Small Firms Response to Tax Enforcement through Audits[1]

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Abstract

Understanding tax non-compliance and the effect of different enforcement strategies is relevant for improving the efficiency and efficacy of tax policy. Using administrative data provided by Chile's tax authority we analyze the dynamic effects of real-world tax audits for the universe of micro and small firms. The results show that audits have significant impacts on the corporate income tax base and sales of audited firms. The largest effects are found among micro firms and last up to one year after the audit. We exploit the rich panel dataset to alleviate concerns about non-random audits.

JEL Codes: H26, H25, H32

1. Introduction

Tax enforcement and compliance are an essential part of a tax system. As Slemrod and Gillitzer (2013) argue, tighter enforcement could be a more socially desirable way to raise revenue than increasing statutory tax rates. Tax authorities have two major, potentially complementary, tax auditing technologies to enforce compliance. The first one is based on third-party reporting, which consists in comparing data reported by taxpayers with data reported by other legal institutions about the taxpayer's activities. This is a very effective strategy (Kleven et al., 2011) and in Chile is done automatically when filing taxes online. Recent theoretical studies argue that its effectiveness can be explained by the additional deterrence effect implied by third-party information reporting (Kopczuk and Slemrod, 2006; Gordon and Li, 2009; Kleven et al., 2016). In fact, experimental evidence shows that the tax compliance rate on third-party reported income is much higher than on self-reported income (Kleven et al., 2011; IRS, 2012) and that threat-of-audit letters have a

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significant effect on self-reported income but not on third-party reported income (Slemrod et al., 2001; Kleven et al., 2011; Pomeranz, 2015).

The second technology is what Slemrod (2016) calls "real-world operational audits", consisting in visiting or summoning the taxpayer to verify the information reported and the invoices and other documents that support it. Even though third-party reporting is preferable as it reduces enforcement costs and increases evasion costs for the taxpayer – as tax evasion of third-party reported income is quite difficult whereas it is much easier when it is self-reported-- it does not fully eliminate tax evasion if there are unverifiable margins and/or if the institutional environment is weak (Carrillo et al., 2017). In that sense, traditional audits are a complementary tax enforcement tool. Furthermore, in the case of collusive tax evasion between employers and employees third party reporting becomes less effective as an enforcement tool and the evidence is that the main enforcement strategy to detect undeclared work is unannounced audits at the work site (Bjorneby et al (2018)).

In this paper, we study the effects of real world operational audits on taxable income for the universe of micro and small Chilean firms. Tax authorities in most countries regularly perform these operational audits and usually collect a non-negligible amount of tax revenue through them (Kopczuk and Slemrod, 2006). The paper contributes to the empirical literature of tax evasion by studying these operational audits, instead of the more commonly studied random audits. Although random audits have important advantages for the econometric identification of their causal effect, they have two important caveats that limit their external validity (Slemrod, 2016). First, randomly selected taxpayers are not representative of the taxpayers usually targeted by tax authorities. Therefore, their reactions to tax audits might be different from the reaction of taxpayers subjected to real world audits. In addition, most taxpayers know they have been randomly selected to be audited; thus, the causal effects of those audits may not be extrapolated to real operational audits.[5] In particular, real-world tax audits are expected to have substantially different effects on taxpayer's perceptions regarding future auditing probabilities than random audits. This is an especially relevant concern when studying dynamic reactions to tax enforcement, as we do in this paper.

We exploit the richness of our dataset to minimize concerns about the identification strategy by using firm fixed-effects, a rich set of controls, and pre-processing of the data with non-parametric matching techniques (Ho et al., 2007). Our results show that firms do respond to audits: taxable income is almost 16 percent larger among audited firms in the audit year and 8 percent in the years after the audit. Most of these effects are concentrated among micro firms and one year after the audit. In fact, we detect no statistically significant effects two and three years after the audit. We find similar results for sales reported, which is the main reason why taxable income increases.

[5] An exception is Kleven et al. (2010).

To the best of our knowledge, there is almost no evidence on the dynamic effects of audits on corporate income tax.[6] Several studies estimate the dynamic effects of audits on personal income taxes in different contexts and find lasting effects of audits on subsequent reported income (Mazzolini et al., 2017; DeBacker et al., 2015a; Advani and Elming, 2017; Kastlunger et al., 2009). The different nature of individual and corporate tax evasion warrants an empirical analysis of the effects of audits on corporate tax. Corporate tax evasion is much more complicated than individual tax evasion because it involves the strategic behavior of the firm's owner and manager (Chen and Cyrus Chu, 2005). Furthermore, in the case of Chile, 22.1% of total tax revenue is collected from corporate taxes while only 8.5% is collected from personal income taxes.

Additionally, most of the empirical evidence in the tax evasion literature has focused on compliance in developed countries for both personal income taxes (Blumenthal et al., 2001; Slemrod et al., 2001; Wenzel and Taylor, 2004; Kleven et al., 2010) and corporate income taxes (Onji, 2009; D'Agosto et al (2017), Almunia and Lopez-Rodrigues, 2018). This paper contributes to a small literature analyzing tax evasion and auditing in emerging economies, where lower levels of state capacity generally hinder the ability of efficient monitoring and potentially lead to high levels of informality and evasion rates. We provide complementary evidence to Pomeranz (2015), who also analyzes tax evasion among firms in Chile. However, Pomeranz focuses on VAT enforcement using randomized experiments, while this paper studies evasion of corporate income tax using real-world operational audits. Similarly, Scartascini (2015) and Doerrenberg and Schmitz (2015) use randomized experiments to evaluate the relative impact of different auditing technologies on VAT in Colombia and on corporate income tax in Slovenia, respectively, whereas Agostini and Martínez (2014) analyze the impact of letters sent to firms by the Chilean tax authority requiring information to enforce diesel taxes.

Our paper is also related to Carrillo et al. (2017) who analyze the tax response of firms in Ecuador to IRS notifications of discrepancies between reported and third-party information. The authors analyze a time frame immediately after the introduction of such notifications and, therefore, their results can be interpreted as short-term effects. Our paper complements their evidence by analyzing an environment in which taxpayers' expectations about tax enforcement has arguable reached a steady state. Contrary to Carrillo et al. (2017), we find that tax revenue does increase because of the audits, so that firms do not fully adjust their reports to maintain their pre-audit base.

The rest of the paper continues as follows. Section 2 describes the data and provides a description of the audit process. Section 3 describes the empirical strategy. Section 4 presents the estimation results. Section 5 concludes.

[6] A recent exception is DeBacker et al. (2015b) which studies firm behavior in the U.S.

2. Data and enforcement actions

In the empirical analysis, we use administrative data from the annual income tax forms filed by all formal firms in Chile from 2009 to 2013. Chile's tax authority, *Servicio de Impuestos Internos* (SII), provided the data. Firms in the tax authority's website fill the annual declarations electronically.

The data contains firms' characteristics including age, size, economic sector and administrative location for tax purposes, as well as all the information reported in the annual tax return, including the tax base and sales. Importantly, the data contains audit flags indicating whether the firm was summoned in any year between 2008 and 2012. The sample consists of about 1.8 million firm/year observations with information on the tax base.

Selection for an audit is based on a two-step procedure. In the first step, the information provided in the annual return is compared with third party information and with information reported by the taxpayer in other tax forms. Any discrepancy between the tax return and this additional information triggers an online warning. The taxpayer may amend the return or provide new information in order to explain the discrepancies. If the discrepancies are not solved, the taxpayer may be subject to an administrative summon. Selection into this second stage depends on the severity and number of discrepancies and the resource constrains of the regional SII offices.[7] Audits are conducted through an in-person interview at a SII office face to face. The taxpayer is notified by regular mail or by email when the taxpayer has previously authorized this.

We construct our estimation sample by restricting the data in several ways. First, we drop all organizations that are not Chilean private for-profit firms. That is, we exclude non-profit organizations, international companies, international organizations and state-owned firms. We also exclude investment funds and pension fund managers.

In addition, in order to circumvent problems associated with dynamic effects, we exclude firms audited in 2008 as well as firms audited more than once during our sample period. Furthermore, we drop the information for 2013 since we do not observe which firms were audited that year.

In addition, we exclude all firms classified as medium and large by the SII.[8] About 7% of firms belong into these categories in our data set and very few of them were audited over

[7] Unfortunately, we do not observe the severity, number and resource criteria used by the SII to categorize tax returns' discrepancies, information that would have allowed us to use a regression discontinuity design. As explained below, we use pre-processing of the data with non-parametric matching techniques to deal with the non-random nature of summons.

[8] Firms are classified by the SII within four broad size categories according to annual sales: micro-enterprises, small businesses, medium-sized businesses, and large businesses. According to this classification, a micro-enterprise had sales below 11 thousand dollars in 2013. In turn, a small firm had annual sales between 11

the sample period. This is expected as the larger corporations can afford the services of big accounting firms and of tax attorneys. Moreover, the SII has a special division devoted to the enforcement of taxes among large taxpayers, using for this purpose different strategies and criteria.[9]

In order to correct for outliers, we also exclude firms that in any given year, and within each size category (micro enterprises and small firms), report a tax base at the top 1% of observations.

Finally, we exclude firms that were notified but not audited; that is, we exclude firms that displayed discrepancies in the first stage of the audit process and then either clarified or fixed them, as well as those that displayed discrepancies but were not selected into an audit given the SII audit selection criteria.

After these restrictions, our data consists of almost 700,000 unique firm/year observations with complete information. Panel A of Table 1 shows the descriptive statistics of the data, split by treatment and control firms.

Several important differences across treatment and control group emerge. First, control firms are smaller, as either measured by tax base or by sales. The average tax base among control units nears 6 million Chilean Pesos (CLP), whereas the tax base among treated units nears 13 million CLP.[10] Consistent with this, almost 82 percent of control firms are classified as micro, compared to only 55 percent of treated firms.

There are also substantial differences with respect to firm age: the average control firm is 10 years older than the average treatment firm. Similarly, there exist some important differences regarding economic sector. Although hotels and restaurants is the most frequent sector for both groups, control groups are substantially more likely to belong to this group (51 percent) than control firms (34 percent).

Finally, control and treated units show a similar pattern of geographic dispersion. The largest difference is in region 15, which corresponds to the eastern area of Santiago, where 16 percent of control firms are located compared to 21 percent of treated units.

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and 116 thousand dollars in 2013. A medium sized firm sold between 116 and 465 thousand dollars in that year. A small number of firms switched from one category to another over the sample period. We assigned the most frequently observed category to all observations of any given firm. Similarly, we assigned the most frequent location and the most frequent economic sector to firms that switched across these categories over the sample years.

[9] Consistent with this, we find no effect of audits on the tax base among these firms. Results are available upon request.

[10] All CLP variables are expressed in 2013 real terms. Nominal variables were deflated by the CPI. For reference, the average exchange rate in 2013 was 495 CLP per US dollar. Therefore, on average, the tax base among control firms equals about 12 thousand dollars, whereas among treated firms equals about 26 thousand dollars.

The differences in observables across treatment and control firms raise concerns about unobserved differences among treated and control groups.

We take advantage of the richness of the dataset to address these concerns in two ways. First, we pre-process our sample using exact matching on the following covariates: age, region, sector, tax regime (general, simplified accounting or presumptive), and firm size dummies (micro or small). In practice, we first discard treated firms for which there is no control firm sharing the same exact combination of observables in the year of treatment. This "common support" sample consists of about 520,000 firm/year observations, corresponding to around 162,000 unique control firms and 29,000 unique treated firms. Next, we match each treated firm to all control firms with the same observable characteristics in the corresponding treatment year. The goal of this procedure is to make the control sample have a similar distribution of characteristics as the treatment sample in the common support. It is important to highlight that this procedure requires an exact match in terms of the age of the firm (in years) as well as in one of 18 possible different regions, 19 possible economic sectors, 2 different accounting regimes, and 2 different tax regimes. The richness of the dataset allows us to perform this high-dimensional exact matching without running into the well-known curse of dimensionality, that generally leads researchers to resort to parametric or semi-parametric methods based on an estimated propensity score (Rubin and Thomas, 1996). In fact, the common support includes 74% of the pre-processed data.

The effect of the pre-processing procedure in covariate balance is shown in Panel B of Table 1. The treatment and control samples are balanced in all the observables used as control variables. Pre-processing the sample renders the distribution of covariates among the control firms to be very similar to that of the treated firms, both in terms of the means and standard deviations.[11] Moreover, by comparing Panel A and Panel B, we see that the treated firms in the common support are highly representative of the treated firms in the raw data. As such, our final estimation sample is highly representative of the type of firms to which the tax authority actually deployed enforcement actions.

In addition, the panel nature of our data allows us to perform a differences-and-differences estimation, and to control for a full set of firm fixed-effects. We provide details of our empirical strategy, carried out in the preprocessed data, in the next section.[12]

3. Empirical strategy

The empirical strategy consists of estimating the effects of enforcement actions on several outcomes using a differences-in-differences approach. Let t_i^* be the year at which firm *i* was audited. For a given outcome of interest Z_{it} for firm *i* in year *t* we estimate:

[11] In fact, exact matching is designed to balance the entire joint distribution of covariates.
[12] A similar procedure was implemented by DeBacker et al. (2015a), who face the same data issues.

$$Z_{it} = \alpha_i + \beta_0 1(t = t_i^*) + \beta_p 1(t > t_i^*) + \varphi_t + \boldsymbol{X}_{it} + \epsilon_{it} \qquad (1)$$

where X are observable characteristics of the firm and the main coefficients of interest are β_0 and β_p which measure the immediate and the future effect of tax enforcement on taxable income. Barring endogeneity concerns, finding $\widehat{\beta_0} > 0$ would mean that, on average, firms increase their reported income once audited. Similarly, $\widehat{\beta_p} > 0$ would indicate that firms increase reported income as a response to past audits, indicating dynamic effects of tax enforcement.

To investigate in more detail the dynamic effects, we then estimate

$$Z_{it} = \alpha_i + \sum_{k=0}^{3} \beta_k 1(t - t_i^* = k) + \varphi_t + \boldsymbol{X}_{it} + \epsilon_{it} \qquad (2)$$

In this specification, the main coefficients of interest are the β_k's, which correspond to the differences-in-differences estimates *k* years after the audit.

The main identifying assumption is that enforcement is not correlated with unobservable trends in the outcome of interest. The fixed effect α_i enables us to control for time-invariant unobservable factors affecting the outcome that may be influence treatment assignment. Moreover, as described in the previous section, we perform exact matching on a rich set of characteristics $\boldsymbol{X}_{it}$ to pre-process the data.

4. Results

Table 2 shows the result of estimating equation (1) on (log) taxable income. Each column corresponds to different combinations of sample definition and specifications. Column (1) shows the result of estimating equation (1) on the full sample and without adding controls. The results show that taxable income is 6.3 percent higher in the year of the treatment but 5.3 percent lower the years after the treatment. Column (2) shows that the estimated effect is very sensitive to restricting the sample to the set of firms in the common support of observables. In fact, when we restrict the sample to the common support, we find a negative impact on taxable income in short term, and not statistically significant effect in the years after the treatment. In column (3) we add the rich set of covariates already described in Section 2. Unsurprisingly, adding these covariates significantly increases the model's fit and significantly decreases the remainder correlation between the treatment variable and the outcome. In this specification, we find that taxable income is 8.6 percent higher in the treatment year and 6.8 percent in the years following the treatment. Column (4) includes firm-specific fixed effects that eliminate concerns regarding time-invariant unobservables that may bias the results. Adding fixed effects allows us to explain almost 90 percent of the variance in taxable income. The results show that taxable income is 15.8 percent larger among treated units in the treatment year and 8 percent in the years after the treatment. Finally, in our preferred specification, we estimate the model in the reweighted sample. As shown in Table 1, in the reweighted sample we achieve covariate balance between treated and control firms. We find that that the contemporary effect of enforcement is 13 percent, and is 2.8 percent in the years after the treatment.

Next, we turn to investigate heterogeneity across different firm sizes. To that end, we estimate equation (1) for firms according to their size. The results are shown in Table 3. Column (1) replicates the results of our preferred specification in Table 2 to facilitate the comparisons. Columns (2) and (3) show that the effects are significant for both micro and small firms with a 15 percent and an 10 percent increase in taxable income respectively. We also find that the dynamic effect is mostly concentrated among micro firms. We estimate a significant dynamic effect equal to 4 percent on micro firms, but no significant effect among small firms.

Table 4 shows the results of estimating equation (2) for the pooled sample and for micro and small firms separately in order to characterize the dynamic effect in more detail. The results show positive effects of the audit only one year after the treatment: micro enterprises increase their tax base by 15.5 percent in the audit year and by 8.4 percent one year after, whereas small firms increase their tax base by 10 percent in the audit year after and by 4.9 percent one year after. The coefficients for two and three years after the audit are statistically equal to zero for both types of firms, except for a negative effect three years after the treatment for small firms. One potential explanation for this finding is a "bomb-crater effect" (Mittone, 2006), which leads taxpayers to believe that the risk of being audited falls immediately after an audit. An alternative explanation is a "loss repair" effect (Andreoni et al., 1998), which makes taxpayers evade more in the future to recover the losses due to the audit.

To further investigate the ways through which tax base increases, we estimate equation (1) and (2) using sales as the dependent variable[13]. The first three columns in Table 5 show the results of estimating equation (1). The results are qualitatively and quantitatively similar to those obtained for the tax base: annual sales are 14.7 percent larger among treated units in the treatment year and 3.6 percent larger in the years after the treatment. These effects are larger for micro than for small firms (16.2 and 5 percent versus 12.4 and a non-statistically 2.6 percent, respectively).

Similar to Table 4, columns (4) to (6) display the results allowing for heterogeneity in the dynamics of the audit effect (equation 2). As in the case of the tax base, we find a positive dynamic impact of the audit on sales that is concentrated one year after the treatment (9.6 percent for micro firms and 6.4 percent for small firms). Moreover, we again find a negative and statistically significant reduction three years after the audit for small firms.

[13] We again drop firms that in any given year report sales at the top 1 percent within size category.

Conclusions

Tax authorities around the globe regularly audit corporate tax returns to detect and prevent tax evasion. In spite of their popularity, the impact of such policies has been seldom studied empirically. In this paper, we estimate the effect of real-world tax audits on the behavior of Chilean firms. The empirical results show that in response to an audit, firms immediately report a larger income tax base and more sales. These effects last up to one year after the audit. The results are consistent with firms updating their perceived audit risk up, reducing noncompliance. However, the positive dynamic effect is short-lived, as we find evidence of negative effects two and three years after the audit. This effect gives support to either a “bomb-crater effect” (Mittone, 2006) or “loss repair” effect (Andreoni et al., 1998) and is consistent with the empirical evidence of Di Porto (2011), who shows that in Italy corporations increase their tax aggressiveness after audits.

Overall, these combined results show that firms react to audits by adjusting their tax base and that this is done, at least partially, through an adjustment in reported sales. Contrary to Carrillo et al. (2017), we thus find that audits do generate higher tax revenue since firms do not fully adjust other margins. We note that the audits that we analyze contemplate a thorough review of the tax return and thus leave little “unverifiable margins” in which firms could adjust.

It is also important to highlight that the dynamic effects we find are conditional on the current audit probabilities among firms in Chile. Drawing conclusions regarding optimal audits in light of these results is beyond the scope of this paper but it is a promising avenue for future research.

Table 1. Descriptive Statistics in Raw and Processed Data

	Panel A: Raw Data				Panel B: Processed Data			
	Control		Treatment		Control		Treatment	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Tax base (million Chilean pesos, 2013)	6.03	12.66	13.26	20.21	11.32	19.25	13.86	20.83
Ln tax base (ln of Tax base +1)	1.30	1.01	1.95	1.20	1.74	1.21	2.00	1.20
Sales (million Chilean pesos, 2013)	38.85	86.48	89.81	215.48	73.71	117.26	91.02	226.67
Ln sales (ln of sales +1)	2.77	1.33	3.70	1.36	3.44	1.44	3.73	1.33
Age (years)	39.48	22.24	29.41	23.27	28.30	22.97	27.99	22.95
Location (SII regional division dummies)								
Region 1	0.02	0.14	0.02	0.13	0.02	0.12	0.01	0.12
Region 2	0.03	0.17	0.04	0.18	0.04	0.18	0.04	0.18
Region 3	0.02	0.13	0.02	0.13	0.01	0.11	0.01	0.11
Region 4	0.04	0.19	0.04	0.20	0.04	0.19	0.04	0.19
Region 5	0.10	0.30	0.10	0.30	0.10	0.31	0.11	0.31
Region 6	0.06	0.24	0.04	0.19	0.04	0.19	0.04	0.19
Region 7	0.06	0.24	0.05	0.21	0.05	0.21	0.05	0.21
Region 8	0.10	0.30	0.09	0.29	0.10	0.30	0.10	0.30
Region 9	0.05	0.21	0.05	0.22	0.05	0.21	0.05	0.21
Region 10	0.05	0.22	0.05	0.21	0.05	0.21	0.05	0.21
Region 11	0.01	0.09	0.01	0.09	0.01	0.08	0.01	0.07
Region 12	0.01	0.09	0.02	0.12	0.01	0.10	0.01	0.10
Region 13	0.07	0.26	0.10	0.30	0.10	0.30	0.11	0.31
Region 14	0.12	0.33	0.09	0.29	0.10	0.30	0.10	0.30
Region 15	0.16	0.37	0.21	0.40	0.22	0.41	0.22	0.41
Region 16	0.07	0.26	0.06	0.24	0.07	0.25	0.07	0.25
Region 17	0.01	0.11	0.01	0.11	0.01	0.09	0.01	0.09
Region 18	0.02	0.14	0.02	0.13	0.02	0.12	0.01	0.12
Sector dummies								
Agriculture	0.00	0.05	0.00	0.05	0.00	0.03	0.00	0.03
Fishing	0.04	0.19	0.05	0.22	0.04	0.20	0.04	0.19
Mining	0.00	0.04	0.00	0.06	0.00	0.04	0.00	0.04
Non-Metallic Manufacturing	0.00	0.05	0.01	0.07	0.00	0.04	0.00	0.03
Metallic Manufacturing	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25
Utilities	0.04	0.20	0.06	0.23	0.05	0.22	0.05	0.23
Construction	0.00	0.04	0.00	0.05	0.00	0.01	0.00	0.02
Retail	0.07	0.25	0.11	0.31	0.11	0.31	0.11	0.31
Hotels and Restaurants	0.51	0.50	0.34	0.47	0.38	0.48	0.37	0.48
Transport and Telecommunications	0.06	0.23	0.05	0.22	0.04	0.21	0.04	0.21
Financial Intermediation	0.03	0.18	0.06	0.23	0.05	0.22	0.05	0.22
Real Estate	0.03	0.16	0.05	0.21	0.04	0.21	0.05	0.21
Public Administration and Defense	0.10	0.29	0.14	0.35	0.15	0.36	0.15	0.36
Education Services	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00
Social and Health Services	0.01	0.09	0.01	0.12	0.01	0.09	0.01	0.09
Other Social Services	0.03	0.16	0.04	0.18	0.04	0.19	0.04	0.19
Residential Management	0.03	0.16	0.03	0.16	0.02	0.15	0.02	0.15
Extraterritorial Organizations	0.00	0.02	0.00	0.02	0.00	0.01	0.00	0.01
Sector NA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Simplified Accounting	0.06	0.24	0.04	0.19	0.03	0.16	0.02	0.15
Presumptive Tax Regime	0.02	0.15	0.03	0.16	0.01	0.11	0.01	0.11
Small firm	0.18	0.39	0.45	0.50	0.43	0.50	0.44	0.50
Micro entreprise	0.82	0.39	0.55	0.50	0.57	0.50	0.56	0.50
Number of Observations	592023		105731		437218		81657	

Table 2. Effect of Audits on Taxable Income

	All observations		Common Support		
	(1)	(2)	(3)	(4)	(5)
Treated	0.649***	0.700***	0.235***		
	(0.008)	(0.009)	(0.008)		
Treated x Year Treated	0.063***	-0.027***	0.086***	0.158***	0.133***
	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)
Treated x After	-0.053***	-0.02	0.068***	0.080***	0.028***
	(0.012)	(0.015)	(0.012)	(0.012)	(0.013)
Year effects	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes
Fixed Effects	No	No	No	Yes	Yes
Reweighted	No	No	No	No	Yes
Observations	697754	518875	518875	518875	518875
R^2	0.049	0.054	0.356	0.854	0.820

Standard errors in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.

Table 3. Size Heterogeneity

	Pooled	Micro firms	Small firms
	(1)	(2)	(3)
Treated x Year Treated	0.133***	0.153***	0.104***
	(0.009)	(0.011)	(0.014)
Treated x After	0.028***	0.040***	0.023
	(0.013)	(0.015)	(0.022)
Year effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Reweighted	Yes	Yes	Yes
Observations	518875	399860	119015
R^2	0.820	0.788	0.743

Standard errors in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.

Table 4. Dynamics Heterogeneity

	Pooled	Micro firms	Small firms
	(1)	(2)	(3)
Treated x Year Treated	0.120***	0.140***	0.093***
	(0.009)	(0.011)	(0.014)
Treated x One year after	0.045***	0.059***	0.037*
	(0.013)	(0.015)	(0.022)
Treated x Two years after	-0.048***	-0.039**	-0.04
	(0.017)	(0.019)	(0.029)
Treated x Three years after	-0.094***	-0.081***	-0.092**
	(0.025)	(0.027)	(0.043)
Year effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Reweighted	Yes	Yes	Yes
Observations	518875	399860	119015
R^2	0.820	0.789	0.743

Standard errors in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.

Table 5. Effect of Audit on Sales

	Pooled	Micro firms	Small firms	Pooled	Micro firms	Small firms
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Year Treated	0.147***	0.162***	0.124***	0.132***	0.149***	0.107***
	(0.008)	(0.010)	(0.011)	(0.008)	(0.010)	(0.011)
Treated x After	0.036***	0.050***	0.026			
	(0.011)	(0.015)	(0.017)			
Treated x One year after				0.053***	0.068***	0.042**
				(0.011)	(0.015)	(0.017)
Treated x Two years after				-0.044**	-0.025	-0.050**
				(0.015)	(0.019)	(0.023)
Treated x Three year after				-0.111***	-0.064**	-0.149***
				(0.021)	(0.028)	(0.032)
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Reweighted	Yes	Yes	Yes	Yes	Yes	Yes
Observations	516221	398117	118104	516221	398117	118104
R^2	0.896	0.821	0.688	0.896	0.821	0.689

Standard errors in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.

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