No Taxation without Information
Deterrence and Self-Enforcement in the Value Added Tax[*]

Dina Pomeranz[†]

Harvard University and NBER

October 2014

Abstract

Claims that the VAT facilitates tax enforcement by generating paper trails on transactions between firms contributed to widespread VAT adoption worldwide, but there is surprisingly little evidence. This paper analyzes the role of third party information for VAT enforcement through two randomized experiments among over 400,000 Chilean firms. Announcing additional monitoring has less impact on transactions that are subject to a paper trail, indicating the paper trail's preventive deterrence effect. This leads to strong enforcement spillovers up the VAT chain. These findings confirm that when taking evasion into account, significant differences emerge between otherwise equivalent forms of taxation.

JEL codes: H26, H25, O17, O38

A fundamental constraint for taxation is that governments need to be able to observe transactions in order to impose a tax on them. A growing 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).[1] Third-party reporting, verifiable paper trails, and whistleblowers are thought to play an important role in facilitating tax enforcement (Kopczuk and Slemrod, 2006; Kleven et al., 2009, 2011; Kumler et al., 2013). The challenge of enforcing taxation is particularly severe in developing countries, where many transactions in the economy are not readily observable by the government, and it has been argued that these limited sources of information can explain some of the key differences in tax systems between developed and developing countries (Gordon and Li, 2009; Besley and Persson, 2012).[2]

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 third-party reported paper trail on transactions between firms, which makes it harder to hide the transaction from the government (e.g. Tait, 1972; Burgess and Stern, 1993; Agha and Haughton, 1996; Kopczuk and Slemrod, 2006). This belief contributed to one of the most significant developments in tax policy of recent decades (Keen and Lockwood, 2010): a striking increase in 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 reported paper trails for tax enforcement, and tests for the self-enforcing properties in the VAT through two randomized field experiments with over 445,000 firms in Chile. A first experiment, the large-scale "Letter Message Experiment," evaluates the effects of the VAT paper trail across the entire economy in a fully developed VAT system. It investigates whether the VAT paper trail lowers the tax payment response to an increase in the perceived audit probability of randomly chosen firms. This would suggest that the paper trail has a preventive deterrence effect on evasion. Since a paper trail facilitates detection of evasion during the audit, one would expect firms to respond more to an increase in the audit probability where a paper trail is present, if evasion levels are equal across transactions. Observing that a given firm responds less on its transactions

---

[1] Information constraints have been a key feature of models in the optimal taxation literature as well. While ability is not observable in these models, most transactions in the economy are typically assumed to be observable.

[2] 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 not only severely restrict funding for basic public infrastructure, they can also lead to significant distortions in the economy. Even in the US, overall tax evasion is estimated to be around 16% (Internal Revenue Service, 2008), a loss similar in size to the entire corporate income tax.

that are covered by a paper trail therefore suggests that the preventive deterrence effect of the paper trail was strong enough to reduce evasion to the point where an increase in the audit probability has a smaller effect. The experiment exploits the fact that the incentive structure built into the VAT system, which generates the paper trail, breaks down at the final production stage, where sales are made to consumers, rather than other firms: 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 Chilean Tax Authority sent letters indicating an increased audit probability to over 100 thousand randomly selected firms. While the letters generate an immediate and strong increase in VAT payments, this effect is much weaker on transactions between firms, where the paper trail is present, than on sales to final consumers, where there is no VAT paper trail. This holds true not only when comparing between firms with different shares of final sales, but also when comparing the response of different types of transactions within a given firm. 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. There is suggestive evidence that a substantial fraction of the higher response in smaller firms may be driven by their higher propensity to sell at the retail level, where the VAT paper trail is absent.[3]

Conceptually, this type of intervention represents an indirect use of randomized experiments, as defined by Khwaja and Mian (2011). It allows studying an existing policy at a large scale, even if the policy itself cannot be randomized. The idea is to test whether randomly induced variation of another factor that interacts with the policy (in our case the perceived audit probability) generates the response that would be predicted based on the underlying mechanism if the policy is effective. The two experiments provide both types of interventions in the classification of Ludwig et al. (2011). The Letter Message Experiment is an (indirect) policy evaluation, aimed at testing whether the policy works on a nation-wide level, while the second experiment, the "Spillover Experiment," is a mechanism experiment, aimed at showing the underlying mechanism that leads the policy to be effective.

The Spillover Experiment was designed to find direct evidence for the self-enforcing mechanism that underlies the findings of the Letter Message Experiment. It examines the transmission of tax enforcement through the VAT paper trail up the production chain. The idea is to jumpstart the effect in a sample where it is not currently well enforced, by injecting

---

[3]In order to test whether the impact of the letter really stems from deterrence, I also compare its effect to both a motivational letter that appeals to tax morale and social norms, and a placebo letter that contains information that is irrelevant for tax compliance. The methodology of analyzing the impact of different letter messages on tax payments was first developed by Slemrod et al. (2001), and has recently been employed by Engstrom and Hesselius (2007), Castro and Scartascini (2013), and Fellner et al. (2013), shedding light on the impact of deterrence and motivational appeals on tax payments by individuals in developed countries.

deterrence into the system and observing how it propagates along the paper trail and up the production chain. Half of a selected sample of 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 for the 1,527 firms that the tax authority was able to audit, information about their pre-treatment trading partners was collected.[4] The randomly administered audit announcement leads to strong spillover effects that increase VAT payments by the suppliers of the treated firms. In line with the asymmetric incentives between clients and suppliers in the self-enforcing mechanism of the VAT, tax payments of client firms do not increase.

Taken together, the two experiments show that for a given firm, the VAT paper trail acts as a substitute to the firm's own audit probability, and globally the paper trail acts as a complement to the audit probability, since its effectiveness gets multiplied through the spillover effects. This represents the first micro-empirical evidence for the self-enforcing properties of the VAT. Previous 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 provide the first evidence of how tax enforcement generates spillovers through firms' trading networks, and together with Rincke and Traxler (2011), they represent one of the first documentations of spillovers in tax enforcement overall. The results imply 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.

This paper also provides 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 (Slemrod and Yitzhaki, 2002). One notable exception is an intervention conducted concurrently to this field experiment by Kleven et al. (2011), who analyze the individual income tax in Denmark and formalize the distinction between third-party and self-reported income. They find that evasion is generally low, except for the small fraction of income for which the government does not already possess third party-reported information. These results confirm related findings for the income tax on a more aggregate level by the US Taxpayer Compliance Measurement Program (TCMP) (Internal Revenue Service, 1996, 2006).[5]

---

[4]For a discussion of attrition at the auditing stage, see the implementation section below.

[5] Alm et al. (2007) study a related question in a lab setting, by experimentally varying the portion of an individual's income that is subject to third-party reporting, and find that cheating increases as individuals earn larger shares of income that are not perfectly detectable.

The paper extends the findings of this literature in several ways. First, it looks at tax compliance by firms rather than individuals. Raising and enforcing tax payments from firms strongly reduces the number of agents the tax authorities must oversee (Kopczuk and Slemrod, 2006), and thus may be a more feasible approach for developing countries to increase tax revenue. Firms can play an important role of aggregators of information that facilitate tax enforcement. 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; Olken and Singhal, 2011; Carrillo et al., 2012; Kleven and Waseem, 2012; Kumler et al., 2013), there is still little micro-empirical evidence.

Third, the findings speak 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. Finally, and particularly relevant for developing countries, the paper finds that third-party records strongly affect tax compliance even in a context where they are not automatically accessible to the tax authority.[6]

The remainder of this paper is structured as follows: Section 2 provides background on the VAT in Chile and on the mechanism of the self-enforcement hypothesis of the VAT, Section 3 describes study design, data and estimation strategy, and Section 4 shows the results of the Letter Message Experiment and the Spillover Experiment in turn. Section 5 concludes.

1   Context

1.1   Background on the VAT and its Use in Chile

A large majority of countries around the world have a VAT, and for many developing countries it represents the largest source of tax revenue. In Chile, the VAT accounts for about half of tax revenues (Servicio de Impuestos Internos, 2010a). Chile has a single 19% VAT rate, which is paid monthly. Firms pay VAT on the difference between total sales and total input costs. The overall tax base – total value added in the production chain – is therefore equivalent to that of a sales tax, which is paid on the entire final value at the

---

[6]For most transactions in Chile, as in many 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.

retail stage.[7] 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. This difference is irrelevant in most standard tax models, but is thought to have significant implications when taking tax administration and tax evasion into account (Slemrod, 2008).

Firms have to document their declared amounts of "tax debit" (sales and other sources of revenue) and "tax credit" (input costs) with the original receipts.[8] If the tax credit is greater than the debit for a given month, the excess can be carried over to the following month as a credit (Servicio de Impuestos Internos, 2011). Firms are allowed to claim rebates, but in practice few do so as it is time consuming and triggers an automatic audit.

A firm's book of purchases therefore contains documentation about the sales of its suppliers. This leads to the third-party paper trail along the production chain (henceforth referred to as "paper trail"), which allows the tax authority to cross-check the two firms' records against each other. In Chile, as in many countries, most firms do not have to submit this information to the tax authority. Only very large firms, and what was at the time of the study a small number who choose to use an online filing system, do so. For all other firms, this information can be accessed by the tax authority through an audit, but not as a matter of routine. For this reason, the tax authority does not generally know which firms trade with one another. The spillover experiment was therefore designed to collect such information.

To prevent fraudulent production of fake input receipts or duplications, all receipts have to be pre-approved and stamped by the Chilean tax authority. These receipts are numbered so that missing sales receipts must be explained at audit. The tax authority uses the part of the cross-checkable information that it has on file (i.e. sales and purchases of large firms and firms that use electronic accounting, as well as information from previous audits) to calculate whether tax declarations display discrepancies with third party reported information. This calculation is performed both for the annual income tax returns and monthly VAT returns. Letters are sent to the taxpayers with the largest discrepancies (about 50 thousand per year), requiring them to explain or correct the situation. Outstanding tax refunds are withheld until that situation has been resolved. Tax officials also regularly visit all retail stores to check that the books are well-kept and that correct receipts are given to customers.

Official estimates of VAT evasion in Chile, based on the comparison of collected VAT to aggregate consumption data from the central bank, have ranged between 27% and 12% since 1990, with significant fluctuations from one year to the next (Servicio de Impuestos Internos, 2010b). Only a few industries are exempt and there is no minimum size threshold for firms

---

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

[8]This collection method is commonly known as the "credit-invoice method." The vast majority of countries with a VAT currently use the credit-invoice method with a few exceptions, such as Japan that uses the subtraction method (Grinberg, 2010).

to be subject to the VAT.[9] This context allows for analysis of the VAT across a large set of different firm types, without the interference of confounding institutional factors such as industry-specific exemptions or varying VAT rates.

1.2   Mechanism of the "Self-Enforcing" Properties of the VAT

The "self-enforcement" hypothesis in the VAT is based on the premise that firms have an incentive to ask their suppliers for receipts because they can deduct input costs from their VAT bill (Agha and Haughton, 1996). This incentive builds the creation of paper trails directly into the tax structure. 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). The buyer acts as the third-party, recording a transaction which creates a liability for the supplier. There is no scope for gains from collusion between two parties in the middle of the VAT chain since the two sides of a transaction in inter-business trade have opposing incentives: the buyer benefits from overstating the input cost, while the seller benefits from understating the sale. This mechanism breaks down at the end of the chain, since the final consumer has no incentive to ask for a receipt.[10]

To illustrate, let us index the firms along the value chain by $i \in 1, 2, \ldots N$, where N is the final firm in the production chain; denote by $\widehat{c}_i$ and $\widehat{s}_i$ the reported amount of input costs and sales of firm i respectively, and denote by c_i and s_i the actual amounts. On a trade between two firms, i and $i+1$, firm $i+1$ would want a receipt from firm i in order to be able to deduct firm i's sale s_i from its input costs c_{i+1}. The key assumption behind the notion that "self-enforcement" breaks down at the retail stage is that all else equal, the cost of evasion will be lower at that point than in the middle of the production chain because firm N is not faced by a firm $N+1$ that would want a receipt. Instead, firm N is faced by a consumer who does not require the receipt. In this case, no collusion is required to omit the declaration of sales by firm N.

There are several reasons why this self-enforcing mechanism might not work in practice. First, as mentioned above, most firms do not have to report this third-party information directly to the government. The self-enforcing mechanism depends on the deterrence effect of a cross-check conducted during a possible audit. The word "self-enforcement" is therefore misleading, since it can be expected to work only in interaction with credible deterrence on

---

[9]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, exports are VAT free.

[10]For this reason, several countries have tried to increase the incentive for the consumer by imposing fines for consumers who do not ask for a receipt, organizing lotteries with consumer receipts, allowing consumers to deduct part of their VAT payments from the income tax, etc. (Naritomi, 2013).

the part of the tax authority. Second, since the mechanism breaks down at the final stage, it can potentially unravel from the bottom if collusion builds up from the final stage.

If the mechanism works, we would expect a preventive deterrence effect on transactions covered by the paper trail. This will affect how firms respond to an increased audit probability in the following way. The information contained in the paper trail facilitates detection of evasion during an audit. At a given level of evasion, one would therefore expect firms to respond more to an increased audit risk on transactions with a paper trail. The anticipation of this ease of detection can lead firms to reduce evasion ex-ante on transactions with a paper trail. If this preventive deterrence effect is strong enough, it may reduce evasion to the point where an increase in the audit probability has a smaller effect on transactions covered by a paper trail (as there is less evasion on these transactions to begin with). At that point, the paper trail and the audit probability will interact in a substitutive way.

Observing firms responding less on transactions with a paper trail therefore suggests that ex-ante evasion was lower on these transactions. The Letter Message Experiment tests whether this is the case.[11] The Spillover Experiment complements the findings of the Letter Message Experiment by showing the mechanism by which the preventive deterrence effect comes about.

We can distinguish two forms of VAT evasion on inter-firm transactions, and spillover effects can operate for both. "Unilateral evasion" occurs when a paper trail is created but firms take the risk of under-reporting their tax obligation despite the fact that this leads to discrepancies between the amounts reported by the buyer and seller.[12] This is based on the hope that the tax authority will not cross-check the records. It does not require any coordinated actions between buyer and seller. Through "collusive evasion" a transaction is either completely omitted from the books of both the seller and the buyer or underreported (with the reported amount matching across the two books). This requires collusion, and as discussed above, reduces overall VAT payments if it is carried through to the end of the production chain.

[Table 1]

Expressed in the notation introduced above firm i can be involved in unilateral VAT evasion in two ways: $\widehat{c}_i > c_i$ or $\widehat{s}_i < s_i$, which correspond to $\widehat{c}_i > \widehat{s}_{i-1}$ and $\widehat{s}_i < \widehat{c}_{i+1}$ respectively. Collusive evasion implies: $\widehat{c}_i \neq c_i$ but $\widehat{c}_i = \widehat{s}_{i-1}$, or $\widehat{s}_i \neq s_i$ but $\widehat{s}_i = \widehat{c}_{i+1}$. In order for a collusive equilibrium to be incentive compatible, it must be the case that

---

[11]As discussed in more detail in the results section, the necessary assumption is that a lower response is not driven by other differences within firms for the type of transactions covered by a paper trail.

[12]In order to reduce tax liability, the seller will tend to understate the value of a transaction, while the buyer will tend to inflate it.

$c_i - \widehat{c}_i \le s_i - \widehat{s}_i$. Intuitively, sales under-reporting must be greater than cost under-reporting for this to be beneficial to the firm. As discussed above, there is no scope for collusion in the middle of the chain due to the fact that firms have opposing incentives in reporting transactions. Collusive evasion must flow up the chain from a firm, which can unilaterally evade taxes. If firm n engages in collusive evasion, then $\widehat{s}_i < s_i$ and $\widehat{c}_i < c_i$ for all $i \ge n$ up to N within the chain of firms engaged in collusive evasion.

Let us now analyze what happens to spillovers for both unilateral and collusive evasion if monitoring is increased with a pre-announcement for firm n at some point within the production chain. In the case of unilateral evasion, the spillovers are symmetric for suppliers and clients. These spillovers act through information: upstream and downstream firms may learn about the audit pre-announcement that firm n received. They may thus update their priors about the probability that their receipts from trades with firm n will be cross-checked against firm n's receipts or that they will be audited themselves. This may lead them to revise their declared transactions to be consistent with those of firm n, thereby reducing evasion.

In the case of collusive evasion, an increase in the audit probability of firm n increases the likelihood that it will insist that the transactions with its trading partners be "on the books." Hence, the audit pre-announcement can lead to the creation of a paper trail. Suppose firm n, which is in the interior of a colluding chain $i, i+1, \dots, N$, is sent an audit pre-announcement. If firm n starts to create a paper trail, this can hurt its supplier firm $n - 1$, as it may cause that firm to increase its declared sales and ultimately its tax liability. Firm $n-1$ may react by simultaneously increasing its declared inputs as well, which may hurt firm $n - 2$ in turn. So the collusion may break all the way upstream. We move from a collusive equilibrium between firms $i, i+1, \dots, N$ to a new partially collusive equilibrium between firms $n + 1, \dots, N$.

The increase in declared sales by firm n benefits the client firm, $n + 1$, which can now declare higher input costs. Thus the client firm can report a lower tax liability.[13] These asymmetric effects are illustrated in Table 1. Reducing collusive evasion by firm n in the middle of the chain does not necessarily increase total government tax revenue. Instead it may simply lead to a transfer from firm n and those upstream of it to those downstream of it. This mirrors the difficulty of evading the VAT through collusion: collusive evasion in the VAT that is not combined with unilateral evasion at the retail stage or in the middle of the chain, simply leads to transfers between firms along the chain, rather than to an increase in overall evasion.

Taking both types of evasion together, the effect of an increase in the audit probability on

---

[13]Depending on the bargaining between firms $n + 1, \dots, N$, some of this gain may be passed down the value chain by $n + 1$ reporting higher sales so that firm $n + 2$ can report higher input costs, etc.

reported tax liability is positive for suppliers and ambiguous for clients, depending on which type of inter-firm evasion dominates. The Spillover Experiment tests whether increasing a firm's audit probability indeed increases VAT payments by its trading partners, and whether this effect is asymmetrically concentrated on its suppliers.

2 Study Design, Data and Empirical Specification

2.1 Study Design: Letter Message Experiment

Both randomized field studies analyzed in this paper were conducted in collaboration with the Chilean Tax Authority ("Servicio de Impuestos Internos"). A research design diagram can be found in Figure 1. The goal of the "Letter Message Experiment" is to evaluate the effectiveness of the VAT paper trail across the entire economy in a fully developed, well-functioning VAT system. The tax authority sent out letters aimed at varying the perceived audit probability of randomly chosen firms.[14] The goal is to test for differential responses by whether transactions are covered by the VAT paper trail, i.e. whether the transactions are between firms or to the final consumers. As discussed in Section 2, if we find that transactions that are subject to a paper trail respond less to an increase in the perceived audit probability, this indicates that the paper trail had a preventive deterrence effect prior to the intervention.

A deterrence message was sent to 102,000 randomly selected firms in a stratified sample among most firms in the country.[15] The letter is aimed at increasing the perceived audit probability by informing firms that they have been randomly selected for analysis and emphasizes that if irregularities are detected, they could be subject to an audit.[16] 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 routinely analyzes all firms, and firms may always be audited if irregularities are detected.[17]

---

[14]As discussed in Section 2.3 below, the letter content was pre-tested extensively to ensure its intended interpretation.

[15]The sample consists of the universe of Chilean firms that were operating in June 2008, had declared a positive amount of VAT for at least one month between July 2007 and June 2008, had a valid postal address, and were not included in the spillover experiment.

[16]The exact language of the letter is as follows: Mr.(s) Taxpayer, This is an informational letter and, therefore, does not require that you take any action vis-a-vis the SII. We would like to inform you that in a process of random selection process among micro, small and medium size enterprises, your firm has been selected for analysis. In the event that any irregularities are detected, you could be summoned 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.

[17]It is important to note that even though used surprisingly frequently in practice, sending out deterrence letters which are not backed up by a corresponding increase in the actual audit probability is not a policy

The impact of the letter is measured by comparing the VAT payments of recipient firms to payments by the 306,600 firms of the study sample that did not receive any letter. The idea is to overcome the challenge that it is inherently difficult to determine the degree of evasion at different points in the production chain based on audits. During an ex-post audit, it is typically not possible to find out how much VAT was evaded by omitting emission of receipts to final consumers. The innovation of this paper is therefore the use of an indirect, "mechanism" experiment to look at firms' responses as indicators for the underlying level of evasion.[18] Since the paper trail makes tax evasion easier to observe, all else equal, one would expect an increase in the audit probability to lead to a stronger response for transactions between firms, where a paper trail is present. If, instead, we observe that alerting firms that they are being monitored has less of an effect for these transactions, this suggests that firms had already internalized the higher probability of detection on inter-firm transactions and had reduced evasion on these transactions prior to receipt of the letter, i.e. the paper trail had a "preventive deterrence effect." Given the large size of the treated group, it is of course plausible that there were spillover effects on the control group. This would bias the estimated effect of the treatment downwards since the spillover effects would reduce evasion in the control group as well.

The goal of the Letter Message Experiment is, however, not to measure the overall effect of the letter on compliance. The intention is to shed light on the effectiveness of the paper trail by analyzing how it interacts with the expected audit probability. This is achieved by comparing the responsiveness of line-items in the VAT declaration that are covered by the paper trail, i.e. transactions between two firms – to line items that are not – i.e. sales to final consumers. We can do this both across firms and for different types of transactions within firms. The latter allows holding constant any difference that may exist across firms such as size, risk aversion, ease of hiding evasion etc.

Finally, the Letter Message Experiment also allows testing predictions about firm size.[19] Kleven et al. (2009), for example, posit that large firms evade fewer taxes since they run a higher risk that a whistle-blower among their employees informs the government about unreported transactions. We might therefore expect that firm size, similarly to the VAT

---

that seems sustainable in the long-run. As a tool of analysis, it can be used to study differential responses by line item or type of tax payer, both for academic research or to optimize audit strategies (see Pomeranz et al., 2014). However, ideally, tax authorities should combine it with real changes in audit policy, since it could otherwise start undermining the agency's credibility. Nevertheless, the strong response to the second wave of this experiment suggests that the letters do not lose their power very quickly.

[18]For further discussion of this approach, see Khwaja and Mian (2011).

[19]The Letter Message Experiment was also used to analyze other firm characteristics not reported in this paper, which are of interest to the tax authority, such as firm age, region, or industry. Based on the analysis of what type of firms are more likely to respond, we developed an instrument to optimize audit strategies (see Pomeranz et al., 2014).

paper trail, has a preventive deterrence effect that leads to a lower response to an increase in the audit probability. At the same time, smaller firms also tend to have a larger share of sales going to final consumers. Controlling for both firm size and share of final sales, we can get a sense of how much of the differential response by size may be driven by the degree of retail sales.[20]

To test whether the impact of the letter stems from deterrence, two additional letters were each sent to a sample of about 18,500 firms: a tax morale letter aimed at affecting perceived social norms and a "placebo" letter to test whether the effect is driven by the simple fact of receiving mail from the tax authority. The placebo letter informs firms about new features on the tax authority's website. The tax morale letter contains a message aimed at increasing the perceived social norm of tax compliance. It emphasizes that Chile has one of the highest levels of tax compliance in the world and encourages firms to continue on this path in order to further develop the country.[21]

2.2 Study Design: Spillover Experiment

While the Letter Message Experiment studies whether for representative firms across the whole economy, the paper trail interacts in a substitutive way with the audit probability, the Spillover Experiment is designed to show the underlying self-enforcing mechanisms in action. It analyzes whether, as predicted by the self-enforcement hypothesis, increased tax enforcement on one firm generates spillovers to its trading partners up the VAT chain. In order to analyze how the self-enforcing mechanism gets triggered by injecting deterrence into the system, the Spillover Experiment focuses on a set of firms where compliance is ex-ante expected to be low.

The sample consists of mostly rural, micro size firms with tax declarations that show patterns suggestive of evasion and much lower declared VAT than comparable firms in the same industry. Many of these firms had submitted VAT declarations regularly each month and had not been found guilty of any infractions, but they continually reported sales smaller than their input costs, without going out of business.[22] This sample was chosen in order to focus on a segment in the economy where both unilateral and collusive evasion could

---

[20]However, it is important to keep in mind that such cross-firm comparisons have to be interpreted with caution, since in contrast to the within-firm estimates, other unobserved firm characteristics may affect the estimates.

[21]Motivational messages have been found to increase voluntary cooperation in some instances. For example, Dal Bó and Dal Bó (2014) find that moral suasion can increase cooperation in the lab, especially when coupled with deterrence, and Fellner et al. (2013) find that social norm letters increase compliance with broadcast tax obligations for those who live in regions with generally low compliance.

[22]Even though sales can temporarily be lower than input costs, for example when firms buy large, costly inputs, over the long-run this pattern raises suspicion.

be expected to be high. The fact that many firms in the sample had zero or negative reported VAT liability created scope for a collusive equilibrium to emerge and to flow up the production chain. Since the self-enforcement mechanism could be expected to be quite weakly enforced ex-ante, increasing deterrence had the potential to jump-start the mechanism.

The intervention of the Spillover Experiment is as follows. 5,600 firms were scheduled for an audit. Half of them were randomly selected to receive a pre-announcement, which informed them that there was going to be an upcoming audit between June and August 2009.[23] The other half did not receive any message. Half a year later, the tax authority started summoning all firms in the sample for an audit. Since the tax authority does not have information on which firms trade with which, information about trading partners was collected during the audits. For the firms the tax authority was able to audit,[24] auditors recorded information about the trading partners from the transaction records in the firms' books of sales and purchases for the three months prior to the mailing of the pre-announcement. This made it possible to identify the firms' main suppliers and clients in a period not yet affected by the treatment. Based on this information, the spillover effects of the audit pre-announcement on suppliers and clients of the treated firms can be measured by comparing their declared VAT to the declared VAT of suppliers and clients of the control firms,[25] before and after the time when the audit pre-announcement was sent out. Finding that trading partners of treated firms increase their declared VAT compared to trading partners of control firms indicates a spillover effect, since whether or not ones' trading partner was treated is randomly assigned.

2.3 Implementation and Data

The sample of firms considered in this study consists of the universe of 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 subsample of 5,600 firms was selected for the Spillover Experiment, the remainder is in the Deterrence Letter Experiment. The only firms

---

[23]The exact language of the letter is as follows: Dear Taxpayer, This is an informational letter that does not require you take any action vis-a-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 the future audit. Later, you can expect a formal notification letter, which will inform you about which SII office you need to attend, what documents you need to bring and the date of the audit.

[24]For a discussion of attrition at the auditing stage see Section 2.3 below.

[25]One potential confounding factor for this estimation strategy could occur if firms that received a pre-announcement selectively removed or changed the trading partners in their books. This is discussed in more detail in Section 2.3, which provides evidence that the number and characteristics of trading partners reported by treated firms do not differ from those of control firms and discusses why differential manipulations between treated and control firms may not be likely in this setting.

that were excluded from the study were very large firms and firms in the Letter Message Experiment that had invalid postal addresses. The main data consists of information from monthly VAT declarations, starting in January 2008. In addition, I merge this data with firm characteristics such as size, number of employees, industry, etc.

Implementation and Summary Statistics: Letter Message Experiment

Most letters in the Letter Message Experiment were sent in early December 2008, affecting tax declarations starting in November 2008, which are due the following month. A smaller, also randomly chosen second wave was mailed five months later, in order to study whether the effectiveness of the letters decreased over time. The text of all letters was pre-tested extensively to ensure their intended interpretation. A series of iterative pre-tests was conducted with almost 100 firm representatives (SME owners, legal representatives, and employees in charge of firms' tax processes) prior to the mailing. The results showed that the letter strongly increased the perceived probability of audit, and thereby the expected cost of evasion. We can therefore interpret the strong, immediate increases in declared taxes following the letters as a response to an increase in the risk of being audited.

In order to increase compliance with treatment assignment in the study sample, we were able to undertake a special procedure with the Chilean Postal Service to exclude firms with invalid postal addresses from both the treatment and the control group of the Letter Message Experiment, leaving a sample of 445,734 firms. Excluding firms with invalid postal addresses from the start allowed us to mitigate the large reduction of statistical power that ensues when a substantial part of the intent-to-treat sample is not reached. In the sample of firms with valid postal addresses, the share of treatment firms that did not receive the letter was 6% compared to 26% in the full universe. Without this procedure from the postal office we would not have known which firms in the control group would have received a letter they had been in the treatment group. Firms with invalid addresses would therefore have had to be maintained in the estimation sample, leading to strongly reduced statistical precision.

Table 2 presents summary statistics for the Letter Message Experiment, compared to the control group. None of the differences are statistically significant at the 10%-level.

[Table 2]

Firms in the Letter Message Experiment pay an average of 264,000 pesos (about 500 USD) in monthly VAT, with a median of about 70,000. The large difference between the mean and the median indicates a very large dispersion in the distribution of tax payments. As discussed below, this dispersion has implications for the empirical specification.

14

Firms in Chile are categorized into five groups by size, based on their revenues during the preceding tax year: micro, small, medium, large-sized firms, and firms with no sales in the preceding tax year.[26] Micro-sized firms are by far the largest group, comprising 74.5% of the firms, followed by small firms (18.2%), medium size (2.8%), and firms with no sales in the preceding year (1.5%). The remaining three percent are new firms that have not yet been classified. Firms are also 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 final consumers is 45.6%.

Implementation and Summary Statistics: Spillover Experiment

The letters containing the audit pre-announcement for the Spillover Experiment were also sent in early December 2008, affecting tax payments starting in November 2008. As discussed above, the sample of the Spillover Experiment consists of mostly rural micro-size firms, which were suspected of evasion. Since many of these small firms were located in remote areas, there was a substantial fraction with no valid postal address. In these cases, the tax authority agreed to deliver the audit pre-announcement in person to the firms' location. They were able to reach 96% of assigned firms, but there were several weeks of delay for part of them.

Table 3, Columns (1) and (2) present summary statistics for the 5,600 firms in the original sample of the Spillover Experiment. None of the differences between treatment and control group are statistically significant at the 10% level. Since this sample was selected for having suspiciously high input costs compared to their sales, it is not surprising that their reported sales/input-ratio is low, averaging only 0.67, and 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 tax deduction from input costs than liabilities from sales, and the median VAT declaration is zero. The sample consists of very small, rural firms, mostly in remote areas: all are micro size, and they are among the smallest even within that category, with an average of 2.3 on the official firm size sub-classification within micro size, which ranges from 2 to 4.[27] Compared to other firms in the country, they also have a smaller share of final sales (16%), and are more likely to be in the agricultural sector (54%).

[Table 3]

---

[26]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 between 1.1 and 4.2 million USD, and large firms over 4.2 million USD.

[27]Given that the official size classification is based on declared sales, and that this sample is suspected of under-declaring true sales, this is likely to be an underestimate of their true size.

The fact that information about the trading partners was obtained during the audits lead to substantial attrition in the final sample for the Spillover Experiment. Due to administrative delays in the delivery of the audit notices, followed by the consequences of a very large earthquake, only about 27% of the 5,600 firms were audited and they provided information about 2,829 trading partners. One potential concern of this attrition is that it might introduce selection bias if it creates differences between the treatment and control group within the remaining sample. Since the attrition was mainly driven by the degree to which an area was affected by the earthquake, it is plausibly exogenous to the randomly assigned treatment. Columns (3) and (4) of Table 3 suggest that this is indeed the case. Treatment and control firms do not differ in their probability of being audited and there is no significant difference in the characteristics of treatment and control firms among the audited sample, apart from a somewhat higher proportion of firms in the agricultural sector.

A separate potential issue is that firms that received a pre-announcement might have selectively removed or changed the trading partners in their books. There are several reasons why this is unlikely in this case. First, as shown in Columns (5) and (6), trading partners of treated firms do not differ from those of control firms, except for a slightly lower share of months in which they failed to submit a declaration. Second, due to the audit method of the Chilean Tax Authority, such differential manipulations are not very likely, since all audited firms, including those in the control group, receive an audit notice in written form weeks before they come into the tax office to get audited. Finally, even if firms had selectively removed their most delinquent trading partners from their books, this would actually lead to a downward biased estimate of the spillover effects.

As the rest of the balance table shows, overall, the trading partners in the Spillover Experiment are substantially larger than the audited firms, both in terms of their VAT paid and their official size category. They have about the same age and a slightly higher share of sales to final consumers, and are much less likely to be in the agricultural sector. 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.

2.4 Empirical Strategy

The main outcome variable is declared VAT, i.e. 19% of declared sales minus declared input costs.[28] The dispersion of monthly declared VAT is very large. It ranges from -

---

[28]If this value is negative because input costs exceed sales in a given month, that amount can be carried over to the next month. All measures used in this paper exclude such carry-overs, so that the analysis focuses on the new transactions of the current month. As a robustness check, I also show a specification that includes carryovers.

800 billion pesos (equivalent to -1.7 billion USD) to 16 billion pesos (33 million USD). For illustration, Figure A1 in the Appendix shows this very large dispersion, even excluding the top and bottom 5% of values. At the same time, there is a large density at zero. Such a large variance and the fat tails of the distribution imply that analyzing the impact of the intervention on the mean of declared VAT does not lead to significant results because the resulting variance is extremely large. I therefore use quantile regressions as well as linear probability models for the likelihood that declared VAT is larger than three key thresholds: (1) zero for the extensive margin, (2) VAT declared in the same month of the previous year, and (3) the predicted value based on the control group.

The latter two specifications have the advantage of not being sensitive to scale. This is important, given that much of the analysis does not focus on the overall impact of the treatment, but rather on the comparison of the response for different types of transactions or firms. Since the amount in pesos will mechanically be larger in larger types of transactions or firms, and the probability of declaring zero VAT will be smaller, both the quantile regressions and the extensive margin are not appropriate to analyze differential responses. For such comparisons, measures that captured relative changes are indicated.

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 post-treatment period. The specification for the quantile regressions is therefore

$$Q_\tau(VAT_{it}|Z_i, t) = \alpha_\tau + \beta_\tau(Z_i \cdot post) + \gamma_\tau Z_i + \partial_t, \tag{1}$$

where Z_i is the treatment assignment dummy indicating that a firm is in the treatment group, ∂_t stands for month fixed effects and $Z_i \cdot post$ indicates treatment, i.e. a firm in the treatment group in the post-treatment period.

The linear probability models are analyzed using the following specifications:

$$T_{it} = \alpha + \beta(Z_i \cdot post) + \gamma Z_i + \partial_t + e_{it}, \tag{2}$$

where T_{it} is the binary tax outcome for individual i in month t. The probability of detecting any positive amount, $T_{it} = LargerZero_{it}$, captures the extensive margin. However, similar to the quantile regression, this measure will vary with the size of transactions or firms. For analysis of differential responses, a measure capturing relative change is required. One specification that satisfies this concern is the linear probability model with the outcome dummy indicating whether declared VAT is higher in the current month compared to the same month a year earlier: $T_{it} = VATIncrease_{it}$. This outcome has several benefits com-

pared to alternative measures. First, as opposed to log specifications or count data models, it is applicable to variables that include both zeros and 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 VAT distribution.[29]

One possible remaining confounding factor could be differential time trends between different types of transactions, which could lead to differences in the probability of an increase compared to the previous year. As a robustness check, I therefore also run a specification with an outcome dummy that indicates whether declared VAT is higher than the predicted value for that firm in that month: $T_{it} = LargerPredicted_{it}$. The prediction is based on median regressions among the firms in the control group and uses as predictors the firms' pre-treatment VAT payments, as well as those characteristics, for which differential treatment effects are evaluated in this paper, such as size and the share of sales to final consumers.[30] For robustness, I also implement alternative estimation strategies for non-linear models following Athey and Imbens (2006) and Blundell and Dias (2009).

When comparing the impact of the letter for different line-items, I use an additional within-firm estimation to hold firm characteristics constant. For example, we can compare sales to input costs by comparing them within each firm and month. An interaction term between the type of transaction and treatment captures the differential impact by line-item. Including the necessary controls, this gives the following regression:

$$LineItemIncrease_{itl} = \alpha + \beta(c_i \cdot post) + c_i + \partial_t + \phi(Z_i \cdot post \cdot l) + \eta(post \cdot l) + \iota_l + e_{itl}, \quad (3)$$

where l indicates the line-item, c_i captures the firm fixed effects controlling for any between-firm variation, and $c_i \cdot post$ captures firm fixed-effects times treatment period, to control for any differential trends within the firms. ϕ therefore represents the coefficient of interest: the differential response of line item l for the treatment group in the treatment period.

All regressions of the Letter Message Experiment include both waves of mailing. Treatment firms are included until four months after treatment (the time during which the effect of the deterrence letter is the strongest), which is February 2009 for the first wave and

---

[29]One of the alternative specifications is quantile regressions 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.

[30]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. 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.

June 2009 for the second, and control firms are included until June 2009.[31] Since some pre-announcements were delivered with delays, a six months post-treatment window is used in the Spillover Experiment. This corresponds to an average of four months after delivering of the letter, to be equivalent to the post-treatment window of the Letter Message Experiment.[32]

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 changes in tax evasion. There may, however, also be a response of real economic activity. The increased tax payments following treatment reduce business profitability and may lead to increased prices, which, in turn, may decrease demand. All this may lead firms to reduce production. The observed increase in declared VAT may therefore be an underestimate of the reduction in evasion resulting from the treatment, since a reduction in production would lead to a decrease in declared VAT.[33]

3 Results

3.1 Letter Message Experiment

The Letter Message Experiment examines how the VAT-generated paper trail interacts with tax enforcement for representative firms across Chile, by testing whether the increase in the expected audit probability induced by the deterrence letter has a lower effect on reporting of transactions that are subject to the VAT paper trail – i.e. transactions between two firms – compared to transactions that are not – i.e. sales to the final consumer. The following section establishes the overall effectiveness of the deterrence message. It then shows that this increase in the perceived audit probability indeed has a smaller impact where the VAT paper trail is present, even when looking at differential effects within firms, holding all firm characteristics constant.

Overall Effectiveness of Deterrence

---

[31]Due to the random assignment, dropping one treatment group at an earlier date does not affect the validity of the results. All specifications include month fixed effects.

[32]Results from a time-varying instrumental variables approach, aimed at calculating the corresponding Treatment-on-the-Treated effect (available upon request), find similar if slightly larger effects than the Intent-to-Treat results reported in the paper. Robustness checks using four post-treatment months of the Intent-to-Treat specification are similar, if somewhat less statistically significant.

[33]If demand is sticky in response to changes in price, the short term response in evasion might be over-estimated (similar to the J-curve phenomenon in trade), while the longer term response would be under-estimated. This means that the decay in the effect of the letters would be slower than it appears in Figure 2.

Panel A in Figure 2 shows the impact of the deterrence letter on declared VAT of recipient firms, compared to the control group, which received no letter. The graph shows the percent difference between medians of the treated and control firms in each month. We see a marked jump in tax payments after receipt of the deterrence letter. The median declared VAT increases by about 12 percent and then slowly decreases to reach the same levels as the control group after about 15 months.[34] This large response speaks to the credibility of the Chilean Tax Authority in shifting firms' perceived monitoring risk and also indicates that firms do have scope to increase their tax declarations in the face of heightened deterrence.

[Figure 2]

Table A1 confirms the dynamics of the effect of the deterrence letter in regression form. The horizontal line marks the time of the mailing, with *t1* indicating the first month affected by the letter and the rows below showing subsequent months. Using the binary variables discussed above, it confirms the same pattern shown for the median in the graphical results above: a marked increase after the mailing of the letter and a steady decline thereafter.[35]

I compare the impact of the deterrence letter to the tax morale and placebo letters to determine whether deterrence drives the effect. Panels B and C of Figure 2 show that in contrast to the deterrence letter, no marked increase is visible at the time of the mailing. The apparent first increase in Panel B happens before mailing, and the second increase almost a year later is very unlikely to be due to the letter. The variance is larger due to the smaller sample size. This comparison shows that it is the content of the deterrence letter that drives the response, not simply the fact of receiving mail from the tax authority.

Table 4 shows the same result in regressions for the mean, median, probability of declaring more than in the same month of the previous year, probability of declaring more than predicted, and probability of declaring any positive amount. All specifications confirm a highly significant impact of the deterrence message, except for the regression using mean VAT, which as expected does not provide statistically significant results.[36] The deterrence letter led to a 1,326 peso increase in the median VAT per month, a 7.6% increase compared

---

[34]Figure A2 shows a similar pattern for the second wave of mailing. It indicates that even five months after the first wave, the deterrence message is perceived as credible. If anything, the treatment effect is stronger, increasing the median by up to 18%. A possible reason 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.

[35]Since the first four months have the strongest overall effect, all subsequent analysis for the Letter Message Experiment is conducted using four post-treatment months. Robustness checks using six post-treatment months, available upon request, show similar effects.

[36]The tax morale letter only has a significant effect on the margin of declaring a positive amount. In line with this, quantile results not shown here find no significant effects overall, except for an increase by those with very low or negative declared VAT. For these firms, receiving a letter about high compliance by others may have a deterrence effect, as it may suggest that the tax authority suspects them of evasion.

to the baseline median. The probability of declaring more than in the same month of the previous year increased by 1.4 percentage points, the probability of declaring more than predicted by 1.42 percentage points, and the probability of declaring any positive amount by 0.53 percentage points. All estimates are significant at the 1% level.

[Table 4]

For robustness, I also report estimates from alternative estimation strategies for the non-linear models in Table A2: Athey and Imbens (2006) for the changes-in-changes estimation on the median VAT and Blundell and Dias (2009) for a non-linear difference-in-difference estimate of the probability of an increase in VAT compared to the previous year. The results remain qualitatively the same. Finally, Table A3, Column (1) shows a robustness check that includes the carry-overs from previous declarations and also finds a very similar effect.

While the main purpose of sending the deterrence letters was to investigate the deterrence mechanism of the paper trail, the direct impact may be of interest for public policy. Tables A4 and A5 in the appendix show cost-benefit calculations for the deterrence letter on micro-size firms and find very high return rates (up to 24-fold marginal return).[37] However, these estimates do not include fixed costs, as well as potential reputational costs, and should therefore be interpreted with caution (see also footnote 17). For micro-size firms, the overall effect size of the letter message as a share of the control group mean is 1.9% and falls within the range of those observed in other deterrence letter message experiments:[38] 2.1% for non-self-employed middle income individuals and 8% for self-employed middle income individuals in Slemrod et al. (2001); 0.5% in Kleven et al. (2011); 3.6% in Hallsworth et al. (2014).

Interaction with the Paper Trail

Having established that the tax authority was able to credibly increase the expected audit probability, this section analyzes the differential impact for transactions covered by the VAT paper trail. It first compares different types of transactions overall and then within firms.

Table 5 displays the effect of the deterrence message for the different types of transactions. The first two columns show the two components of the VAT: sales and input costs. By definition, input costs are based on transactions between firms and are therefore subject to a paper trail. If the paper trail has a preventive deterrence effect, one would therefore expect less of a response for input costs than for sales. In line with the self-enforcement hypothesis, Columns (1) and (2) indeed show a significant response in sales, but not in input costs.

---

[37]The cost-benefit analysis focuses on micro-size firms since the values for larger firms are very noisy.

[38]This value is calculated for micro-size firms, winsorized at the 5th and 95th percentile.

The probability of increasing declared sales compared to the previous year increases highly significantly by 1.17%, while there is an insignificant coefficient of 0.16% for input costs.[39]

[Table 5]

The Chilean tax forms allow me to further disentangle the effect by distinguishing two types of sales: intermediary sales to other firms and final sales to consumers. Columns (3) and (4) show that the effect is again concentrated on the type of transaction not covered by the paper trail, the sales to the final consumer. The probability of increasing final sales compared to the previous year goes up by 1.33% and is highly significant, while there is only a statistically insignificant effect of 0.12% for intermediate sales. Figure A3 shows the effects in graphical form.

The above analysis excludes retail firms that sell only to final consumers, and upstream firms that sell only to other firms. This limits the degree to which the differential response is driven by cross-firm variation, since by definition, pure retailers cannot respond on intermediate sales or upstream firms on final sales. When including the entire universe of firms in the analysis (Appendix Table A6), results are still consistent with the self-enforcing hypothesis. We see a much stronger response on sales than on inputs, and on final sales than on intermediate sales. In this sample, there is some response on inter-firm sales. However, this specification has to be interpreted with caution, since many other characteristics that can affect the response, such as firm size, tax morale, risk aversion, etc. could be correlated with being a retailer or an upstream firm.

Although Table 5 only compares transactions among firms that have both types of sales, we can go one step further in ruling out spurious effects stemming from variation between firms by confirming these findings in a within-firm estimation. The regressions in Table A7 follow the specification of Equation (3) in Section 2.4. It uses a data set that contains an observation for each line item for each firm in a given month. Including firm fixed effects and firm fixed effects times post-treatment period allows comparing the response between different line items within a firm.[40] Column (1) compares the effect between sales and input costs, and confirms that the response is clearly concentrated on sales. Column (2) compares the impact between final and intermediary sales, and again finds a much stronger response in final sales.

---

[39]This of course does not mean that only 1.13% of the firms responded to treatment, as firms may for many other reasons be already above that threshold without the treatment, or far below it. The interpretation of these coefficients is the percent of firms that were pushed over the threshold of last year's payment due to the increase from treatment. These coefficients unfortunately do not have a very intuitive interpretation, but they are well equipped to show relative responses between different types of firms or transactions.

[40]To address the large computational demands of two sets of high-dimensional fixed effects, Stata routine reg2hdfe was used (Guimaraes and Portugal, 2010).

These results confirm that the finding of a lower response on transactions covered by a paper trail is not driven by heterogeneity between firms. As discussed in Section 1.2, considering that at a given level of evasion, an audit can be expected to be more effective where a paper trail is present, the fact that the response is *lower* on these transactions suggests that the paper trail had a preventive deterrence effect, leading to *ex ante* lower levels of evasion on transactions covered by the paper trail prior to the intervention.

While we can hold firm characteristics constant, the necessary assumption for this inference is that the lower response is not driven by a difference within firms between final sales and other transactions. There are of course differences between final and intermediate transactions, since it is not randomly assigned whether the client is a consumer or firm. We can also not test directly whether firms interpret the letter to target final sales in particular, though this did not seem to be the case based on the interviews I conducted when pre-testing the letters. The Spillover Experiment in Section 3.2 will therefore complement the findings of the Letter Message Experiment by showing the underlying mechanism in action.

Interaction with Firm Size

The following section shows how the treatment effect varies with firm size and how this relates to the degree with which firms' sales are subject to the VAT paper trail. This analysis necessarily implies comparison across firms, as opposed to the VAT paper trail where we can analyze types of transactions within a given firm. Nevertheless, there are interesting correlations.

Table 6 analyzes how the treatment effect varies with both firm size and the share of sales going to final consumers. Column (1) shows that, consistent with the findings in the previous section, firms with a larger share of final sales respond more strongly to the deterrence letter. Columns (2) and (3) of Table 6 look at two different measures of firm size: the official two-digit Chilean size classification, based on firms' revenues in the preceding year, and the log of the number of employees in the pre-treatment year. Figure A4 also displays the impact based on three median regressions, one for each of the official Chilean size categories: micro, small and medium. This is consistent with the idea of Kleven et al. (2009) that larger firms evade fewer taxes since collusion is harder with a large number of employees. There may, of course, be many other reasons for this differential response by size.[41] With respect to the

---

[41]Firms of different sizes may for example vary in their priors about the underlying audit probability, risk aversion, use of other sources of paper trails such as electronic billing, etc. An additional reason for lower evasion may be that in small firms, the person making the evasion decision (e.g. whether or not to give a receipt) is also the residual claimant of the tax money saved, while in large firms, this decision is usually made by an employee who does not benefit directly.

VAT, the question arises whether the fact that small firms have a higher proportion of retail sales may be driving the differential response by firm size to some extent.

[Table 6]

Columns (4) and (5) include interactions of treatment with both the share of final sales and a size measure. The coefficients on the size measures are reduced significantly. In Panel A, looking at the probability of declaring more than in the previous year, coefficients are reduced by about 40 percent, but stay significant, while in Panel B, looking at the probability of declaring more than predicted, they are reduced by about two thirds and are no longer statistically significant. The coefficient on final sales stays highly significant in all specifications.

Since the specification in Panel B controls for trends for these different groups of firms over time, it provides the more reliable estimate. This would suggest that almost two thirds of the larger response in smaller firms can be explained by the degree to which their sales go to final consumers. However, this type of specific magnitude resulting from a horse-race regression between two correlated variables of course needs to be interpreted with much caution.[42] Nevertheless, the results suggest that a significant part of the higher evasion in smaller firms may be driven by a weaker VAT paper trail.

3.2   Spillover Experiment

The Letter Message Experiment showed that in Chile overall, in general equilibrium across the whole country, an increase in deterrence generates less of a response on transactions already covered by a VAT paper trail, suggesting that the paper trail has a preventive deterrence effect. The Spillover Experiment complements these findings and is designed to find direct evidence for the underlying self-enforcing mechanism. As discussed in Section 2.2 above, the experiment injects deterrence into the system in a subsample of firms where compliance is believed to be low, to test for spillovers along the production chain. I measure such spillovers by comparing VAT declarations for trading partners of the treated firms to trading partners of the control firms before and after the audit pre-announcement.

[Table 7]

Table 7 shows the impact of the audit pre-announcement for all trading partners. There is a significant increase in declared VAT showing that the pre-announcement had compliance

---

[42]If one of the variables is measured with more measurement error than the other, attenuation bias will lead it to look relatively less important.

effects beyond the treated firm, propagating through the network of their trading partners.[43] These results represent the first experimental evidence of tax enforcement generating spillovers to other firms. Given these strong spillover effects, it might be in the interest of tax authorities to take these indirect effects into account when designing an audit strategy. However, the overall spillover effects cannot by themselves establish that the channel is the VAT chain. They could also simply result from a perception of a general increase in the audit risk by firms that are in communication with the treated firms.

Columns (3) and (4) of Table 7 therefore test for the asymmetry in the prediction of the VAT self-enforcement hypothesis. The spillover effects are shown separately for clients and suppliers.[44] In line with the predictions, there are strong increases in declared VAT for suppliers, and no significant effects for client firms. This establishes the directionality of the spillover effects up the VAT chain. We can now rule out that the spillovers are simply the results of general conversations about the audit pre-announcement with trading partners.

One remaining concern is that this differential effect might be driven by the fact that client and supplier firms are clearly different from each other. I test for robustness of this result by including a series of control variables and their interaction with treatment, treatment period, etc. in Columns (5) and (6) of Table 7. The included control variables are firm size, the sales-input ratio (a proxy the tax authority uses to assess suspicion of evasion), share of sales to final consumers, and a categorization of whether the firm's industry is classified by the tax authority as "hard-to-monitor." The findings remain robust even after inclusion of the control variables, indicating that it is not the different nature of supplier and client firms, but rather their position in the VAT chain that seems to be driving the result.[45] Figure A5 shows graphical evidence of the impact of the pre-announcement on VAT payments by trading partners.

The findings 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 up the production chain. Monitoring a firm increases tax payments by its suppliers. 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

---

[43]For robustness, I also report non-linear difference-in-difference estimates following Blundell and Dias (2009) in Table A2 Columns (7)-(9).

[44]The very small number of trading partners that show up both as a client and as a supplier of some of the audited firms are recorded both as a client and as a supplier. All results are clustered at the level of the audited firm. Any spillovers between the treatment and control group can be expected to lead to a downward bias of the estimates.

[45]Table A3 shows a robustness check including carry-overs from previous VAT declarations. As expected, this introduces more noise, since the firms in the spillover experiment had large carry-overs from the pre-treatment period. The results on the differential spillover effects are robust, though less significant.

effectiveness of an increase in the audit probability of one firm, by increasing VAT payments by others.

Third, the mere existence of information through the paper trail – not surprisingly – is not by itself self-enforcing in an environment where the risk of cross-checks is low. Prior to the audit announcement, self-enforcement was incomplete at best among this sample of firms, even though a VAT system was in place. The small firms in this sample, mostly located in remote areas, were probably correctly anticipating that the risk of the tax authority double-checking their declarations was low. The additional deterrence effect resulting from the pre-announced audits was necessary to trigger the effectiveness of the VAT paper trail, indicating that it is only the interaction of information with deterrence that leads to effective tax enforcement.

4   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 paper trails for taxation. It provides the first micro-empirical evidence for the self-enforcing power of the paper trail in the VAT and for spillovers in tax enforcement through firms' trading networks more generally, and shows that in line with a growing recent literature, 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 Letter Message Experiment looks at the entire economy of Chile and investigates the deterrence effect of the VAT paper trail, incorporating the general equilibrium effects of this long established policy. It finds that holding firm characteristics constant, transactions that are already subject to the VAT paper trail respond much less to an increase in the perceived audit probability. Since for a given level of evasion, an audit can be expected to be more effective where a paper trail is present, the fact that the response is lower suggests that the paper trail had a preventive deterrence effect, leading to lower levels of evasion on transactions covered by the paper trail. In line with predictions of Kleven et al. (2009), the Letter Message Experiment also finds a stronger response for smaller firms. Controlling for the degree of retail sales reduces this differential response considerably, suggesting that a significant part of the higher evasion among smaller firms may be driven by a weaker paper trail.

The Spillover Experiment looks at the underlying dynamic that creates the preventive deterrence effect of the paper trail, and is designed to show the self-enforcing mechanism in action. It finds that as predicted by the self-enforcement hypothesis, increasing the audit

probability of firms suspected of evasion generates spillovers up the VAT paper trail that lead to an increase of their suppliers' tax payments. These multiplier effects indicate that globally the VAT paper trail acts as a complement to the audit probability.

The combined findings also show that while in Chile overall, the VAT paper trail seems to be highly effective, the mere existence of a VAT system, in the absence of credible deterrence, does not lead to "self-enforcement" – as exemplified by the low compliance among the sample of the Spillover Experiment prior to the intervention. It is the interaction of information with deterrence that leads to effective tax enforcement.

These results have a number of implications for public finance in developing countries and for tax policy in general. First, and most broadly, in line with findings from the literature on corruption and illegal capture of public funds, such as Reinikka and Svensson (2004), the conclusions 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 for tax collection over other forms of taxation, such as a retail sales tax. 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. Further research is required to investigate the effectiveness of such mechanisms, as well as the dimensions of generalizability of these findings. For example, the Chilean Tax Authority has a reputation for being highly effective and having low levels of corruption. On one hand, the paper trail may be more important in high corruption environments as it reduces the discretion of tax officials (e.g. Baurer, 2005). At the same time, the paper trail may be less effective where enforcement can be circumvented by paying off the tax auditor.

Third, the spillover effects are relevant for the design of optimal audit strategies. When choosing which firms to audit, a tax authority may not only want to consider the expected impact on the audited firm, but also the multiplier effect through a firm's trading network. In particular, the higher response in final sales and spillovers along the supply chain suggest that 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. At the same time, enforcement at the final sales stage is also more costly, given the absence of a paper trail and the smaller average firm size. Further research is required to analyze how to optimally allocate audit probabilities to different nodes in the network.

Fourth, as Shahe 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-sectorial distortions between formal and informal sectors. De Paula and Scheinkman (2010) 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 potentially 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 market. If the evasion rate is higher for downstream firms, a flat VAT rate will result in upstream firms paying a higher effective tax rate. This 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. On the other hand, if small firms can evade more, this may lead firms to stay inefficiently small to reduce their effective tax burden. 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 (e.g. 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 the 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 divisions 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.
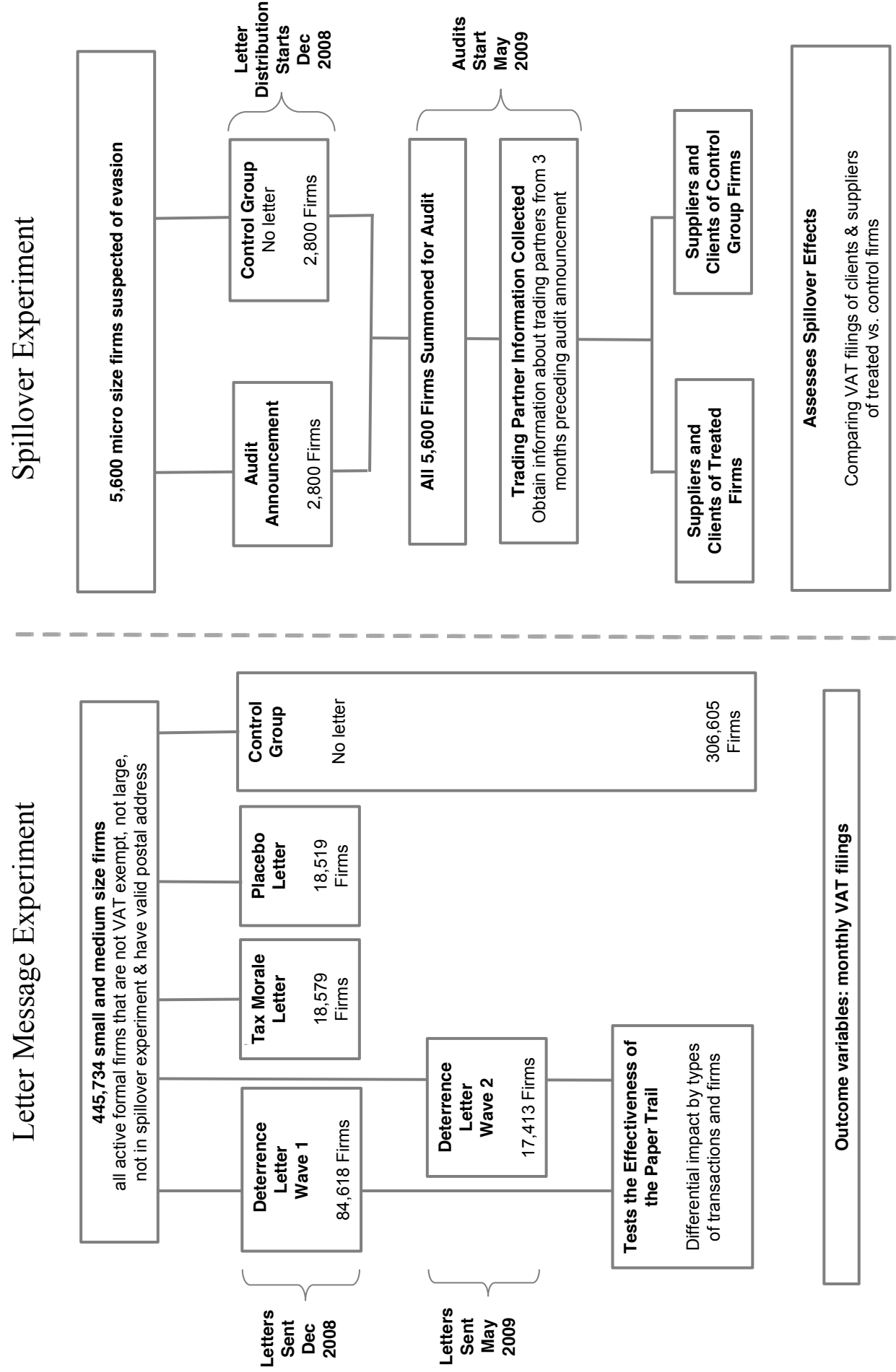
5 References

Adimark, "Imagen del Servicio de Impuestos Internos," Santiago de Chile 2006.

Agha, Ali and Jonathan Haughton, "Designing VAT Systems: Some Efficiency Considerations," *The Review of Economics and Statistics*, 1996, *78* (3), 303–308.

Allingham, Michael G. and Agnar Sandmo, "Income Tax Evasion: A Theoretical Analysis," *Journal of Public Economics*, 1972, *1* (3-4), 323–338.

Alm, James, John A. Deskins, and Michael McKee, "Do Individuals Comply On Income Not Reported By Their Employer?," *Research Paper Series No. 07-34, Andrew Young School of Policy Studies*, 2007.

Andreoni, James, Brian Erard, and Jonathan Feinstein, "Tax Compliance," *Journal of Economic Literature*, 1998, *36* (2), 818–860.

Athey, Susan and Guido W. Imbens, "Identification and Inference in Nonlinear Difference-in-Differences Models," *Econometrica*, 2006, *74* (2), 431–497.

Baurer, Lewis I., "Tax Administrations and Small and Medium Enterprises (SMEs) in Developing Countries," *The World Bank*, 2005.

Besley, Tim and Torsten Persson, "Public Finance and Development," in A. J. Auberach and M. S. Feldstein, eds., *Draft Chapter for the Handbook of Public Economics*, 2012.

Bird, Richard and Pierre-Pascal Gendron, *The VAT in Developing and Transitional Countries*, Cambridge University Press, 2007.

Blundell, Richard and Monica Costa Dias, "Alternative Approaches to Evaluation in Empirical Microeconomics," *Journal of Human Resources*, 2009, *44* (3), 565–640.

Burgess, Robin and Nicholas Stern, "Taxation and Development," *Journal of Economic Literature*, 1993, *31* (2), 762–830.

Carrillo, Paul, M. Shahe Emran, and Anita Rivadeneira, "Do Cheaters Bunch Together? Profit Taxes, Withholding Rates and Tax Evasion," *Working Paper No. 2011-02, Institute for International Economic Policy*, 2012.

Castro, Lucio and Carlos Scartascini, "Tax Compliance and Enforcement in the Pampas: Evidence from a Field Experiment," *IDB Working Paper Series 472, Inter-American Development Bank*, 2013.

Dal Bó, Ernesto and Petro Dal Bó, "Do the Right Thing: The Effects of Moral Suasion on Cooperation," Technical Report 2014.

Das-Gupta, Arindam and Ira N. Gang, "Value Added Tax Evasion, Auditing and Transactions Matching," in J. McLaren, ed., *Institutional Elements of Tax Design and Reform*, Washington, D.C.: The World Bank Technical Paper No. 539, 2003.

De Paula, Aureo and Jose A. Scheinkman, "Value-Added Taxes, Chain Effects, and Informality," *American Economic Journal: Macroeconomics*, 2010, *2* (4).

Ebrill, Liam, *The Modern VAT*, International Monetary Fund, 2001.

Emran, M. Shahe and Joseph Stiglitz, "On Selective Indirect Tax Reform in Developing Countries," *Journal of Public Economics*, 2005, *89* (4), 599–623.

Engel, Eduardo, Alexander Galetovic, and Claudio E. Raddatz, "Estimacion de la Evasion del IVA Mediante el Metodo de Punto Fijo," *Estudios Tributarios. Internal Revenue Service, Santiago, Chile*, 1998.

_ , _ , and _ , "A Note on Enforcement Spending and VAT Revenues," *Review of Economics and Statistics*, 2001, *83* (2), 384–387.

Engstrom, Per and Patrik Hesselius, "The Information Theory: Theory and Application," 2007.
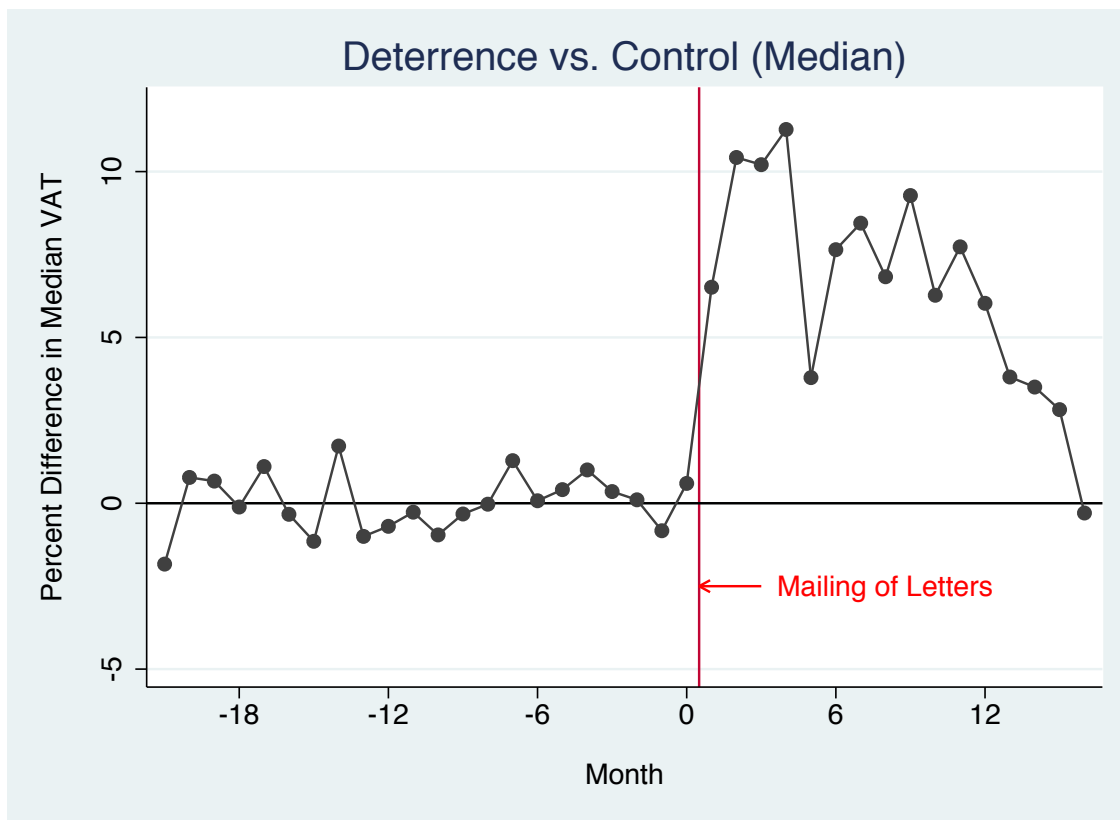
Feld, Lars and Bruno S. Frey, "Trust Breeds Trust: How Taxpayers are Treated," *Economics of Governance*, 2002, *3* (2), 87–99.

Fellner, Gerlinde, Rupert Sausgruber, and Christian Traxler, "Testing Enforcement Strategies in the Field: Legal Threat, Moral Appeal and Social Information," *Journal of the European Economic Association*, 2013, *11* (3), 634–660.

Fisman, Raymond and Shang-Jin Wei, "Tax Rates and Tax Evasion: Evidence from Missing Imports in China," *Journal of Political Economy*, 2004, *112*, 471–500.

Gordon, Roger H. and Wei Li, "Tax Structures in Developing Countries: Many Puzzles and a Possible Explanation," *Journal of Public Economics*, 2009, *93* (7-8), 855–866.

Grinberg, Itai, "Where Credit is Due: Advantages of the Credit-Invoice Method for Partial Replacement VAT," *Tax Law Review*, 2010, *63* (2).

Guimaraes, Paulo and Pedro Portugal, "A Simple Feasible Alternative Procedure to Estimate Models with High-Dimensional Fixed Effects," *Stata Journal*, 2010, *10* (4), 628–649.

Hallsworth, Michael, John A List, Robert D Metcalfe, and Ivo Vlaev, "The Behavioralist as Tax Collector: Using Natural Field Experiments to Enhance Tax Compliance," *NBER Working Paper*, 2014.

Hemming, Richard and John Kay, "The United Kingdom, In The Value Added Tax: Lessons from Europe," in H.J. Aaron, ed., *H.J. Aaron, ed.*, Chicago: Washington: Brookings Institution, 1981.

Internal Revenue Service, "Federal Tax Compliance Research: Individual Income Tax Gap Estimates for 1985, 1988, and 1992," IRS Publication - Washington, D.C 1415 (Rev. 4-96) 1996.

— , "Updated Estimates of the Tax Year 2001 Individual Income Tax Underreporting Gap," Overview - Washington, D.C. 2006.

— , "Congressional Justification," Washington, D.C. 2008.

Keen, Michael and Ben Lockwood, "The Value Added Tax: its Causes and Consequences," *Journal of Development Economics*, 2010, *92* (2), 138–151.

— **and Stephen Smith**, "VAT Fraud and Evasion: What Do We know and What Can Be Done?," *National Tax Journal*, 2006, *LIX(4)*, 861–887.

Khwaja, Asim Ijaz and Atif Mian, "Rent Seeking and Corruption in Financial Markets," *Annual Review of Economics*, 2011, *3* (1), 579–600.

Kleven, Henrik J. and Mazhar Waseem, "Behavioral Responses to Notches: Evidence from Pakistani Tax Records," *Working Paper Series, London School of Economics*, 2012.

— , **Claus T. Kreiner, and Emmanuel Saez**, "Why Can Modern Governments Tax So Much? An Agency Model of Firms As Fiscal Intermediaries," *NBER Working Paper No. 15218*, 2009.

— , **Martin Knudsen, Claus T. Kreiner, Soren L. Pedersen, and Emmanuel Saez**, "Unwilling or Unable to Cheat? Evidence from a Tax Audit Experiment in Denmark," *Econometrica*, 2011, *79* (3), 651–692.

Kopczuk, Wojciech and Joel Slemrod, "Putting Firms into Optimal Tax Theory," *The American Economic Review: Papers and Proceedings*, 2006, *96* (2), 130–134.

Kumler, Todd J., Eric A. Verhoogen, and Judith Frías, "Enlisting Employees in Improving Payroll-Tax Compliance: Evidence from Mexico," *NBER Working Paper No. 19385*, 2013.

Ludwig, Jens, Jeffrey R. Kling, and Sendhil Mullainathan, "Mechanism Experiments and Policy Evaluations," *NBER Working Paper No. 17062*, 2011.

Mideplan, "Resultados CASEN 2009: Nuevas Cifras de Distribucion del Ingreso," Ministerio de Planificación - Gobierno de Chile 2010.

Naritomi, Joana, "Consumers as Tax Auditors," *Working Paper, London School of Economics*, 2013.

Nellor, David, "The Effect of the Value-Added Tax on the Tax Ratio," *IMF Working Papers 8747, International Monetary Fund*, 1987.

Olken, Benjamin A. and Monica Singhal, "Informal Taxation," *American Economic Journal: Applied Economics*, 2011, *3* (4).

Pomeranz, Dina, Cristobal Marshall, and Pamela Catellon, "Randomized Tax Enforcement Messages: A Policy Tool for Improving Audit Strategies," *Tax Administration Review*, 2014, *36*, 1–21.

Reinikka, Ritva and Jakob Svensson, "The Power of Information: Evidence from a Newspaper Campaign to Reduce Capture," *Policy Research Working Paper No. 3239, The World Bank*, 2004.

Rincke, Johannes and Christian Traxler, "Enforcement Spillovers," *The Review of Economics and Statistics*, 2011, *93* (4), 1224–34.

Schneider, Friedrich, Andreas Buehn, and Claudio E. Montenegro, "Shadow Economies All Over the World: New Estimates for 162 Countries from 1999 to 2007," *World Bank Discussion Papers, The World Bank*, 2010.

Servicio de Impuestos Internos, "Ingresos Tributarios Anuales 1993 a 2009," Santiago de Chile 2010.

_ , "Tasa de Evasion IVA 1990-2008," Subdireccion de Estudios. Santiago de Chile 2010.

_ , "Impuesto al Valor Agregado (IVA)," `http://www.sii.cl/portales/inversionistas/imp_chile/impuestos_iva_ing.htm,` April 2011. Accessed: 19/11/2012.

Slemrod, Joel, "Does It Matter Who Writes the Check to the Government? The Economics of Tax Remittance," *National Tax Journal*, 2008, *61* (2), 251–75.

_ **and Shlomo Yitzhaki**, "Tax Avoidance, Evasion, and Administration," in A. J. Auberach and M. S. Feldstein, eds., *Handbook of Public Economics*, 2002.

_ **, Marsha Blumenthal, and Charles Christian**, "Taxpayer Response to an Increased Probability of Audit: Evidence from a Controlled Experiment in Minnesota," *Journal of Public Economics*, 2001, *79* (3), 455–483.

Tait, Alan, *Value Added Tax*, London, New York: McGraw-Hill, 1972.

United States Office of Management and Budget, "Budget of the United States Government, Fiscal Year 2009," Washington, DC 2008.

Figure 1: Research Design Diagrams

Spillover Experiment

5,600 micro size firms suspected of evasion

Letter Distribution Starts Dec 2008

Audit Announcement — 2,800 Firms

Control Group — No letter — 2,800 Firms

Audits Start May 2009

All 5,600 Firms Summoned for Audit

Trading Partner Information Collected — Obtain information about trading partners from 3 months preceding audit announcement

Suppliers and Clients of Treated Firms

Suppliers and Clients of Control Group Firms

Assesses Spillover Effects — Comparing VAT filings of clients & suppliers of treated vs. control firms

Letter Message Experiment

445,734 small and medium size firms — all active formal firms that are not VAT exempt, not large, not in spillover experiment & have valid postal address

Deterrence Letter Wave 1 — 84,618 Firms

Tax Morale Letter — 18,579 Firms

Placebo Letter — 18,519 Firms

Control Group — No letter — 306,605 Firms

Letters Sent Dec 2008

Deterrence Letter Wave 2 — 17,413 Firms

Letters Sent May 2009

Tests the Effectiveness of the Paper Trail — Differential impact by types of transactions and firms

Outcome variables: monthly VAT filings

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 for each type of letter: (median VAT treatment group - median VAT control group) / (median VAT control group), normalizing the average of pre-treatment months 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 Figure A2.

Table 1: Responses to Increase in Audit Probability: Collusive and Unilateral Evasion

Position in supply chain	Collusive Evasion		Unilateral Evasion	
Supplier	Sales ↑	VAT ↑	Sales ↑	VAT ↑
Treated firm	Inputs ↑ Sales ↑	VAT (↑)	Inputs ↓ Sales ↑	VAT ↑
Client	Inputs ↑	VAT ↓	Inputs ↓	VAT ↑

Notes: "Collusive evasion" stands for the type of evasion where a transaction is omitted from the books of both the seller and the buyer firm. "Unilateral evasion" 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: Letter 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 VAT (mean)	264,029	3,105	305	-10,565
	(1,871)	(3,744)	(7,869)	(7,472)
Monthly VAT (median)	17,511	-188	-415	-7
	(115)	(231)	(491)	(462)
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. The constant term captures the value for the control group. Monthly VAT winzorized at the top and bottom 0.1% to deal with extreme outliers. Columns (2)-(4) show the difference of the treatment groups to the control group. None of the differences are statistically significant at the 10%-level. Monetary amounts are in Chilean pesos, with 500 pesos approximately equivalent to 1 USD. 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. All other observations are monthly for ten months prior to treatment (January 2008-October 2008), except for median tax paid, for which observations are averaged over four months prior to treatment.

Table 3: 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	1,069,113	29,274
	(11,370)	(12,326)	(6,271)	(12,988)	(82,548)	(114,952)
Monthly VAT (median)	0	0	0	0	301,230	10,750
	(0)	(0)	(0)	(0)	(24,656)	(34,396)
Firm age in months	131	-0.43	139	-3.33	126	2.01
	(1.19)	(1.69)	(2.18)	(3.13)	(1.77)	(2.51)
% Non-filed declaration	1.09	0.1	0.40	-0.21	1.31	-0.43*
	(0.13)	(0.18)	(0.11)	(0.13)	(1.94)	(0.24)
Size category	2.28	-0.00	2.34	0.02	5.87	0.65
	(0.01)	(0.01)	(0.02)	(0.03)	(0.62)	(0.9)
% Retail firms	6.82	-0.54	7.27	-1.16	2.26	-0.63
	(0.48)	(0.66)	(0.94)	(1.28)	(0.49)	(0.6)
% Intermediary firms	66.3	-0.46	63	1.01	35.7	-2.37
	(0.89)	(1.27)	(1.76)	(2.46)	(1.52)	(2.13)
% Final sales	15.7	-0.35	19.4	-2.99	22.7	0.15
	(0.65)	(0.91)	(1.35)	(1.82)	(1.1)	(1.46)
Sales/input	0.68	-0.01	0.73	-0.03	1.64	-0.15
	(0.01)	(0.01)	(0.02)	(0.02)	(0.1)	(0.11)
% Agriculture	54	0.71	45	5.34**	18.2	1.72
	(0.94)	(1.33)	(1.81)	(2.55)	(1.32)	(1.84)
% Audited	27	0.46	100	0		
	(0.84)	(1.19)	(.)	(.)		
% Suppliers					56.5	-0.0032
					(1.46)	(2.11)
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 were 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 ten months prior to treatment (January 2008-October 2008), except for median tax paid, for which observations are averaged over ten months prior to treatment. The constant terms shown in Columns (1), (3) and (5) capture the values for the control group. Columns (2), (4) and (6) show the difference of the treatment group to the control group. Monetary amounts are in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. 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). *** = p<0.01, ** = p<0.05, * = p<0.1

Table 4: Letter Message Experiment: Intent-to-Treat Effects on VAT Payments by Type of Letter

	(1) Mean VAT	(2) Median VAT	(3) Percent VAT > Previous Year	(4) Percent VAT > Predicted	(5) Percent VAT > Zero
Deterrence letter X post	-1,114	1,326***	1.40***	1.42***	0.53***
	(2,804)	(316)	(0.12)	(0.10)	(0.09)
Tax morale letter X post	-1,840	262	0.40	0.30	0.44**
	(6,082)	(666)	(0.25)	(0.22)	(0.20)
Placebo letter X post	835	383	-0.11	-0.19	-0.14
	(6,243)	(687)	(0.26)	(0.23)	(0.20)
Constant	268,810***	17,518***	47.50***	48.27***	67.30***
	(1,799)	(112)	(0.07)	(0.07)	(0.06)
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	No	Yes	Yes	Yes
Treatment Assignment	No	Yes	No	No	No
Number of observations	7,892,076	1,221,828	7,892,076	7,892,076	7,892,076
Number of firms	445,734	445,734	445,734	445,734	445,734
Adjusted R^2	0.40		0.14	0.28	0.47

Notes: Column (1) shows a regression of the mean declared VAT on treatment dummies, winsorized at the top and bottom 0.1% to deal with extreme outliers. Column (2) shows a median regression of average VAT before treatment and in 4 months after each treatment wave. Columns (3)-(5) show linear probability regressions of the probability of an increase in declared VAT compared to the same month in the previous year, the probability of declaring more than predicted and the probability of declaring any positive amount. Observations are monthly in Columns (1) and (3)-(5) for ten months prior to treatment and four months after each wave of mailing. The four months after the second wave excludes firms treated in the first. Coefficients and standard errors of the linear probability regressions are multiplied by 100 to express effects in percent. Monetary amounts are in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. Standard errors in parentheses, robust and clustered at the firm level for Columns (1) and (3)-(5). *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Impact of Deterrence Letter on Different Types of Transactions

	(1) Percent Sales > Previous Year	(2) Percent Input Costs > Previous Year	(3) Percent Intermediary Sales > Previous Year	(4) Percent Final Sales > Previous Year
Deterrence letter X post	1.17***	0.16	0.12	1.33***
	(0.22)	(0.21)	(0.19)	(0.21)
Constant	55.39***	53.25***	38.37***	45.04***
	(0.13)	(0.13)	(0.12)	(0.12)
Month fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	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
Adjusted R^2	0.25	0.22	0.30	0.32

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. Sample of firms that have both final and intermediary sales in the year prior to treatment. The four months after the second wave excludes firms treated in the first wave. Coefficients and standard errors are 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 6: Interaction of Firm Size and Share of Sales to Final Consumers

Panel A:	Percent VAT > Previous Year				
	(1)	(2)	(3)	(4)	(5)
Deterrence letter X final sales share	1.61***			1.48***	1.43***
	(0.26)			(0.27)	(0.26)
Deterrence letter X size category		-0.17***		-0.10***	
		(0.04)		(0.04)	
Deterrence letter X log employees			-0.45***		-0.29**
			(0.11)		(0.12)
Deterrence letter	0.68***	2.63***	1.66***	1.49***	0.92***
	(0.16)	(0.29)	(0.13)	(0.35)	(0.19)
Constant	47.53***	48.87***	47.50***	48.89***	47.53***
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Final sales share X post	Yes	No	No	Yes	Yes
Size measure X post	No	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes	Yes
Observations	7,308,631	7,116,590	7,340,994	7,084,823	7,308,631
Number of firms	406,834	396,135	408,636	394,367	406,834
Adjusted R^2	0.14	0.14	0.14	0.14	0.14

Panel B:	Percent VAT > Predicted				
	(1)	(2)	(3)	(4)	(5)
Deterrence Letter X final sales share	1.51***			1.51***	1.44***
	(0.23)			(0.25)	(0.24)
Deterrence Letter X size category		-0.10***		-0.03	
		(0.03)		(0.04)	
Deterrence Letter X log employees			-0.28***		-0.11
			(0.10)		(0.11)
Deterrence Letter	0.74***	2.15***	1.57***	1.00***	0.83***
	(0.14)	(0.26)	(0.12)	(0.32)	(0.16)
Constant	48.48***	49.79***	48.26***	50.01***	48.48***
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Final sales share X post	Yes	No	No	Yes	Yes
Size measure X post	No	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	7,308,631	7,116,590	7,340,994	7,084,823	7,308,631
Number of firms	406,834	396,135	408,636	394,367	406,834
Adjusted R^2	0.28	0.26	0.29	0.26	0.28

Notes: Regression of the probability of monthly declared VAT being higher than in the same month of the previous year (Panel A) and on being higher than predicted (Panel B). Coefficients and standard errors are multiplied by 100 to express effects in percent. Sample includes all firms in the deterrence treatment and in the control group. The four months after the second wave excludes firms treated in the first. Number of observations vary due to missing observations for some variables. Final sales share is not defined for firms with zero sales in preceding year, size category is not available for new firms. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.   39

Table 7: Spillover Effects on Trading Partners' VAT Payments

|	(1) Percent VAT > Previous Year	(2) Percent VAT > Predicted	(3) Percent VAT > Previous Year	(4) Percent VAT > Predicted	(5) Percent VAT > Previous Year	(6) Percent VAT > Predicted
Audit announcement X post	2.41**	2.03*				
	(1.14)	(1.11)				
Audit announcement X supplier X post			4.28***	3.92***	4.14***	3.83***
			(1.54)	(1.50)	(1.52)	(1.52)
Audit announcement X client X post			-0.26	-0.28	-0.14	-0.28
			(1.64)	(1.51)	(1.67)	(1.55)
Supplier X post			-0.64	0.34	-1.11	0.60
			(1.62)	(1.59)	(1.67)	(1.64)
Constant	52.07***	49.06***	52.07***	49.06***	52.75***	50.11***
	(0.95)	(0.94)	(0.95)	(0.94)	(0.96)	(0.96)
Controls X post	No	No	No	No	Yes	Yes
Controls X audit announcement X post					Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	45,264	45,264	45,264	45,264	44,288	44,288
Number of firms	2,829	2,829	2,829	2,829	2,768	2,768
Adjusted R^2	0.05	0.11	0.05	0.11	0.05	0.10

Notes: Regressions for trading partners of audited firms. Column (1), (3) and (5) shows the probability of an increase in declared VAT since the previous year, Column (2), (4) and (6) shows the probability of declaring more than predicted. The controls in Columns (5) and (6) are firm sales, sales/input-ratio, share of sales going to final consumers, and industry categorized as "hard-to-monitor." Observations are monthly for ten months prior to treatment and six months after the audit announcements were mailed. Coefficients and standard errors are multiplied by 100 to express effects in percent. Robust standard errors in parentheses, clustered at the level of the audited firm. *** p<0.01, ** p<0.05, * p<0.1.

A   Additional Figures
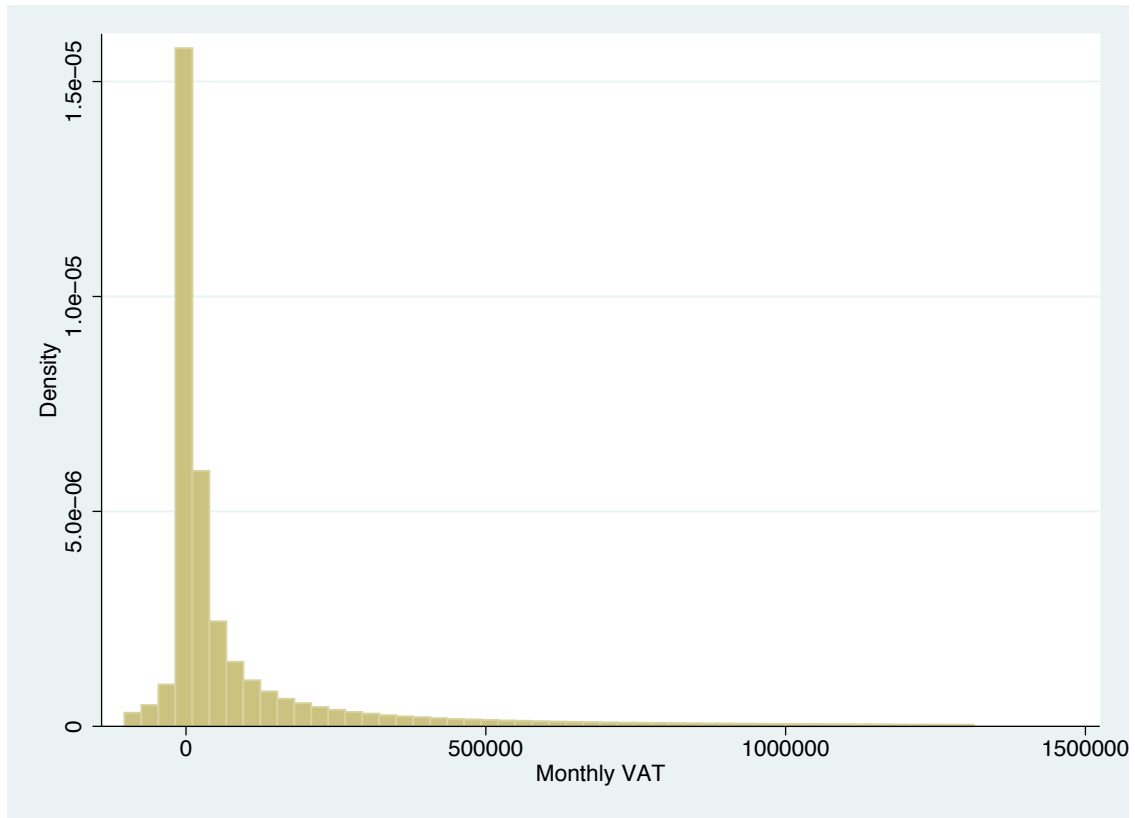
Figure A1: Distribution of monthly declared VAT for the full study sample, twelve months prior to treatment to twelve months after treatment, excluding the top and bottom 5%.
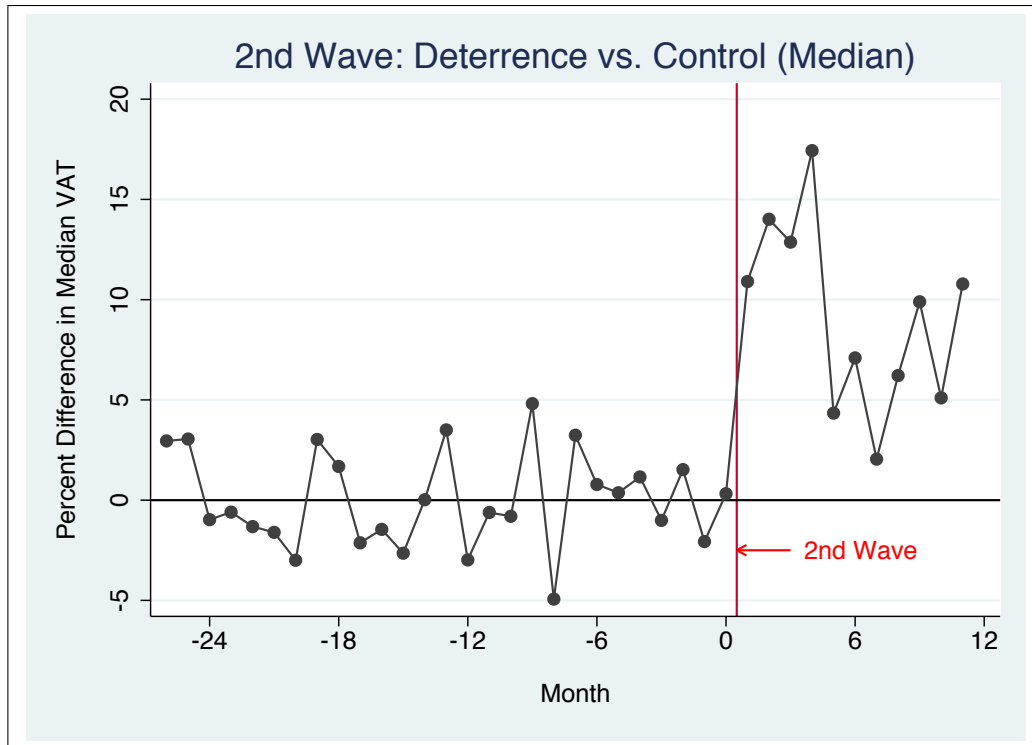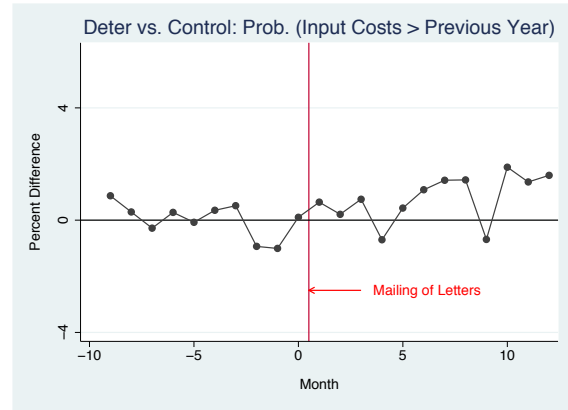
Figure A2: Impact of Deterrence Letter: Second Wave of Mailing

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 the average of pre-treatment months 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. Since the second wave of mailing is much smaller than the first, the figure shows a more noisy pattern than the first wave displayed in Figure 2, Panel A.
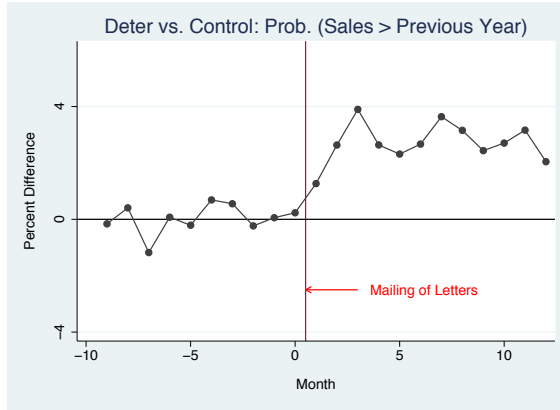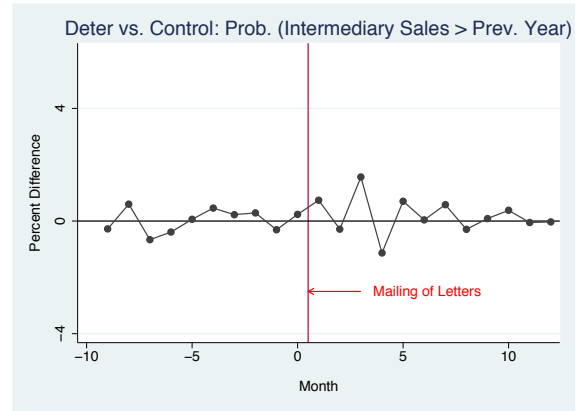
Panel A: Sales

Panel B: Input Costs

Panel C: Final Sales

Panel D: Intermediary Sales

Figure A3: Impact of Deterrence Letter on Different Types of Transactions

Notes: This figure plots the percent difference between deterrence letter and control group of the probability that a line item (total sales, total input costs, intermediary sales or final sales) is larger than in the same month of the previous year, normalizing the average of pre-treatment months percent difference to zero. The y-axis indicates time, with monthly observations, and zero indicates the last month before the mailing of the letters. Figures show the first wave of mailing.
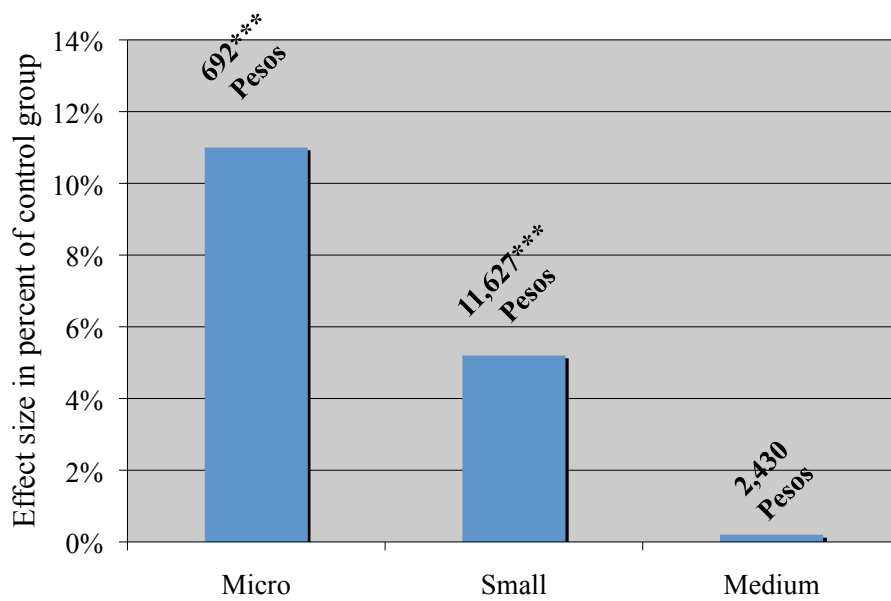
Figure A4: Impact by Firm Size (Median Regression)

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 letter treatment group of a median regression of mean monthly VAT payments in the four months following treatment. The height of the bar indicates the effect in percent relative to the mean in the control group in that size category. *** = p<0.01, ** = p<0.05, * = p<0.1
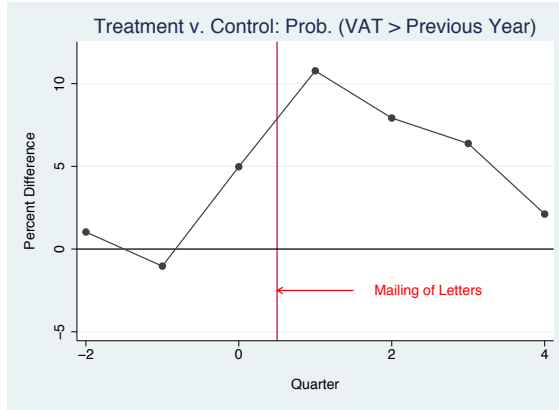
Panel A: Suppliers
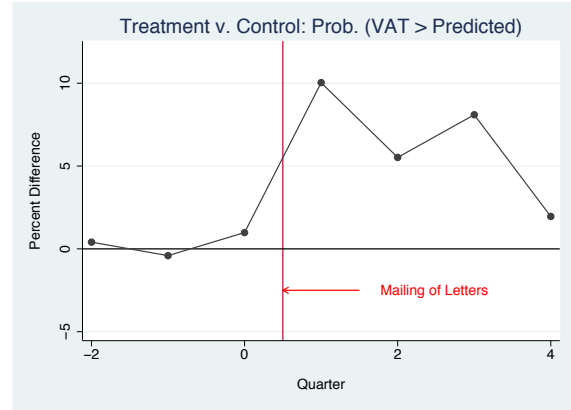
Panel B: Suppliers
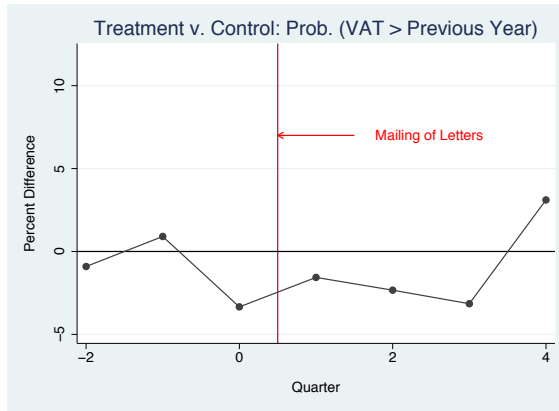
Panel C: Clients
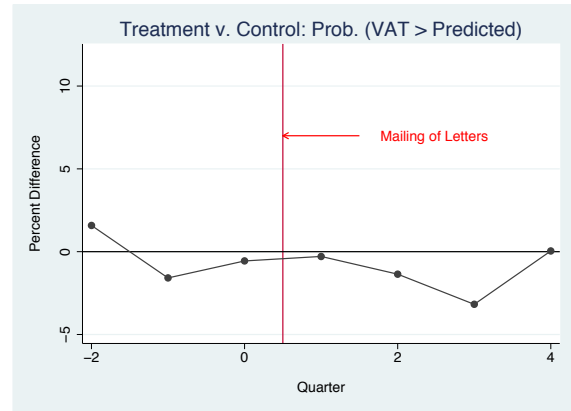
Panel D: Clients

Figure A5: Spillover Effects on Trading Partners' VAT Payments, Quarterly

Notes: This figure plots the percent difference between the group receiving a pre-announcement and the control group of the probability that declared VAT is larger than in the same month of the previous year (Panels A and C) and the probability that declared VAT is larger than predicted (Panels B and D), normalizing the average of pre-treatment months percent difference to zero. The y-axis indicates time, with observations aggregated at the quarterly level because monthly observations are very noisy, and zero indicates the last quarter before the mailing of the letters.

B   Additional Tables

Table A1: Deterrence Letter Experiment: Monthly Effects on VAT Payments

	(1)	(2)	(3)
	Percent VAT > Previous Year	Percent VAT > Predicted	Percent VAT > Zero
Deterrence X t-5	-0.02	-0.12	-0.07
	(0.17)	(0.18)	(0.17)
Deterrence X t-4	0.26	0.03	0.02
	(0.17)	(0.18)	(0.17)
Deterrence X t-3	0.10	-0.29*	-0.03
	(0.17)	(0.18)	(0.17)
Deterrence X t-2	0.05	-0.30*	-0.14
	(0.17)	(0.18)	(0.17)
Deterrence X t-1	0.19	-0.07	-0.09
	(0.17)	(0.18)	(0.17)
Deterrence X t1	1.07***	1.17***	0.48***
	(0.17)	(0.18)	(0.17)
Deterrence X t2	1.76***	1.73***	0.56***
	(0.17)	(0.18)	(0.17)
Deterrence X t3	1.46***	1.30***	0.48***
	(0.17)	(0.18)	(0.17)
Deterrence X t4	1.64***	1.21***	0.46***
	(0.17)	(0.18)	(0.17)
Deterrence X t5	0.99***	0.83***	-0.15
	(0.17)	(0.18)	(0.17)
Deterrence X t6	0.94***	0.72***	0.12
	(0.17)	(0.18)	(0.17)
Deterrence X t7	0.88***	0.59***	0.04
	(0.17)	(0.18)	(0.17)
Deterrence X t8	0.92***	0.63***	0.17
	(0.19)	(0.20)	(0.20)
Deterrence X t9	0.85***	0.75***	0.31
	(0.19)	(0.20)	(0.20)
Deterrence X t10	0.87***	0.68***	0.10
	(0.19)	(0.20)	(0.20)
Deterrence X t11	0.73***	0.82***	0.16
	(0.19)	(0.20)	(0.20)
Deterrence X t12	0.77***	0.51***	0.12
	(0.19)	(0.20)	(0.20)
Constant	47.00***	48.26***	65.31***
	(0.09)	(0.09)	(0.09)
Month fixed effects	Yes	Yes	Yes
Number of obs.	6,859,747	6,859,747	6,859,747
Number of firms	408,636	408,636	408,636
R^2	0.004	0.000	0.005

Notes: Each column shows a linear probability regression on interaction terms of being assigned to receive a deterrence letter with month dummies. Coefficients and standard errors are multiplied by 100 to express effects in percent. Sample includes all firms in the deterrence treatment and the control group. Robust standard errors in parentheses, clustered at the firm level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A2: Robustness Checks of Intent-to-Treat Effects on VAT Payments

	Letter Message Experiment						Spillover Experiment		
	Median VAT			Percent VAT > Previous Year			Percent VAT > Previous Year		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Deterrence letter X post	1,258*** (287)			1.96*** (0.27)					
Tax morale letter X post		36 (522)			0.49 (0.57)				
Placebo letter X post			476 (603)			-0.52 (0.56)			
Audit announcement X Post							3.99 (2.75)		
Audit announcement X supplier X post								7.17** (3.54)	
Audit announcement X client X post									0.78 (3.92)
Number of observations	782,446	644,024	643,908	782,446	644,024	643,908	5,658	3,196	3,196
Number of firms	391,223	322,012	321,954	391,223	322,012	321,954	2,829	1,598	1,598

Notes: Non-linear estimation strategies based on Athey and Imbens (2006) (Columns (1)-(3)) and Blundell and Dias (2009)(Columns (4)-(9)). Columns (1)-(6) show robustness checks for Table 4, Columns (7)-(9) for Table 7. Observations affected by the other treatments are excluded, since all treatment groups are non-overlapping. Coefficients and standard errors of the Blundell Costa-Dias regressions are multiplied by 100 to express effects in percent. Monetary amounts are in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Effects Including Carry-Overs

	Letter Experiment	Spillover Experiment: Trading Partners	
	(1)	(2)	(3)
	Percent VAT > Previous Year	Percent VAT > Previous Year	Percent VAT > Previous Year
Deterrence letter X post	1.35***		
	(0.137)		
Tax morale letter X post	0.237		
	(0.296)		
Placebo letter X post	-0.413		
	(0.297)		
Audit announcement X supplier X post		3.49*	3.29*
		(1.8)	(1.79)
Audit announcement X client X post		-2.01	-2.10
		(2.08)	(2.12)
Supplier X post		-1.17	-1.38
		(2.03)	(2.12)
Constant	46.71***	49.63***	50.40***
	(0.075)	(0.93)	(0.94)
Controls X post	No	No	Yes
Controls X audit announcement X post	No	No	Yes
Month fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Number of observations	7,892,076	45,264	44,288
Number of firms	445,734	2,829	2,768
Adjusted R^2	0.18	0.13	0.13

Notes: Regressions of the probability that monthly declared VAT including carry-overs from previous declarations is higher than in the same month of the previous year. Column (1) corresponds to Column (3) of Table 4. Columns (2) and (3) correspond to Columns (3) and (5) of Table 7. The controls in Column (3) are firm sales, sales/input-ratio, share of sales going to final consumers, and industry categorized as "hard-to-monitor." Coefficients and standard errors are 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 A4: Intent-to-Treat Effects For Cost-Benefit Calculations (for Micro-Size Firms)

	Four Months Post-Treatment	Twelve Months Post-Treatment
	(1)	(2)
Deterrence x post	2,523**	1,360
	(1,086)	(931)
Tax morale x post	3,231	1,259
	(1,997)	(1,657)
Placebo x post	-2,934	-1,649
	(1,996)	(1,795)
Constant	58,872	58,841
	(621)	(642)
Adjusted R^2	0.16	0.16
Winsorized at 5th and 95th Percentile		
Deterrence x post	1,550***	1,093***
	(182)	(175)
Tax morale x post	585	783
	(392)	(367)
Placebo x post	379	463
	(401)	(369)
Constant	48,163	48,134
	(120)	(125)
Adjusted R^2	0.47	0.47
Month fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Number of observations	5,879,337	6,916,536
Number of firms	332,048	314,388

Notes: Regressions of mean declared VAT on treatment dummies among micro-size firms. Observations are monthly for ten months prior to treatment and four months or twelve months after treatment, respectively. Column (2) excludes firms in the second wave of mailing because there are not 12 post-treatment months available for these firms. The first set of estimates are winsorized at the top and bottom 0.1% to deal with extreme outliers, as is in Table 4. Monetary amounts are in Chilean pesos, with 500 Chilean pesos approximately equivalent to 1 USD. Robust standard errors in parentheses, clustered at the firm level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: Cost-Benefit Calculations for Micro-Size Firms

(1) Winsorization	(2) Number of months	(3) Monthly effect (Pesos)	(4) Overall effect (Pesos)	(5) Overall effect (USD)	(6) Number of firms	(7) Total additional tax revenue (USD)	(8) Additional tax revenue net of mailing cost (USD)	(9) Marginal return rate (USD)
0.1 Percent	4	2,523	10,092	19	75,994	1,452,522	1,376,528	18-fold
5 Percent	4	1,550	6,200	12	75,994	893,505	816,360	11-fold
5 Percent	12	1,093	13,116	25	63,041	1,565,996	1,502,955	24-fold

Notes: 102,031 firms received a deterrence letter. Out of these, 75,994 are micro-size (i.e. firms with up to 100,000 USD in annual sales.) When calculating the additional tax revenue for twelve post-treatment months, firms from the second wave of mailing are excluded because there are not 12 post-treatment months available for these firms. The cost of sending a letter through certified mail was 528 pesos or approximately 1 USD.

Additional Cost Considerations

In addition to the marginal cost of mailing for each letter, the tax authority also incurred a fixed cost of setting up the intervention. Namely, there was a time cost of the tax authority staff in the development of the specific wording of the letter, informing their representatives on how to respond to inquiries to the letter, etc.

Finally, and importantly, as noted in footnote 17 of the paper and in Pomeranz et al. (2014), sending out deterrence letters that are not backed up by a strong increase in the audit probability can lead to a reputational cost for the tax authority and can undermine its deterrence power. To make this a sustainable policy, the tax authority has to increase the probability of audits, with corresponding costs. To calculate the full cost of the intervention, one would therefore need to include the reputation cost or alternatively the cost of additional audits. However, the strong response to the second wave of this experiment, 5 months after the first wave, suggests that the deterrence letters did not lead to an immediate loss in deterrence power.

Table A6: Impact of Deterrence Letter on Different Types of Transactions, Sample Including Pure Retailers and Intermediary Firms

	(1) Percent Sales > Previous Year	(2) Percent Input Costs > Previous Year	(3) Percent Intermediary Sales > Previous Year	(4) Percent Final Sales > Previous Year
Deterrence x post	1.01***	-0.02	0.22**	0.90***
	(0.12)	(0.12)	(0.10)	(0.10)
Constant	50.07***	48.94***	29.40***	29.03***
	(0.07)	(0.07)	(0.06)	(0.06)
Month fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Number of observations	7,340,994	7,340,994	7,340,994	7,340,994
Number of firms	408,636	408,636	408,636	408,636
Adjusted R^2	0.28	0.25	0.38	0.47

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 the full sample including pure retailers and intermediary firms. Coefficients and standard errors are multiplied by 100 to express effects in percent. Sample includes all firms in the deterrence treatment and the control group. Robust standard errors in parentheses, clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Differential Impact of Deterrence Letter within Firms by Type of Transaction

	Percent Line-Item > Previous Year	
	(1)	(2)
	Comparing Sales vs. Input Costs	Comparing Final vs. Intermediary Sales
Deterrence letter X post X sales dummy	1.26***	
	(0.17)	
Deterrence letter X post X final sales dummy		1.23***
		(0.23)
Sales dummy	1.98***	
	(0.51)	
Final sales dummy		5.64***
		(0.12)
Sales dummy X post	Yes	
Final sales dummy X post		Yes
Firm fixed effects X post	Yes	Yes
Month fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Number of observations	4,785,058	4,785,058
Number of firms	133,156	133,156
R^2	0.32	0.24

Notes: Regression of the probability of the line item being higher than in the same month of the previous year. Each observation is one of two line items for a given firm in a given month: Sales and input costs in Column (1), final and intermediate sales in Column (2). Column (1) compares the impact on sales with the impact on input costs within a given firm. Column (2) does the same for final sales vs. intermediate sales. The reg2hdfe routine (Guimaraes and Portugal, 2010), used to produce the large number two-level fixed effects (firm fixed effects and firm fixed effects X post), does not produce a constant term. The four months after the second wave exclude firms treated in the first. Coefficients and standard errors are multiplied by 100 to express effects in percent. Sample contains all firms that have both final and intermediary sales 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.