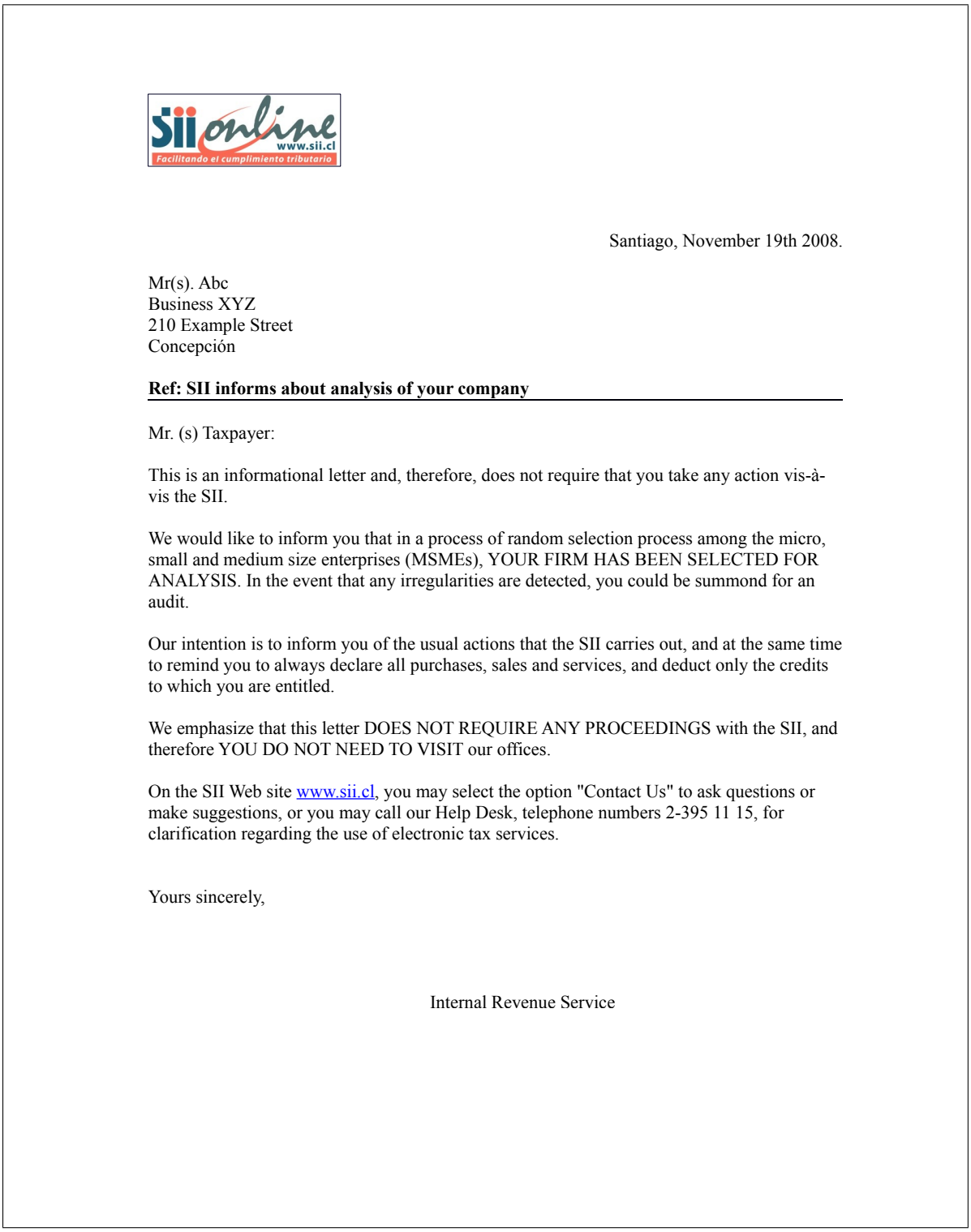
Also, remember that you can declare, correct or amend your earlier statements in the SII Virtual Office on the internet (www.sii.cl).

Yours sincerely,

Internal Revenue Service

Figure A1: Audit Announcement

B Letter Templates (Translated from Spanish)



Santiago, November 19th 2008.

Mr(s). Abc
Business XYZ
210 Example Street
Concepción

Ref: SII informs about analysis of your company

Mr. (s) Taxpayer:

This is an informational letter and, therefore, does not require that you take any action vis-à-vis the SII.

We would like to inform you that in a process of random selection process among the micro, small and medium size enterprises (MSMEs), YOUR FIRM HAS BEEN SELECTED FOR ANALYSIS. In the event that any irregularities are detected, you could be summond for an audit.

Our intention is to inform you of the usual actions that the SII carries out, and at the same time to remind you to always declare all purchases, sales and services, and deduct only the credits to which you are entitled.

We emphasize that this letter DOES NOT REQUIRE ANY PROCEEDINGS with the SII, and therefore YOU DO NOT NEED TO VISIT our offices.

On the SII Web site www.sii.cl, you may select the option "Contact Us" to ask questions or make suggestions, or you may call our Help Desk, telephone numbers 2-395 11 15, for clarification regarding the use of electronic tax services.

Yours sincerely,

Internal Revenue Service

Figure A2: Deterrence Letter

Santiago, November 19rd 2008

Mr. (s)
Business XYZ
210 Example Street
Concepción

Did you know that Chile has one of the highest levels of tax compliance in the world?

Dear Taxpayer:

We wish to inform you that 98.3% of the taxes in our country are paid on a voluntary basis. This has allowed Chile to have one of the highest levels of tax compliance worldwide, according to information from the Organization for Economic Cooperation and Development (OECD).

This success is a result of the fact that the majority of Chileans declare and pay taxes in accordance with the provisions of the law.

We invite you to continue on this path and together achieve the country we all want, a Chile with more and more integrity, solidarity and development.

Yours sincerely,

Internal Revenue Service

Figure A3: Tax Morale Letter

Santiago, November 19th 2008.

Mr(s) Abc
Business XYZ
210 Example Street
Concepción

Ref: Visit the Web site of the Internal Revenue Service (www.sii.cl)

Dear Taxpayer:

We want to tell you that the SII Virtual Office now has an option called "Electronic Tax Folder", a tool that allows taxpayers to bring together in a single electronic document, statements and tax information that they are required to submit to banks or other institutions.

For more information, you can visit the SII Virtual Office website (www.sii.cl), menu "Tax Status", option "Electronic Tax Folder".

In our Virtual Office you will also find online deals, news, economic indicators, tax information, FAQs and guidelines for most of your dealings with the SII, such as tax returns, changes in information and payment of contributions.

Take advantage of the internet: visit www.sii.cl.

Yours sincerely,

Internal Revenue Service

Figure A4: Placebo Letter

C Second Wave of Mailing

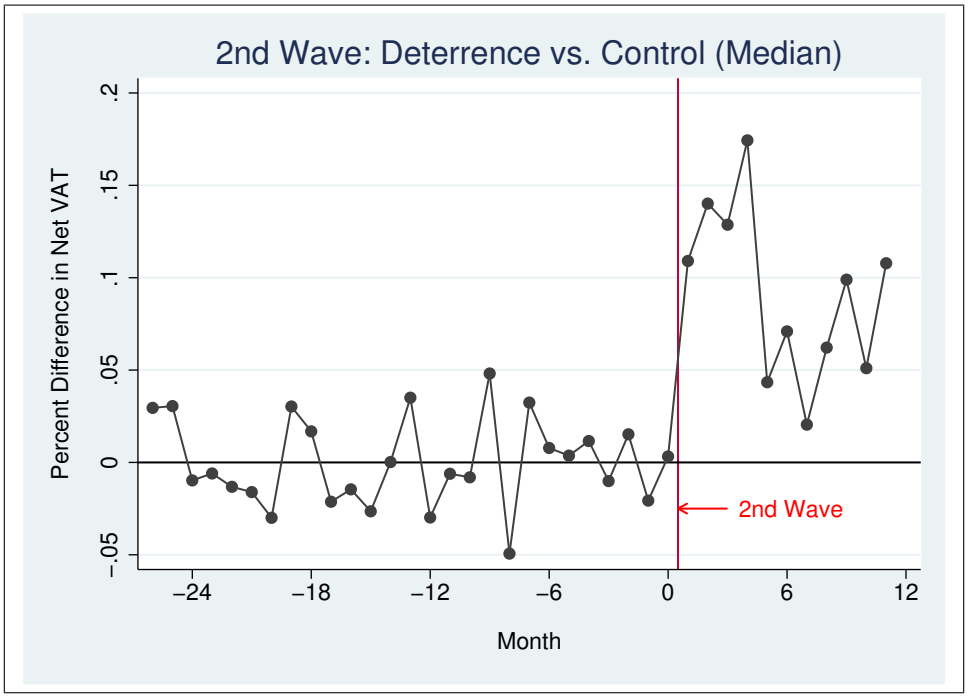


Figure A5: Impact of Deterrence Letter: Second Wave of Mailing

Notes: This figure plots the monthly percent difference between the medians of the treatment and the control group of the deterrence letter for the second wave of mailing: (median VAT treatment group - median VAT control group) / (median VAT control group), normalizing pre-treatment percent difference to zero. The y-axis indicates time, with monthly observations, and zero indicates the last month before the mailing of the letters. The vertical line marks mailing of the letters. The figure shows the first wave of mailing. Since the second wave of mailing is much smaller than the first, these figures show a much more noisy pattern.

D Additional Tables and Robustness Checks

D.1 Implementation Spillover Experiment

Table A1: How many firms received the audit announcement when?

	(1)	(2)
Month	Number of firms	Percent
Oct 2008	20	0.72
Nov 2008	1,124	40.14
Dec 2008	81	2.90
Jan 2009	5	0.18
Feb 2009	253	9.04
Mar 2009	1,083	38.68
Apr 2009	123	4.40
May 2009	3	0.10
Jun 2009	0	0.00
Jul 2009	2	0.08
Never	106	3.79
Total	2,800	100

Notes: This table indicates how many firms received the letter in time to affect which tax month. Any letter that arrives for example between November 12th and December 12th 2008 is considered to affect the tax declaration of November 2008, which is due on December 12th for most firms. The 20 firms that received the letter in time for October 2008 are from the pilot for the study, which was also conducted in a clean randomized fashion out of the same sample of 5,600 firms. Letters that arrived by January were distributed by mail, later letters were distributed in person by the tax authority.

D.2 Instrumental Variables and Non-Compliance

A series of time-varying instruments allow for the treatment-on-the treated analysis of the impact of the letter, taking into account that not all firms that were assigned to treatment actually received the letter, and among those who received the letter, some received it at later dates.[23] If the rate of non-compliance is different for different types of firms or transactions, this can lead to potential biases when comparing the impact between these types of firms or transactions.

To create a treatment-on-the-treated estimator with different intensities of the first stage over time - taking into account the number of firms that received the letter in each month - a series of time-varying instruments is constructed. Each instrument consists of an interaction of the number of months since mailing of the letter with treatment assignment. The outcome variables of the first stage consist of two dummies: the variable of interest, indicating whether the month is within the first four months after the firm received the letters, and a second dummy, indicating whether a firm received the letter more than four months ago. This latter dummy absorbs the "remainder" months, so that the coefficient the first dummy compares post-treatment outcomes to the pre-treatment the period only. The following tables show results of IV regressions.

[23] In the Deterrence Message Experiment 5.8% of firms did not receive the letter, and 3.1% received it late. The dispersion over time is even greater in the Spillover Experiment, where the last firm received the letter only eight months after the first. Table A2 shows how many firms received the audit announcement in which month. The timing of the arrival of letters is driven by the quality of the addresses and the remoteness of the location.

Table A2: Spillover Experiment: Effects on Trading Partners' VAT Payments
IV Estimates: Treatment on the Treated

	(1)	(2)
	Percent > Past Year	Percent > Predicted
Audit Announcement X Supplier X Post	6.08***	5.97***
	(2.13)	(1.93)
Audit Announcement X Client X Post	0.39	0.86
	(2.16)	(2.11)
Constant	47.96***	42.98***
	(1.97)	(2.04)
Supplier	Yes	Yes
Supplier X Audit Announcement	Yes	Yes
Month fixed effects	Yes	Yes
Audit Announcement X Supplier X Later months	Yes	Yes
Audit Announcement X Client X Later months	Yes	Yes
Supplier X Later months	Yes	Yes
Supplier X Post	Yes	Yes
Audit Announcement	Yes	Yes
Number of observations	73,554	73,554
Number of firms	2,829	2,829
R-squared	0.005	0.003

Notes: Treatment-on-the-treated effects using time-varying instruments for the first four months after receipt of the audit announcement for linear probability models of the probability of a tax increase since the previous year (Column 1), the probability of declaring more than predicted (Column 2). Coefficients and standard errors multiplied by 100 to express effects in percent. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Deterrence Letter Experiment: Effects on Different Types of Transactions IV Estimates: Treatment on the Treated

	(1)	(2)	(3)	(4)
	Percent Sales > Previous Year	Percent Input Costs > Previous Year	Percent Intermediary Sales > Previous Year	Percent Final Sales > Previous Year
Deterrence Letter X	1.28***	0.10	0.11	1.51***
Post	(0.24)	(0.23)	(0.21)	(0.23)
Constant	55.41***	53.23***	38.38***	45.03***
	(0.14)	(0.14)	(0.14)	(0.15)
Month fixed effects	Yes	Yes	Yes	Yes
Deterrence Letter	Yes	Yes	Yes	Yes
Later months X Deterrence Letter	Yes	Yes	Yes	Yes
Number of observations	2,663,120	2,663,120	2,663,120	2,663,120
Number of firms	133,156	133,156	133,156	133,156
R-squared	0.016	0.014	0.008	0.007

Notes: Treatment-on-the-treated effects using time-varying instruments for the first four months after receipt of the letter on the probability of the line-item (total sales, total input costs, intermediary sales, and final sales) being higher than in the same month the previous year, among firms that have sales to both other firms and final consumers in the period prior to treatment. Coefficients and standard errors multiplied by 100 to express effects in percent. Robust standard errors in parentheses, clustered at the firm level. *** = p<0.01, ** = p<0.05, * = p<0.1

D.3 Alternative Post-Treatment Time Window

Table A4: Differential Spillovers: Suppliers vs. Clients - 4 post-treatment months

	(1)	(2)
	Percent VAT > Previous Year	Percent VAT > Predicted
Audit Announcement	3.57**	3.19**
X Supplier X Post	(1.62)	(1.58)
Audit Announcement	-0.71	-1.03
X Client X Post	(1.69)	(1.63)
Constant	51.25***	46.55***
	(1.18)	(1.23)
Controls	No	No
Controls X Audit Announcement	No	No
Controls X Audit Announcement X Post	No	No
Controls X Post	No	No
Supplier	Yes	Yes
Supplier X Post	Yes	Yes
Supplier X Audit Announcement	Yes	Yes
Month fixed effects	Yes	Yes
Audit Announcement	Yes	Yes
Number of observations	39,606	39,606
Number of firms	2,829	2,829
R-squared	0.003	0.003

Notes: Linear probability models of the probability of an increase in declared VAT since the previous year and the probability of declaring more than predicted. Time period includes 4 months after mailing of the letters. Robust standard errors in parentheses, clustered at the level of the audited firm. All coefficients and standard errors multiplied by 100 to express effects in percent. *** p<0.01, ** p<0.05, * p<0.1

Table A5: Deterrence Letter Experiment: Effects on Different Types of Transactions 6 post-treatment months

	(1)	(2)	(3)	(4)
	Percent Sales > Previous Year	Percent Input Costs > Previous Year	Percent Intermediary Sales > Previous Year	Percent Final Sales > Previous Year
Deterrence X	1.08***	0.08	0.10	1.28***
Post	(0.21)	(0.20)	(0.18)	(0.20)
Constant	55.41***	53.30***	38.38***	45.03***
	(0.14)	(0.14)	(0.14)	(0.15)
Month fixed effects	Yes	Yes	Yes	Yes
Deterrence letter	Yes	Yes	Yes	Yes
Number of observations	2,658,841	2,658,841	2,658,841	2,658,841
Number of firms	133,156	133,156	133,156	133,156
R-squared	0.01	0.01	0.01	0.01

Notes: Regression of the probability of the line-item (total sales, total input costs, intermediary sales, and final sales) being higher than in the same month the previous year, among firms that have both final and intermediary sales in the period prior to treatment. Coefficients are multiplied by 100 to be interpretable as percent, for ease of reading. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

D.4 Logit and Probit

Table A6: Differential Spillovers: Suppliers vs. Clients

Panel A: Logit

	(1)	(2)
	Percent > Previous Year	Percent > Predicted
Audit Announcement	0.19***	0.16***
X Supplier X Post	(0.06)	(0.06)
Audit Announcement	-0.04	-0.03
X Client X Post	(0.06)	(0.06)
Constant	0.05	-0.14***
	(0.05)	(0.05)
Controls	No	No
Controls X Audit Announcement	No	No
Controls X Audit Announcement X Post	No	No
Controls X Post	No	No
Supplier	Yes	Yes
Supplier X Post	Yes	Yes
Supplier X Audit Announcement	Yes	Yes
Month fixed effects	Yes	Yes
Audit Announcement	Yes	Yes
Number of observations	45,264	45,264
Number of firms	2,829	2,829
Pseudo R-squared	0.002	0.002

Panel B: Probit

	Percent > Previous Year	Percent > Predicted
Audit Announcement	0.12***	0.10***
X Supplier X Post	(0.04)	(0.04)
Audit Announcement	-0.02	-0.02
X Client X Post	(0.04)	(0.04)
Constant	0.03	-0.09***
	(0.03)	(0.03)
Controls	No	No
Controls X Audit Announcement	No	No
Controls X Audit Announcement X Post	No	No
Controls X Post	No	No
Supplier	Yes	Yes
Supplier X Post	Yes	Yes
Supplier X Audit Announcement	Yes	Yes
Month fixed effects	Yes	Yes
Audit Announcement	Yes	Yes
Number of observations	45,264	45,264
Number of firms	2,829	2,829
Pseudo R-squared	0.002	0.002

Notes: Logit regressions in Panel A and probit regressions in Panel B of the probability of an increase in declared VAT since the previous year and the probability of declaring more than predicted. The controls in Columns (2) and (4) are firm sales, sales/input-ratio, final sales share and industry categories as "hard-to-monitor." Time period includes 6 months after mailing of the letters. Robust standard errors in parentheses, clustered at the level of the audited firm. All coefficients and standard errors multiplied by 100 to express effects in percent. *** p<0.01, ** p<0.05, * p<0.1

Table A7: Deterrence Letter Experiment: Effects on Different Types of Transactions

Panel A: Logit

	(1)	(2)	(3)	(4)
	Percent Sales > Previous Year	Percent Input Costs > Previous Year	Percent Intermediary Sales > Previous Year	Percent Final Sales > Previous Year
Deterrence X	0.046***	0.003	0.005	0.057***
Post	(0.009)	(0.008)	(0.009)	(0.009)
Constant	0.217***	0.132***	-0.473***	-0.199***
	(0.006)	(0.006)	(0.006)	(0.006)
Month fixed effects	Yes	Yes	Yes	Yes
Deterrence letter	Yes	Yes	Yes	Yes
Number of observations	2,392,529	2,392,529	2,392,529	2,392,529
Number of firms	133,156	133,156	133,156	133,156
Pseudo R-squared	0.011	0.009	0.006	0.005

Panel B: Probit

	(1)	(2)	(3)	(4)
	Percent Sales > Previous Year	Percent Input Costs > Previous Year	Percent Intermediary Sales > Previous Year	Percent Final Sales > Previous Year
Deterrence X	0.029***	0.002	0.003	0.035***
Post	(0.005)	(0.005)	(0.005)	(0.005)
Constant	0.136***	0.083***	-0.295***	-0.125***
	(0.004)	(0.004)	(0.004)	(0.004)
Month fixed effects	Yes	Yes	Yes	Yes
Deterrence letter	Yes	Yes	Yes	Yes
Number of observations	2,392,529	2,392,529	2,392,529	2,392,529
Number of firms	133,156	133,156	133,156	133,156
Pseudo R-squared	0.011	0.009	0.006	0.005

Notes: Logit and probit regressions of the probability of the line-item (total sales, total input costs, intermediary sales, and final sales) being higher than in the same month the previous year, among firms that have both final and intermediary sales in the period prior to treatment. Coefficients are multiplied by 100 to be interpretable as percent, for ease of reading. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

D.5 Estimates Based on Post-Treatment Observations

Table A8: Differential Spillovers: Suppliers vs. Clients

	(1)	(2)
	Percent > Previous Year	Percent > Predicted
Audit Announcement X	2.85**	3.88**
Supplier	(1.42)	(1.57)
Audit Announcement X Client	-2.26	-0.96
	(1.51)	(1.73)
Constant	49.86***	46.50***
	(1.12)	(1.29)
Supplier	Yes	Yes
Number of observations	16,974	16,974
Number of firms	2,829	2,829
R-squared	0.002	0.004

Notes: Column (1) shows the probability of a tax increase since the previous year and Column (2) the probability of declaring more than predicted for post-treatment observations only. Robust standard errors in parentheses, clustered at the level of the audited firm. All coefficients and standard errors multiplied by 100 to express effects in percent. *** p<0.01, ** p<0.05, * p<0.1

Table A9: Deterrence Letter Experiment: Effects on Different Types of Transactions

	(1)	(2)	(3)	(4)
	Percent Sales > Previous Year	Percent Input Costs > Previous Year	Percent Intermediary Sales > Previous Year	Percent Final Sales > Previous Year
Deterrence X	0.970***	-0.308	0.002	1.443***
Post	(0.248)	(0.237)	(0.233)	(0.253)
Constant	45.427***	44.189***	32.295***	36.984***
	(0.115)	(0.110)	(0.108)	(0.117)
Number of observations	509,916	509,916	509,916	509,916
Number of firms	127,479	127,479	127,479	127,479
R-squared	0.000	0.000	0.000	0.000

Notes: Regression of the probability of the line-item (total sales, total input costs, intermediary sales, and final sales) being higher than in the same month the previous year, among firms that have both final and intermediary sales in the period prior to treatment for post-treatment observations only. Coefficients are multiplied by 100 to be interpretable as percent, for ease of reading. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

E Research design diagrams