Taxes, Prisons, and CFOs: The Effects of Increased Punishment on Corporate Tax Compliance in Ecuador[1]

(Preliminary Draft: Comments welcome)

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Extended Abstract:

This paper takes advantage of a rich firm level data set from Ecuador to analyze the effects of a reform to the tax code during 2007 that introduced stricter punishment for tax evasion. Our dataset contains actual tax-return and financial-statement data for the universe of corporations in Ecuador both before and after the reform (from 2003 to 2007). At its core, the new legislation introduced reclusion from 1 to 6 years as a punishment for non-compliance, and made a firm's CFO liable for tax-crimes.

We study the effects of stricter punishment on corporate income taxes both at the intensive and extensive margins. For the intensive margin we use the subset of firms that belong to the tax-net for consecutive years, and for the extensive margin we focus on the entry into and exit from the tax net using the universe of firms across different years. We combine a difference-in-difference approach with the DiNardo, Fortin and Lemieux (DFL) decomposition method. This allows us to estimate the heterogeneous effects of the reform across the distribution of firms. To construct appropriate counterfactual trends across the distribution for the year when the reform took place, we use the changes observed in consecutive years from 2003-2006.

We find that, at the intensive margin the reform led to an average 10% increase in real corporate tax payments, after controlling for firm characteristics and the growth in taxes between 2003 and 2006. This positive effect was somewhat stronger at the middle of the tax distribution. In contrast, at the extensive margin, we find that the probability of entry into the tax net increased as a result of increased punishment, however most of the firms that entered the tax net claimed zero taxes.

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1. Introduction

Tax administration and compliance have long been the weakest links in tax reform in developing countries. In the last few decades, significant tax reform has been implemented in a large number of developing countries under the structural adjustment and stabilization programs of the World Bank and IMF. The focus of the reform has, however, been on the tax structure, i.e., tax bases and rates, most notably on introducing value-added tax (VAT), and reducing trade taxes (especially import tariff). Although most of the tax reform programs include some administrative components, they are usually accorded only a supportive role at best. The widely implemented tax reform program that instituted VAT as a main source of government revenue has, however, failed to compensate for the revenue lost due to drastic reduction in tariff rates, especially in poor developing countries (see, Keen and Baunsguard (2007), Rajaraman (2004)).[2] This difficult fiscal predicament makes it especially important to understand the revenue effects of tax administration reform in developing countries; given a certain tax structure, how much difference a better monitoring (higher audit probability) or an increase in the punishment can make to the government revenue? While the theory of tax enforcement and compliance is well-developed, the corresponding empirical literature is limited at best, especially in the context of developing countries where the required data are rarely available (among the few studies available, see Das-gupta and Mookherjee (1998), Mclaren (2003)). Even in the context of developed countries, attempts to establish an empirical link between better administration and the extent of evasion have been less than satisfactory because of difficulties in getting data from actual tax-returns, and identification challenges due to unobserved heterogeneity. This paper provides evidence on the effects of higher punishment (both monetary and non-monetary) on corporate income tax revenue by analyzing a rich firm level data set from Ecuador, where imprisonment for 1 to 6 years was introduced in 2007 as a punishment for tax evasion as the center piece of a new tax enforcement regime.

There is a rich body of theoretical work on tax evasion in economics literature (see, Allingham and Sandmo (1972), Srinivasan (1973), Cowell (1990), Dasgupta and Mookherjee (1998), Slemrod and Yitzhaki (2002), among others). The basic model as developed by Allingham and Sandmo (1972) adopts the `crime and punishment' framework that grew out of the influential work of Becker (1964). In the standard model with risk neutrality, the decision to

[2] Most of the governments in developing countries have turned to domestic borrowing to cover the revenue shortfall. This has caused a dramatic increase in domestic debt in developing countries with the attendant consequences such as crowding out of private sector credit (see Emran and Farazi, 2009).

evade taxes depends on the expected cost (probability of getting caught times the punishment) and benefit (savings from tax evasion). Interestingly, most of the theoretical and policy discussion on tax administration reform focuses on the incentives of the tax inspectors, and the possible collusion between the tax payer and tax inspector (corruption). The literature, especially the empirical work, has largely ignored the role of higher punishment given a monitoring regime.

A widely discussed implication of the Becker type models is that the optimal fine is maximal, because improving monitoring intensity (auditing) is costly, but fines actually raise revenue for the government.[3] There is a large literature that discusses a variety of explanations for the fact that we, in general, do not observe maximal fine in the real world (for a survey of the literature, see Polinsky and Shavell (2000)). One important factor which is especially relevant for the developing countries is credit constraint, or more generally wealth constraint. The threat of a large fine has little bite if the tax payer does not have the money to pay the fine.[4]

Since the scope for monetary punishment is relatively limited in developing countries, non-monetary punishment schemes including imprisonment for tax evasion assumes added importance. If the threat of imprisonment is credible enough, it might affect the behavior of tax payer without requiring significant expansion in prison space. A credible threat of the imprisonment, not the actual imprisonment, is important for tax payers' behavior.[5] Thus from a policy perspective, the threat of imprisonment seems to be an attractive instrument to improve taxpayer compliance in developing countries.

However, note that the effects of higher punishment depend on the extent of corruption in tax administration; if most of the tax inspectors are corrupt, a higher punishment can make things worse for both the tax payer and government (Stiglitz, 2010). Since the threat of imprisonment increases the bargaining power of tax inspector vis. a vis. the tax payer, the bribe amount will in general go up. But this may not increase government revenue as the inspector gets rich at the expense of both taxpayer and government. The effect of a higher punishment for tax evasion on government revenue is thus an empirical question. However, to best of our knowledge, there is no empirical work in the current economics literature that attempts to isolate the causal effects of introducing higher punishment on the behavior of tax payers and tax revenue in a developing country.

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[3] One can keep the expected cost unchanged by increasing the fine arbitrarily while reducing the monitoring (auditing) close to zero.

[4] An important part of the problem is lack of information on the ability to pay. For example, most of the firms in developing countries operate in the informal economy with little verifiable accounting data. This makes it especially difficult to use monetary punishment as a credible threat.

[5] In a repeated game model, one should not observe any imprisonment along the equilibrium path.

This paper takes advantage of a rich firm level data set from Ecuador to analyze the effects of a reform to the tax code in Ecuador during 2007 that introduced higher punishment for tax evasion. Although the 2007 reform imposed both monetary and non-monetary punishment, at its core was a highly costly and visible non-monetary punishment. The new legislation introduced reclusion from 1 to 6 years as a punishment for non-compliance, and made a firm's general manager (CFO) and anyone else involved in the tax evasion scheme liable for a criminal offense.[6] Note that imprisonment is a fundamentally different form of punishment that entails potential social cost (loss of reputation in the society) for the family as a whole in addition to the cost to the evader himself/herself. Because of this special nature of imprisonment as a punishment for tax evasion, it stood out as the salient feature of the 2007 tax reform. [7]

Our dataset contains actual tax-return and financial-statement data for the universe of corporations in Ecuador both before and after the reform (from 2003 to 2007). We study the effects of the higher punishment on corporate income tax both at the intensive and extensive margins. For the intensive margin we use the subset of firms that belong to the tax-net for consecutive years, and for the extensive margin we focus on the entry into and exit from the tax net using the universe of firms across different years. A focus of our empirical analysis is to understand possibly heterogeneous effect of higher punishment across the distribution of the firms. We combine a difference-in-difference (DID) approach with the DiNardo, Fortin and Lemieux (DFL) decomposition method which allows us to estimate the heterogeneous effects of the reform without imposing any arbitrary functional form. Although most of the empirical literature on tax reform deals with the mean effects, the mean (average) effects might mask important heterogeneity across different firms. The possibility of heterogeneous effects has implications for both revenue and equity effects of a tax reform. There are plausible theoretical reasons to expect that the effects of a higher punishment will vary significantly across different firms. For example, the large firms are likely to be politically connected, and thus the effective probability of audit may be less for them. On the other hand, large firms might be already under enhanced monitoring, say as part of an LTU, and they are less likely to evade taxes to begin with. Note that if a firm is not evading taxes before the implementation of the 2007 legislation, the higher punishment will have little or no effects on its observed behavior (i.e., it is already at a corner solution with the amount of tax evaded equal to zero). Even if a large firm is not at the corner solution to begin with, the threat of imprisonment may still have relatively little effect on

[6] The higher monetary punishment for tax evasion was in the form of surcharges (20 percent) and higher interest rates on arrears,

[7] Most of the discussion in the media and among general people was also focused on this particular aspect of tax reform.

it. The reason is that the large firms are likely to have substantial bargaining power vis. a vis. the tax inspector. The smaller firms may respond different because the audit resources are usually not concentrated on the lower tail of firm distribution. The new startup firms are usually smaller in size, and they are likely to be significantly more risk takers and thus may discount the cost of higher punishment. The firms in the middle might be most vulnerable, as they are visible to the tax inspectors and they also lose a lot if the owner is imprisoned. They might also not have the political connections or legal resources of the large corporations. The incidence of a higher punishment thus can be quite different across firms of different size.

The 2007 reform in Ecuador offers us an excellent opportunity to understand the impact of higher punishment as a tax enforcement instrument in a developing country for a number of reasons. A major difficulty in estimating the effects of higher punishment on tax evasion is that tax reforms are usually omnibus reform that implements an array of changes simultaneously such as changes in types of taxes (for example, introduction of VAT) and rates (changes in tariff and VAT rates are common), and improvements in monitoring of tax payers (higher audit rates) and anti- corruption strategies (for example, higher wages for tax inspectors). This makes it almost impossible to isolate the effects of a particular component of the reform, for example the effects of higher punishment given the tax structure and the audit regime. The 2007 reform in Ecuador has important advantages in this regard; the corporate income tax rates were left unchanged, and there were no improvements in the monitoring of the tax payers.[8]

In the context of our data set, where we rely on variations over time for estimating the revenue effect of the reform, the estimates might be misleading if the year after reform was exceptional in terms of economic performance. For example, if the economy experiences strong positive productivity shock, the corporate tax revenue might be driven by higher growth and sales revenue of the firms rather than by the threat of imprisonment. Fortunately, 2007 was not above the trend in terms of economic performance, the growth rate in GDP was 3.6% and in manufacturing value added was 3.8% in 2007 compared to 4.8% (GDP) and 4.7% (manufacturing value added) for the period 2003-2006. Thus an increase in the revenue between 2006 and 2007 is not likely to be driven by common positive shocks to growth such as

[8] The evidence in fact shows that the auditing intensity was lower in 2007 compared to some of the earlier years (in the Northern Regional Office). Thus the higher revenue in 2007 cannot be due to increased monitoring. This also means that when we use the data from earlier years to estimate the counterfactual trend, it might over-estimate the trend for 2007.

productivity or price shocks.[9] As an additional layer of caution, we control for the sales revenue of a firm in the regressions.[10]

To estimate the effect of the higher punishment, we use difference-in-difference (DD) and difference-in-difference-in-difference (DDD) approaches, exploiting the fact that we have data on a number of years before the reform was implemented (2003-2006). The main identification challenge is that the effects of the reform in 2007 on revenue may be confounded by other secular factors such as economic growth and small but steady improvements in tax administration.[11] The increase in the revenue between 2006 and 2007 thus cannot be attributed solely to the increased punishment for tax evasion. We need to construct the counterfactual trend in revenue growth that would have been observed in the absence of the 2007 reform. The availability of data for four years before the reform allows us to understand the underlying trend with a measure of confidence, and also to construct the counterfactual trend in a variety of ways. For example, we calculate alternative trend lines across the distribution of firms using data from different pairs of consecutive years, and then take the upper envelope of the different trends as a conservative estimate of the counterfactual trend in corporate tax revenue in the absence of the reform. An alternative is to use the average of the different trends as an estimate of the counterfactual trend. The average may represent the counterfactual trend more faithfully if the year-to-year changes in corporate income tax revenue are affected significantly by transitory shocks. To address the possibility that the underlying trend is not linear, and may be a convex function of time (i.e., the year-to-year revenue changes are increasing in time in the absence of the reform), we implement a triple difference (DDD) approach. The main conclusions of the paper are thus based on a combination of DDD and DFL decomposition method.

At the intensive margin, results suggest that there was a large and positive mean effect (about 10 percent) of increased punishment on the growth rate of corporate income taxes of firms that file positive taxes in consecutive years; this effect was somewhat larger for the firms at the

[9] One important price shock especially for government revenue in Ecuador is oil price. To make sure that our results are not driven by the oil revenue, we exclude the firms in the oil sector.

[10] We have data on the sales revenue reported by a firm in the tax return. We would expect a firm to report higher sales when declaring higher tax liability as a response to the higher punishment. This implies that by controlling for the sales revenue, we might be underestimating the effects of punishment. It is interesting that we still find substantial effects of higher punishment on corporate income tax revenue.

[11] Note that in most of the available studies in economics that use difference-in-difference method, data consists of only one period before the treatment (usually baseline surveys in evaluation studies), which makes it difficult to estimate the underlying trend in the absence of intervention, and one cannot test the validity of the "parallel trend" assumption. The fact that we have data for four periods before the "intervention" (i.e., 2007 reform) makes it easier for us to estimate the counterfactual trend. One might argue that compared to a standard difference in difference set-up our data set suffers from the fact that we do not have a control group in the same period of time. However, observe that most of the objections to the validity of difference in difference estimates emanates from the fact that the control group may not be a good representation of treatment group in the counterfactual state. In our case, the control group is exactly the same firms, but observed in a different time period.

right tail of the tax distribution (90th percentile). But there was no significant mean effect when the sample also includes firms that claim zero taxes in their tax returns. In fact, our evidence suggests that increased punishment decreased growth rate of corporate income taxes at lower quantiles (-37 % at the 30th quantile), but increased it for higher quantiles (12 percent at the 70th quantile, and 14 percent. at the 90th quantile). These results suggest that focusing on mean impacts can mask important heterogeneity in the impact of tax reforms.

Results for the extensive margin suggest that the overall probability of entry into the tax-net increased as a result of increased punishment as compared to the period (2005-06). However, when considering the changes in the probability of entering the tax-net of firms that pay positive taxes, we notice that it actually decreased. Thus, while a number of firms began to file taxes in 2007, most of them claimed zero taxes.

The rest of the paper is organized as follows. Section 2 provides institutional details about Ecuador's Tax Administration and the 2007 reform. Section 3 describes the data. In section 4, we present the empirical strategy to measure the mean impact of the 2007 reform at the intensive margin. Section 5 measures the impact at several points of the tax distribution. In Section 6, we discuss effects of the reform at the extensive margin. Sections 7 and 8 present results using alternative functional forms (levels rather than natural logarithms) and robustness checks. Finally, the latter section concludes.

2. Taxation in Ecuador

In this section, we provide some institutional details about Ecuador, its Tax Administration (SRI) and, most importantly, the 2007 Tax Reform.

2.1. General background

Ecuador is a developing country in South America. In 2006, its per capita GDP was close to $3,200, lower than most of the other countries in this continent except Bolivia and Paraguay. Ecuador's economy relies heavily on the oil industry. Oil exports accounted for about 55 percent of its total exports and more than 25 percent of the Central Government revenue came from oil-related royalties in 2006.

Ecuador suffered a severe financial crisis at the end of 1999 that precipitated a collapse of the banking system and a contraction of more than 6 percent in GDP. In January, 2000, a presidential decree approved a series of structural reforms to address the ongoing crisis including the adoption of the US dollar as the legal currency of the country (i.e., "dollarization" in popular

parlance). A slow economic recovery followed. Higher oil prices and increased remittances helped the country achieve an average annual growth rate close to 5 percent from 2001 to 2006.

In Ecuador, all firms are taxed 25% of their profits. Moreover, all corporations are required to distribute 15% of pre-tax profits among their employees. Although these tax obligations may seem high, the typical corporate tax burden in Ecuador is lower than in other countries in the region.

Taxable income (profit) is defined as the sum of ordinary and extraordinary revenues subject to tax, less production costs and other discounts and deductions.[12] Profits are taxed equally regardless of whether they are retained or distributed. However, special provision laws may allow for additional tax breaks.[13]

The fiscal year in Ecuador coincides with the calendar year (ending December 31). All corporations must file an annual profit tax return at the end of the tax year –between February 1st and May 10th– according to a deadline that varies with the corporation's tax registration number. Each corporation assesses its own profit tax, but tax authorities usually revise those assessments on subsequent inspections within specified time limits (Deloitte 2010).

Virtually all revenue received by corporations is subject to income tax withholding (Deloitte 2010).[14] The tax withheld is usually an advance payment of the recipient's profit-tax and may be used to offset the total annual tax due. The SRI has the power to adjust withholding rates without approval of the legislature.

The SRI classifies corporations into different groups, which may face higher levels of monitoring. The most relevant is the group composed of Large Taxpayer Units (LTUs). LTU are locally known as "special taxpayers" and include firms of greatest economic importance (usually measured by their sales and employment levels) within each sub-region in the country.

2.2. The 2007 Tax Reform

During the last decade, taxation in Ecuador has faced a number of profound changes. Taxation in Ecuador may be classified into three periods: (a) a period of policy instability accompanied by deep administrative reform between 1997 and 2003; (b) a period of relative

[12] Companies in certain sectors such as petroleum, construction, urban development, and real estate dealings, may compute taxable income in accordance with special rules (Deloitte 2010).

[13] For instance, there is a Hydrocarbons Law specific for the oil sector; Tourism and Mining Laws provide their own set of tax breaks; and Regional-development Laws offer tax incentives for investments in certain provinces (EIU, 2006).

[14] The withholding system is a mechanism where most companies (those that are designated by the SRI to be withholding agents) are required to deduct and withhold a fixed percentage of the payments they make to other firms. We refer to this fixed percentage as the withholding rate. This deduction takes place only if the payment is taxable income for those who receive it. Every month, withholding agents must report and transfer all withholdings to the tax authority. Firms can deduct their withheld funds from their tax liability.

status-quo –lack of structural tax reform accompanied by a consolidation of earlier administrative improvements– between 2003 and 2006; and (c) a major tax reform in 2007. To avoid cluttering the text, in this section we focus on the 2007 reform (details about earlier reforms can be requested from the authors).

In 2007, reducing tax evasion and improving tax collection became a primary objective of a new presidential administration. At the end of December 2007, the Ecuadorian constitutional assembly passed a major tax reform bill, known as "Reform for Tax Equity". The 2007 Tax Reform introduced a large number of changes affecting several types of taxpayers (i.e. individuals and corporations) and several types of taxes (i.e. VAT, income tax, and profit tax). However, at the core, the reform toughened sanctions for non-compliance with tax law and granted new enforcement tools to the SRI (EIU 2009). Changes introduced –which are relevant for corporations– are summarized in Table A1.

The new measures came into effect in January 2008; however, given that tax-returns for a given year are filed in April of the following year, some parts of the new legislation –in particular more severe sanctions for tax evasion– effectively apply to the 2007 tax-year.

The most notable aspect of the 2007 tax reform is the introduction of more rigorous sanctions. New penalties include: suspension of activities, definitive seizure, suspension or cancelation of the tax-payer id-number, and minor ordinary reclusion from 1 to 6 years. Moreover, legal actions can no longer be extinguished through the payment of tax obligations as occurred prior to 2007. In addition, while in the past only a company's legal representative or accountant were responsible for tax-crimes, now this responsibility extends to anyone in the company who may have been part of a tax evasion scheme, and in particular the CFO. Finally, the new legislation increased fines and surcharges charged to firms that fail to pay their tax obligations.[15]

Besides tougher sanctions, the 2007 tax reform introduced a few other changes. For example, corporations are impeded from transferring profit to related parties located in tax-heavens; firms are prevented from claiming large deductions for interest payments on loans from related-parties on foreign countries; and tougher controls on leasing practices are imposed.[16] On the administrative front, during 2007, the SRI also took advantage of some enforcement tools at its disposal to tackle evasion. While the tax administration exercised its power to decree a hike in

[15] First of all, the interest rate for tax arrears has increased from 1.1 to 1.5 times the reference rate (the 90-day active referential interest rate of the Central Bank). In addition, a surcharge of 20% of the principal must now be paid for non-declared income that is discovered by the SRI through tax assessments. Prior to the reform, taxpayers were charged only the additional tax assessed plus interest.

[16] At that time, some firms used to sell a machine at a loss, and then lease the same machine in order to deduct from its taxes both the loss amount and the rental cost.

withholding rates,[17] there is no evidence of significant improvements in monitoring efforts during this period.[18]

3. Data Description

3.1. Sample Description

The data used in this paper were obtained from SRI administrative records. The database contains information on every line-item of the universe of income tax returns filed by public and private corporations in Ecuador. Regardless of profitability, all corporations are required to file income taxes using Tax-Form 101, which contains information on the firm's balance sheet and income statement.[19]

We focus exclusively on firms that were created in 2003 or earlier. This latter step is important to control for the impact of firm creation and to control for tax policies that encourage firm closure and reorganization as new ventures. We also excluded from the sample firms that belong to the public sector and firms that operate in the oil industry because they are subject to special taxation rules.[20] All nominal variables were deflated to allow for meaningful comparisons across years using the CPI from the Ecuadorian Central Bank.[21]

As it was mentioned in the introduction, we analyze the impact of the reform on both on the intensive margin –firms that belong to the tax net in consecutive periods– and the extensive margin –the probability of entering/exiting the tax net. For the intensive margin analysis we focus on two different samples: Sample A includes only firms that file positive taxes in consecutive years; while Sample B includes firms that file taxes in consecutive periods regardless of the amount filed (i.e. including zero tax-returns).

Table A2 presents summary statistics by year for the main variable of interest –reported corporate income taxes, both in levels and logarithms. Note that when we measure the tax revenue in logarithms, what we are estimating is the effects of the reform on the growth rate in tax revenue. This may be helpful for comparison of different firms across the distribution as the revenue performance is normalized. There are also some practical advantages in using logarithm

[17] In July 2007 there was a hike in withholding rates for most economic sectors (from 1 to 2 percent), excluding the transport sector.
[18] Indeed, SRI records suggest that in the Northern Regional Office, where the majority of firms are located, the number of audits during 2007 (year of the reform) were actually lower than that of prior years.
[19] This form can be requested from the authors.
[20] We also excluded from our sample observations with negative revenues, where declared profit is greater than revenue and with other inconsistencies.
[21] Applying the same deflator to all regions in Ecuador is appropriate as most corporations are concentrated in the two major provinces –Guayas and Pichincha. Results are similar when using other price measures (i.e., the wholesale price index from World Development Indicators).

for the empirical analysis.[22] However, one can argue that as a measure of overall revenue effects of the 2007 reform, it might be too stringent a metric, because there can be significant increase in the average revenue (per firm) without a perceptible increase in the growth rate. Also, it is much easier to have higher growth when one starts with low tax revenue in the base year, and thus measuring performance in terms of growth rates can be misleading, especially if there are a lot of small firms with low tax obligations. One might find strong effects on the growth rate of revenue driven by the small firms, while the total tax revenue response might be rather muted. To understand the effects of 2007 reform, we thus look at both the changes in levels of and growth rates of corporate tax revenue. However, the conclusions do not vary significantly across log and level measures of tax revenue, and for the sake of brevity we focus the discussion on the results from log measure. Note that if we find an effect of the increased punishment on revenue using logarithmic measure of revenue, it is a sufficient condition for a positive revenue effect in levels.

From Table A2 a few interesting patterns emerge. First, notice that there is an underlying positive trend from period t to period (t+1) over the span of pre-reform period (2003-2006). Such a trend suggests that it is not unusual to observe an increase in the average corporate tax revenue in Ecuador. Second, we notice that mean reported taxes between 2003 and 2006 are increasing at a similar annual rate. Thus, mean reported taxes for each of those years do not seem unusual, suggesting that a large increase in corporate tax revenue between 2006 and 2007 cannot be explained by unusually low taxes in 2006. Indeed, it is not surprising that reported taxes between 2003 and 2006 do not deviate much from the trend due to the fact that no major tax policy reforms were introduced during this period[23].

Table A3 presents summary statistics for firms' observable characteristics. The choice of the observable characteristics is motivated by a close reading of the Ecuadorian tax law, and also the literature on corporate tax compliance and effective tax rates (ETRs)[24]. The variables include revenue and total assets as measures of the size of the firm and its performance. The effect of these variables on tax compliance of a firm is, however, not unambiguous (Rice, 1992; Hanlon, 2005; Spooner, 1986; Kim and Limpaphayom, 1998). Other explanatory variables include capital

[22] As shown in the summary statistics, the data have some outliers, and the variance is very high in the tax variable. Transforming the dependant variable to logarithms has the advantage of reducing data variability and making the tax distribution less skewed. Less variability in the data helps to improve the precision of the estimates; while a less skewed distribution allows presenting the results of the quantile analysis graphically. Moreover, given that quantiles are robust to monotonic transformations of the dependant variable, results should be comparable whether the dependant variable is in logs or levels.

[23] In particular, corporate income tax rates remained unchanged.

[24] As pointed out by Slemrod (2005), to the extent that low effective tax rates (ETRs), defined as total tax expense divided by pre-tax earnings, are a proxy for aggressive tax positions, variables that explain differences in ETRs may help explain other measures of tax non-compliance as well. See Rego (2003) for a summary of this literature.

intensity, leverage, and purchases. These variables should reduce tax payments due to the deductibility of capital investments, interest payments, and costs (Gupta and Newberry, 1997; Porcano, 1986; Stickney and McGee, 1982).

4. Effects of 2007 Reform: Intensive Margin

4.1. Empirical Strategy

In the introduction, we discussed a host of issues that arise in estimating the effects of higher punishment for tax evasion with panel data, and also underscored some important advantages of the 2007 reform in Ecuador. The fact that there were no changes in the corporate tax rate, and that the monitoring regime (audits) did not improve in 2007, makes it an excellent case study to understand the effects of higher punishment. As discussed in the introduction, the central issue in identifying the effects of higher punishment using panel data is how to eliminate the bias due to the counterfactual trend reliably. We use a difference-in-difference (DD) and difference-in-difference-indifference (DDD) estimators for this purpose. The richness of the panel data also allows us to check the validity of the DD and DDD estimates using a set of placebo DD and DDD estimates.

Difference in the Distribution of Corporate Income Taxes (D)

A large rightward shift in the distribution of corporate income taxes between 2006 and 2007 would provide some prima facie evidence suggesting that higher punishment improved tax compliance. However, as shown by the summary statistics, mean corporate income taxes are increasing over time, which suggests that the entire tax distribution (or most of it) may also be shifting to the right over time. Therefore, the difference in corporate income taxes between 2006 and 2007 would not provide an unbiased estimate of the effect of higher punishment; but instead, estimates would be biased upwards.

The upward trend in corporate income taxes may be driven by macroeconomic factors such as positive demand (local demand or export market demand) and productivity shocks. Firms may experience greater sales levels and higher profit margins due to higher demand or lower costs (positive productivity shocks). Moreover, in the specific context of Ecuador, slow but steady improvements in tax administration that were initiated in 1997 with the new independent tax administration may also be important in driving the trend.[25]

[25] The recent literature has highlighted the potential role of social and moral costs of non-compliance as an important element in understanding the observed behavior of tax payers. Many researchers have invoked these more cultural factors to reconcile the fact that the compliance level is too high given the monitoring intensity and severity

Difference-in-difference (DD)

The critical question for a credible estimate of the effects of higher punishment for tax evasion thus is how to reliably estimate the underlying trend in reported corporate income taxes. A widely used approach to eliminate the bias in the estimate from the single difference is to use a difference-in-difference (henceforth DD) that yields the appropriate estimate under the assumption that the underlying trends are linear and parallel. The DD estimator compares taxes reported in the most recent pre-reform and post-reform years (treatment), to another set of pre-reform years (control).[26] The change in reported taxes for the control group is a counterfactual measuring the tax increase that would have occurred in the absence of the reform.

DD estimates of the effect of higher punishment would be unbiased if the factors underlying the trend in corporate income taxes would change at a constant rate over time (in the absence of the reform). It is unlikely, however, that all factors underlying the trend would normally change at a constant rate. Thus, DD estimates of the impact of the reform may be improved upon by explicitly controlling for those firm characteristics for which data is available[27].

Difference –in-difference-in-difference (DDD)

The identifying assumption of the DD approach is that given the definition of treatment, nothing other than the 2007 Tax Reform affected log reported taxes between 2006 and 2007[28]. For our application, the DD identifying assumption requires that the year-on-year change in omitted variables is the same for any time period. Throughout the span of our dataset there have been no major changes in most tax policy parameters[29], making the identification assumption more plausible. However, there may be some other (unobservable) factors that may be affecting the year-on-year shifts in reported taxes differently in different periods. For instance, the Ecuadorian tax authority has been making continuous improvements in tax administration; yet it

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of punishment, and Allingham-Sandmo model seems to grossly underestimate the expected compliance. However, note that such cultural norms do not change in the span of a few years, and thus cannot be important in understanding the differences in revenue performance in 2007 relative to the immediate previous years. In an interesting recent paper Saez et al (forthcoming) show that the compliance can be explained in terms of third party enforcement without taking recourse to cultural factors.

[26] As noted before, the firms in the control group in our analysis are exactly the same firms in the treatment group, only observed in a different time period (close approximation to a parallel world!).

[27] Moreover, controlling for firm characteristics such as firm sales and firm purchases may also help to capture the effect of macro variables such as GDP and exports.

[28] The DD identifying assumption would be satisfied if the tax shift between 2006 and 2007 would have been parallel to the tax shift for the control group in the absence of the reform (i.e. the trend in reported taxes was linear in pre-reform years, and would have remained unchanged in the absence of the reform).

[29] Other than the 2007 Tax reform and changes in withholding rates.

is unlikely that such tax improvements are constant year-on-year. If there is learning by doing, the efficiency gains from such improvements may be convex.

In particular, one might worry that the tax shift betweens adjacent pairs of years may be larger in more recent years[30]. Such an upward trend (in tax shifts) would result in DD estimates for the effect of higher punishment that are biased upward, as we would (incorrectly) attribute to the higher punishment some of the tax increase that resulted from other factors, such as slow but steady efficiency gains in tax administration. We eliminate this bias by constructing a difference-in-difference-in-difference estimator (DDD).

4.2. Mean Analysis: Did Higher Punishment Increase Average Corporate Income Tax Payment?

Given that most of the extant tax literature has focused on mean effects of tax reform; prior to presenting the results for different *quantiles* of the tax distribution, it may be useful to discuss the OLS estimates of the impact of the 2007 Reform. In all models, the dependant variable is log corporate income taxes and explanatory variables are those described earlier. Table B1 presents mean estimates for the three models discussed above (D, DD, and DDD).

First, we present estimates for the mean difference (D) in log corporate income taxes between 2006 and 2007. Results are available for the two different samples: Panel A includes firms reporting positive taxes in consecutive years; while Panel B includes all firms filing taxes in consecutive years, regardless of the amount filed. For both samples, the *growth* rate of corporate income taxes during 2006-07 is statistically significant and large in magnitude (a 23% and 14% increase in corporate income taxes for Panel A and Panel B, respectively).

We also estimate the mean impact of increased punishment based on a difference-in-differences estimator (DD)[31] as obtained from the following specification:

$$\Delta Y_{i,t} = \alpha_0 + \delta^{DD} R_i + \Delta X_{i,t}\beta + u_{i,t} \qquad (1)$$

Where $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-1}$ are first-differences of the dependant variable, and $\Delta x_{i,t} = x_{i,t} - x_{i,t-1}$ are first-differences of the covariates (shown on Table A3); R_i equals one for the reform period 2006-07 and zero the control period 2005-06; and δ^{DD} is an average treatment effect on the treated where all treated firms are weighted equally. In essence, equation (1) consists of a pooled

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[31] As emphasized in Athey and Imbens (2006), the DD approach results in a parameter of the form:

$$\delta^{DD} = \{E[Y_i \mid X, t_{07}, \tau_1] - E[Y \mid X, t_{06}, \tau_1]\} - \{E[Y \mid X, t_{06}, \tau_0] - E[Y \mid X, t_{05}, \tau_0]\}$$

Where Y_i is the log of reported taxes, t is the time period, τ is the treatment status.

regression of stacked differenced observations.[32] DD estimates for Panel A suggest that the *growth* rate of corporate income for the treatment (2006-07) was 15 p.p. above the one for the control (2005-06)[33]. On the contrary, DD estimates for Panel B suggest that the *growth* rate of reported corporate income taxes was not any higher in 2006-07.[34]

Finally, we estimate the effect increased punishment using a difference-in-difference-in-difference (DDD) estimator based on the following model:

$$\Delta Y_{i,t} = \gamma_0 + \gamma_1 I(2006-07)_i - \gamma_2 I(2004-05)_i + \Delta X_{i,t}\beta + u_{i,t} \qquad (2)$$

Where $I(2006-07)_i$ and $I(2004-05)_i$ are indicators for the shift taking place in 2006-07 and 2004-05, respectively[35]. Here, the possible bias from differences in the pre-reform trends for the treatment and control groups is addressed by computing two DD estimators simultaneously. Specifically, $\hat{\gamma}_1$ would be an unbiased estimator of the effect of increased punishment if not for factors such as increased tax administration efficiency over time. Subtracting $\hat{\gamma}_2$ helps to control for the potential bias[36]. The estimate of most interest is therefore, $\hat{\delta}^{DDD} = \hat{\gamma}_1 - \hat{\gamma}_2$, a difference-indifference-in-difference estimator. DDD estimates for Panel A suggest that the mean *growth* rate of corporate income taxes may be lower than those obtained from previous models (a 10% growth rate after controlling for underlying trends)[37].

One way to test if the previous mean estimates of the effect of higher punishment are reliable is to run similar regressions, but using a pair of pre-reform years as a placebo treatment group. Table B2 presents results for the D, DD, and DDD models using a placebo treatment group. As expected due to the upward trend in mean corporate income taxes, D estimates are statistically significant for both samples (Panel and Panel B) even when using a placebo

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[32] In the model, control variables are first-differences of firms' characteristics.

[33] Given that the dependant variable is in logarithms, we are estimating a proxy for the difference in the growth rate of reported taxes between the treatment and control groups. However, some computational differences arise. Most importantly, the parameter estimated when using logs is:
$(2007-2006)/2006 - (2006-2005)/2005$, which differs from the parameter of interest:
$[(2007-2006) - (2006-2005)]/2006$. However, given that the tax distribution is higher over time, the use of logs would actually underestimate the impact of the 2007 Reform.

[34] However, the lack of an increase in average tax growth when firms reporting zero-taxes are included in the sample, does not necessarily imply that the 2007 Reform had no effect in the mean *level* of reported corporate income taxes.

[35] Conditional on X, the coefficient γ_1 measures the mean difference between the shift in 2006-07 and the 2005-06 shift (omitted category); while the coefficient γ_2 measures the mean difference between the shift in 2004-05 and the 2005-06 shift (omitted category) .

[36] We test for the presence of a linear trend in tax shifts (whether α_1 is significant) using the following specification:
$\Delta Y_{i,t} = \alpha_0 + \delta R_i + \Delta X_{i,t}\beta + \alpha_1 trend + u_{i,t}$

[37] The sample including firms claiming zero taxes may not present any indication of higher growth rates over time, as there is some year-on-year variability in the margin of filing a zero or non-zero tax return.

treatment group. However, results for the DD and DDD model suggest that our approach is valid. For Panel A, while the DDD effect of increased punishment was statistically significant for the actual treatment group (2006-07); it is not statistically significant for the placebo treatment group (2005-06)[38]. For Panel B, results suggest that the *mean* growth rate of corporate income taxes did not increase as a result of increased punishment (coefficients are not statistically significant neither for the actual treatment group nor for the placebo).

Mean estimates suggest that there was a large and positive mean effect of increased punishment on the growth rate of corporate income taxes of firms that file positive taxes in consecutive years; but there was no significant mean effect when the sample also includes firms that claim zero taxes in their tax returns. What explains such seemingly inconsistent results between sample A and sample B? Are mean estimates telling the whole story of the impact of increased punishment at the intensive margin? Or is there something that mean impacts miss?

Limitations of the mean analysis

Mean estimates are instructive; however, they have some important limitations for our application. First, results from Panel B (including firms filing zero tax returns) may be biased due to censoring (about 30% of the firms report cero taxes)[39]. Secondly, there is large variability in the data; and mean estimates are more adversely affected by outliers than median (or quantile) estimates. Finally, the mean estimates may hide important heterogeneity throughout the tax distribution. As a result, in the next section we use methods that impose little parametric assumptions, and allow us to obtain estimates of the impact of increased punishment at every point of the distribution, even when censored observations are frequent.

5. Quantile Analysis (Intensive Margin)

5.1. The Tax Distribution

In this section we go beyond the mean effect of increased punishment, by analyzing its effect for different quantiles of the tax distribution. However, before proceeding with a more formal analysis, we first present a descriptive analysis, which highlights the basic pattern of our investigation. Table B3 summarizes the tax distribution for 2006, the distribution for 2007, and

[38] For Panel A, DD estimates for the placebo are statistically significant. This is consistent with the finding that the trend in levels of corporate income taxes is convex rather than linear.

[39] While non-linear models could be used to take account for the censoring, strong functional form assumptions about the nature of the censoring problem are generally needed and results may depend upon these parametric assumptions.

the difference between the two (both in levels and in logarithms). If the increased punishment reduced tax evasion in Ecuador; we would expect to observe that the tax distribution in 2007 (year of the reform) shifted to the right relative to tax distribution in 2006 (pre-reform). As before, we focus on two samples: Panel A includes only firms declaring positive taxes; while Panel B includes all firms, even those declaring zero taxes (roughly 30% of firms claim zero taxes).

We observe a large percentage-increase in the *mean level* of corporate income taxes between 2006 and 2007 (31% and 33% for Panel A and Panel B, respectively). However, the mean increase in corporate income taxes hides the fact that there were different levels of tax increase for different quantiles of the tax distribution. For Panel A, the tax shift had an inverted-U shape. At the 10^{th} quantile taxes increased by 27%, compared to 37% for the 75^{th} quantile; and 28% for the 99^{th} quantile. Instead, for Panel B, taxes increased 31% for the 75^{th} quantile; and 27% the 99^{th} quantile.

Could the large rightward shift in the distribution of corporate income taxes between 2006 and 2007 be explained by firm characteristics? The bottom of Table B3 presents summary statistics and percentage changes for explanatory variables. Large changes in explanatory variables between 2006 and 2007 would provide some support for attributing the rightward shift in corporate income taxes to changes in firms' economic conditions. We observe that the percentage change of the explanatory variables between 2006 and 2007 is much smaller than the percentage change in corporate income taxes.

5.2. Difference in the Tax Distribution / DFL (D-DFL)

The shift in the distribution of corporate income taxes between 2006 and 2007, after controlling for firms' observable characteristics, is a (crude) measure of the impact of increased punishment on tax compliance. How much of the rightward shift in the tax distribution between 2006 and 2007 remains "unexplained" after netting out the effect of firms' economic characteristics? To answer this question, we rely on a straightforward decomposition method developed first by DiNardo, Fortin and Lemieux (1996) (DFL), and further extended by Lemieux (2002), and Leibbrandt, Levinshon and McCrary (2009)[40]. The methodology decomposes the

[40] The estimation of counterfactual distributions using semi-parametric methods has received a significant amount of attention in the recent literature. In particular, a method using quantile regressions and simulations by Machado and Mata (2005) has been widely applied to analyze counterfactual distributions of wages (Albrecht et al. 2003, Arulampalam et al. 2007), home prices (McMillen 2008), income (Nguyen et al. 2007), and homeownership rates (Carrillo and Yezer 2009), among others.

difference in log reported taxes between 2006 and 2007[41] into a part that is "explained" by firms' endowments and a part that remains "unexplained".

Figure B1 shows the cumulative density function of log reported taxes for 2006 and 2007. We would like to construct a counterfactual –how the distribution of log reported taxes in 2006 would look like if the individual firm characteristics (i.e. real-income, assets, financial ratios, etc.) were the same as in 2007. The counterfactual distribution is estimated by re-weighting the pre-reform sample to mimic the distributions of firms' attributes as they were after the reform[42]. In the figure, the counterfactual distribution is shown between the actual distributions for 2006 and 2007.

The counterfactual distribution decomposes the overall difference in log reported taxes between 2006 and 2007 into a part that is "explained" by firm characteristics, and a part that remains "unexplained". As emphasized in DFL (1995), the decomposition is summarized as follows:

$$\underbrace{Q_\theta[Y \mid t_{07}] - Q_\theta[Y \mid t_{06}]}_{\text{Overall}} = \underbrace{\{Q_\theta[Y \mid t_{07}] - Q_\theta^{6\to 7}[Y \mid t_{06}]\}}_{\textit{Unexplained}} - \underbrace{\{Q_\theta[Y \mid t_{06}] - Q_\theta^{6\to 7}[Y \mid t_{06}]\}}_{\textit{Explained}} \quad (3)$$

Where $Q_\theta[Y \mid t_i]$ is the θ^{th} quantile of the distribution of log reported taxes in year i; $Q_\theta^{i\to j}[Y \mid t_i]$ is the θ^{th} quantile of the distribution of log income taxes in year i, if firm attributes where identical to those in year j (counterfactual distribution). The first term to the right of equation (3) is the portion of the overall shift in the distribution of log corporate income taxes that remains "unexplained"; while the second term is the portion of the overall shift that can be explained by differences in endowments[43].

Figure B1 shows that the share of the overall tax shift that is explained by endowments is generally small, but its magnitude increases for higher quantiles. The share that is explained by firms' characteristics increases for higher quantiles because of two reasons: (1) the explained shift (numerator) is larger for higher quantiles, and (2) the overall shift (denominator) is smaller for higher quantiles. Consequently, we would expect that estimates of the difference (D) between the distributions of corporate income taxes in 2006 and 2007, after netting out the effect of observable firm characteristics, are large in magnitude. Indeed, these crude estimates of the impact of increased punishment should be large because most of the overall tax shift cannot be

[41] To keep our exposition self-contained, we provide a careful description of the DFL approach in the Technical Appendix.
[42] Technical details are presented in Appendix 1.
[43] The unexplained tax shift is represented graphically by the horizontal distance between the 2007 tax distribution and the counterfactual; while the explained shift is the distance between the 2006 tax distribution and counterfactual. Given that the counterfactual is very close to the 2006 distribution, it is apparent that much of the shift in tax collection can not be explained by observable firm characteristics.

explained by firms' characteristics. Moreover, given that the magnitude of the tax shift varies for different quantiles, we would also expect that the impact of increased punishment would be heterogeneous.

5.3. Difference-in-Differences combined with DFL (DD-DFL)

A single DFL decomposition of the tax shift between 2006 and 2007 as calculated earlier –although it controls for the effect of firms' observable characteristics– is not enough to identify an unbiased impact of the 2007 Tax Reform. As previously discussed, the upward trend in corporate income taxes (due to omitted variables) may lead to an upward bias. We eliminate this bias by constructing a difference-in-difference (DD) estimator that is based on the DFL methodology (called DD-DFL estimator).

The DD-DFL estimator compares taxes reported in the most recent pre-reform and post-reform years (treatment), to another set of pre-reform years (control). In our application, the time-difference in corporate income taxes for the treatment period is calculated as the "unexplained" shift from a DFL decomposition of log taxes between 2006 and 2007. Similarly, the time-difference in corporate income taxes for the control period is calculated as the "unexplained" shift from a DFL decomposition of log taxes between 2005 and 2006[44]. The change in reported taxes for the control group is an estimate of the tax increase that would have occurred in the absence of the reform. Thus, the DD parameter of interest has the following form:

$$\delta_{\theta}^{DD-DFL} = \underbrace{\{Q_{\theta}^{7}[Y \mid t_{07}] - Q_{\theta}^{6\to 7}[Y \mid t_{06}]\}}_{Treatment} - \underbrace{\{Q_{\theta}^{6}[Y \mid t_{06}] - Q_{\theta}^{5\to 6}[y \mid t_{05}]\}}_{Control} \qquad (4)$$

Where $\hat{\delta}_{\theta}^{DD-DFL}$ is the DD-DFL estimator for the impact of the reform at quantile θ; and other notation is the same as before. Figure B2 presents multiple DFL decompositions for different years.

5.4. Difference-in-Difference-in-Difference combined with DFL (DDD-DFL)

As previously mentioned, the tax shift betweens pairs of years appears to be larger in more recent years. This may occur, for instance, if the SRI has been gaining additional efficiency at tackling evasion each year. Such an upward trend would result in DD-DFL estimates for the effect of increased punishment that are biased upward, as we would attribute to increased

[44]An alternative would be to reweight all the distributions (other than that of 2007) to have the same observable characteristics as the distribution in 2007. Results for this approach are similar, and are available from the authors upon request.

punishment some tax increase that resulted from other factors, such as efficiency gains in tax administration. To account for such possibility, we also focus on the following parameter of interest:

$$\delta_{\theta}^{DDD-DFL} = \left[\{Q_{\theta}^{6}[Y \mid t_{07}] - Q_{\theta}^{6\to 7}[Y \mid t_{06}]\} - \{Q_{\theta}^{6}[Y \mid t_{06}] - Q_{\theta}^{5\to 6}[y \mid t_{05}]\}\right] - \left[\{Q_{\theta}^{6}[Y \mid t_{06}] - Q_{\theta}^{5\to 6}[Y \mid t_{05}]\} - \{Q_{\theta}^{5}[Y \mid t_{05}] - Q_{\theta}^{4\to 5}[y \mid t_{04}]\}\right] \quad (5)$$

Where $\hat{\delta}_{\theta}^{DDD-DFL}$ is a difference-in-difference-in difference estimator (DDD-DFL) of the impact of the reform for quantile θ.

5.5. Results

Table B4 and Figure B3 present quantile estimates of the impact of the 2007 Tax Reform using the three models discussed above (D-DFL, DD-DFL, and DDD-DFL). As before, results are available for two different samples: Panel A includes firms reporting positive taxes in consecutive years; while Panel B includes all firms filing taxes in consecutive years, regardless of the amount filed.

First, we present estimates for the difference (D-DFL) in log corporate income taxes between 2006 and 2007, after netting out the effect of firm' endowments. For both samples (Panel A and panel B), although the percentage change in corporate income taxes during 2006-07 was generally positive and statistically significant; its magnitude was higher at the middle of the distribution and slightly smaller at the tails. For Panel A, firms' real taxes increased by 26% at the 30th quantile, but only 18% at both the 10th and 90th quantiles. For Panel B, firms' real taxes increased by 29% at the 70th quantiles, but real taxes did not increase at the 30th quantile, and they increased by only 28% at the 90th quantile. For both of the samples, the large rightward shift observed at (almost) every quantile of the tax distribution, is consistent with the finding that the *mean* difference in log corporate income taxes was positive, large and significant.

We also estimate the mean impact of increased punishment based on a difference-in-differences estimator combined with DFL decompositions (DD-DFL) to control for firms' characteristics. DD-DFL estimates for Panel A suggest that the *growth* rate of corporate income for the treatment period (2006-07) was above the one for the control period (2005-06). Indeed, the tax growth rate in 2006-07 was roughly 16 p.p. above the growth rate in 2005-06 for most quantiles (17p.p. higher at the 30th quantile, but only 11p.p. higher at the 10th quantile and 10p.p. higher at the 80th quantile). The mean estimate of the effect of higher punishment was 15p.p. For the sample including all firms (Panel B), the reform lead to a decrease in the growth rate of corporate income taxes at lower quantiles (-37p.p. at the 30th quantile), but to an increase for

higher quantiles (12 p.p. at the 70th quantile, and 14 p.p. at the 90th quantile). These results suggest that mean impacts cannot tell the whole story for sample B. Indeed, mean DD results for panel B suggests that on average the growth rate of corporate income taxes was the same in 2006-07 and 2005-06. In reality, however, the growth in corporate income taxes was actually higher for some quantiles, but lower for others. This heterogeneity also explains why the mean estimate was lower for the sample including all firms as opposed to the sample including firms that declared positive taxes.

Finally, we estimate the effect of the tax Reform using a difference-in-difference-in-difference estimator combined with DFL. DDD-DFL estimates for Panel A suggest that the effect of increased punishment on the mean *growth* rate of corporate income taxes was large in magnitude for the middle of the distribution, but not for the tails. For Panel B, DDD-DFL estimates are large in magnitude, although they are not statistically significant.

One way to test if the previous mean estimates of the impact of the reform are reliable is to run similar regressions, but using a pair of pre-reform years as a placebo treatment group. Table B5 presents results for the D/DFL, DD-DFL, and DDD-DFL models using a placebo treatment group. As expected due to the upward trend in mean reported taxes, D/DFL estimates for the placebo are statistically significant and large in magnitude for both samples (Panel and Panel B). However, DD-DFL and DDD-DFL estimates for both samples are generally small and not statistically significant.

Quantile results suggest that increased punishment improved compliance for firms that were already paying a large amount of taxes (higher quantiles); but it decreased compliance for firms that paid little or no taxes (lower quantiles). Moreover, quantile results suggest, that mean impact miss part of the story.

6. Extensive Margin

The analysis thus far has focused exclusively on the intensive margin –firms that belong to the tax net in consecutive periods. However, given that substantive legislative changes took place in Ecuador since 2007, it may not be appropriate to assume that the choice to file a tax return has remained unchanged. Thus, in this section we examine the extensive margin by focusing on the probability of entering and exiting the tax net[45]. Where firm entry is defined as

[45]For the extensive margin analysis we measure the difference in the conditional probability of firm entry into the tax-net, and the conditional probability of exit out of the tax-net, between the treatment and control periods. We calculate D, DD, and DDD in the probability of entry and exit; where:

$1(File_t = 0 \quad \& \quad File_{t+1} = 1) = F(X_{t+1}\beta)$ and firm exit is defined as $1(File_t = 1 \quad \& \quad File_{t+1} = 0) = F(X_t\beta)$.

However, it is important to consider that filing a tax return does not necessarily mean that tax revenues will increase. Many of the firms that file a tax return claim zero-income or zero-taxes. Thus, it is possible that the new firms entering the tax net are filing a tax return but are not actually paying any taxes. Thus, we also define an alternative form of tax net, the net of firms that pay positive taxes. In this case, we define firm entry as $1((Tax_t = 0 \quad or \quad File_t = 0) \quad \& \quad Tax_{t+1} > 0) = F(X_{t+1}\beta)$, and firm exit as $1(Tax_t > 0 \quad \& \quad (File_{t+1} = 0 \quad or \quad Tax_{t+1} = 0)) = F(X_t\beta)$.

Table B6 presents results for the extensive margin. We find that the probability of entry into the tax net increased as a result of increased punishment; while the probability of exit decreased slightly between 2006 and 2007, as compared to the period (2005-06). Similarly, Table B7 presents results for changes in the probability of entering/exiting the net of firms that pay positive taxes. Interestingly, we notice that the probability of entering this alternative tax net actually decreased. Indeed, while a number of firms began to file taxes in 2007, most of them claimed zero taxes. Thus, at the extensive margin, it seems that higher punishments also led to a contraction of the tax net of firms that pay positive taxes by reducing the tax payments of smaller firms or by pushing smaller firms out into the shadow economy.

7. Impact on the Level of Tax Revenue

Throughout the main section of the paper, the dependant variable of interest was transformed to logarithms; and as a result, the coefficients of interest were interpreted as growth rates (or differences in growth rates). What can differences in growth rates tell us about the actual impact of increased punishment on the *level* of corporate income taxes? For Panel A, DD estimates show that the growth rate of corporate income taxes was higher in 2006-07 than in 2005-06 for every quantile of the distribution. Consequently, we expect that the *level* of

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$D = P(Entry_6 \mid Filing_7) - P(Entry_5 \mid Filing_6)$

$DD = [P(Entry_6 \mid Filing_7) - P(Entry_5 \mid Filing_6)] - [P(Entry_5 \mid Filing_6) - P(Entry_4 \mid Filing_5)]$

$DD = \{[P(Entry_6 \mid Filing_7) - P(Entry_5 \mid Filing_6)] - [P(Entry_5 \mid Filing_6) - P(Entry_4 \mid Filing_5)]\} -$
$\{[P(Entry_5 \mid Filing_6) - P(Entry_4 \mid Filing_5)] - [P(Entry_4 \mid Filing_5) - P(Entry_3 \mid Filing_4)]\}$

and where:

$D = P(Exit_7 \mid Filing_6) - P(Exit_6 \mid Filing_5)$

$DD = [P(Exit_7 \mid Filing_6) - P(Exit_6 \mid Filing_5)] - [P(Exit_6 \mid Filing_5) - P(Exit_5 \mid Filing_4)]$

$DD = \{[P(Exit_7 \mid Filing_6) - P(Exit_6 \mid Filing_5)] - [P(Exit_6 \mid Filing_5) - P(Exit_5 \mid Filing_4)]\} -$
$\{[P(Exit_6 \mid Filing_5) - P(Exit_5 \mid Filing_4)] - [P(Exit_5 \mid Filing_4) - P(Exit_4 \mid Filing_3)]\}$

corporate income taxes increased as well.

For Panel B, DD estimates show that the growth rate of corporate income taxes was lower in 2006-07 as compared to 2005-06 for the bottom quantiles of the tax distribution, but growth rates were higher for the top quantiles. Interestingly, the heterogeneity in the effect of higher punishment suggests that on average the *level* of corporate income taxes increased, even though there was no effect for the *log* of corporate income taxes. Results differ for *levels* and *logs* because a large decrease in growth rates at the bottom of the distribution translates into only a small decrease in tax levels. Indeed, estimates when the dependant variable is measured in levels show that results are equivalent to those predicted from log estimates (see Table C1 to Table C4).

8. Robustness Checks

8.1. Alternative Control Groups

In this section we define alternative control groups and alternative models in order to show that the quantile DD results obtained earlier are robust to reasonable changes in specification. The results reported are for the logarithm of revenue.

If the trend in reported taxes was roughly linear prior to the reform, any single pair of pre-reform years may be used as a reliable control group. However, if there was some period-to-period variation in log reported taxes, then the choice of the years to be used as control period matters. In the main section of the paper the control group was defined as the most recent pair of pre-reform years (2005-06), because this control period is conservative if tax shifts are increasing over time. Nevertheless, it would be reassuring to find that our results are robust to using alternative control periods, such as the following:

i. *The shift in 2004-05*. This control period would be preferable if the shift in 2005-06 turned out to be unusually small (which would bias results upwards).
ii. The *average* tax shift occurring between *every* pair of pre-reform consecutive years. This control period would be preferable if there were some ad-hoc period to period in tax shifts (as it exploits multiple periods).
iii. *The upper envelope* amongst the shifts between all the pairs of years. This control period gives the most conservative DD estimates[46].

[46]Estimates (for Panel A) are smaller when the control group is a more recent period, suggesting that the tax shift between pairs of years may be trending upward over time. Thus, DDD estimates are the most reliable.

Figure D1 shows DD estimates using the different control groups described above, as well as benchmark estimates using the control group presented in the main sections of this paper (2005-06). In general results are qualitatively similar regardless of the control group chosen. However, using the period 2005-06 as the control, results in estimates that are almost as conservative as the envelope.

8.2 Effects of Increase in Withholding Rates

The SRI has the power to change withholding rates without the need of Congress approval. In mid 2007 withholding rates have been increased for most sectors. Thus, it is possible that the large tax increase between 2006 and 2007 (even after netting out the effect of observable firm characteristics and the trend) is partly due to changes in withholding rates rather than due to increased punishment. However, given that withholding rates have not changed for all sectors, we can test whether sectors that have not been subject to hikes in withholding rates also experienced a larger increase in the growth rate of corporate income taxes between 2006 and 2007. In particular, withholding rates for the transport sector have remained constant, and corporations in the LTU are not subject to withholding.

Tables D1 and D2 present mean DD estimates of effect increased punishment for different sectors. We find that the mean DD effect for the transport sector, which was not subject to changes in withholding rates, is statistically significant and comparable in magnitude to the overall sample (the mean DD effect for the transport sector is 11% compared to 15% for the overall sample. We also calculate the difference in the mean DD effect between the transport sector and other sectors. We find that with the exception of agriculture, the difference between transport and other sectors is not statistically significant. Indeed, given that the magnitude of the estimates is similar for all sectors; there is some evidence that the increase in corporate income taxes is not driven by changes in withholding rates (see also Figure D2).

8.3 Estimates from Traditional QDID

The methodology presented in this paper (DD-DFL) has a number of advantages: (i) it allows defining the control group in a flexible manner; (ii) it is robust to censoring; and (iii) it does not impose a linear relationship between the covariates and the dependant variable for which there is no theoretical justification. Nevertheless, it is important to compare the results obtained using the DD-DFL methodology with those from a more traditional quantile DD specification. We estimate the impact increased punishment using Koenker and Bassett's (1978) quantile regression[47]:

$$(\hat{\alpha}_\theta, \hat{\beta}_\theta, \hat{\delta}_\theta^{QDID}) = \arg\min_{\alpha,\beta,\delta} \sum \rho_\theta \left(y_{i,t} - \alpha_1 Treat + \alpha_2 Post + \delta^{QDD} Treat \cdot Post + X_{i,t}\beta \right) \quad (8)$$

Where δ_θ^{QDID} is a treatment effect on the treated at a given quantile θ. The treatment period is 2006-07 and the control period is 2004-05. Figure D1 shows that DD results are qualitatively similar when using the traditional QDD model.

9. Conclusions

This paper analyzes the effects of a reform to the tax code in Ecuador during 2007 that introduced stricter punishment for tax evasion. At its core, the new legislation introduced reclusion from 1 to 6 years as a punishment for non-compliance, and made a firm's CFO liable for tax-crimes. We take advantage of a rich firm level administrative data set from Ecuador with actual tax-return and financial-statement data for the universe of corporations both before and after the reform (from 2003 to 2007).

We study the effects of stricter punishment on corporate income taxes both at the intensive and extensive margins. At the intensive margin, increased punishment seems to increase, on average, the growth rate of corporate income taxes of firms that file positive taxes in consecutive years. However, there are no significant mean effects when the sample also includes firms that claim zero taxes in their tax returns. This estimate masks the fact that, for this sample, growth rate of corporate income taxes diminished at lower quantiles but increased at the right tail of the tax distribution. These results suggest that focusing on mean impacts can mask important heterogeneity in the impact of tax reforms. Results for the extensive margin suggest that, while a number of firms began to file taxes after the reform, most of them claimed zero taxes.

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[47] Treat=1 for years 2006 and 2007; Post=1 for years 2005 and 2007; Post*Treat=1 for year 2007; the omitted category is year 2004. The model uses only data for 2004-05 and 2006-07. For the sample including firms filing zero tax returns, we also estimate a simple extension of the previous model, which is robust to censoring without imposing parametric assumptions (Buckhinsky; and Powell): $(\hat{\alpha}_\theta, \hat{\beta}_\theta, \hat{\delta}_\theta^{QDID}) = \arg\min_{\alpha,\beta,\delta} \sum \rho_\theta \min\left(y_c, y_{i,t} - \alpha_1 Treat + \alpha_2 Post + \delta^{QDID} Treat \cdot Post + X_{i,t}\beta \right)$

A. Descriptive Tables and Figures

Table A1: 2007 Reforms to the Tax Code

Articles		Pre-reform	Post-reform
(1)	Minor ordinary reclusion as a punishment for tax crimes	Not available (milder forms of prison available for firm's legal representative only)	1-6 years depending on the crime
(2)	Responsibility for tax crimes	Only Firm's legal representative	Also the CFO + anybody who controls the firm's economic activity
(3)	End to tax-related legal actions	Payment of tax debt, death, or prescription	Payment does not end a tax-related legal action
(4)	Interest rate for tax arrears	1.1 times the TAR	1.5 times the TAR
(5)	Surcharge for non-declared income discovered by the SRI through tax assessments	Pay only the additional taxes assessed plus interest	Pay a 20% surcharge in addition to pre-reform amount
(6)	Requirement to contest SRI tax assessments at court	No guarantee requirements	Payment of 10% of the amount demanded

Table A2: Dependant Variable, Summary Statistics

	PANEL A: Firms with Tax>0					
	2003	**2004**	**2005**	**2006**	**2007**	**All years**
Log(Real Tax)						
mean	6.36	6.59	6.80	7.05	7.44	6.83
SD	2.45	2.42	2.42	2.41	2.40	2.45
max	17.18	17.06	17.03	16.91	17.68	17.68
min	0	0	0	0	0	0
Real Tax ($ Ths.)						
mean	15.10	16.90	21.33	25.06	35.03	22.37
SD	282.76	269.10	310.24	327.58	522.33	350.46
max	29,015	25,589	24,997	22,042	47,506	47,506
min	0	0	0	0	0	0
Obs.	17,968	17,915	17,276	16,744	15,922	85,825
	PANEL B: Firms with Tax≥0					
	2003	**2004**	**2005**	**2006**	**2007**	**All years**
Log(Real Tax)						
mean	4.15	4.53	4.79	5.12	5.44	4.77
SD	3.62	3.65	3.71	3.75	3.88	3.74
max	17.18	17.06	17.03	16.91	17.68	17.68
min	0	0	0	0	0	0
Real Tax ($ Ths.)						
mean	9.86	11.62	15.04	18.20	25.59	15.62
SD	228.65	223.28	260.74	279.38	446.75	293.05
max	29,015	25,589	24,997	22,042	47,506	47,506
min	0	0	0	0	0	0
Obs.	27,505	26,054	24,493	23,056	21,791	122,899

Notes: The dependant variable is real taxes. When real tax is measured in logs, we add one unit in order to include firms that generated no taxes. The sample used in this table differs from that in Table B3 because here the sample is not balanced to include only firms that file taxes in consecutive years.

Table A3: Explanatory Variables, Summary Statistics

	PANEL A: Firms with Tax>0				PANEL B: Firms with Tax≥0			
	Mean	**SD**	**Max**	**Min**	**Mean**	**SD**	**Max**	**Min**
Real Assets ($ Ths.)	1,319	12,837	1,132,842	0	1,179	13,674	1,514,545	0
Real Income ($ Ths.)	1,906	13,909	957,818	0	1,498	11,919	957,818	0
Leverage	0.63	1.31	261	0	0.73	6.98	1,744	0
Fixed to total assets	0.24	0.29	1	0	0.26	0.30	1	0
1(Purchases>0)	0.51	0.50	n.a.	n.a.	0.46	0.50	n.a.	n.a.
Real Purchases ($ Ths.)	710	7,383	582,408	0	544	6,281	582,408	0

Notes: Explanatory variables have been selected following the literature on corporate tax compliance and ETRs. Real assets are a proxy for firm size. Real income is a proxy for firm profitability. Leverage is measured as the ratio of total debt (sum of current and noncurrent liabilities) to total assets. Fixed to total assets is measured as the ratio of net property, plant, and equipment to total assets. Purchases are defined as the dollar value of annual local purchases (most purchases are tax deductable). The sample used in this table differs from that in Table B3 because here the sample is not balanced to include only firms that file taxes in consecutive years.

B. Main Results Tables and Figures

Table B1: Intensive Margin, Mean Results (logs)

	Based on the year of the Reform	
	PANEL A: Firms with Tax>0	PANEL B: Firms with Tax≥0
DDD	**0.103*** **	**-0.036**
	(0.022)	(0.046)
Obs.	42,130	66,558
R-sq	0.187	0.083
DD	**0.150*** **	**0.007**
	(0.013)	(0.027)
Obs.	27,730	42,991
R-sq	0.186	0.087
D	**0.233*** **	**0.144*** **
	(0.015)	(0.030)
Obs.	27,198	41,632
R-sq	0.718	0.366

Notes: Robust SE in parenthesis. The dependant variable is log(Real tax+1). Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets, 1(Purchases), and log(Real Purchases). The treatment period is 2006-07; data from prior periods is used as control. The parameters of interest are defined as follows: $\delta^{D} = E[Y \mid X, t_{07}] - E[Y \mid X, t_{06}]$; $\delta^{DD} = \{E[Y \mid X, t_{07}] - E[Y \mid X, t_{06}]\} - \{E[Y \mid X, t_{06}] - E[Y \mid X, t_{05}]\}$; and $\delta^{DDD} = (\{E[Y \mid X, t_{07}] - E[Y \mid X, t_{06}]\} - \{E[Y \mid X, t_{06}] - E[Y \mid X, t_{05}]\}) - (\{E[Y \mid X, t_{06}] - E[Y \mid X, t_{05}]\} - \{E[Y \mid X, t_{05}] - E[Y \mid X, t_{04}]\})$

Table B2: Intensive margin, Placebo Mean Results (logs)

	Based on the Placebo	
	PANEL A: Firms with Tax>0	PANEL B: Firms with Tax≥0
DDD	**0.031**	**0.074**
	(0.023)	(0.045)
Obs.	42757	70445
R-sq	0.195	0.080
DD	**0.047*** **	**0.042**
	(0.013)	(0.026)
Obs.	28531	45742
R-sq	0.176	0.080
D	**0.090*** **	**0.141*** **
	(0.015)	(0.029)
Obs.	28262	44350
R-sq	0.712	0.349

Notes: Robust SE in parenthesis. The dependant variable is log(Real tax+1). Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets, 1(Purchases), and log(Real Purchases). The placebo treatment period is 2005-06; while data from prior periods is used as control. The parameters of interest are defined as follows: $\delta^{D} = E[Y \mid X, t_{06}] - E[Y \mid X, t_{05}]$; $\delta^{DD} = \{E[Y \mid X, t_{06}] - E[Y \mid X, t_{05}]\} - \{E[Y \mid X, t_{05}] - E[Y \mid X, t_{04}]\}$; and $\delta^{DDD} = (\{E[Y \mid X, t_{06}] - E[Y \mid X, t_{05}]\} - \{E[Y \mid X, t_{05}] - E[Y \mid X, t_{04}]\}) - (\{E[Y \mid X, t_{05}] - E[Y \mid X, t_{04}]\} - \{E[Y \mid X, t_{04}] - E[Y \mid X, t_{03}]\})$

Table B3: The 2006-07 Tax Shift

	PANEL A: Taxes>0				PANEL B: Taxes≥0			
	2006	2007	Change	% Change	2006	2007	Change	% Change
Real Tax [$ Thousands]								
mean	30	39	9	**31%**	20	27	7	**33%**
SD	(363)	(561)			(294)	(457)		
max	22,042	47,506			22,042	47,506		
1st quantile	0.004	0.004	0.001	**18%**	0	0	0	**0%**
10th quantile	0.078	0.099	0.021	**27%**	0	0	0	**0%**
25th quantile	0.389	0.524	0.135	**35%**	0	0	0	**0%**
50th quantile	1.776	2.444	0.668	**38%**	0.563	0.738	0.176	**31%**
75th quantile	6.893	9.452	2.560	**37%**	3.662	4.983	1.321	**36%**
90th quantile	25.144	34.262	9.118	**36%**	14.816	20.171	5.356	**36%**
99th quantile	411.252	525.509	114.257	**28%**	270.105	354.595	84.491	**31%**
Log(Real Tax)								
mean	7.37	7.67	**0.30**		5.40	5.54	**0.15**	
SD	(2.31)	(2.33)			(3.72)	(3.88)		
max	16.91	17.68			16.91	17.68		
1st quantile	1.52	1.65	**0.13**		0.00	0.00	**0.00**	
10th quantile	4.37	4.61	**0.23**		0.00	0.00	**0.00**	
35th quantile	5.97	6.26	**0.30**		0.00	0.00	**0.00**	
50th quantile	7.48	7.80	**0.32**		6.33	6.61	**0.27**	
75th quantile	8.84	9.15	**0.32**		8.21	8.51	**0.31**	
90th quantile	10.13	10.44	**0.31**		9.60	9.91	**0.31**	
99th quantile	12.93	13.17	**0.25**		12.51	12.78	**0.27**	

	Mean Values and S.D.				Mean Values and S.D.			
	2006	2007	Change	% Change	2006	2007	Change	% Change
Explanatory Vars.								
Real Assets [$ Ths.]	1,707	1,833	126	**7%**	1,439	1,517	78	**5%**
	(13,330)	(14,256)			(14,462)	(14,639)		
Real Income [$ Ths.]	2,543	2,781	238	**9%**	1,901	2,061	160	**8%**
	(16,647)	(18,355)			(13,840)	(15,216)		
Leverage	0.65	0.61	-0.042	**-6%**	0.67	0.68	0.009	**1%**
	(1.15)	(0.36)			(1.26)	(1.47)		
fixed to total assets	0.25	0.26	0.003	**1%**	0.28	0.28	0.003	**1%**
	(0.28)	(0.28)			(0.30)	(0.30)		
1(Purchases>0)	0.56	0.56	-0.003	**-1%**	0.50	0.49	-0.005	**-1%**
	(0.50)	(0.50)			(0.50)	(0.50)		
Purchases [$ Ths.]	942	1,032	90	**10%**	691	752	62	**9%**
	(8,990)	(9,449)			(7,408)	(7,773)		

Note: Standard deviations in parenthesis.

Table B4: Intensive Margin, Quantile Results (logs)

	Based on the year of the Reform								
	PANEL A: Firms with Tax>0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**0.057**	**0.088**	**0.090**	**0.096**	**0.083**	**0.106**	**0.065**	**0.083**	**0.210***
	(0.148)	(0.115)	(0.078)	(0.080)	(0.069)	(0.071)	(0.096)	(0.098)	(0.125)
DD	**0.111***	**0.151****	**0.169*****	**0.158*****	**0.125*****	**0.135*****	**0.117*****	**0.100***	**0.133***
	(0.059)	(0.063)	(0.046)	(0.041)	(0.036)	(0.035)	(0.044)	(0.059)	(0.070)
D	**0.188*****	**0.244*****	**0.262*****	**0.257*****	**0.243*****	**0.243*****	**0.237*****	**0.197*****	**0.180*****
	(0.035)	(0.021)	(0.023)	(0.024)	(0.025)	(0.027)	(0.027)	(0.027)	(0.036)
	PANEL B: Firms with Tax≥0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**NA**	**NA**	**-0.148**	**-0.123**	**0.010**	**0.012**	**0.079**	**0.049**	**0.088**
			(0.692)	-0.123	(0.115)	(0.096)	(0.075)	(0.087)	(0.111)
DD	**NA**	**NA**	**-0.365**	**-0.049**	**0.088**	**0.091**	**0.118*****	**0.107****	**0.138****
			(0.381)	(0.106)	(0.062)	(0.056)	(0.046)	(0.050)	(0.066)
D	**NA**	**NA**	**-0.057**	**0.190*****	**0.267*****	**0.262*****	**0.283*****	**0.280*****	**0.282*****
			(0.151)	(0.058)	(0.037)	(0.029)	(0.023)	(0.032)	(0.035)

Notes: Bootstrapped SE in parenthesis. Dependant variable is log(Real tax+1). Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets, 1(Purchases), and log(Real Purchases). The treatment period is 2006-07; while data from prior periods is used as control. The parameters of interest are defined as follows: $\delta_\theta^D = Q_\theta^7[Y \mid t_{07}] - Q_\theta^{6\to 7}[Y \mid t_{06}]$; $\delta_\theta^{DD} = \{Q_\theta^7[Y \mid t_{07}] - Q_\theta^{6\to 7}[Y \mid t_{06}]\} - \{Q_\theta^6[Y \mid t_{06}] - Q_\theta^{5\to 6}[Y \mid t_{05}]\}$; and $\delta_\theta^{DDD} = \left(\{Q_\theta^7[Y \mid t_{07}] - Q_\theta^{6\to 7}[Y \mid t_{06}]\} - \{Q_\theta^6[Y \mid t_{06}] - Q_\theta^{5\to 6}[Y \mid t_{05}]\}\right) - \left(\{Q_\theta^6[Y \mid t_{06}] - Q_\theta^{5\to 6}[Y \mid t_{05}]\} - \{Q_\theta^5[Y \mid t_{05}] - Q_\theta^{4\to 5}[Y \mid t_{04}]\}\right)$, for each quantile θ.

Table B5: Intensive Margin, Placebo Quantile Results (logs)

	Based on the Placebo								
	PANEL A: Firms with Tax>0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**0.001**	**0.105**	**0.136**	**0.077**	**0.001**	**-0.023**	**0.002**	**-0.064**	**-0.251****
	(0.122)	(0.126)	(0.108)	(0.085)	(0.081)	(0.078)	(0.077)	(0.097)	(0.114)
DD	**0.054**	**0.063**	**0.079***	**0.062**	**0.042**	**0.029**	**0.051**	**0.017**	**-0.077**
	(0.083)	(0.072)	(0.046)	(0.052)	(0.049)	(0.037)	(0.042)	(0.047)	(0.071)
D	**0.077****	**0.093*****	**0.093*****	**0.099*****	**0.118*****	**0.108*****	**0.121*****	**0.097*****	**0.047**
	(0.036)	(0.032)	(0.025)	(0.023)	(0.026)	(0.016)	(0.024)	(0.027)	(0.037)
	PANEL B: Firms with Tax≥0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**NA**	**NA**	**-0.743**	**0.270**	**0.250****	**0.144**	**0.013**	**0.015**	**0.000**
			(0.955)	0.270	(0.126)	(0.092)	(0.084)	(0.095)	(0.107)
DD	**NA**	**NA**	**-0.217**	**0.074**	**0.078**	**0.078**	**0.040**	**0.058**	**0.050**
			(0.443)	(0.103)	(0.055)	(0.053)	(0.046)	(0.046)	(0.050)
D	**NA**	**NA**	**0.308***	**0.239*****	**0.180*****	**0.171*****	**0.165*****	**0.173*****	**0.144*****
			(0.169)	(0.074)	(0.040)	(0.032)	(0.025)	(0.029)	(0.031)

Notes: Bootstrapped SE in parenthesis. Dependant variable is log(Real tax+1). Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets, 1(Purchases), and log(Real Purchases). The placebo treatment period is 2005-06; while data from prior periods is used as control. The parameters of interest are defined as follows: $\delta_\theta^D = Q_\theta^6[Y \mid t_{06}] - Q_\theta^{5\to 6}[Y \mid t_{05}]$; $\delta_\theta^{DD} = \{Q_\theta^6[Y \mid t_{06}] - Q_\theta^{5\to 6}[Y \mid t_{05}]\} - \{Q_\theta^5[Y \mid t_{05}] - Q_\theta^{4\to 5}[Y \mid t_{04}]\}$; and $\delta_\theta^{DDD} = \left(\{Q_\theta^6[Y \mid t_{06}] - Q_\theta^{5\to 6}[Y \mid t_{05}]\} - \{Q_\theta^5[Y \mid t_{05}] - Q_\theta^{4\to 5}[Y \mid t_{04}]\}\right) - \left(\{Q_\theta^5[Y \mid t_{05}] - Q_\theta^{4\to 5}[Y \mid t_{04}]\} - \{Q_\theta^4[Y \mid t_{04}] - Q_\theta^{3\to 4}[Y \mid t_{03}]\}\right)$, for each quantile θ.

Table B6: Extensive Margin (Actual Tax Net)

	Based on the year of the Reform (Actual Tax Net)	
	Probability of Entry	Probability of Exit
DDD	**0.040*** **	**-0.005**
	(0.007)	(0.005)
obs.	179,717	169,906
R-sq	0.205	0.102
DD	**0.044*** **	**-0.0078** **
	(0.004)	(0.003)
obs.	137,944	129,976
R-sq	0.097	0.031
D	**0.051*** **	**0.00001**
	(0.002)	(0.002)
Obs.	94,617	88,203
R-sq	0.104	0.032

Notes: Robust SE in parenthesis. Linear probability model. Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets, 1(Purchases), and log(Real Purchases). For firm entry analysis, the dependant variable is $1(File_t = 0 \quad \& \quad File_{t+1} = 1)$. For firm exit analysis, the dependant variable is $1(File_t = 1 \quad \& \quad File_{t+1} = 0)$.

Table B7: Extensive Margin (Net of Taxpayers where Tax>0)

	Based on the year of the Reform (Net of Taxpayers where Tax>0)	
	Probability of Entry	Probability of Exit
DDD	**-0.03** **	**0.03** **
	(0.013)	(0.013)
obs.	85,131	80,003
R-sq	0.293	0.244
DD	**-0.004**	**0.021*** **
	(0.007)	(0.007)
obs.	65,683	62,035
R-sq	0.057	0.053
D	**0.003**	**0.018*** **
	(0.004)	(0.004)
Obs.	45,116	42,587
R-sq	0.066	0.060

Notes: Robust SE in parenthesis. Linear probability model. Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets, 1(Purchases), and log(Real Purchases).

Figure B1: DFL 2006-07

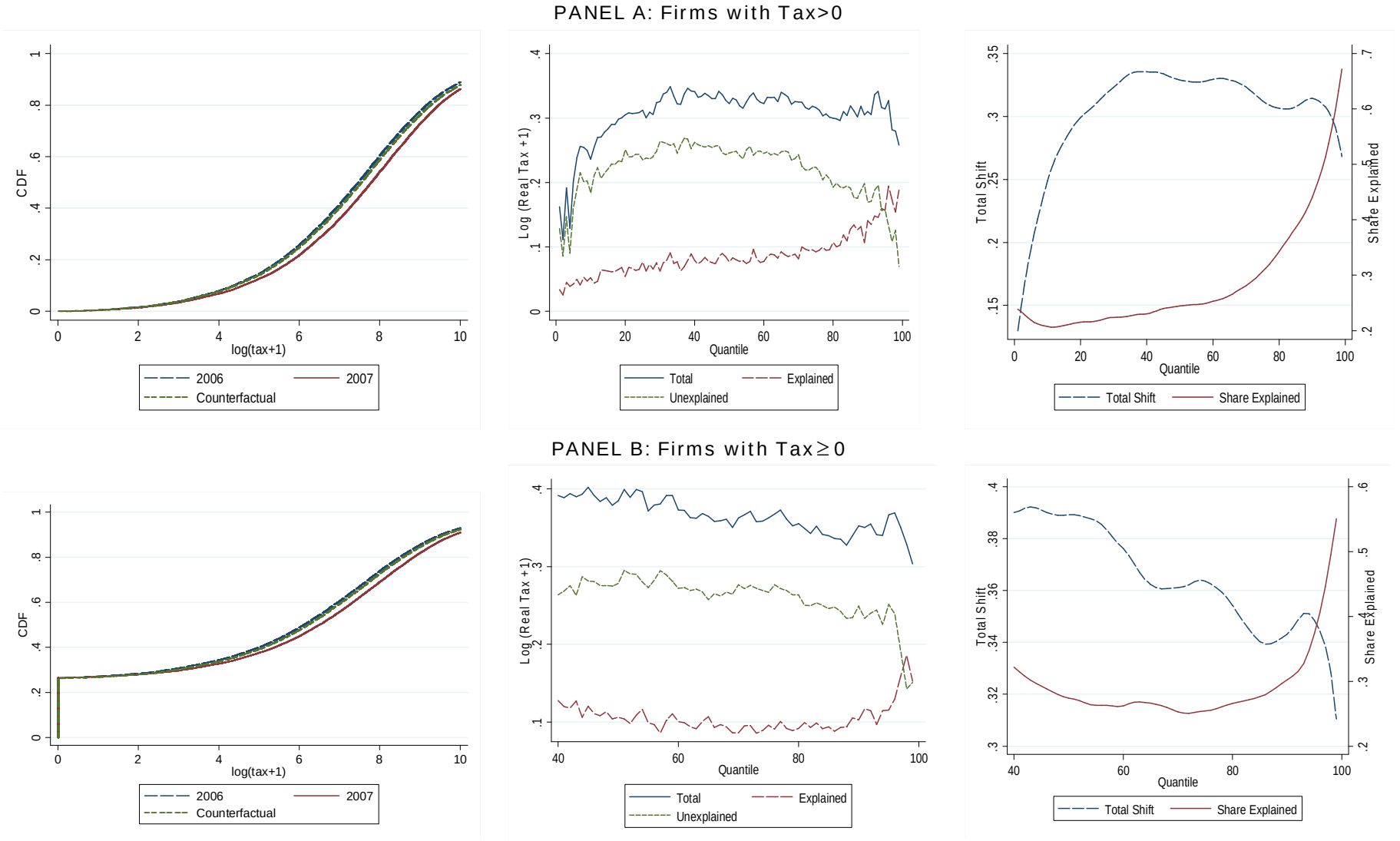


Figure B2: DFL Multiple Years

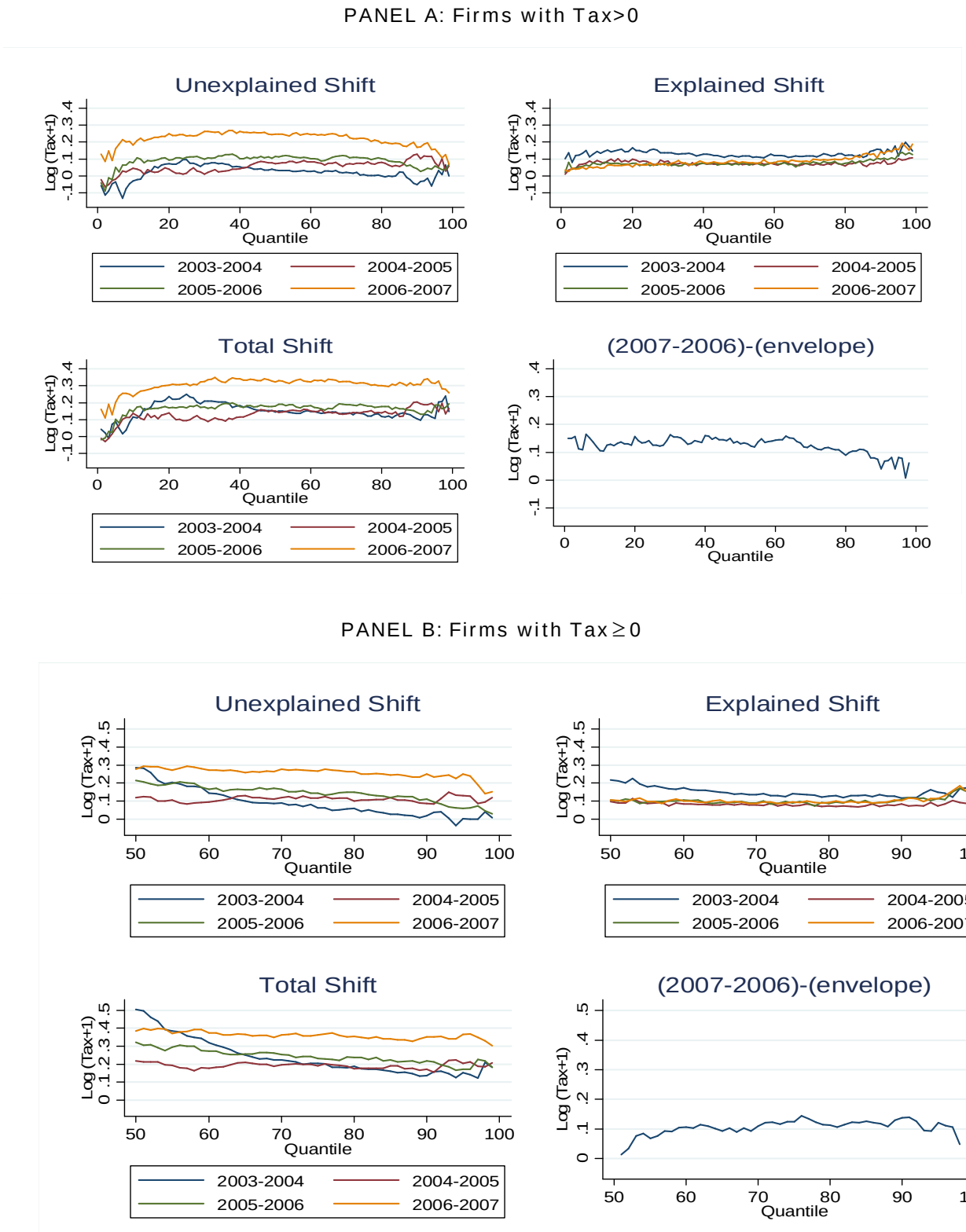
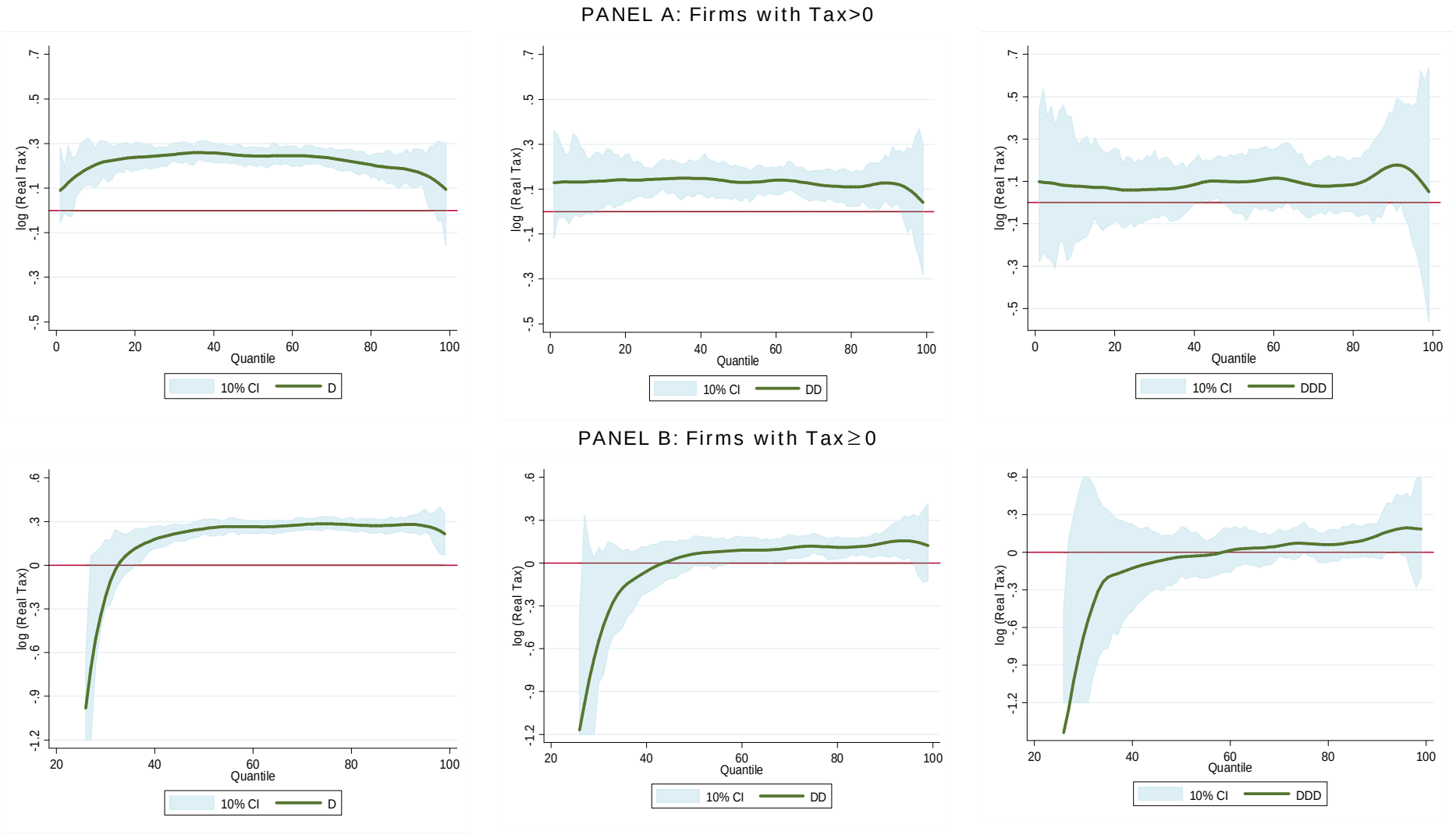


Figure B3: Intensive Margin, Quantile Results (logs)



C. Impact on the Level of Tax Revenue

Table C1: Intensive Margin, Mean Results in Levels [$ Thousands]

	Based on the year of the Reform	
	PANEL A: Firms with Tax>0	PANEL B: Firms with Tax≥0
DDD	**2.172*** **	**1.642*** **
	(0.545)	0.529
Obs.	41,783	66,200
R-sq	0.011	0.005
DD	**2.159*** **	**1.547*** **
	(0.324)	(0.294)
Obs.	27,475	42,731
R-sq	0.012	0.005
D	**2.118*** **	**1.809*** **
	(0.449)	(0.314)
Obs.	26,942	41,372
R-sq	0.253	0.193

Notes: Robust SE in parenthesis. Excludes corporations with Real Taxes above 500,000. Dependant variable is Real Tax ($ Thousands).

Table C2: Intensive Margin, Placebo Mean Results in Levels [$ Thousands]

	Based on Placebo	
	PANEL A: Firms with Tax>0	PANEL B: Firms with Tax≥0
DDD	**-1.122**	**-0.778**
	0.819	0.540
Obs.	42,477	70,157
R-sq	0.005	0.003
DD	**-0.029**	**-0.101**
	(0.301)	(0.277)
Obs.	28,326	45,529
R-sq	0.007	0.003
D	**0.477**	**0.715*** **
	(0.398)	(0.270)
Obs.	28,055	44,138
R-sq	0.226	0.171

Notes: Robust SE clustered by firm in parenthesis. Excludes corporations with Real Taxes above 500,000. Dependant variable is Real Tax ($ Thousands).

Table C3: Intensive Margin, Quantile Results in Levels [$ Thousands]

	Based on the year of the Reform								
	PANEL A: Firms with Tax>0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**0.010**	**0.042**	**0.091*** **	**0.179*** **	**0.261** **	**0.497*** **	**0.639** **	**0.966***	**5.142***
	(0.009)	(0.027)	(0.033)	(0.057)	(0.108)	(0.187)	(0.302)	(0.534)	(2.898)
DD	**0.013** **	**0.056*** **	**0.130*** **	**0.233*** **	**0.344*** **	**0.600*** **	**0.924*** **	**1.343*** **	**4.153*** **
	(0.006)	(0.014)	(0.026)	(0.053)	(0.080)	(0.134)	(0.253)	(0.400)	(1.457)
D	**0.017*** **	**0.076*** **	**0.173*** **	**0.316*** **	**0.517*** **	**0.874*** **	**1.443*** **	**2.246*** **	**5.398*** **
	(0.004)	(0.011)	(0.019)	(0.028)	(0.055)	(0.085)	(0.189)	(0.382)	(1.201)
	PANEL B: Firms with Tax≥0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**NA**	**NA**	**-0.008**	**-0.008**	**0.050**	**0.106**	**0.343** **	**0.521** **	**1.970** **
			(0.006)	-0.008	(0.037)	(0.082)	(0.142)	(0.252)	(0.921)
DD	**NA**	**NA**	**-0.006**	**0.010**	**0.095*** **	**0.207*** **	**0.497*** **	**0.935*** **	**2.713*** **
			(0.006)	(0.016)	(0.033)	(0.068)	(0.126)	(0.240)	(0.685)
D	**NA**	**NA**	**-0.002**	**0.041*** **	**0.169*** **	**0.370*** **	**0.843*** **	**1.716*** **	**4.438*** **
			(0.003)	(0.010)	(0.024)	(0.042)	(0.081)	(0.174)	(0.652)

Notes: Bootstrapped SE in parenthesis. Excludes corporations with Real Taxes above 500,000. Dependant variable is Real Tax ($ Thousands).

Table C4: Intensive Margin, Placebo Quantile Results in Levels [$ Thousands]

	Based on the Placebo								
	PANEL A: Firms with Tax>0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**0.001**	**0.017**	**0.054**	**0.056**	**0.032**	**-0.033**	**0.126**	**-0.139**	**-4.143***
	(0.007)	(0.022)	(0.038)	(0.060)	(0.105)	(0.172)	(0.297)	(0.533)	(2.301)
DD	**0.003**	**0.014**	**0.039***	**0.055**	**0.084**	**0.102**	**0.286****	**0.377**	**-0.988**
	(0.004)	(0.010)	(0.022)	(0.036)	(0.058)	(0.092)	(0.132)	(0.238)	(1.122)
D	**0.004***	**0.019****	**0.043*****	**0.083*****	**0.173*****	**0.274*****	**0.518*****	**0.903*****	**1.244***
	(0.003)	(0.009)	(0.015)	(0.018)	(0.031)	(0.067)	(0.117)	(0.224)	(0.705)
	PANEL B: Firms with Tax≥0								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
DDD	**NA**	**NA**	**0.001**	**0.019**	**0.069****	**0.120***	**0.083**	**0.248**	**0.111**
			(0.003)	0.019	(0.029)	(0.067)	(0.123)	(0.290)	(1.072)
DD	**NA**	**NA**	**0.002**	**0.018***	**0.045****	**0.101****	**0.154***	**0.415****	**0.744**
			(0.002)	(0.010)	(0.021)	(0.041)	(0.084)	(0.162)	(0.619)
D	**NA**	**NA**	**0.004*****	**0.031*****	**0.074*****	**0.164*****	**0.346*****	**0.781*****	**1.725*****
			(0.001)	(0.005)	(0.014)	(0.025)	(0.043)	(0.105)	(0.470)

Notes: Bootstrapped SE in parenthesis. Excludes corporations with Real Taxes above 500,000. Dependant variable is Real Tax ($ Thousands).

D. Robustness Checks

Table D1: Intensive Margin, Mean DD by Sector (logs)

	PANEL A: Firms with Tax>0								
	Transport	Agriculture	Fishing	Manufacturing	Construction	Commerce	Hotels & Restaurants	Real State	Other Services
DD	**0.108*** **	**0.340*** **	**0.177***	**0.131*** **	**0.218*** **	**0.148*** **	**0.049**	**0.133*** **	**0.249*** **
	(0.040)	(0.066)	(0.098)	(0.032)	(0.059)	(0.021)	(0.085)	(0.026)	(0.087)
Comparison with Transport	**N.A.**	**0.232*** **	**0.069**	**0.023**	**0.110**	**0.040**	**-0.059**	**0.025**	**0.141**
		(0.077)	(0.106)	(0.051)	(0.071)	(0.045)	(0.093)	(0.047)	(0.096)

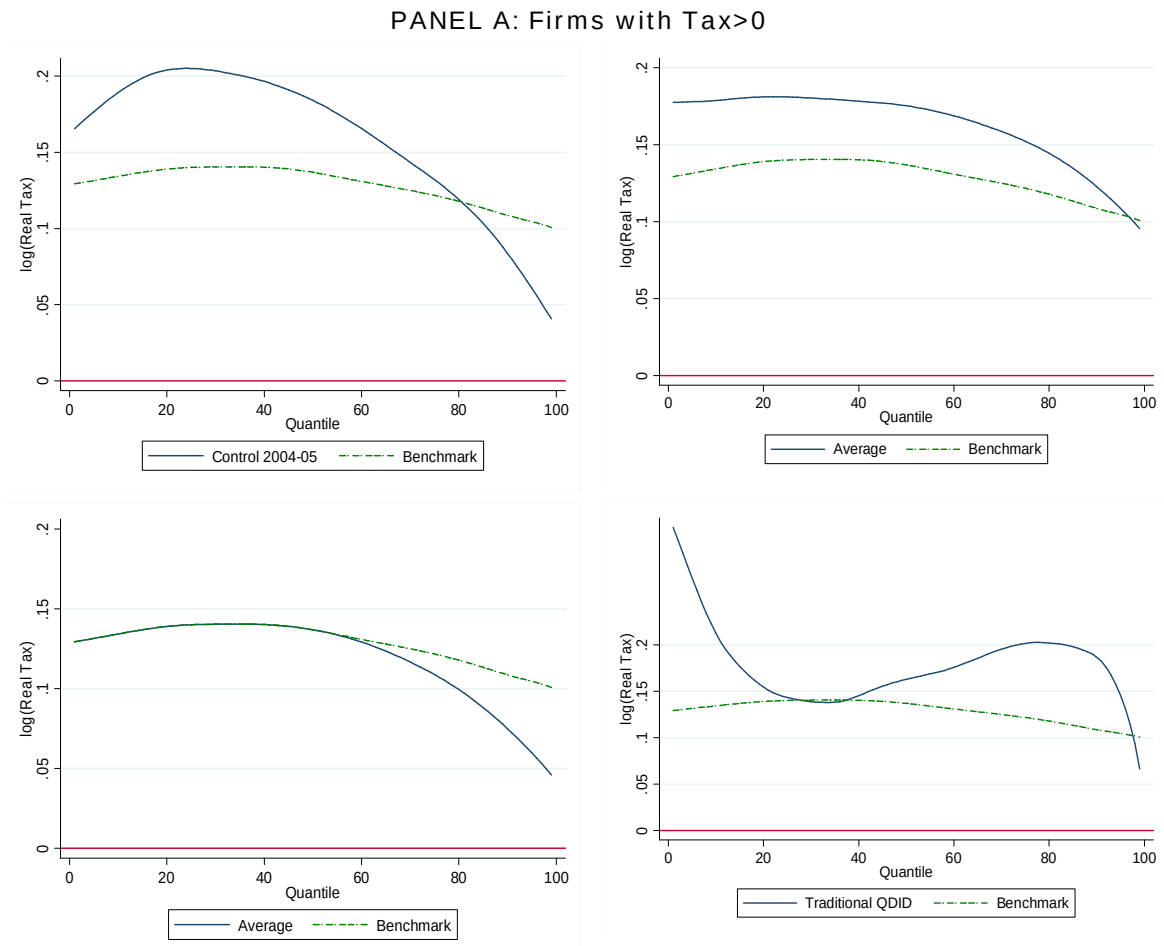
Notes: Robust SE in parenthesis. The dependant variable is log(Real tax+1). Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets capital-intensity, 1(Purchases), and log(Real Purchases). The treatment period is 2006-07; while data from prior periods is used as control.

Table D2: Intensive Margin, Mean DD by Sector (levels)

	PANEL A: Firms with Tax>0								
	Transport	Agriculture	Fishing	Manufacturing	Construction	Commerce	Hotels & Restaurants	Real State	Other Services
DD	**0.907***	**1.121**	**5.453*** **	**3.879*** **	**2.942*** **	**2.297*** **	**0.463**	**1.619*** **	**0.064**
	(0.537)	(0.822)	(1.681)	(1.394)	(1.065)	(0.589)	(1.274)	(0.498)	(1.417)
Comparison with Transport	**N.A.**	**0.214**	**4.546** **	**2.972** **	**2.035***	**1.390***	**-0.444**	**0.713**	**-0.843**
		(0.982)	(1.765)	(1.494)	(1.190)	(0.795)	(1.383)	(0.730)	(1.517)

Notes: Dependant variable is the first difference of Real Tax ($ Thousands). Excluding firms with Real Tax>500,000.

Figure D1: Alternative Control Groups and Alternative Models



Notes: The dependant variable is log(Real tax+1). Explanatory variables include: log(Real Income), log(real Assets), leverage, fixed to total assets, 1(Purchases), and log(Real Purchases).

Figure D2: DD by Sector (logs)

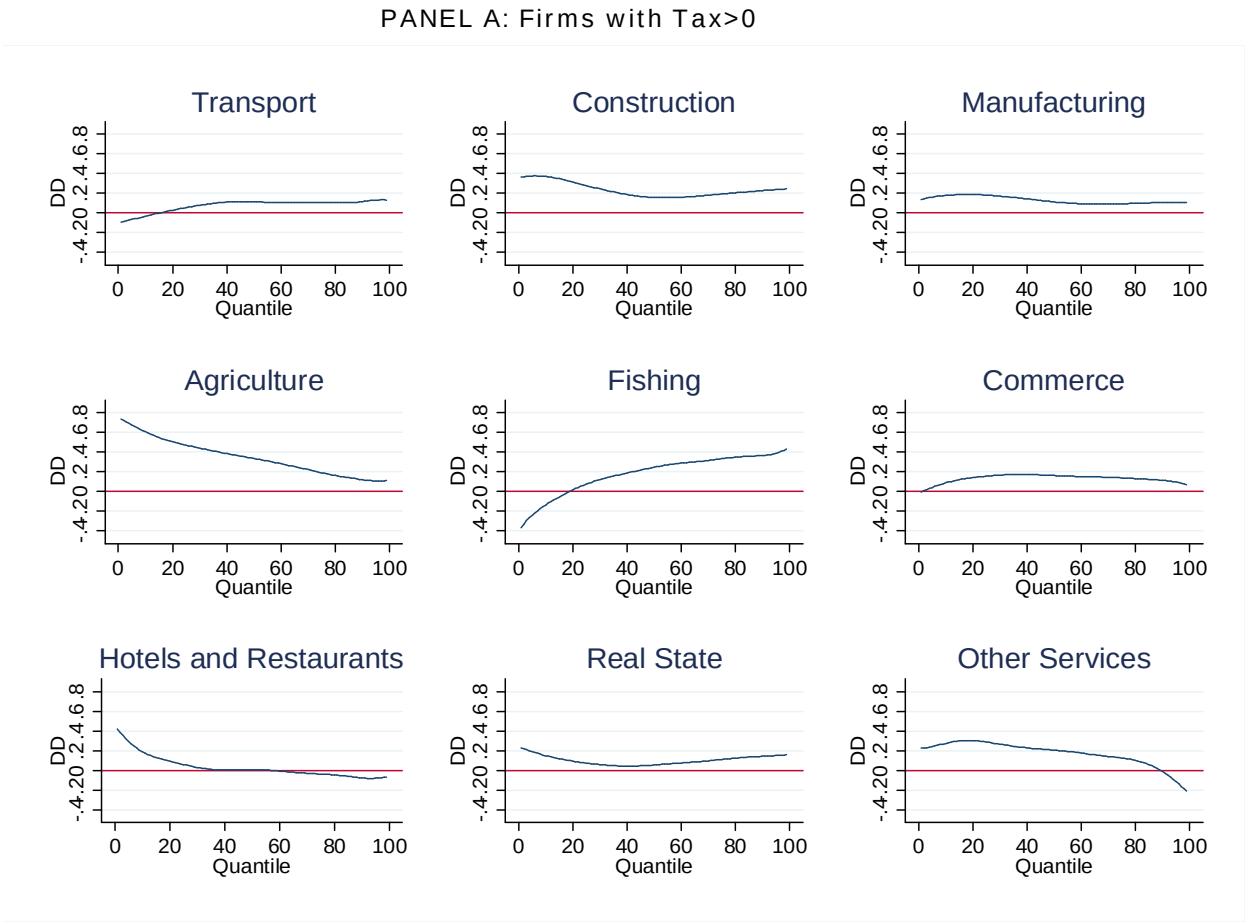
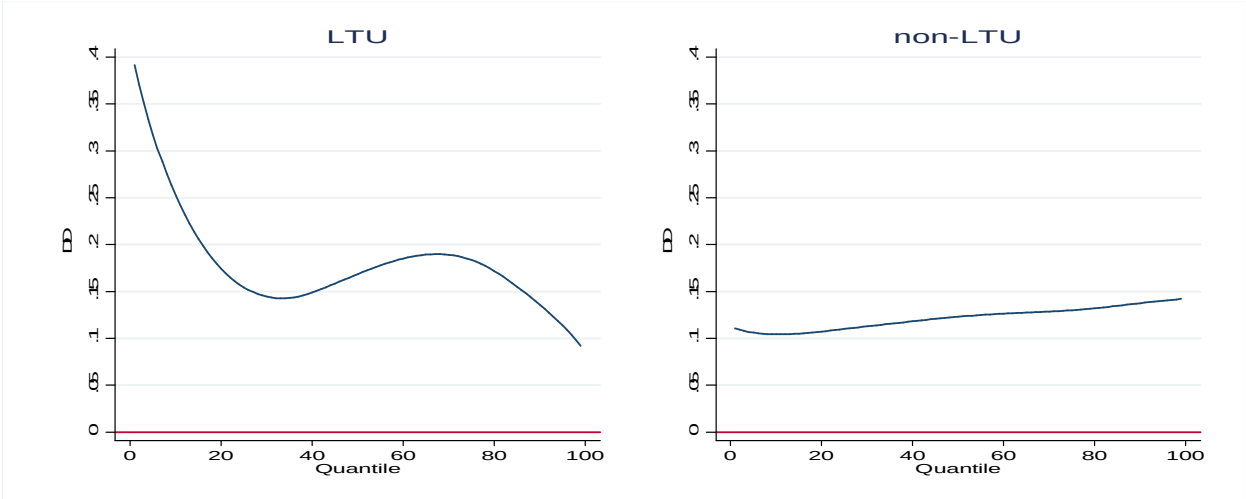


Figure D3: DD by Tax Scheme (logs)



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Appendix 1: DFL Methodology

To keep our exposition self-contained, we provide a careful description of the DFL approach using the same notation as in Leibbrandt, Levinshon and McCrary (2009). Let Y be our variable of interest (log taxes) and t_0 and t_1 refer to the two mutually exclusive periods we analyze. The cumulative probability function Y in period t_0 is defined as

$$F(y \mid T = t_0) = \Pr\{Y \leq y \mid T = t_0\} = \int F(y \mid x, T = t_0) h(x \mid T = t_0) dx \qquad (1)$$

Where T is a random variable describing the period from which an observation is drawn and x is a particular draw of observed attributes of individual characteristics from a random vector X. $F(y \mid x, T = t_0)$ is the (conditional) cumulative distribution of Y given that a particular set of attributes x have been picked, and $h(x \mid T = t_0)$ is the probability density of individual attributes evaluated at x. The cumulative probability function of Y in period t_1 is defined similarly.

Suppose we like to assess how the distribution of Y (taxes) in period t_0 (2006) would look like if the individual attributes x (real-income and assets, for example) were the same as in period t_1 (2007). We denote this counterfactual as $F_{t_0 \to t_1}$ and express it symbolically as[48]

$$F_{t_0 \to t_1} = \int F(y \mid x, T = t_0) h(x \mid T = t_1) dx \qquad (2)$$

In order to compute such counterfactual, Bayes' rule is used to obtain $h(x \mid T = t) = P(X = x) P(T = t \mid X = x) / P(T = t)$, and DFL recognized

$$\frac{h(x \mid T = t_1)}{h(x \mid T = t_0)} = \frac{\dfrac{P(T = t_1 \mid X = x)}{P(T = t_1)}}{\dfrac{P(T = t_0 \mid X = x)}{P(T = t_0)}} = \frac{\dfrac{P(T = t_1 \mid X = x)}{1 - P(T = t_1 \mid X = x)}}{\dfrac{P(T = t_1)}{1 - P(T = t_1)}} = \tau_{t_0 \to t_1}(x) \qquad (3)$$

One may use expression (3) to substitute $h(x \mid T = t_1)$ in equation (1) and obtain that

$$F_{t_0 \to t_1}(y) = \int F(y \mid x, T = t_0) h(x \mid T = t_0) \tau_{t_0 \to t_1}(x) dx \qquad (4)$$

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[48] The subscript "$t_0 \to t_1$" indicates that the attributes data from period t_0 will be "replaced" by data from period t_1.

Notice that this expression differs from equation (1) only by $\tau_{t_0 \to t_1}(x)$. DFL refer to $\tau_{t_0 \to t_1}(x)$ as "weights" that should be applied when computing the counterfactual distribution of our variable of interest. However, given that the weights are unknown, they need to be estimated.

To be specific, as in Leibbrandt, Levinshon and McCrary (2009), we summarize the estimation algorithm for the counterfactual given that a random sample of N_0 and N_1 observations for periods t_0 and t_1 is available:

1) Estimate $P(T = t_1)$ using the share of observations where $T_i = t_1$ to obtain $\hat{P}(T_i = t_1) = N_1 / (N_0 + N_1)$.

2) Estimate $P(T = t_1 \mid X = x)$, by estimating a logit model for both periods. The dependent variable equals one if $T_i = t_1$, and explanatory variables include the vector of individual attributes x_i.

3) For the subsample of observations where $T_i = t_0$, estimate the predicted values from the logit $\hat{P}(T_i = t_1 \mid X = x_i) = \dfrac{\exp\{x_i \hat{\beta}\}}{1 + \exp\{x_i \hat{\beta}\}}$, where $\hat{\beta}$ is the parameter vector from the logit regression. Then, compute the estimated weights as

$$\hat{\tau}_{it_0 \to t_1} = \frac{\dfrac{\hat{P}(T_i = t_1 \mid X = x_i)}{1 - \hat{P}(T_i = t_1 \mid X = x_i)}}{\dfrac{\hat{P}(T_i = t_1)}{1 - \hat{P}(T_i = t_1)}}$$

4) For the subsample of observations where $T_i = t_0$, compute a weighted empirical cumulative distribution function. While there are many available options to do this task, we use the simplest non-parametric option

$$\hat{F}_{t_1 \to t_0}(z) = \frac{\sum_{i=1}^{N_1} 1[y_i \le z] \hat{\tau}_{it_1 \to t_0}}{\sum_{i=1}^{N} \hat{\tau}_{it_1 \to t_0}},$$

where $1[.]$ is the indicator function.

It is useful to analyze the differences between the distributions of interest at each quantile. Specifically, define $Q_\theta[y \mid T = t]$ as the θ^{th} quantile of the distribution of Y

in period t. Similarly, let $Q_\theta^*[y \mid T = t_0]$ be the θ^{th} quantile of the counterfactual distribution of Y if individual attributes where identical to those in t_0 and notice that Q_θ^* is implicitly defined by $\hat{F}_{t_0 \to t_1}(z)$.

To assess how much of the changes in the distribution of Y between period t_0 and t_1 can be "explained" by changes in individual attributes x, we may compute

$$Q_\theta[y \mid t_1] - Q_\theta[y \mid t_0] = \{Q_\theta[y \mid t_1] - Q_\theta^*[y \mid t_0]\} - \{Q_\theta[y \mid t_0] - Q_\theta^*[y \mid t_0]\} \quad (6)$$

The first term measures the "unexplained" part of the changes in the distribution of Y. The second term in parenthesis is the portion of the changes that can be explained by differences in endowments.

There is only one subtle difference between our approach and DFL's: rather than computing the counterfactual probability density function, we estimate the counterfactual cumulative distribution. This is a natural choice because the counterfactual cumulative distribution remains valid even in cases when the dependent variable is censored (as is the case for taxes).