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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 firms. The results show that audits have significant impacts on the corporate income tax base and sales of audited firms. The effects are mostly concentrated 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. 2010) and in Chile is done automatically when filing taxes online. Recent theoretical studies argue that this 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. 2015). Indeed, 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 significant effect on self-

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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.

In this paper, we study the effects of "real world operational audits" on taxable income for the universe of 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 that are usually targeted by tax authorities. Therefore, their reactions to tax audits might be different than the one of taxpayers subjected to real world audits. Also, 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 15 percent larger among audited firms in the audit year and 7 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.

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 et al. 2015;

[5] An exception is Kleven et al. (2010).
[6] A recent exception is DeBacker et al. (2015b) which studies firm behavior in the U.S.

Kastlunger et al. 2009). The different nature of individual and corporate tax evasion warrants an empirical analysis of the effects of audits in 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% of tax revenue is collected from corporate taxes while only 8% 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; Pomeranz 2015). This paper contributes to a small literature analyzing tax evasion and auditing in developing countries, 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 Martinez (2014) analyze the impact of letters sent by the Chilean tax authority to firms 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 taxpayer's expectations about tax enforcement has arguable reached a steady state. Contrary to Carrillo et al. (2017), we find that tax revenue does increase as a consequence 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.

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. The data was provided by Chile's tax authority, Servicio de Impuestos Internos (SII). The annual declarations are filled electronically by firms in the tax authority's website.

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 this has been previously authorized by the taxpayer.

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.

Also, 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 the

[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 a matching strategy 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-sized 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 and 116 thousand dollars in 2013. A medium sized firm sold between 116 and 465 thousand dollars in that

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 sample consists of nearly 700,000 unique firm/year observations. However, for the empirical analysis we restrict our sample using exact matching. In practice, we discard treated firms for which there is no control firm sharing the same observables in the year of treatment. Also, we discard control firms that cannot be matched to any treatment firm in any year. Matching is done on the following covariates: age, region, sector, tax regime (general, simplified accounting or presumptive), and firm size (micro or small). This "common support" sample consists of about 610,000 firm/year observations, corresponding to around 120,000 control firms and 24,000 unique treated firms.

Table 1 shows descriptive statistics of the final sample, 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 is almost 13 million CLP.[10] Consistent with this, almost 83 percent of control firms are classified as micro, compared to only 58 percent of treated firms.

There are also substantial differences with respect to firm age: control firms are on average 10 years older than treatment firms. Similarly, there exist some important differences regarding economic sector. Although hotels and restaurants is the most frequent sector for

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.

both groups, control groups are substantially more likely to belong to this group (56 percent) than control firms (36 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 18 percent of treated firms are located compared to 22 percent among treated units.

The differences in observables across treatment and control firms raise concerns about unobserved differences among treated and control groups. As explained in the next section, we take advantage of the wealth of the dataset to control for persistent differences across firms using a fixed-effects regression in order to alleviate some of those concerns.[11]

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:

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

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

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

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 set of characteristics $\boldsymbol{X}_{it}$ to pre-process the data (Ho et al. 2007). In particular, we drop treated

[11] A similar procedure was implemented by DeBacker et al. (2015a), who face the same data issues.

firms for which there are no control firms sharing the same set of characteristics $\boldsymbol{X}_{it}$ in year t. Conversely, we discard control observations that cannot be matched to any treated unit. As stated previously, matching is done on age, region, sector, tax regime, and firm size.

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 year after the treatment. Column (2) shows that these results are very similar when restricting the sample to the common support. In column (3) we add the rich set of covariates already described in Section 2. Unsurprisingly, adding these covariates significantly increases the model fit and decreases the remainder correlation between the treatment variable and the outcome. In this specification, we find that taxable income is 13.3 percent higher in the treatment year and 3.4 percent in the years following the treatment. Finally, in our preferred specification, we include 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 percent larger among treated units in the treatment year and 7 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 different firm sizes. 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. However, we can only detect significant dynamic effects among micro firms.

Table 4 shows the results of estimating equation (2) for micro and small firms in order to characterize the dynamic effect in more detail. The results show positive effects of the audit after one year only: micro enterprises increase their tax base by 14.8 percent in the audit year and by 6.2 percent one year after, whereas small firms increase their tax base by 10 percent in the audit year after and by 3.7 percent one year after. The coefficients for two and three years after are statistically equal to zero.

To further investigate the ways through which tax base increases, we turn to estimate similar models using sales as the outcome. In Table 5 we show the results of estimating equations (1) and (2) on (log) annual sales.[12] The first three columns show the results of estimating equation (1). The results are qualitatively and quantitatively similar to those obtained for the tax base: annual sales are 17 percent larger among treated units in the treatment year and 9 percent larger in the years after the treatment.

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

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 positive effects of the audit on sales after one year only among the smallest firms. We also find an increase in the sales of small firms one year after. However, we now find a negative and statistically significant reduction three years after the audit.

Conclusions

Tax authorities around the globe regularly 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 higher income tax base and 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.

Overall, these combined results show that firms react to audits by increasing their tax base and that this is done, at least partially, through an increase in 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 endogenous to 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. Basic Statistics

	Control Group		Treatment Group	
	Mean	Standard deviation	Mean	Standard deviation
Tax base (million Chilean pesos, 2013)	5.96	12.75	12.98	20.12
Ln tax base (ln of Tax base +1)	1.28	1.01	1.92	1.20
Sales (million Chilean pesos, 2013)	38.39	88.17	88.06	221.56
Ln sales (ln of sales +1)	2.76	1.32	3.67	1.36
Age (years)	38.09	21.99	28.48	23.01
Location (SII regional division)				
Region 1	0.015	0.121	0.015	0.121
Region 2	0.030	0.170	0.034	0.182
Region 3	0.012	0.108	0.013	0.114
Region 4	0.033	0.179	0.038	0.191
Region 5	0.107	0.309	0.104	0.306
Region 6	0.055	0.228	0.038	0.191
Region 7	0.058	0.234	0.046	0.210
Region 8	0.108	0.310	0.094	0.292
Region 9	0.043	0.203	0.046	0.211
Region 10	0.047	0.212	0.046	0.210
Region 11	0.004	0.061	0.007	0.082
Region 12	0.006	0.076	0.013	0.114
Region 13	0.074	0.261	0.103	0.304
Region 14	0.130	0.336	0.095	0.293
Region 15	0.178	0.382	0.217	0.412
Region 16	0.075	0.263	0.065	0.247
Region 17	0.010	0.100	0.010	0.099
Region 18	0.015	0.122	0.015	0.123
Sector				
Agriculture	0.001	0.024	0.002	0.041
Fishing	0.032	0.175	0.046	0.208
Mining	0.001	0.026	0.002	0.046
Non metallic manufactures	0.001	0.026	0.002	0.046
Metallic manufactures	0.057	0.232	0.065	0.247
Utilities	0.033	0.179	0.052	0.222
Construction	0.000	0.011	0.001	0.028
Retail	0.065	0.247	0.107	0.309
Hotels and restaurants	0.556	0.497	0.358	0.480
Transport and telecommunications	0.044	0.204	0.045	0.208
Financial intermediation	0.030	0.171	0.055	0.227
Real estate	0.029	0.168	0.049	0.216
Education services	0.101	0.301	0.147	0.354
Health services	0.005	0.070	0.011	0.106
Other social services	0.025	0.155	0.035	0.183
Residential management	0.023	0.149	0.024	0.152
Other organizations	0.000	0.005	0.000	0.014
Simplified accounting	0.040	0.195	0.027	0.162
Presumptive tax regime	0.016	0.124	0.021	0.143
Size				
Small firm	0.173	0.378	0.424	0.494
Micro entreprise	0.827	0.378	0.576	0.494
Number of observations	510767		97683	

Table 2. Effect of Audits on Taxable Income

	All observations	Common Support	Common Support	Common Support
	(1)	(2)	(3)	(4)
Treated	0.649***	0.636***	0.201***	
	(0.008)	(0.008)	(0.007)	
Treated x Year Treated	0.063***	0.065***	0.133***	0.153***
	(0.008)	(0.008)	(0.007)	(0.009)
Treated x After	-0.053***	-0.050***	0.034***	0.069***
	(0.012)	(0.013)	(0.011)	(0.012)
Controls	No	No	Yes	Yes
Fixed Effects	No	No	No	Yes
Observations	697754	608450	608450	608450
R^2	0.049	0.05	0.342	0.856

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	0.153***	0.151**	0.106***
	(0.009)	(0.010)	(0.014)
Treated x After	0.069***	0.041***	0.024
	(0.012)	(0.014)	(0.020)
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Observations	608450	478590	129860
R^2	0.856	0.825	0.762

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	0.150***	0.148***	0.100***
	(0.008)	(0.010)	(0.014)
Treated x One year after	0.084***	0.062***	0.037*
	(0.012)	(0.014)	(0.021)
Treated x Two years after	0.037**	0.001	-0.013
	(0.015)	(0.017)	(0.027)
Treated x Three year after	0.007	-0.033	-0.063
	(0.021)	(0.024)	(0.039)
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Observations	608450	478590	129860
R^2	0.856	0.826	0.762

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	0.173***	0.159***	0.126***	0.169***	0.155***	0.117***
	(0.007)	(0.010)	(0.011)	(0.007)	(0.010)	(0.011)
Treated x After	0.088***	0.049***	0.031*			
	(0.010)	(0.014)	(0.016)			
Treated x One year after				0.106***	0.071***	0.050***
				(0.011)	(0.014)	(0.016)
Treated x Two years after				0.051***	0.007	-0.022
				(0.013)	(0.017)	(0.021)
Treated x Three year after				0.016	-0.016	-0.111***
				(0.013)	(0.024)	(0.029)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	605045	476145	128900	605045	476145	128900
R^2	0.919	0.874	0.702	0.856	0.826	0.762

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

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