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# How do Top Earners Respond to Taxation? Own- and Cross-Tax Base Responses, Efficiency, and Inequality

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# How do Top Earners Respond to Taxation? Own- and Cross-Tax Base Responses, Efficiency, and Inequality

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**Abstract**

This paper presents new evidence on how top income earners respond to changes in the personal labor income tax schedule, uncovering both own- and cross-tax base responses within a unified framework. For identification, we exploit a 2012 tax reform in Uruguay that generated quasi-random variation in top marginal rates within the top 1% of the labor income distribution. Our empirical approach relies on a difference-in-differences identification strategy and administrative records linked at the individual level across multiple tax bases. We estimate an own-tax base intensive margin elasticity of 0.77 and extensive margin semi-elasticity of 2.64. Extensive margin responses are mostly driven by taxpayers shifting from the personal labor income tax base toward corporate income or capital income tax bases (semi-elasticities of -0.79 and -0.75, respectively). Our preferred estimates suggest that the reform was effective in increasing tax revenues, with efficiency costs representing 27% of the projected increase. However, it had limited impact on inequality, most likely due to its narrow scope and income shifting toward tax bases with lower and flat rates. Overall, our results indicate that policy efforts aiming to reduce inequality by increasing top marginal tax rates should also focus on limiting income shifting opportunities to strengthen their redistributive effects.

*JEL Classification:* H21, H24, H30, J22, O23

*Keywords:* Income taxation, top income earners, tax reform, reported income supply, income-shifting

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# 1 Introduction

A substantial increase in income and wealth concentration (Chancel et al., 2022), combined with an increasing need to finance public spending, has revitalized the debate about how to tax top income earners (TIEs). The size and mechanisms that drive TIEs' responses to taxation, e.g., changes in labor supply versus tax avoidance/evasion responses, should be key inputs in this discussion because of their efficiency and inequality implications.

Most evidence shows that income taxation induces substantial behavioral responses, especially among high-income taxpayers (Neisser, 2021). However, existing studies present at least two critical challenges. First, ideally, one would like to study how TIEs respond to taxation based on exogenous variation in tax rates within the TIEs' group. This is rare in real-life settings. Most existing studies are based on broader segments of the earnings distribution, which creates concerns about the comparability between affected and unaffected groups, in particular, due to mean reversion and secular trends on inequality (Jakobsen and Søgaard, 2022; Saez et al., 2012). Second, due to data limitations, very few studies analyze behavioral responses across tax bases. Since TIEs have several opportunities for tax planning, accounting for income shifting is important to understand the welfare and inequality implications of taxation (Piketty et al., 2014; Chetty, 2009; Slemrod, 1998).

This paper contributes with novel empirical evidence that addresses these limitations in a unified setting. We exploit a unique reform to Uruguay's progressive personal labor income tax (PLIT) schedule that took place in 2012. This reform created variation in the tax rates within, roughly, the top 1% of the labor income distribution and left unchanged all other relevant bases. This reform is very well suited to analyze how TIEs respond to taxation. First, even within the top 1%, some TIEs experienced an increase in their tax rates while other did not. Second, the reform affected the tax rate differential between tax bases, increasing the incentives for income shifting. Finally, the tax reform was salient, simple, and similar in size to others used in previous studies (e.g., Saez 2017). Combined with detailed individual-level microdata, this is a unique setting to dig into the individual responses to income taxation within a unified framework. Our findings show that TIEs respond across multiple margins and tax bases, and that failing to account for some of them can lead to inaccurate assessments of the efficiency and redistributive effects of tax reforms.

We start by developing a simple theoretical model in which individuals earn labor income and decide how much to report under the PLIT base and how much to shift to other tax bases. Individuals face heterogeneous fixed and variable shifting costs, leading to sorting across different tax mixes. We show that the efficiency costs associated with increasing marginal tax rates depend on both own- and cross-base intensive and extensive margin elasticities.

Using 2009-2015 tax records for the universe of Uruguayan taxpayers, we then implement a difference-in-differences design to estimate the reduced-form effects of the reform. The 2012 tax reform split the top two PLIT brackets (with marginal rates of 22% and 25%) into three, adding a new top marginal rate of 30%. This change creates four alternate income zones within the top 1%, with only two of them experiencing increases in marginal tax rates. Our research design compares TIEs who were more or less likely to be affected by the reform, over time. Tax records can be linked at the individual level across PLIT, corporate, and capital income tax bases to capture the relevant own- and cross-tax base responses. Building on our theoretical framework, we estimate the full set of own- and cross-base aggregate elasticities using a two-stage least squares approach that instruments (predicted) changes in net-of-tax rates with the difference-in-differences interactions. Finally, we use the estimated elasticities to quantify the efficiency costs of the reform and explore its effects on income inequality.

Our empirical analysis yields two main findings. First, we document a decline in reported gross labor income among treated TIEs, explained both by intensive and extensive margin responses (elasticities of 0.77 and 2.64, respectively). This decline reflects inter-temporal and concurrent responses and it is driven mostly by the highest TIEs. Our estimated intensive margin elasticity falls within the typical $[0, 1]$ range documented in the meta-study by Neisser (2021), but exceeds the average estimate (0.287), likely due to our focus on TIEs. Second, we also document significant cross-tax base responses, as taxpayers fully exit the PLIT base toward corporate and capital income tax bases (elasticities of -0.79 and -0.75, respectively). We do not find evidence of TIEs leaving the three tax bases altogether.

We supplement our main analysis with exploratory estimates based on social security labor histories. This exploratory analysis suggests that the reform led to a 2% reduction in *reported* hours worked and a 1.2% decline in the number of income sources. We interpret this as an upper bound on real labor supply responses given the limitations in measuring hours worked from administrative records. A back-of-the-envelope calculation suggests that the observed decline in gross labor income can be roughly decomposed in thirds: income shifting, *reported* hours, and other margins not captured in our analysis (e.g., tax evasion).

Overall, our estimates suggest that the reform increased tax revenues, with efficiency losses representing about 27% of the projected mechanical gain. Our results also illustrate how accounting for extensive and cross-base responses is key: ignoring extensive margin responses would lead to a 25% underestimation of efficiency costs, whereas omitting income shifting responses would overstate them by 26%. Finally, we also estimate that, if anything, the reform had limited impact on income concentration, likely due to its narrow scope and shifting toward less progressive tax bases. From a policy perspective, these results suggest that policy efforts should also be focused on reducing incentives to switch between tax bases in order to strengthen the redistributive effects of increasing tax rates (e.g., by closing loopholes and opportunities for arbitrage).

Our paper's main contribution is to the broad literature on how individuals respond to taxation (e.g., Neisser 2021; Saez et al. 2012), with a specific focus on TIEs. Our contribution to this literature is twofold. First, we provide what is, to our knowledge, the first unified analysis of taxpayer responses to taxation across the three major tax bases, i.e., labor, corporate, and capital, using detailed administrative individual-level microdata. Prior studies have typically focused on specific margins of adjustment, such as own-base intensive margin responses (e.g., Miao et al. 2024; Kleven and Schultz 2014; Gruber and Saez 2002; Auten and Carroll 1999; Feldstein 1995), intertemporal shifting (Miller et al., 2024; Kreiner et al., 2016), personal-to-corporate income shifting (e.g., Goolsbee 2000; Gordon and Slemrod 2000; Slemrod 1995), personal-to-capital income shifting (e.g., Pirttilä and Selin 2011; Alstadsæter and Jacob 2016; Harju and Matikka 2016), or extensive margin responses (e.g., Kleven et al. 2013).[1] In contrast, we estimate the full set of intensive and extensive margin responses across tax bases within a unified empirical and theoretical framework. Our granular, longitudinal, and individually linked data also allow us to study the mechanisms better. This includes studying anticipatory vs. concurrent responses, heterogeneity by treatment intensity or employment type, total income, and exploratory analysis on labor supply. This unified, broad, and flexible approach, combined with a detailed empirical analysis allows us not only to provide a clearer description of the anatomy of behavioral responses to taxation, but to assess more precisely their efficiency and inequality implications. As our study shows, a narrow focus on a single margin or tax base may be misleading.

Second, we use a transparent empirical approach that provides compelling reduced-form visual evidence of the effects of the reform and supports our identification assumption, something relatively rare in the related literature (Miao et al., 2024). Most existing studies typically rely on variation across broader income ranges, which requires controlling for pre-reform income to address mean reversion and inequality trends (Weber, 2014b; Gruber and Saez, 2002; Auten and Carroll, 1999), but prevents visual validation of parallel trends (Jakobsen and Søgaard, 2022), can remove much of the identifying variation in short panels with one reform (Saez et al., 2012), and often leads to unstable estimates (Neisser, 2021; Weber, 2014b; Giertz, 2010; Kopczuk, 2005). The reform we study has two features that help us avoid these issues. First, by focusing on a more homogeneous group, it is less likely that treated and control groups are affected differently by mean reversion or heterogeneous secular trends

[1] Some of these papers consider two of these margins. For example, Miao et al. (2024) and Kleven and Schultz (2014) complement their analysis of own-base intensive responses with some discussion of income shifting, but they include it mainly as a potential channel within a broader discussion on mechanisms.

in inequality. Second, the 2012 reform introduced non-monotonic changes in marginal tax rates even within the top 1%. This helps reduce endogeneity concerns that typically affect instruments based on predicted tax changes using base-year income. These are particularly important when tax rate changes are monotonic in income and income is serially correlated, as discussed in Weber (2014b). Still, to be conservative, we treat the predicted change in the net-of-tax marginal rate as endogenous and instrument it using a difference-in-differences interaction term, where treatment is defined based on the full pre-treatment period. Altogether, these features allow us to offer a clean and transparent comparison between treated and control groups, over time, without controlling for pre-reform income.

Our paper also contributes to a more specific and growing strand of literature that uses quasi-experimental designs and administrative data to study behavioral responses to taxation in developing or low- and middle-income countries. While recently there has been an increase in the number of studies focusing on lower-income settings, most of this work focuses on firm behavior (e.g. Waseem 2018), and studies examining individual responses remain scarce (Pomeranz and Vila-Belda, 2019). Furthermore, only a handful of studies have focused specifically on top income earners (TIEs) (e.g., Jouste et al. 2024; Axelson et al. 2024; Tortarolo et al. 2020 for Uganda, South Africa, and Argentina, respectively).[2] Hence, one contribution of our paper is to provide new evidence to this thin but relevant literature.

One might be concerned about the external validity of using Uruguay as a laboratory to study behavioral responses of TIEs to taxation. However, we study these responses in a tax system that is quite representative of other tax structures in the world. Progressive personal income taxes combined with a differential treatment of corporate and capital income, often taxed at lower or flat rates, are common features of many tax systems, and this is also the case in Uruguay. More specifically, the own- and cross-base responses we analyze are similar to those studied in a wide range of contexts, including unified tax systems like in the United States (e.g., Goolsbee 2004; Gruber and Saez 2002; Feldstein 1995), dual income tax systems in the Nordic countries (e.g., Alstadsæter and Jacob 2016; Harju and Matikka 2016; Kleven and Schultz 2014), and lower-income countries (e.g., Axelson et al. 2024; Jouste et al. 2024; Waseem 2018; Sivadasan and Slemrod 2008). While the specific way in which these responses actually materialize depends on legal and administrative details, the underlying incentives and mechanisms are fairly general: taxpayers respond to differences in tax rates by shifting income across tax bases, either from personal to corporate income, or by adjusting the wage-dividend mix in closely held firms (Selin and Simula, 2020).

The paper is organized as follows. In Section 2, we describe the main characteristics

[2] In related research, Londoño-Vélez and Avila-Mahecha (2024) study the behavioral responses of high-net-worth individuals to personal wealth taxes in Colombia.

of Uruguay's TIEs, its tax structure, and the 2012 tax reform. In Section 3, we present our simple theoretical framework. In Section 4, we present the data used for the empirical analysis as well as our sample selection criteria. Section 5 describes our research design, and Section 6 reports our main results. In Section 7 we discuss our preferred estimates for the efficiency costs and inequality implications of the reform, while Section 8 concludes.

# 2 Institutional Background

## 2.1 Top Income Earners and Income Composition

Uruguay is an upper-middle-income country with 3.5 million inhabitants and a GDP per capita of USD 20,200 (PPP, 2015). Tax revenues represent 27.3% of the GDP, which is high compared to the Latin American average (22.6%), but lower compared to OECD (33%).[3] In terms of income concentration, recent estimates show that Uruguayan TIEs earn about 15% of all income (Burdín et al., 2022). While among the lowest in Latin America, a region with high income concentration (Alvaredo, 2010; Alvaredo and Londoño Velez, 2014; Flores et al., 2020; Morgan, 2017), this share is similar to that of other higher-income countries such as the U.S. (Piketty, 2003; Atkinson, 2007; Atkinson et al., 2011).

In Uruguay, working-age individuals have three main sources of income: labor, corporate, and capital. The tax reform analyzed in this paper targeted individuals approximately in the top 1% of the *labor* income distribution. As a result, our analysis excludes individuals who primarily earn business or capital income, even if they are in the top 1% overall. Nevertheless, over 81% of taxpayers in our sample also belong to the top 1% of the *total* income distribution. Table 1 reports descriptive statistics for four groups: the universe of taxpayers with positive total income, the top 1% of the total income distribution, the top 1% of the gross labor income distribution, and the final sample used in our empirical analysis. Across all groups, labor income is by far the largest component of total income, ranging from 71.6% in the top 1% of total income to 95.5% in the top 1% of labor income. Capital and corporate income play a much smaller role, even within the upper tail of the distribution. For example, among taxpayers in the top 1% of gross labor income, 88.9% report labor income only, only 13.3% report any capital income, and, on average, it represents only 2.7% of total income. When considering the top 1% of total income, these shares raise up to 21.7% and 15.6%, respectively. A similar situation is observed when considering corporate income.

Table 1 shows that individuals in the top 1% of labor income earn, on average, 908 BPC in total yearly income (approximately USD 126,000, PPP-adjusted), of which 833 BPC cor-

[3] OECD.stats: `https://stats.oecd.org/`

responds to labor income.[4] The average tax payment on labor income is 125 BPC, implying an effective tax rate of 15%. These individuals are around 49 years old, predominantly male (about 72%), with roughly 37% reporting some self-employment income and about 38% receiving labor income from multiple sources. TIEs are disproportionately employed in financial and insurance services, as well as in human health and social work.[5]

## 2.2 The Pre-Reform Tax Structure

Direct taxation represents approximately 35% of total tax revenues, with most of the remaining 65% coming from a widespread value added tax. Personal and corporate income taxes account for about 75% of direct tax revenues, split evenly between them. The remaining 25% comes from smaller sources such as property, pension, and non-resident taxes. For personal income taxation, Uruguay implemented in 2007 a dual system, similar to those in Nordic countries, that treats labor and capital income separately. Within this dual system, the labor income tax accounts for roughly 85% of revenues.[6]

**Personal Income Tax on Labor (PLIT)**. Before the reform, the PLIT applied five progressive rates between 0% and 25% to wages and self-employment income. Taxpayers can claim itemized and non-itemized deductions (e.g., social security contributions), also subject to a progressive schedule. Final tax liability is calculated by applying the progressive rates separately to gross labor income and deductions, then computing the difference.[7] Panel (a) in Table 2 describes the PLIT structure, while Figure 1 overlaps it with the gross labor income distribution. Two things are worth noting. First, only individuals above 70th percentile actually pay PLIT, which is not atypical in developing countries due to the relatively large exemption thresholds (Jensen, 2022). Second, the top two income brackets overlap almost perfectly with the top 1% of the gross labor income distribution. This is precisely the income range affected by the tax reform we study in this paper.

**Capital Income Tax**. As shown in Panel (b) of Table 2, the capital income tax consists of flat rates applied without deductions to various sources of individual capital income. These rates remained unchanged throughout our analysis period: 3% on bank deposit interest, 7% on dividends and other financial income, and 12% on real estate rents. As usual, dividends are also taxed at the corporate level. This means that if the owner of an incorporated firm opts for paying out profits as dividends, these are first subject to a 25% corporate income

[4] BPC is the monetary unit used by the PLIT tax law and is adjusted annually by CPI. As reference, the BPC/USD exchange rate in 2011 was 1 BPC = 117 USD, and in 2011 the PPP conversion factor was 0.8.

[5] Additional details on income composition and sectoral participation are provided in Appendix A.

[6] Further details on Uruguay's tax structure and tax bases are provided in Appendix A.

[7] Since brackets are adjusted annually by CPI, we should not expect "bracket creep" (Saez, 2003).

tax, resulting in an effective tax rate of approximately 30%. However, as discussed later, unincorporated self-employed who opt into corporate taxation as an alternative to PLIT are exempt of capital income tax to avoid double taxation. Individuals must file capital income tax returns only if they were not subject to withholding. Due to bank secrecy rules, substantial capital income, such as interest from bank deposits or dividends from anonymous companies, is not attributable to specific taxpayers (henceforth, non-nominative). Appendix A shows that the share of nominative dividends over total dividends increased in our period from about 50% in 2009 to 62% in 2015. Our main analysis focuses on nominative capital income, as it corresponds to the capital income items that we can link at the individual level.

**Corporate Income Tax**. Panel (c) of Table 2 describes the corporate income tax schedule, which consists of a flat 25% rate applied to business net profits. Unincorporated self-employed workers registered as sole proprietorships or partnerships earning below a certain threshold (i.e., BPC 4,000) may opt to pay corporate income tax on their profits instead of the PLIT and are exempt from capital income tax on dividends to avoid double taxation.[8] For tax purposes, business profits can be computed using either a presumptive or real regime. Most self-employed workers who opt for the corporate regime choose the presumptive method ($\sim$ 80%), in which the 25% rate applies to 48% of gross business revenues, resulting in an effective tax rate of 12% on total gross revenues. The remaining 20% use the real regime which requires detailed reporting (e.g., full balance sheets) and consists of a flat 25% rate on net business profits. Switching from PLIT to corporate taxation involves minimal administrative costs (i.e., filing a form), but taxpayers choosing corporate taxation must wait three years before returning to PLIT. Because of its flat rate, corporate taxation becomes increasingly beneficial relative to PLIT as income rises. In 2010, 12.8% of self-employed TIEs opted for corporate taxation over PLIT, but this share exceeded 40% among those with the highest income. Appendix A provides additional descriptive statistics.

Two additional features about Uruguay's tax structure are worth noting. First, as in most countries, the incentives to shift between tax bases involve more than just a simple comparison between tax rates across bases. For instance, labor income is subject to mandatory social security contributions of about 20%, while opting into the corporate tax allows self-employed individuals to contribute on a lower presumptive base. In addition, collecting income as dividends avoids these contributions entirely. As a result, the decision to report income under one tax base or another is complex and it also depends on how individuals value access

[8] This tax treatment of dividends is not uncommon in the international context. For example, in the United States, distributed profits from S Corporations are not subject to the dividend tax. Instead, income is passed through to individual shareholders, who pay taxes on profits as ordinary income.(Kennedy et al., 2022; Kopczuk and Zwick, 2020; Goolsbee, 2004)

to benefits like public pensions and health insurance.

Second, despite legal and administrative differences in how tax bases are defined and the required steps to switch between them, the structure described above and the opportunities for income shifting are broadly representative of tax systems worldwide. Conceptually, progressive personal income taxes combined with opportunities to access more favorably taxed bases, such as capital or corporate income, are common in many countries. One particular feature of the Uruguayan system that is worth to be noted is that self-employed individuals can opt into corporate income taxation without formally incorporating their business, which implies relatively low transaction costs. While this may be a specific institutional feature, the literature has documented strong income shifting responses across a wide range of settings, including those where switching tax treatment requires formal incorporation. The fact that large responses are observed even in contexts with higher administrative and legal costs suggests that such behavior does not depend on low transaction costs. That said, the responses estimated in the Uruguayan context may be seen as an upper bound relative to settings where switching is more costly.[9]

## 2.3 The 2012 Tax Reform: Changes in the PLIT

The 2012 tax reform split the top two PLIT brackets into three, adding a new top marginal rate of 30%. It exclusively affected taxpayers roughly in the top 1% of the gross labor income distribution, creating four income zones subject to different changes in marginal tax rates. We denote these as G1:G4, where G1 represents the lower-income zone and G4 the higher-income zone. Panels (a) and (b) in Figure 2 illustrate how the reform changed marginal and effective tax rates, respectively. Zones G2 and G4 faced increases in marginal tax rates from 22% to 25% and 25% to 30%, respectively, implying reductions in marginal net-of-tax rates

[9] The income-shifting margins studied in this paper are conceptually similar to those analyzed in other settings. For instance, the U.S. literature on transitions from S corporations to C corporations (Auten et al., 2016; Goolsbee, 2004; Gordon and Slemrod, 2000; Slemrod, 1995). In those settings, individuals and firms respond to differences in tax treatment by (easily) changing tax status without altering their legal structure (Kopczuk and Zwick, 2020). In our case, while the provision applies to unincorporated self-employed individuals rather than incorporated firms, the underlying incentive is comparable: taxpayers can access a more favorable tax schedule with relatively low frictions and without changing their legal form. Furthermore, although incorporation is not required, the income shifting behavior studied in this paper is similar to that documented in the literature on organizational responses to tax incentives (e.g., Tazhitdinova 2020; Waseem 2018; Romanov 2006), as it reflects the incentive for the self-employed to move income into flat-rate tax regimes to avoid progressive personal income taxation. PLIT to capital income tax shifting corresponds to the type of behavior analyzed in studies such as Harju and Matikka 2016; Alstadsæter and Jacob 2016; Pirttilä and Selin 2011. One illustrative example of this, is that all the margins we analyze broadly match the responses analyzed in Auten et al. (2016) for the historical review in the U.S. context. Furthermore, these are the specific mechanisms underlying the income shifting model in Selin and Simula (2020) which describe how taxpayers respond to differences in tax rates by shifting income across tax bases.

by 4% and 7%. Zones G1 and G3 saw no change in marginal rates. Regarding effective tax rates, only G1 remained unaffected, while G2, G3, and G4 experienced increases of up to 3%, rising with income. Importantly, tax rates on corporate and capital income remained unchanged, making these tax bases, all else equal, relatively more attractive than PLIT.

This reform offers two advantages that help mitigate the typical concerns about mean reversion and secular trends in inequality in studies about behavioral responses to taxation. These concerns are most common in studies that exploit tax reforms where changes in tax rates are strongly correlated with income levels (Saez et al., 2012). First, our analysis focuses on a relatively homogeneous group of TIEs, which limits the possibility that differences between treated and control groups are driven by mean reversion or heterogeneous long-run trends in inequality. For instance, in Appendix A, we show that the top 1%, 0.5%, and 0.1% income shares remained stable over the period of analysis, while the top 5% and 10% income shares declined steadily. This, not only supports the validity of our identification strategy which will be discussed in Section 5, but also highlights potential issues that can arise when individuals in the 90-99 or 95-99 percentiles are used as control groups for those in the top 1%, a common approach in the related literature. Second, the reform generated non-monotonic changes in marginal tax rates even within the top 1%. This reduces endogeneity concerns typically associated with instruments based on predicted tax changes using pre-reform income, especially in settings where income is serially correlated (Weber, 2014b).[10]

Finally, given the timing of the reform's announcement and approval, some taxpayers may have anticipated the changes. The reform was first announced on September 12, 2011, but only approved after six months of debate on May 25, 2012 (Law 18.910), retroactive to January 1, 2012. Due to the ruling party's absolute majority, taxpayers may have felt confident about its approval as soon as it was announced. Moreover, with three months remaining in the fiscal year and over six months before the tax filing deadline, some taxpayers may have adjusted their behavior, accelerating income realizations to benefit from lower pre-reform rates. We account for these potential anticipatory responses both when discussing extensions to the theoretical framework as well as in the empirical analysis.

# 3 Conceptual Framework

In this section, we present a simple conceptual framework to guide our empirical analysis. Our model extends Selin and Simula (2020) to allow both for extensive and intensive margin

[10] More specifically, Weber (2014b) argues that endogeneity concerns are mitigated when a tax reform affects some individuals but not others within a given income class. We believe our setting is similar in spirit, as the reform created alternate zones with/without changes in marginal net-of-tax rates within the top 1%.

income-shifting responses simultaneously. We focus on individuals who earn labor income and can shift part of it between tax bases. While we present this in a way that is compatible with Uruguayan tax structure, our model fits income shifting opportunities broadly. In a nutshell, our model illustrates how an increase in the top tax rate on personal labor income tax can lead TIEs to: (i) reduce reported earnings in the affected tax base (own-base intensive margin response), (ii) exit the affected tax base (own-base extensive margin response), or (iii) shift earnings to alternative tax bases (cross-base intensive and extensive margin responses).[11]

**Setup.** Consider a population of individuals who maximize a quasi-linear utility function, $u = c - v(h)$, where $c$ represents consumption, $h$ denotes labor supply, and $v(h)$ is a convex function capturing, for instance, the disutility of effort.[12] For simplicity we assume that there are only two tax bases: $b \in \{l, k\}$. This assumption rules out pure tax evasion, as all income must be reported to base $l$, $k$, or a combination of the two. We discuss this and other extensions at the end of this section. By default, labor income, denoted by $y(h)$, is reported to tax base $l$ which follows a progressive tax schedule with total tax liability given by $T_l(y)$ and a marginal tax rate of $T_l'(y)$. However, at some cost, individuals can engage in income shifting by reallocating part of their earnings, $a \leq y(h)$, to the alternative base $k$ where income is taxed at a flat rate $\tau_k$. In the Uruguayan setting, tax base $l$ can be thought of as the PLIT base, while $k$ represents the capital income tax base.

**Compliance and shifting costs.** Tax compliance is costly. First, reporting income to a tax base $b$ involves a fixed cost $\gamma_b$. Intuitively, fixed costs can be thought of as administrative costs for complying with tax regulations or the administrative burden of organizing income sources or separate bookkeeping for different tax bases.[13] Shifting income to tax base $k$ also has a variable cost $R_k(a)$, represented by a convex function finite at $a = y$. These can be thought of as typical costs associated with tax avoidance, including, but not limited to, increased audit risk and penalties, which tend to increase with the amount avoided or evaded.

**Heterogeneity.** We define three sources of heterogeneity. First, individuals differ in their labor market productivity $\omega$, such that $y = \omega h$. Second, we also allow for heterogeneous costs of effort $v(h)$. Without loss of generality, neither of these are tax base specific. Third, we also allow individuals to be different in their base-specific compliance technology, $\theta_b$. For

[11] This model can be seen as a combination of the frameworks in Sections 4.1 and 5.1 of Selin and Simula (2020), with the addition of tax-base-specific fixed costs. We thank Håkan Selin for this insight.

[12] For simplicity, given the lack of evidence of significant income effects, we follow the usual approach in related literature and use a quasi-linear utility function. See Saez et al. (2012) for a more in depth discussion.

[13] In the related literature, examples of fixed costs are the disutility of gathering information about the tax law, the time and effort spent to fill tax documents, costs associated to entering self-employment (Selin and Simula, 2020) or costs of incorporation (Tazhitdinova, 2020) which, as we discussed in Section 2, do not apply to the Uruguayan setting.

instance, one could think of $\theta_k$ as the set of parameters that define the shifting cost function (i.e., $\gamma_k$, and $R_k$). These heterogeneity parameters are randomly drawn from a distribution function $F(\lambda)$ with total mass 1, where $\lambda = \{\omega, v, \theta_l, \theta_k\}$.

**Individual Choices.** Individuals can choose between three types of reporting behavior. Some may report all of their income to tax base $l$ (i.e., non-shifters), some might shift completely to tax base $k$ (i.e., full shifters), while others opt for reporting to both tax bases (i.e., partial shifters). Equations (1) and (2) characterize the labor supply decisions for non-shifters and full-shifters, respectively.

$$v'(h) = (1 - T_l'(\omega h))\omega \tag{1}$$

$$v'(h) = [(1 - \tau_b) - R_k'(\omega h)]\,\omega. \tag{2}$$

These conditions show that individuals supply labor until the marginal benefit of an additional hour equals its marginal cost. The key difference is that, for non-shifters, the marginal benefit of an extra hour is given by the marginal net-of-tax rate multiplied by the productivity factor whereas full shifters must also account for the marginal cost of shifting income to tax base $k$, $R_k'(\omega h)$.

Partial shifters decide both on the labor supply and the amount of income shifted. Equations (3) and (4) report the first-order conditions with respect to $h$ and $a$, respectively:

$$v'(h) = (1 - T_l'(\omega h))\omega \tag{3}$$

$$R_k'(a) = T_l'(\omega h) - \tau_k \tag{4}$$

Equation (3) is similar to equations (1), and (2). Equation (4) illustrates that partial shifters will continue shifting income from tax base $l$ to tax base $k$ until the marginal cost of shifting equals the marginal tax rate differential.[14]

To analyze extensive margin decisions, we consider the indirect utility functions associated with non-, partial, and full shifting denoted by subscripts $ns$, $ps$, and $fs$, respectively:

$$V_{ns} = \omega\, h_{ns}^* - T_l \;\; \omega\, h_{ns}^* \;\; - \gamma_l - v \;\; h_{ns}^* \;\; . \tag{5}$$

$$V_{fs} = (1 - \tau_k)\,\omega\, h_{fs}^* - \gamma_k - R_k \;\; \omega\, h_{fs}^* \;\; - v \;\; h_{fs}^* \;\; . \tag{6}$$

$$\begin{aligned} V_{ps} = {} & (\omega\, h_{ps}^* - a_{ps}^*) - T_l \;\; \omega\, h_{ps}^* - a_{ps}^* \;\; - \gamma_l \\ & + (1 - \tau_k)\, a_{ps}^* - \gamma_k - R_k \;\; a_{ps}^* \;\; - v \;\; h_{ps}^* \;\; . \end{aligned} \tag{7}$$

[14] At the optimum, partial shifters are indifferent between placing an additional dollar of labor income in base $l$ or in base $k$ ($T_l'(y) = R_k'(a) + \tau_k$). As a result, their marginal net-of-tax rate on labor is the same as for a non-shifter, leading to the same labor-supply condition. The cost of shifting arises only in determining how much to shift but does not affect the marginal decision of how many hours to work.

where $h^*$ and $a^*$ are solutions to equations (1):(4), accordingly. These indirect utility functions define a series of sorting conditions:

$$V_{ns} \geq V_{fs} \quad \text{and} \quad V_{ns} \geq V_{ps} \quad \Rightarrow \quad \text{Non-shifter.} \tag{8}$$

$$V_{fs} > V_{ns} \quad \text{and} \quad V_{fs} \geq V_{ps} \quad \Rightarrow \quad \text{Full shifter.} \tag{9}$$

$$V_{ps} > V_{ns} \quad \text{and} \quad V_{ps} > V_{fs} \quad \Rightarrow \quad \text{Partial shifter.} \tag{10}$$

Appendix B discusses these conditions in more detail and shows they can be expressed as fixed cost thresholds that separate non-shifters, partial shifters, and full shifters.

Consider an increase in the marginal tax rate $T_l'(y)$. Our model yields the following predictions. First, it reduces labor supply among non-shifters and partial shifters, as shown in equations (1) and (3). Second, it increases the amount of income shifted by partial shifters as shown in equation (4). Third, as shown by Equations (8):(10), it makes non-shifting less attractive, reducing the share of non-shifters and increasing the share of full shifters. The effect on the share of partial shifters is ambiguous: partial shifting becomes relatively more attractive than non-shifting, but less attractive than full shifting.

**Welfare and efficiency costs.** We define a social welfare function $W$ as the sum of indirect utilities across all individuals, $V(\lambda, \tau)$, plus a lump-sum transfer $g$ funded by tax revenues. Budget balance must hold, $G = R$, where total transfers are $G = \int_\Lambda g \, dF(\lambda)$. Hence,

$$W = \int_\Lambda \left[V(\lambda) + g\right] \, dF(\lambda), \tag{11}$$

$$R = \int_\Lambda T_l(y_l(\lambda)) \; I_l(\lambda) \; dF(\lambda) + \int_\Lambda \tau_k y_k(\lambda) \; I_k(\lambda) \; dF(\lambda), \tag{12}$$

where $I_b(\lambda)$ indicates reporting any income to tax base $b$. By the envelope theorem, a small change in $T_l'(y)$ does not affect taxpayers' indirect utilities at the margin, as they were already optimizing. Hence, the deadweight loss associated with that change can be expressed as the sum of revenue changes in each tax base due to behavioral responses:

$$\begin{aligned} \frac{dW}{d\tau_l} = & \underbrace{\int_\Lambda \left( I_l(\lambda) \, \tau_l \, \frac{\partial y_l(\lambda)}{\partial \tau_l(\lambda)} + y_l(\lambda) \, \tau_l^e(\lambda) \, \frac{\partial I_l(\lambda)}{\partial \tau_l(\lambda)} \right) dF(\lambda)}_{\text{Own-tax base response}} \\ & + \underbrace{\int_\Lambda \left( I_k(\lambda) \, \tau_k \, \frac{\partial y_k(\lambda)}{\partial \tau_l(\lambda)} + y_k(\lambda) \, \tau_k \, \frac{\partial I_k(\lambda)}{\partial \tau_l(\lambda)} \right) dF(\lambda)}_{\text{Cross-tax base response}} \end{aligned} \tag{13}$$

where changes in income $\frac{\partial y_l(\lambda)}{\partial \tau_l(\lambda)}$ are multiplied by a constant marginal tax rate, $T_l'(y) = \tau_l$, which we assume is the *top* pre-reform marginal rate. This assumption simplifies the analysis considerably and, if anything, lead to overestimate the welfare losses associated with the reform (see Appendix B for details). Each term in Equation (13) reflects both intensive and extensive margin responses. For own-tax base responses, the intensive margin captures changes in reported income among taxpayers who continue reporting to base $l$, while the extensive margin captures revenue losses from those who stop reporting to base $l$. For cross-tax base responses, the intensive margin reflects changes among taxpayers already reporting to base $k$, and the extensive margin captures gains from those who start reporting to base $k$. Appendix B shows that we can re-write this expression as a sum of weighted average elasticities, expressed in revenue-equivalent terms:

$$\frac{dW}{d\tau_l} = -\frac{\tau_l}{1-\tau_l} Y_l \quad \bar{\epsilon}_{l,l} + \frac{\bar{\tau}_l^e Y_l}{\tau_l Y_l} \bar{\eta}_{l,l} + \frac{\tau_k Y_k}{\tau_l Y_l} (\bar{\epsilon}_{l,k} + \bar{\eta}_{l,k}) \tag{14}$$

Equation (14) decomposes the welfare effect into a sum of intensive and extensive margin elasticities, each weighted and scaled into revenue-equivalent terms. The first term, $\bar{\epsilon}_{l,l}$, corresponds to the intensive margin elasticity with respect to the marginal net-of-tax rate $1 - \tau_l$, aggregated using income weights. These income weights reflect the greater revenue impact of responses by higher-income taxpayers. $\bar{\eta}_{l,l}$, is a term capturing revenue losses associated with extensive margin responses and is defined as $\bar{\eta}_{l,l} = \bar{\mu}_{l,l} \cdot \bar{\varsigma}_{l,l}$. Here, $\bar{\mu}_{l,l}$ is the extensive margin elasticity with respect to the effective net-of-tax rate $1 - \tau_l^e$, aggregated using revenue weights. Similar to income weights, revenue weights address the larger revenue impact of higher-income taxpayers responses, but in this case accounting for the progressive structure of the tax schedule. $\bar{\varsigma}_{l,l}$ adjusts for how changes in the *marginal* rate translate into changes in the *effective* rate, since extensive margin responses are driven by the effective rate, but we are interested in the welfare effects associated with changes in the *marginal* tax rate.

The final term accounts for cross-tax base responses, with $\bar{\epsilon}_{l,k}$ and $\bar{\eta}_{l,k}$ capturing weighted intensive and extensive responses in base $k$, respectively.[15] Finally, it is worth noting that each elasticity is multiplied by a scaling factor to express it in revenue-equivalent terms relative to the own-base intensive margin effect. Specifically, $\bar{\eta}_{l,l}$ is scaled by $\frac{\bar{\tau}_l^e Y_l}{\tau_l Y_l}$, while cross-base responses are scaled by $\frac{\tau_k Y_k}{\tau_l Y_l}$, where $Y_b$ is the total income reported to tax base $b$.

**Extension to multiple tax bases.** So far, for simplicity, we have allowed only for two tax bases. However, this framework is sufficiently flexible to accommodate for a larger number and broader definitions of tax bases. For instance, one could define an alternative tax base $e$ representing tax evasion where $\tau_e = 0$ and $\gamma_e$ and $R_e(a_e)$ are some fixed and variable expected costs of tax evasion. Similarly, one could think of inter-temporal responses as an additional tax base where the tax structure is the pre-reform structure, and there are some fixed and

[15] Because the alternative tax base consists of a proportional tax, income and revenue weights are equivalent.

variables costs of anticipating income to avoid higher future rates. Hence, a more general expression for the welfare costs of a tax reform can be written as:

$$\frac{dW}{d\tau_l} = -\frac{\tau_l}{1-\tau_l} Y_l \sum_{b \in B} \frac{\tau_b Y_b}{\tau_l Y_l} \cdot \bar{\epsilon}_{l,b} + \frac{\tau_b^e Y_b}{\tau_l Y_l} \cdot \bar{\eta}_{l,b} \tag{15}$$

where $B$ represents all possible tax bases (e.g. $B = \{l, k, e, ...\}$). Regardless the number and nature of tax bases, the efficiency costs of the tax reform can be expressed as the sum of weighted aggregate intensive and extensive margin elasticities across all tax bases. This is the key insight of our model.

**Application to the 2012 tax reform.** The 2012 tax reform introduced changes in the top marginal rates of the PLIT base. Our data allow us to focus on three tax bases: PLIT ($l$), capital income tax ($k$), and corporate income tax ($c$). We acknowledge that there may be other margins of response (e.g., income shifting toward non-nominative dividends). However, none of these are observable in our data. Hence, our welfare estimates should be interpreted as an upper bound on the actual efficiency costs of the reform. In Section 6, after presenting our main results, we discuss in detail what is and what is not included in our estimates and the implications for our efficiency estimates.

We will therefore estimate the following own-base elasticities. The *own-base intensive margin* elasticity ($\bar{\epsilon}_{l,l}$), which reflects changes in reported gross labor income among those who continue to report to the PLIT base. For non-shifters, this may include changes in labor supply or unobserved evasion/avoidance. For partial shifters, it may also capture a more intense shifting toward other bases. In all cases, we measure this response through changes in gross labor income reported to base $l$. The *own-base extensive margin* elasticity ($\bar{\mu}_{l,l}$), which captures revenue losses from taxpayers who stop reporting to the PLIT base. This could be due to shifting their entire income to capital or corporate income tax bases, or other behaviors such as fiscal migration or full tax evasion through offshore accounts. These responses are captured through changes in the probability of reporting any income to base $l$. For welfare analysis, we will also estimate $\bar{\varsigma}_{l,l}$, which captures how changes in the *marginal* net of tax rates affect *effective* net of tax rates.

We are also interested in a similar series of cross-base elasticities. The *cross-base intensive* elasticities ($\bar{\epsilon}_{l,k}$ and $\bar{\epsilon}_{l,c}$) reflect increases in reported income to the capital and corporate tax bases, respectively, among TIEs who remain in those bases. This includes wage earners who reclassify a larger part of their income as dividends, or self-employed who report more of their revenues under the corporate tax base. The *cross-base extensive* elasticities ($\bar{\mu}_{l,k}$ and $\bar{\mu}_{l,c}$) capture revenue gains from taxpayers who, in response to the reform, begin reporting to these alternative tax bases for the first time.

# 4 Data Sources and Sample Selection

## 4.1 Data

**TAX Records.** Our analysis relies mostly on tax records provided by Uruguay's Tax Agency (*Direccion General Impositiva*). Tax records cover the universe of registered wage earners and self-employed workers from 2009 to 2015. They include PLIT, corporate, and capital income tax returns, employer-reported statements on employee activity, and firm-level data. Similar to the U.S. 1040 form, PLIT returns provide line-by-line information, including earned income, tax withholdings, and deductions. They also report gender and date of birth. Employer statements on employee activity contain information similar to personal labor income tax returns, but reported by employers, similar to W-2 forms in the U.S. Corporate tax records consist of annual returns, which, for unincorporated businesses, can be linked to other individual-level data using a common identification number. However, due to the lack of ownership information, we are unable to link incorporated firms to their owners. This prevents us from directly studying whether the tax reform led to changes in businesses' organizational form, profit distribution, or consumption within the firm (e.g., as discussed in Kopczuk and Zwick 2020). This limits our ability to understand in detail how income shifting operates from the firm's perspective, but it does not affect our welfare assessments.[16] Capital income tax records include information on dividends, real estate rents, and other financial returns reported to this tax base. As discussed in Section 2, bank secrecy laws and other legal restrictions prevent certain capital income items from being attributed to specific individuals. These non-nominative capital income items are only available in aggregate. Additional details are provided in Appendix C. Finally, firm-level records contain information on the number of employees, firm age, location, industry, and sector.

**SSA Records.** We supplement our analysis with employer-employee administrative records from Uruguay's Social Security Administration (SSA). These data include earnings, hours, and days worked for the universe of workers registered with the SSA between 2000 and 2015. Among other advantages, SSA records allow us to observe certain variables for a longer period, which helps validate our empirical strategy. However, SSA data are not ideal for analyzing individual responses to taxation, as they cover only a subset of gross labor income components, i.e., those subject to third-party reporting. A further limitation is that SSA

[16] More specifically, while we cannot observe whether top income earners incorporate, if they are distributing profits, these are still recorded in the capital income tax base, even though we cannot link firms to individual owners. Similarly, shifting wages from PLIT to non-distributed profits would appear in our data as a reduction in gross labor income reported to the PLIT base, with no counterpart in the corporate income tax base, as profits remain within the firm and untaxed.

records can only be matched to tax records for individuals covered by the National Health Insurance system, representing approximately 75% of our sample of interest. In Section 6 we show this does not have any practical implications for our analysis.[17]

### 4.2 Sample Selection

We created a longitudinal dataset that tracks individuals across tax bases from 2009 to 2015. The full sample includes taxpayers who reported positive labor income in at least one year during the pre-treatment period (2009-2011). Since our focus is on top income earners, we restrict the sample to individuals who reported annual labor income of at least 600 BPC - roughly the 99th percentile of the 2011 labor income distribution - at least once between 2009 and 2011 ($N = 18,930$). This threshold is precisely the starting point of the fifth bracket of the PLIT schedule, making it a natural cutoff for our analysis. We exclude individuals who reported *total* income below 300 BPC - approximately the 95th percentile of 2011's gross labor income distribution - at least once during the same period. This restriction yields a sub-sample of 15,511 taxpayers who consistently remain in the upper part of the income distribution, without strongly limiting upward or downward mobility. Finally, we drop 859 individuals who changed income groups in every year from 2009 to 2011, and 174 individuals whose income grew more than 100% in 2010. These restrictions exclude taxpayers with extreme movement patterns in the pre-treatment period and allow us to obtain more precise estimates.[18] Furthermore, we show that our results are not driven by these decisions. Our final dataset contains 14,478 unique taxpayers, whose individual characteristics described in column (4) of Table 1 are almost identical to those of the top 1% of the gross labor income distribution before applying any restriction, reported in column (3) and Section 2.

## 5 Identification Strategy

### 5.1 Differences-in-Difference Approach

To analyze the reduced form effects of the 2012 tax reform, we implement a difference-in-differences (DiD) design that compares TIEs who were more or less likely to be affected by the reform, over time. Defining treatment and control groups based solely on base-year income can create some endogeneity concerns (Weber, 2014b). Hence, our preferred strategy defines treatment and control groups using the full pre-reform period (2009-2011). In general, we

[17] Appendix C contains more details about the SSA data and sample and compares it with the TAX sample.
[18] We provide more details on sample selection criteria and present a detailed description of the pre-reform income mobility patterns in Appendix C.

follow a conservative approach. For intensive margin responses, we define treated taxpayers as those who belonged to either G2 or G4 in at least one pre-treatment year, since these zones experienced increases in marginal tax rates. The control group includes taxpayers who were never in G2 or G4. For extensive margin responses, where the relevant rate is the *effective* rather than the *marginal* net of tax rate, we define treated individuals as those ever observed in G2, G3, or G4 during the pre-treatment period, while the control group consists of those who were always in G1. We denote these variables as $treat^{MTR}$ and $treat^{ATR}$, respectively.

To investigate the dynamics of individual outcomes before and after the 2012 tax reform, we estimate the following reduced-form DiD regression specification:

$$\Delta \log y^b_{it} = \sum_{t=2010}^{2015} \beta_t \cdot \mathbb{1}\{\text{year} = t\} \cdot \text{treat}_i^{\text{MTR}} + \delta_t + \varepsilon_{it} \tag{16}$$

$$\Delta \mathbb{1}(y^b_{it} > 0) = \sum_{t=2010}^{2015} \gamma_t \cdot \mathbb{1}\{\text{year} = t\} \cdot \text{treat}_i^{\text{ATR}} + \delta_t + \nu_{it} \tag{17}$$

where $\Delta \log y^b_{it}$ denotes the log-change in gross income reported to tax base $b$ from year $t-1$ to year $t$, and $\Delta \mathbb{1}(y^b_{it} > 0)$ captures the change in reporting status to tax base $b$, taking values in $\{-1, 0, 1\}$.[19] More specifically, $y^b_{it}$ can refer to log gross labor income reported to the PLIT ($y^l_{it}$), log revenues reported in the corporate income tax base ($y^c_{it}$), or log capital income reported to the capital income tax base ($y^k_{it}$). These definitions are described in detail in Appendix C. Equation (16) is used for intensive margin responses. The coefficients of interest, $\beta_t$, measure the yearly differential log-change in gross labor income for treated taxpayers relative to control taxpayers. Equation (17) has a similar interpretation but for extensive margin responses. Both equations are estimated using base-year income and revenue weights, ensuring consistency between the reduced-form and elasticity estimates discussed next.[20] Standard errors are clustered at the individual level.

The key identification assumption is that treated and control groups would have followed parallel trends in the absence of the reform, which we support with graphical evidence of parallel pre-trends. In addition, we conduct a series of validation, robustness, and sensitivity checks to assess how reliable our results are. We focus on endogenous selection into treatment, extensions of the pre-treatment period using alternative data sources, and mean

[19] $\Delta \log y^b_{it}$ is winsorized at the 1st and 99th percentiles. Reporting to a tax base is defined as 1 if the share reported to the base exceeds 1% of total income, and 0 otherwise, to exclude incidental reporting. Sensitivity checks to winsoring decisions are reported in Appendices D and E.

[20] More details on weights are discussed in Appendix C. It is important to note here, however, that weights were winsored at the 95% level, which corresponds to the 99.95 percentile of the gross labor income distribution, since our sample comprises the top 1% income earners. This decision aims to prevent that regression estimates are almost exclusively based on a handful of individuals with extreme outlier income/revenue values. In Appendices D and E, we conduct exhaustive sensitivity analyses that validate this decision.

reversion. Finally, it is important to note that all units are affected simultaneously. Hence, the interpretation of our estimates is not affected by recent concerns about two-way fixed effects and difference-in-differences estimators in contexts with staggered treatment adoption and heterogeneous treatment effects (e.g., De Chaisemartin and D'Haultfoeuille 2022).

## 5.2 Estimating Elasticities

Estimates obtained from Equations (16) and (17) capture the reduced-form effects of the reform. However, assessing its efficiency costs requires estimating both intensive and extensive margin elasticities. Hence, we need to relate the reduced-form responses, identified using the quasi-experimental variation generated by the reform, to changes in the net-of-tax rates. The Wald estimators below formalize this relationship, providing empirical estimates of the intensive and extensive margin elasticities based on changes between $t-1$ and $t$:

$$\bar{\epsilon}_{l,b} = \frac{\mathbb{E}\ \log \frac{y^b_{it}}{y^b_{i,t-1}}\ treat^{mtr}=1\ - \mathbb{E}\ \log \frac{y^b_{i,t}}{y^b_{i,t-1}}\ treat^{mtr}=0}{\mathbb{E}\ \log\ \frac{1-\tau^l_{i,t}}{1-\tau^l_{i,t-1}}\ \ treat^{mtr}=1\ - \mathbb{E}\ \log\ \frac{1-\tau^l_{i,t}}{1-\tau^l_{i,t-1}}\ \ treat^{mtr}=0}. \tag{18}$$

$$\bar{\mu}_{l,b} = \frac{\mathbb{E}\ 1\{y^b_{i,t}>0\} - 1\{y^b_{i,t-1}>0\}\ treat^{atr}=1\ - \mathbb{E}\ 1\{y^b_{i,t}>0\} - 1\{y^b_{i,t-1}>0\}\ treat^{atr}=0}{\mathbb{E}\ \log\ \frac{1-\tau^{e,l}_{i,t}}{1-\tau^{e,l}_{i,t-1}}\ \ treat^{atr}=1\ - \mathbb{E}\ \log\ \frac{1-\tau^{e,l}_{i,t}}{1-\tau^{e,l}_{i,t-1}}\ \ treat^{atr}=0}. \tag{19}$$

We estimate these elasticities and their standard errors using 2SLS regressions. Intuitively, the numerator in each expression reflects the reform's reduced-form effect on income reporting behavior, while the denominator captures the change in the corresponding net-of-tax rate induced by the reform, serving as the first stage. These first-stage estimates are obtained from specifications similar to Equations (16) and (17), but with changes in net-of-tax rates as the dependent variable. In practice, we use the following weighted 2SLS specification:

$$\Delta log\ y^b_{it} = \alpha + \bar{\epsilon}_{l,b} \cdot \Delta\log(1-\tau^l_{it}) + \bar{\epsilon}^{\,+}_{l,b} \cdot \Delta\log(1-\tau^l_{i,t+1}) + \lambda_t + v_{it} \tag{20}$$

$$\Delta\mathbb{1}(y^b_{i,t}>0) = \alpha + \bar{\mu}_{l,b} \cdot \Delta\log(1-\tau^{e,l}_{i,t}) + \bar{\mu}^{\,+}_{l,b} \cdot \Delta\log(1-\tau^{e,l}_{i,t+1}) + \lambda_t + v_{it} \tag{21}$$

Three things are worth noting. First, in the simplest of the worlds, $\bar{\epsilon}_{l,b}$ and $\bar{\mu}_{l,b}$ would reflect the aggregate short-run intensive and extensive margin elasticities to changes in the marginal and effective net-of-tax rates, $\Delta \log(1-\tau_t)$ and $\Delta \log(1-\tau^e_t)$. However, as discussed in Section 2, the reform was announced six months before its approval, giving taxpayers time to adjust their behavior in advance. To account for this, we include two additional terms,

$\Delta \log(1-\tau_{t+1})$ and $\Delta \log(1-\tau^{e}_{t+1})$. The parameters $\bar{\epsilon}_{l,b}$ and $\bar{\epsilon}^{+}_{l,b}$ thus capture concurrent and anticipatory intensive margin elasticities, while $\bar{\mu}_{l,b}$ and $\bar{\mu}^{+}_{l,b}$ capture the corresponding extensive margin semi-elasticities. While we estimate separate anticipatory and concurrent elasticities, it is important to note that they both reflect responses to the same underlying tax change (i.e., the 2012 reform). Hence, we interpret this decomposition mostly as a descriptive exercise to characterize the timing of behavioral responses, rather than as separate estimates of structural parameters. In fact, for welfare analysis, we compute the overall short-run elasticity as the sum of the two terms, since they both represent different parts of the same behavioral response to a single change in the tax schedule.

Second, the endogenous variables $\Delta \log(1-\tau^{l}_{it})$ and $\Delta \log(1-\tau^{e,l}_{it})$ (and their forward terms), are defined as *predicted* changes in net-of-tax rates due to changes in the tax schedule, computed using base-year income. Further interpretation on this is provided in Section 5.3. Since the tax code changed only once between 2009-2015, elasticities are estimated using data from 2011 and 2012. Concurrent elasticity estimates are obtained by comparing changes in reporting decisions from 2011 to 2012 between treated and control taxpayers, scaled by predicted changes in net-of-tax rates over the same period. Anticipatory elasticities correspond to changes in reporting decisions from 2010 to 2011, scaled by predicted changes in tax rates from 2011 to 2012. Hence, technically, the reduced-form equations of this model correspond to Equations (16) and (17) using 2010-2011 and 2011-2012 data. Additionally, we estimate longer-run elasticities with a similar approach, and adding up all 2011-2015 coefficients.

Third, the four excluded instruments are defined as the (reduced-form) interactions of $treat^{MTR}$ and $treat^{ATR}$ with 2011 and 2012 dummies. For example, the instrument for estimating the concurrent intensive margin elasticity, $\bar{\epsilon}_{l,b}$, is $treat^{MTR} \times \mathbb{1}(year = 2012)$. Finally, as in the DiD analysis, elasticity estimates are weighted by income (intensive margin) or revenue (extensive margin), and standard errors are clustered at the individual level.[21]

## 5.3 Interpretation of the estimates

Our estimates should be interpreted as intention-to-treat (ITT) effects. This reflects both our definition of treatment status, which uses data for the whole pre-treatment period, and the use of predicted, rather than realized, changes in net-of-tax rates as the endogenous vari-

[21] Our 2SLS specification uses two instruments, i.e., the treatment indicator interacted with year dummies for 2011 and 2012, both constructed from the same treatment variable. While this implies that the instruments are highly correlated across the panel, it is key to note that they do not activate in the same year. Each endogenous variable is instrumented using variation that is specific to its own period (i.e., the instruments and the corresponding endogenous variable are 0 if it is not the active year). Hence, identification comes from period-specific variation. This is equivalent to estimating two separate 2SLS regressions by year.

ables.[22] An alternative approach would be to define treatment status based on 2011 income and use *observed* changes in net-of-tax rates as the endogenous variable. This would yield local average treatment effect (LATE) estimates for "compliers," i.e., taxpayers who remain in the same bracket before and after the reform (Jakobsen and Søgaard, 2022). However, this approach faces several challenges. Bracket mobility may be common, especially in the presence of mean reversion, and extensive margin responses complicate the analysis even further, since tax rates are undefined for individuals not reporting income in a given base. As a result, observed net-of-tax rate changes may not be defined for some taxpayers, making extensive margin elasticities hard to estimate without strong assumptions.

To maintain consistency across tax bases and margins, we focus primarily on ITT estimates. As emphasized by Kawano et al. (2016) and Kumar and Liang (2020), ITT elasticities are often the most relevant parameters for evaluating policy changes.[23] However, for completeness, we also report intensive margin estimates using actual changes in marginal net-of-tax rates. In this context, ITT elasticities can be viewed as lower bounds for treatment-on-the-treated effects, while estimates based on actual changes may serve as upper bounds. The gap between them reflects the extent of bracket mobility (see Kawano et al. 2016 for further discussion). Although we return to this in the results section, it is worth noting here that ITT and LATE estimates are very similar in our case, suggesting limited bracket mobility. As a result, the distinction between the two becomes less relevant in this setting.

# 6 Results

This section presents our main empirical findings. For each tax base, we begin with graphical evidence supporting the parallel trends assumption, showing the raw evolution of key outcomes for treatment and control groups, alongside the corresponding DiD estimates. For transparency, raw data figures are shown without any adjustments, whereas DiD estimates are based on Equations (16) and (17) and use income or revenue weights depending on the margin. This ensures that the DiD estimates align precisely with the numerators of the Wald estimators in Equations (18) and (19). Following this graphical analysis, we present the estimated intensive and extensive margin elasticities in table format, as defined in Equations (20) and (21). In all cases, we report the reduced-form, first-stage, and (2SLS) elasticity estimates separately. Additional results including unweighted figures, alternative specifications using outcomes in levels, and DiD coefficients are presented in Appendix D through F.

[22] This approach is common in the literature. See, for example, Miao et al. (2024) and Kawano et al. (2016).

[23] Weber (2014a) discusses formally the policy-relevant parameters and how they depend on income dynamics. In general, ITT estimates tend to be closer to the relevant policy parameter than LATE estimates.

## 6.1 Own-Tax Base Responses: Personal Labor Income Tax Base

First, consider responses in the intensive margin. Panel (a) in Figure 3 depicts the evolution of log gross labor income reported to the PLIT base for treatment and control groups separately, while panel (b) depicts the reduced-form DiD coefficients using log-changes as the dependent variable. The graphical analysis suggests three main findings. First, there is no evidence of differential pre-trends, which is critical to validate the DiD approach. While one of the limitations of the TAX data is that it only contains information back to 2009, that both groups show similar growth trends until 2011 is reassuring. Second, a diverging pattern between the two groups arises the year in which the reform is announced. During 2011, the anticipation year, TIEs in the treatment group increase their income reported to the PLIT base. However, in 2012, the year in which the reform is enacted, treated TIEs reduce substantially their reported income relative to TIEs in the control group. Furthermore, the drop observed in 2012 seems larger than the increase observed in 2011, which suggest an overall net negative effect of the reform on PLIT reported income. These dynamics are consistent with previous evidence from Foremny et al. (2019), who show a large anticipatory response by liberal professionals before the implementation of PLIT in 2007. Finally, the effect consolidates relatively quickly: once the negative gap opens up in 2012, it remains constant throughout the years covered in the TAX data. Consistently, all DiD coefficients in the years following 2012 bounce around 0 and are statistically insignificant at a 1% level.[24]

Table 3 reports our baseline elasticity estimates. The table is structured into four main panels: Panel (a) presents reduced-form estimates, Panel (b) reports first-stage coefficients, Panel (c) shows the short-run 2SLS elasticity estimates, and Panel (d) reports the longer-run elasticity. Column (1) in panel (a) documents a concurrent reduced-form effect of $-0.047$ log points ($p$-value $< 0.001$). The anticipation effect in 2011 is also statistically significant ($p$-value $<0.001$), though smaller in magnitude (0.012 log points). This confirms that while there is some evidence of inter-temporal shifting, the concurrent response is larger and more than offsets the anticipated amount. Panel (b) shows that the average predicted change in the marginal net-of-tax rate due to the reform is approximately 0.045 log points ($p$-value $< 0.001$). Using these first-stage estimates, Panel (c) reports two elasticity estimates: the anticipation elasticity, $\bar{\epsilon}_{l,l}^{+} = -0.28$, and the concurrent elasticity, $\epsilon_{l,l}^{t} = 1.05$ ($p$-values $<$ 0.001). Together, these imply a combined short-run elasticity of 0.77 ($p$-value $< 0.001$). In other words, a 1% decrease in the marginal net-of-tax rate reduces reported gross labor income by 0.77%, accounting for both anticipation and concurrent responses. This is our

[24] Note that the DiD specifications use log changes relative to the previous year. Coefficients therefore reflect changes in the gap over time. For example, a negative coefficient in one year followed by a null effect in the following means that the gap opened and then remained stable.

preferred estimate for $\bar{\epsilon}_{l,l}$, as defined in Section 3. Finally, Panel (d) shows that the longer-run intensive margin elasticity is 0.66 ($p$-value=0.024), and very close to the short-run elasticity, as anticipated by the graphical analysis.

As noted in Section 5, our baseline estimates correspond to ITT estimates. In appendix D, we present additional estimates using *observed* marginal net-of-tax rates as the endogenous variable. Despite the conceptual differences between the two approaches, the results are extremely similar. Using *observed* marginal net-of-tax rates we obtain an intensive margin elasticity is 0.74 ($p$-value = 0.007), only slightly smaller than our 0.77 baseline ITT estimate. The fact that these two estimates are so close suggests that bracket mobility is limited in this setting, making the distinction between ITT and TOT less relevant for this context.

Panels (c) and (d) in Figure 3 present our graphical analysis of extensive margin responses. As explained in Section 5, treatment and control groups are defined now by changes in the *effective* tax rate. Given our sample construction, most taxpayers report income to the PLIT base between 2009-2011. However, it is important to highlight that parallel trends are not guaranteed by construction, since the sample is selected based on consistently being in the top 5% of *total* income, rather than PLIT income. This means that individuals may enter or exit the PLIT base across years, as long as their total income remains within the top 5%. Looking at the post-reform period, the share of TIEs reporting to the PLIT income declines gradually over time, but this decline is larger for individuals in the treatment group. Two additional patterns distinguish extensive margin dynamics from intensive margin dynamics. First, there is no clear evidence of anticipatory exits from the PLIT base. Second, although the bulk of the response is observed in 2012, the dynamics of the post-treatment coefficients suggest that the response continues to grow over time. Both patterns are consistent with the idea that leaving a tax base entirely may involve greater frictions than adjusting at the intensive margin, causing extensive margin responses to take longer to build up.

Column (4) of Table 3 reports our baseline estimates for extensive margin elasticities. For the full sample, the reduced-form estimates show that the reform led to a concurrent 3.8 percentage point decrease in the probability of reporting income to the PLIT base (p-value $< 0.001$). Combined with a first-stage estimate of $-0.014$ (p-value $< 0.001$), this implies a large concurrent extensive margin semi-elasticity with respect to the *effective* net-of-tax rate of about 2.69 (p-value $< 0.001$). Estimates for anticipatory responses are close to zero and precisely estimated (-0.047, p-value = 0.406). Together, these effects yield a total short-run elasticity of 2.64 (p-value $< 0.001$), which we take as our preferred estimate of $\bar{\mu}_{l,l}$.[25] As

[25] We recover the scale-up factor needed for welfare analysis, $\bar{\varsigma}_{l,l}$, by taking the ratio of the first-stage coefficient from the extensive margin estimates to the first-stage coefficient from the intensive margin elasticities, $\bar{\varsigma}_{l,l} = \frac{-0.014}{-0.045} = 0.31$. Intuitively, this ratio indicates that for each percentage point increase in the marginal net-of-tax rate, the effective net-of-tax rate rises by 0.31%.

anticipated in the graphical analysis, the response on the extensive margin builds over time, and the longer-run elasticity estimate is 5.08 ($p-value$<0.001). Given these dynamics, our welfare analysis will include sensitivity checks based on different time horizons.[26]

Next, we broadly discuss potential threats to identification and describe a series of robustness tests to assess the validity of our baseline empirical strategy. Appendix D discusses these tests in detail and provides all supporting evidence.

**Endogenous selection into treatment.** Endogenous selection into treatment could be a relevant source of bias in the presence of anticipation effects since TIEs may change their income brackets, and hence their treatment assignment, as a result of anticipatory responses. To test whether this type of behavior explains our responses, we implement a series of robustness checks based on alternative sample criteria that restrict the analysis to taxpayers with more stable income and avoid using 2011 data for treatment assignment. These tests yield results consistent with our baseline, although smaller in magnitude since they are restricted mechanically to TIEs with more stable income. It is worth noting that we still observe anticipatory responses, even when preventing taxpayers from switching brackets in 2011. This suggests that anticipation responses occurs mostly within, rather than across, tax brackets, which is entirely consistent with the progressive nature of the tax schedule

**Extending pre-reform period.** To assess pre-trends beyond the short TAX pre-reform window, we replicate our analysis using SSA data going back to 2000. Despite covering only a subset of gross labor income (i.e., income subject to third-party reporting), these data are useful to analyze the more general income dynamics in a longer period. The analysis using this supplementary data source supports the parallel trends assumption and reinforce our main findings. In addition, we also note that the intensive margin anticipatory response in the *SSA* data goes in the opposite direction to the one observed in the *TAX* data. We attribute this difference to the nature of third-party reporting. If TIEs want to avoid higher future tax rates through inter-temporal shifting, they may prefer using income sources that are less visible to the tax authority on the detriment of third-party reported income.

**Mean reversion.** To rule out concerns about mean reversion more formally, we implement a series of visual and regression-based tests using both TAX and SSA data. In particular, we follow the non-parametric pre-trend validation test proposed by Jakobsen and Søgaard (2022), estimate specifications with pre-*TAX* data income decile fixed effects (Auten and Carroll, 1999; Saez et al., 2012), and rely on alternative samples restricted to taxpayers with more stable pre-reform income. Overall, all these tests support our identification strategy

[26] As discussed in Section 5.3, tax rates are not defined for taxpayers who drop out of the tax base. As a result, we cannot estimate TOT-like elasticities on the extensive margin.

and suggest that mean reversion is not a major concern in our setting.

**Other robustness tests.** Finally, we also conduct a number of additional robustness checks, including specifications with sector and sector-year fixed effects, alternative weighting schemes, winsorizing thresholds, and sample selection criteria (e.g., sample restrictions, exclusion of G3, among others). The results are generally robust across specifications. The only consistent pattern that is worth noting is that magnitude of the response changes depending on the weight put on higher income individuals, which anticipates some of the heterogeneous responses discussed in the next section. When estimating the efficiency costs of the reform in Section 7 we conduct some bounding exercises based on the most extreme cases.

## Heterogeneous Responses

**Higher vs. lower intensity of treatment.** We begin by analyzing whether behavioral responses differ by treatment intensity, leveraging that the tax reform introduced a smaller increase in marginal tax rates in income zone G2 and a larger one in income zone G4. Panels (a) and (b) in Figure 4 present graphical evidence where the lower-intensity group consists of treated TIEs who never entered G4 in the pre-treatment period, while the higher-intensity group includes those who entered G4 at least once. The graphical evidence is striking: most of the observed response is driven by taxpayers in the higher-intensity group, both along the intensive and extensive margins. For instance, the reduced-form DiD coefficients for 2012 intensive margin responses are -0.011 and -0.096 for lower- and higher-intensity groups, respectively, and the difference between these coefficients is statistically significant ($p-value<$ 0.001). Similarly, the extensive margin reduced-form estimates for 2012 are -0.008 and -0.070 ($p-value<$ 0.001). Two additional points are worth noting. First, while we refer to these groups as experiencing lower and higher treatment intensity, this intensity is inherently correlated with income. As a result, we cannot disentangle whether the heterogeneous responses are due to differences in the magnitude of the tax rate change (e.g., Chetty et al. 2011) or to higher-income taxpayers being more sophisticated or facing more or easier margins of adjustment. However, even when we cannot separate these explanations, the observed heterogeneity is informative for understanding the anatomy of the response. Second, our focus here is on reduced-form patterns of heterogeneity rather than estimated elasticities. This is because our baseline elasticity estimates already assign different weights to individuals based on income/revenue, and Equation (15) remains valid even in the presence of heterogeneous behavioral elasticities (Saez et al., 2012). For conciseness, we only report the reduced-form figures in the main text, but full results are reported in Appendix F.

**Wage Earners vs. Self-Employed.** In Panels (c) and (d) in Figure 4, we explore heterogeneous responses by employment type. This is a standard comparison in the literature, as self-employed workers have more flexibility to adjust and typically show greater responses. However, our graphical evidence shows that is not the case and, if anything, top income wage earners exhibit larger responses than self-employed workers. While this may seem counter-intuitive, it reflects our *labor*-focused sample selection criteria. As discussed in Sections 2 and 4, self-employed individuals in Uruguay can opt into corporate income taxation, which applies a flat 25% rate on net profits. Beyond a certain threshold, this regime dominates (the progressive) PLIT, leading many high-income self-employed to shift away from the PLIT base even before the 2012 reform. These are not part of our analysis since they do not report income to the PLIT base. Notably, as shown in Appendix A, they also have significantly higher incomes than the self-employed workers who remain in our analysis sample. The fact that self-employed in our sample did not opt for corporate taxation in the pre-reform period, and earn less on average than those who did, suggest that they may face greater frictions to switch regimes or have weaker incentives to respond. As a result, smaller responses for self-employed workers likely reflect this selection. This concern does not arise for wage earners, who cannot report income to the corporate tax base unless they become self-employed first. In any case, while not negligible, the differences between both groups are substantially smaller than the ones observed when comparing lower- vs. higher-intensity group. Again, since our goal here is to explore potential sources of heterogeneity we focus on reduced-form patterns of heterogeneity. However, for completeness, full results are reported in Appendix F, as well as graphical evidence for the raw data.

## 6.2 Cross-Tax Base Responses: Corporate Income Tax, Capital Income Tax, and Tax Mixes

In Section 6.1 we documented TIEs' intensive and extensive margin responses in the PLIT base. Now, we analyze whether and how TIEs adjusted their reporting behavior to other tax bases such as capital and corporate income tax bases.

### Capital Income Tax

The 2012 tax reform left the capital income tax base unchanged making it relatively more attractive compared to PLIT. We present evidence on TIEs' responses in the capital income tax base using the same approach as for PLIT. Figure 5 depicts graphical evidence on the evolution of capital income by treatment group, while columns (2) and (5) in Table 3 report the estimated cross-base elasticities. First, consider responses on the intensive margin, i.e., changes in capital income among taxpayers who were already reporting to this tax base before the reform. Figure 5 shows significant differences in the evolution of capital income between treatment and control groups in 2010, before the tax reform was even announced, suggesting that the parallel trends assumption may not hold in this case. Furthermore, as shown by the raw data, the large and statistically significant anticipation elasticity is entirely driven by the 2010 differences returning to normal.[27] Outside of these spurious differences, TIEs do not seem to change their capital income reporting behavior in the intensive margin. Even when the point estimates for the elasticities are large (e.g., 2.26), these are very imprecisely estimated (S.E.=1.7, $p - value = 0.200$) due to the small sample size: less than 15% of the TIEs in our analysis sample reported capital income in the pre-reform period.

Responses at the extensive margin are much clearer. First, we find support for the parallel trends assumption. Second, both the graphical evidence and the elasticity estimates suggest that the 2012 tax reform increased the share of taxpayers reporting to the capital income tax base. We estimate a statistically significant 2012 reduced-form effect of 1.1p.p., which implies a short-run extensive margin semi-elasticity of -0.75 ($p - value < 0.001$). In other words, a 1% decrease in the effective net-of-tax PLIT rate increases the probability of reporting any income to the capital income tax base by 0.75p.p.

Two additional findings are worth mentioning. First, most of the adjustment took place in 2012, with some additional effects in 2013, but no clear signs of anticipatory behavior or further adjustments in 2014 or 2015. This translates into a longer-run elasticity of -1.1 ($p$-value = 0.001), which is slightly larger than the short-run elasticity of -0.75 (p-value < 0.001). Second, the reduced-form analysis included in Appendix F on heterogeneous responses suggests that, again, most of the response is driven by the high-intensity treatment group. When looking at differences by employment type, we do not find major differences.

#### Corporate Income Tax

Figure 6 and columns (3) and (6) of Table 3 focus on cross-base responses in the corporate income tax base. As for the capital income tax base, we find no evidence of intensive margin responses. The graphical evidence shows that income reported to this base evolved similarly across groups, and the estimated short-run cross-base intensive margin elasticity is negligible (-0.04 with $p$-value = 0.981). It is important to note that this analysis corresponds to TIEs categorized as self-employed workers, since all TIEs reporting any income to the corporate income tax in the pre-reform period are classified as self-employed. Only 533 TIEs in our analysis sample reported income to this tax base in the pre-treatment period, which limits statistical power and makes the estimates imprecise.

[27] Moreover, the direction of this response goes in the opposite direction of theoretical predictions: treated taxpayers appear to shift away from the capital income tax base in anticipation of the reform.

Regarding extensive margin behavior, our graphical analysis shows evidence of both anticipatory and concurrent responses. Panel (d) in Figure 6 reports reduced-form effects on the probability of reporting to the corporate income tax base of 0.6p.p. and 0.4p.p. ($p$-values $< 0.001$ and $= 0.036$), respectively. These effects translate into a statistically significant short-run semi-elasticity of -0.79 ($p$-value $< 0.001$). This means that a 1% decrease in the effective net-of-tax rate in the PLIT base increases the probability of reporting income to the corporate tax base by 0.79p.p. It is worth noting that our model does not explicitly predict anticipatory cross-base responses. However, one potential explanation for such behavior may be one-time frictions in switching tax bases. In that case, the increase in the effective net-of-tax rate induced by the reform may change shifting decisions for some TIEs, making full shifting the most valuable option. Those who now find this type of shifting behavior optimal may be better off by shifting as early as possible. In terms of timing, most of the response appears concentrated in 2011 and 2012, although the graphical evidence suggests some additional shifting later on. This is reflected in a long-run elasticity of -1.0 ($p$-value $<$ 0.001), which is slightly larger than the short-run estimate of -0.8.

Finally, we also explore heterogeneous effects by intensity of treatment and employment type. Again, we find that most of the response is driven by the higher-intensity treatment group. When examining differential responses by employment type, we now observe that self-employed workers are somewhat more likely to shift to corporate taxation compared to wage earners, but estimates are more imprecisely estimated. In this case, potential larger responses by self-employed are likely explained by differences in the cost of shifting: moving from PLIT to corporate taxation is relatively less costly for self-employed workers, while wage earners would first need to become self-employed in order to switch tax bases. We present additional details, including graphical evidence and full elasticity estimates, in Appendix F.

### Tax Mixes

Overall, the analysis of cross-tax base responses shows that TIEs affected by the reform were more likely to start reporting income to the capital and corporate income tax bases. In this context, it is useful to recall two key predictions from our model. First, the model predicts that an increase in the PLIT marginal tax rate should lead to an unambiguous increase in the share of full shifters, as full shifting becomes more attractive relative to both non-shifting and partial shifting. Second, the model yields an ambiguous prediction regarding partial shifters, due to two opposing forces: on the one hand, partial shifting becomes more attractive relative to non-shifting; on the other hand, it becomes less attractive relative to full shifting.

Figure 7 illustrates how the reform affected these groups, and shows results that are aligned with these predictions. Panel (a) shows DiD coefficients using changes in taxpayer type (non-shifter, partial shifter, full shifter) as the outcome. The figure shows a permanent increase in the share of full shifters at the expense of non-shifters. It also shows a temporary increase in the share of partial shifters during the anticipatory period, likely reflecting short-run frictions in exiting the PLIT base, as discussed earlier. This effect, however, is short-lived and disappears by 2012. Panel (b) examines whether taxpayers shifted differentially to capital versus corporate income but finds no major differences across these two alternatives. Panels (c) and (d) focus on the share of total income reported to each base. Panel (c) shows a clear increase in the average share of capital and corporate income as a percentage of total income. Panel (d) focuses on income shares reported by partial shifters, which reveals a more nuanced picture. Overall, and consistent with the main analysis, these patterns suggest that most of the shifting occurred along the extensive margin, although we cannot rule out that some partial shifters increased income reported to the corporate base.

## 6.3 Other Margins of Response

In this section, we conduct some exploratory analyses to better understand the mechanisms behind these behavioral responses. In particular, we start by providing estimates on changes in total income as a summary measure, and then we briefly discuss whether these can be associated with labor supply responses (e.g. through hours worked, number of employers, and retirement decisions), or other mechanisms that are harder to test in our data. For conciseness, we report the corresponding figures and tables in Appendix G.

**Total Income.** Looking at changes in total income allow us to summarize responses across all the margins analyzed into a single measure. While this approach may inform about the nature of behavioral responses, it is not well suited to evaluate the efficiency and inequality implications of the reform, given that income is taxed differently depending on the tax base. We find that even when the combined effect after adding labor, capital, and corporate income together is about 33% smaller than the response estimated based on labor income alone (5.7%), the 2012 tax reform still led to a 3.8% decline in total income. Appendix G shows that this reduction is, at least in the short-run, mostly driven by total income intensive margin responses. However, it is also worth mentioning that there is a small effect of on the probability of reporting to any tax base in the last two years (for 2015, the point estimate is -0.01.p., $p-value$=0.018). Overall, these results suggest that full shifters do not consider other tax bases not included in our analysis as substitutes for PLIT, at least in the short run. Hence, it is key to understand what drives the total income responses documented in the intensive margin.

**Real labor supply responses.** As discussed in our model, changes in PLIT reporting can result from income shifting, real labor supply adjustments, or tax evasion. We have already discussed the income shifting margin exhaustively. Now, using two supplementary datasets, we conduct some exploratory analysis to study whether real labor supply responses may explain at least part of the intensive margin responses observed in total income.

First, although tax records do not contain information on hours worked, SSA records do. The main concerns with using reported hours in our setting are data quality, the relevance of hours as a margin of adjustment for top earners, and the possibility that observed changes reflect collusive reporting rather than real behavioral responses. While these are valid concerns, SSA records are reported by employers, subject to enforcement, and recent validation exercises (Bergolo et al. 2024, following Lachowska et al. 2022) suggest that the quality of Uruguayan data on hours worked is generally high. Using these data, while still acknowledging all these limitations, we find suggestive evidence that treated TIEs reduced their reported hours worked by around 2% in response to the reform.

As discussed above, part of this response may reflect strategic reporting rather than actual changes in labor supply. In particular, given Uruguay's labor market regulations, which do not allow reductions in compensation without reductions in hours worked, collusive arrangements would mechanically show up in the data as changes in reported hours. Hence, we also explore changes in the number of employers or income sources reported in the SSA. While still a proxy for labor supply, this measure can reflect adjustments in work commitment, especially in settings where hours are less flexible (Tazhitdinova, 2022). Furthermore, responses along this margin are less likely to reflect collusive employer-employee arrangements as they would require full under-the-table payments. Our estimates go in the same direction as for *reported* hours and suggest that TIEs in the treatment group reduced the number of income sources following the 2012 tax reform. This suggests that at least part of the reduction in hours worked could be explained by actual changes in labor supply decisions.

Finally, we also analyze retirement decisions. This is an alternative margin of response for TIEs, particularly for those close to retirement age. For this analysis, we rely on pension tax records. Overall we do not find significant responses, especially in the short run. However, we do estimate a small positive effect on the share of treated TIEs entering the pension system in the last two years of the period. This could account for part of the longer-run drop in total income reporting discussed above, although this should remain mostly speculative.

Acknowledging all the limitations already discussed, a back-of-the-envelope calculation suggests that of the total reduction in labor income (5.7%), roughly 33% ($\frac{5.7\%-3.8\%}{5.7\%}$) can be attributed to income shifting, and about 36% to reductions in *reported* hours worked ($\frac{2.1\%}{5.7\%}$), with at least part of it being explained by actual changes in labor supply. The residual 31% is not accounted for by any the tax bases covered so far in our analysis. We now turn to discuss other potential margins of response that may have been triggered by the reform, evaluate whether they can account for this residual, and assess their implications for welfare analysis.

**Other responses.** First, some dividends are, by law, non-nominative, meaning they are taxed but cannot be attributed to specific individuals, and are therefore excluded from our analysis. While individual-level data is not available, Appendix A presents descriptive evidence on aggregate tax revenue dynamics. Over the period covered by our data, we do not observe substantial changes in revenues from non-nominative dividends. If anything, their relative weight declines compared to nominative dividends. Thus, partial shifting to non-nominative dividends is unlikely to explain a large share of the residual response.

Second, international fiscal migration could be also possible. However, this type of response is more likely to operate through the extensive margin. Given that we do not observe large exits of TIEs from the three tax bases considered, and given the higher financial and administrative costs associated with it, we believe it most likely plays a minor role.

Third, our analysis does not provide evidence on potential changes in profit retention within firms, such as through investments or other forms of non-distributed profits. This is a margin that has been documented in the literature as a possible strategy to avoid both labor income and dividend taxation (Miller et al., 2024; Kopczuk and Zwick, 2020) but we cannot test directly due to the lack of ownership data.[28] However, this margin is already accounted for by changes in the PLIT base, as it represent a shift from wages to non-distributed profits.[29] Hence, even if it is relevant for understanding the specific mechanisms for income shifting, it does not explain the 31% residual response.

Fourth, part of the residual response could reflect outright illegal tax evasion, i.e., a reduction in gross labor income that was previously reported to the PLIT base but is now kept off the books. Unfortunately, we do not have data that allow us to test this mechanism directly. In fact, given the relatively limited state capacity in our setting, we cannot rule out that a significant share of the unexplained response corresponds to evasion.

Finally, it is important to note that lacking information to test for this type of responses limits our understanding of the nature of the behavioral responses. However, from an ef-

[28] It is worth noting that this mechanism does not apply to self-employed individuals who opted into corporate taxation without incorporating (about 80%), as the simplified regime exempts them from dividend taxation to avoid double taxation, similar to the treatment of pass-through entities in the U.S. tax system.

[29] Consider, for example, an owner-manager who reduces their wage and retains income within the firm to avoid both higher PLIT rates and the combined corporate and dividend taxation. In our data, this would appear as a reduction in gross labor income reported to PLIT, with no corresponding increase in other tax bases. Ideally, we would compare the behavior of firms owned by treated and non-treated TIEs, but the lack of ownership links prevents this analysis. This margin includes both TIEs who were already owners of incorporated firms before the reform, and those who incorporated their businesses in response to the reform and retained profits within the newly incorporated firm.

ficiency perspective, treating the entire residual as a revenue loss makes our estimates an upper bound of the efficiency costs. If part of this response instead reflects shifting to other tax bases not captured in our analysis, the actual efficiency costs would be lower than the estimates we report in the following section.

# 7 Welfare and Inequality Implications

## 7.1 Welfare Analysis

In this section we use the elasticities estimated in Section 6 to quantitatively assess the efficiency costs of the tax reform. To make the interpretation easier, we divide Equation (15) by $\frac{dM}{d\tau_L} = Y_l$ to express everything as a percent of the projected effect of the reform.

$$\underbrace{\frac{\frac{dW}{d\tau_l}}{\frac{dM}{d\tau_L}}}_{27.0\%} = -\underbrace{\frac{\tau_l}{1-\tau_l}}_{0.333}\left[\underbrace{\bar{\epsilon}_{l,l}}_{0.770} + \frac{\tau_k Y_k}{\tau_l Y_l} \cdot \underbrace{\bar{\epsilon}_{l,k}}_{0} + \frac{\tau_c Y_c}{\tau_l Y_l} \cdot \underbrace{\bar{\epsilon}_{l,c}}_{0} + \frac{\bar{\tau}_l^e Y_l}{\tau_l Y_l} \cdot \underbrace{\bar{\eta}_{l,l}}_{0.249} + \frac{\bar{\tau}_k^e Y_k}{\tau_l Y_l} \cdot \underbrace{\bar{\eta}_{l,k}}_{-0.125} + \frac{\bar{\tau}_c^e Y_c}{\tau_l Y_l} \cdot \underbrace{\bar{\eta}_{l,c}}_{-.082}\right] \tag{22}$$

Using estimates from our preferred specification, we estimate that the efficiency costs of the reform amount to 27.0% of the projected mechanical increase in tax revenues. As described in Section 3, we set $\tau_l = 0.25$ and use the short-run elasticity estimates reported in Table 3. For intensive margin responses, we use $\bar{\epsilon}_{l,l} = 0.770$, and we ignore cross-base intensive margin responses. For extensive margin responses, $\bar{\eta}_{l,b}$, we use our preferred $\bar{\mu}_{l,b}$ estimates for each tax base: $\bar{\mu}_{l,l} = 2.643$; $\bar{\mu}_{l,k} = -0.746$; and $\bar{\mu}_{l,c} = -0.788$, and we rescale them by $\bar{\varsigma}_{l,l} = \frac{0.014}{0.045} = 0.31$, which corresponds to the ratio of intensive- and extensive-margin first-stage coefficients. We also compute the revenue-equivalent adjustment factors $\frac{\tau_b Y_b}{\tau_l Y_l}$ and $\frac{\bar{\tau} b^e Y_b}{\tau_l Y_l}$ directly from the data, by dividing tax revenues in each base by $\tau_l \times Y_l$, which is the unit in which own-base intensive margin elasticities are expressed.

To analyze the sensitivity of our estimates, we also estimate efficiency losses under alternative scenarios. For instance, we replicate the analysis using longer term elasticities, we use estimates that do not restrict weights, and we incorporate short-run losses in government revenues associated with reduced mandatory social security contributions due to income shifting. Furthermore, we consider even more extreme assumptions such as unrestricted weights for own-base elasticities and setting all shifting elasticities to zero. While our estimates vary in size depending on the specific assumption, the maximum efficiency loss estimated under any of these scenarios is 57.7%, corresponding to the latter. Furthermore, we calculate that for behavioral losses to fully offset the projected increase in tax revenues, own-base intensive and extensive margin elasticities should be 2.5 and 5.5, combined with zero shifting elasticities;

this is a highly unrealistic scenario based on our set of results.

Our welfare analysis has several implications. First, the efficiency costs of taxation are smaller than the projected mechanical increase in tax revenues. This suggests that the new top tax rates remain on the "correct" side of the Laffer curve and that the reform increased tax revenues. Second, the magnitude of the efficiency costs associated with this reform is comparable to estimates from recent tax increases on high-income earners in both developed (e.g., Saez 2017) and developing countries (e.g., Jouste et al. 2024). Third, our results illustrate the importance of conducting an exhaustive analysis across all relevant tax bases to properly estimate the efficiency costs of a reform. For example, ignoring extensive margin responses when focusing only on own-tax base adjustments would lead to an underestimation of welfare losses by roughly 25%. Similarly, if we had focused on intensive and extensive responses within the PLIT base *only*, we would have overestimated efficiency losses by about 26% relative to what we estimate when also adding income shifting responses.

## 7.2 Inequality Analysis

Finally, it is worth exploring how the reform affected income inequality itself. Changes in inequality may operate through two distinct channels: mechanical and behavioral. On the mechanical side, a more progressive tax structure leads to lower post-tax inequality, absent any behavioral responses. However, behavioral responses, such as changes in labor supply or evasion decisions, also change the pre- and post-tax income distribution. To capture both effects, we implement a very simple microsimulation model that accounts for these behavioral responses. Using our elasticity estimates, we simulate counterfactual pre- and post-tax income vectors under reform and no-reform scenarios, assigning individual-level responses based on estimated probabilities derived from observed behavior and individual characteristics. As our goal is to illustrate how inequality may have changed across the full income distribution, we pool all individuals in the administrative records (TIEs and non-TIEs) with individuals with informal or zero income by combining our TAX records with data from Burdín et al. (2022), which provide income estimates for the entire adult population based on household survey data (further details in Appendix H).

Table 4 presents a series of estimates that analyze these potential effects under three alternative scenarios for labor and total income. First, rows (a) and (d) assume that all behavioral responses that are not attributed to income shifting correspond to a reduction in labor supply. Focusing on total income, our estimates suggest that in this extreme scenario the 2012 reform led to a small reduction in the total income post-tax Gini of 0.002, and a 0.3p.p. reduction in the top 1% share, compared to the without reform scenario. This reduction is due to a combination of three features: 1) a more progressive PLIT structure, 2) a reduction in pre-tax income inequality due to the negative effect on labor earnings (as reflected by estimates for pre-tax labor income), 3) higher-income earners responding the most. Second, in rows (b) and (e), we consider the polar opposite, and attribute all non-shifting responses to tax evasion. In this case, the reform increases both the post-tax total income Gini and top 1% share, as there are no changes in pre-tax labor income, and all behavioral responses imply shifting away from a progressive tax base, to tax bases with lower and flat rates, or even tax evasion. Finally, in rows (c) and (f) we model our back-of-the-envelope estimate discussed in Section 6.3, and split the reduction in labor income as 33% of income shifting, 36% as labor supply, and 31% as tax evasion. Our estimates show, if anything, a very small reduction in income inequality, but mostly negligible. For instance, post-tax Gini index goes from 0.548 to 0.547, and post tax top 1% share goes from 13.5% to 13.4%. These effects are more muted compared to scenario 1, as labor income responses, which are a key driver of inequality reduction, are half of the size.

The main takeaway from this simple exercise is that the redistributive impact of progressive tax reforms hinge on: (1) the magnitude of real labor supply responses, and (2) the availability and incentives for income shifting, including the relative advantages of alternative bases, and outright evasion. Importantly, across all scenarios fiscal externalities can significantly erode the reform's intended redistributive effects.

# 8 Conclusions

Using a unique policy experiment induced by a tax reform in Uruguay and granular individual-level tax records, this paper analyzes the behavioral responses of TIEs to taxation within a unified theoretical and empirical framework. We start from a simple but broad theoretical model and show that the efficiency costs of a tax reform depend on intensive and extensive margin elasticities across tax bases. We then use a difference-in-differences design to estimate the effects of a reform that increased marginal tax rates for individuals in the top 1% of the labor income distribution. Leveraging the richness of our data, we examine behavioral responses across both margins and tax bases and provide empirical estimates for the key elasticities derived in our model. These estimates are then used to quantify the reform's efficiency costs and its effects on inequality. Our results show that TIEs respond strongly to increases in marginal tax rates, with aggregate elasticities of 0.77 at the intensive margin and 2.64 at the extensive margin. We document substantial inter-temporal shifting and find that higher-income individuals are the most responsive. On the cross-base margin, we observe that much of the extensive margin response can be explained by full shifting to capital and corporate tax bases, with extensive margin elasticities of -0.79 and -0.75, respectively. We estimate that the reform increased tax revenues, though with efficiency costs amounting to 27% of the projected mechanical gain. However, the reform had limited effects on income concentration, primarily due to its narrow scope and the availability of income-shifting opportunities and evasion into lower-taxed, flat-rate tax bases.

It is important to acknowledge several limitations of our study. First, our analysis of inequality effects depends strongly on whether reductions in reported labor income reflect real labor supply responses or outright evasion. While we provide some exploratory evidence, we recognize the limitations of using reported hours from administrative data and highlight the need for further research, particularly on collusive employer-employee arrangements (Bjørneby et al., 2021). Second, our research design identifies behavioral responses in the short to medium run but is not suited to capture long-run effects, such as those related to career progression, entrepreneurship, or innovation (Best and Kleven, 2012; Akcigit et al., 2022). Third, we do not explore the bargaining margin, which may be particularly relevant for top managers; tax changes could affect their bargaining power or the private returns from rent-seeking within firms (Piketty et al., 2014; Rothschild and Scheuer, 2016). These caveats point to promising avenues for future work. Studying the response of TIEs to income and wealth taxation within a unified framework or examining how individual characteristics and social preferences shape behavioral responses could also yield important insights. Making further progress in understanding TIEs' behavior also requires better data transparency and closer collaboration between researchers and tax administrations.

Our findings have important policy implications for the debate on optimal and efficient tax designs. In particular, they highlight that increasing top marginal tax rates as a way to reduce income inequality may require additional efforts to limit income shifting across tax bases. Otherwise, the potential for redistribution may remain limited despite higher revenues. Given the scale of evasion/avoidance responses, tax authorities may prioritize improving administrative capacity, closing loopholes, and limiting arbitrage across tax bases. Furthermore, our results are particularly important for low- and middle-income countries, where taxing TIEs and wealth holders is particularly challenging. Despite weaker enforcement capacities, TIEs in these contexts appear to have access to sophisticated tax-planning strategies comparable to those in advanced economies (Londoño-Vélez and Avila-Mahecha, 2024). These countries face the challenge of strengthening fiscal capacity and increasing tax progressivity without introducing large distortions to economic incentives.

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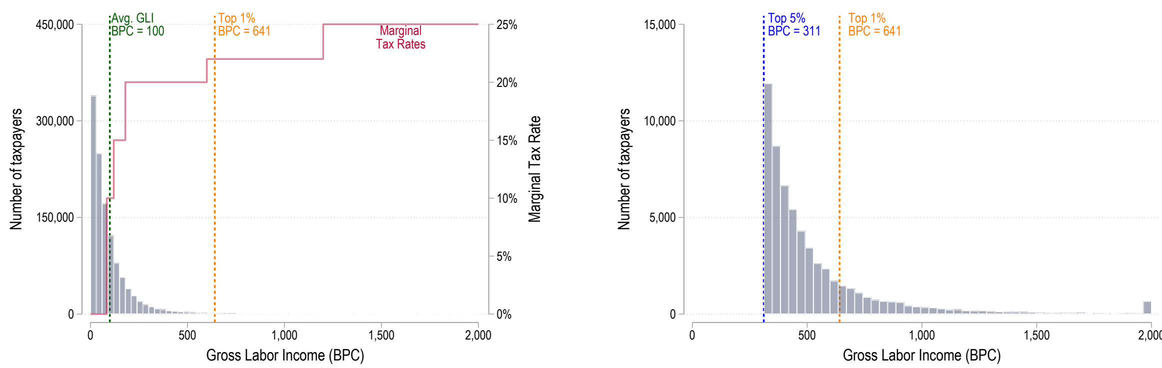
# Figures

Figure 1: Gross Labor Income (GLI) Distribution and the 2011 PLIT schedule (2010)

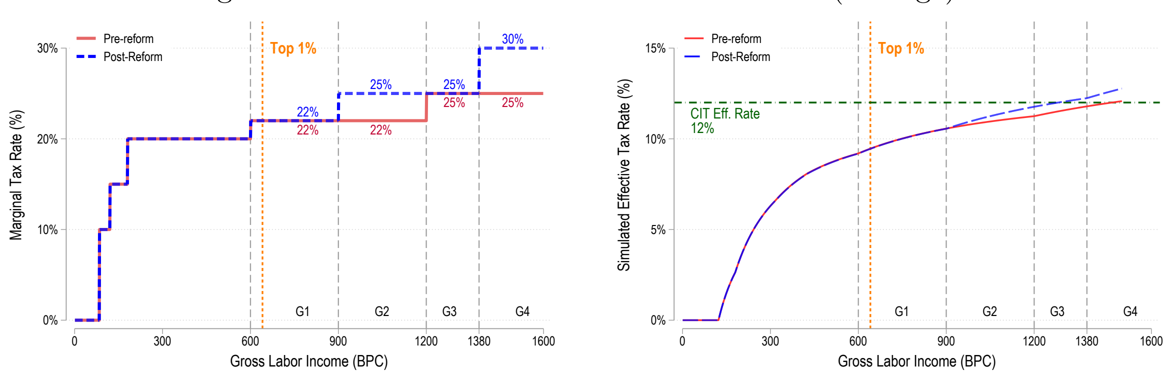
a. Full Range b. Top 5%




Notes: This figure illustrates the 2010 gross labor income distribution (GLI) in the *TAX* sample (blue bars) for those who reported positive values. GLI is expressed in BPC, which is the monetary defined for the tax/deduction schedule. For reference purposes, the BPC/USD exchange rate in 2011 was 1 BPC = 117 USD, and in 2011 the PPP conversion factor was 0.8. In panel (a), the figure includes the full support of the gross labor income variable with winsoring at 2,000 BPC. In addition, the figure also reports the marginal tax rates for the PLIT schedule in the pre-reform period. These are represented by the step-wise solid red lines. Vertical green and orange dashed lines represent the average gross labor income and percentile 99th as a reference. Panel (b) zooms into the top 5% of the GLI distribution. For reference, the blue vertical dashed line informs the value of percentile 95th.

Figure 2: Tax Variation Created by the 2012 Tax Reform

a. Marginal Tax Rates b. Effective (average) Tax Rates

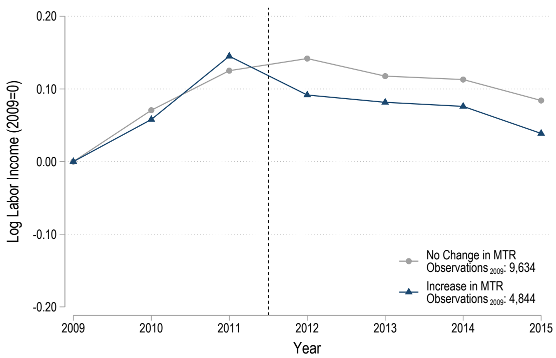



Notes: This figure shows the tax variation in marginal and effective tax rates induced by the 2012 tax reform. All panels depict gray dashed vertical lines that correspond to the four income zones described in Section 2 denoted as G1:G4. Panel (a) shows PLIT marginal tax rates before and after the reform. The dashed blue line indicates the pre-reform rates, while the red solid line indicates the post-reform rates. Panel (b) does the same for the (simulated) PLIT effective tax rate. For comparison purposes we also report the effective corporate tax rate (dashed green line). The simulated scenario illustrates effective rates for different values of gross labor income. Effective tax rates are simulated based on the following assumptions: mechanical deductions at a 15% rate (e.g., payroll taxes) and itemized deductions corresponding to one child. All values are expressed in BPC. For reference purposes, the BPC/USD exchange rate in 2011 was 1 BPC = 117 USD, and in 2011 the PPP conversion factor was 0.8.

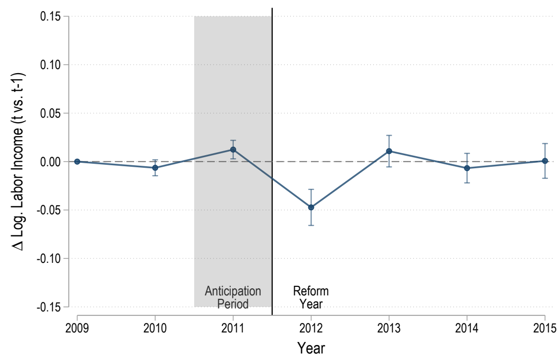
## Figure 3: Own-Tax-Base Responses: Graphical Evidence

### Intensive Margin: Log. Gross Labor Income

a. Normalized Raw Trends (2009=0)

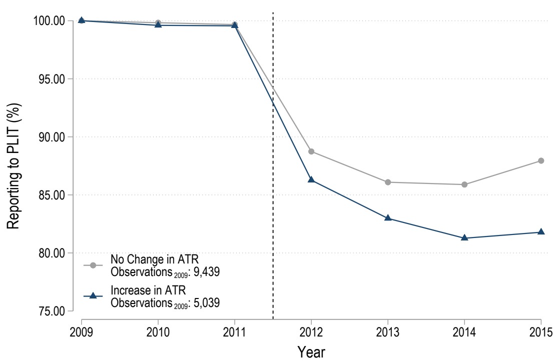



b. DiD: $\Delta\ log\ y^l_{i,t}$

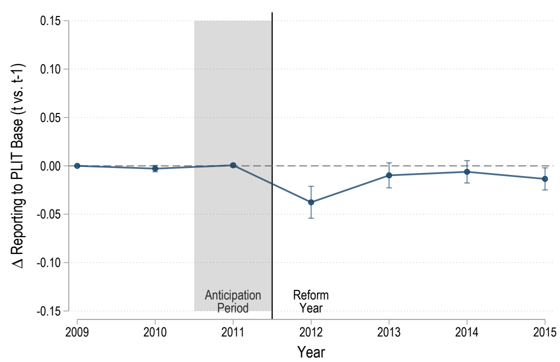



### Extensive Margin: Reporting to PLIT base

c. Raw Trends




d. DiD: $\Delta\mathbb{1}(y^l_{i,t} > 0)$

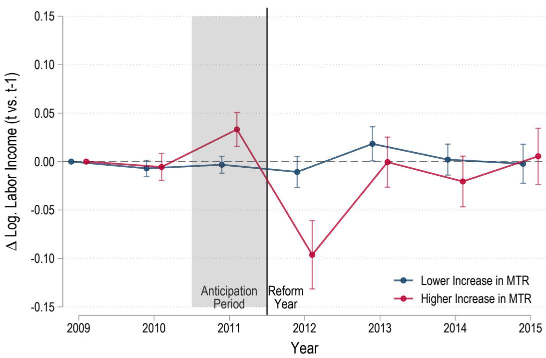



Notes: This figure illustrates the own-tax base reduced-form effects of the 2012 tax reform. Panels (a) and (b) focus on intensive margin responses. Panel (a) depicts the raw evolution of log gross labor income from 2009 to 2015, normalized to 2009 values, for treated and control TIEs without further adjustments. Estimates in blue represent TIEs in the treatment group, defined in Section 5 as $treat^{MTR} = 1$, and estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (b) reports dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change in gross labor income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. As described in Section 5, these correspond to income-weighted estimates, described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (c) and (d) focus on extensive margin responses. Panel (c) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the PLIT base, with the number of observations in 2009 shown in the bottom right corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (d) reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^l_{i,t} > 0)$, with $\mathbb{1}(y^l_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT base. As such, $\Delta\mathbb{1}(y^l_{i,t} > 0)$ takes values -1, 0, or 1. As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in columns (4) and (8) of Table D.1, Appendix D.

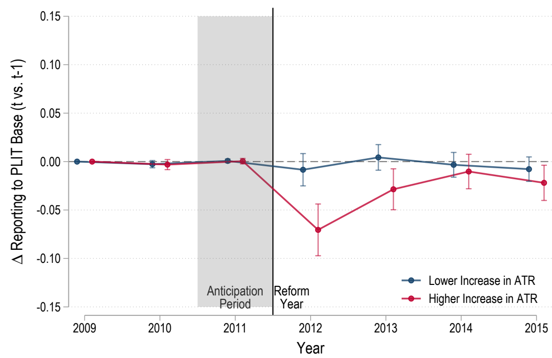
Figure 4: Own-Tax-Base Heterogeneous Responses: Graphical Evidence

By Treatment Intensity

a. DiD: $\Delta \; log \; y^l_{i,t}$

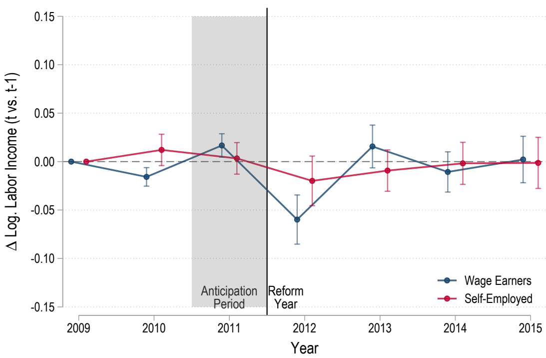



b. DiD: $\Delta \mathbb{1}(y^l_{i,t} > 0)$

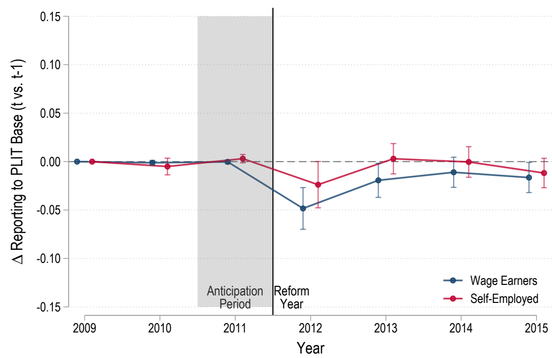



By Employment Type

c. DiD: $\Delta \; log \; y^l_{i,t}$




d. DiD: $\Delta \mathbb{1}(y^l_{i,t} > 0)$

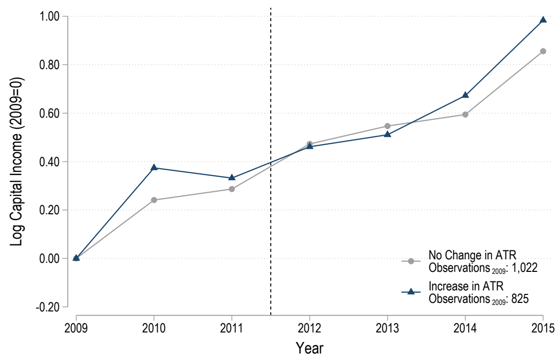



Notes: This figure illustrates own-base heterogeneous responses along two dimensions of heterogeneity: intensity of treatment and employment type. Panels (a) and (b) report the dynamic DiD coefficients for intensive and extensive margin outcomes, respectively, splitting the sample by intensity of treatment. As defined in Section 6, the higher-intensity group includes treated TIEs who entered G4 at least once during the pre-treatment period, while the lower-intensity group includes treated TIEs who never entered G4. Econometric specifications used to estimate coefficients reported in panels (a) and (b) are based on Equations (16) and (17), using the exact same specifications described for panels (b) and (d) in the notes for Figure 3. The only difference is that we now split $treat^{MTR}$ and $treat^{ATR}$ into three categories: control, lower-treatment (blue), and higher-treatment intensity (red). Hence, we use the control group as the reference group. Panels (c) and (d) present analogous estimates for wage earners (blue) and self-employed TIEs (red). Wage earners are defined as individuals with no self-employment income in 2009-2010, while self-employed workers report any self-employment income in that period. In this case, we estimate Equations (16) and (17) separately for both groups and report the corresponding estimates jointly in the figure. As in the baseline analysis, estimates are weighted using income or revenue weights, depending on the margin. Confidence intervals are reported at the 99% level, with standard errors clustered at the individual level. The vertical black line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, including figures for the raw data, are reported and discussed in detail in Appendix F.

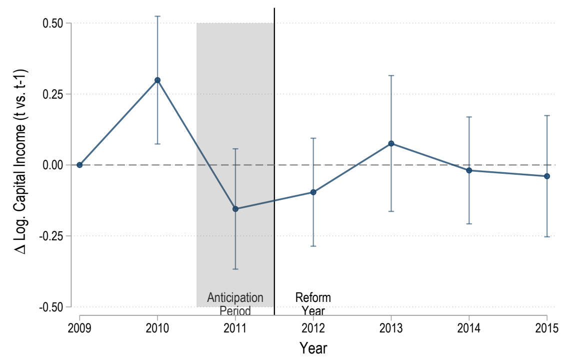
Figure 5: Cross-Tax Base Responses - Personal Income Tax on Capital Base

Intensive Margin: Log. Capital Income

a. Normalized Raw Trends (2009=0)

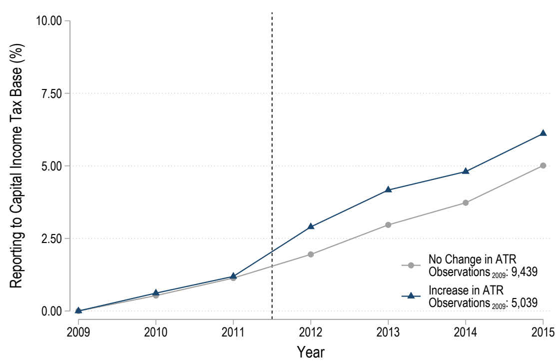



b. DiD: $\Delta\ log\ y^k_{i,t}$

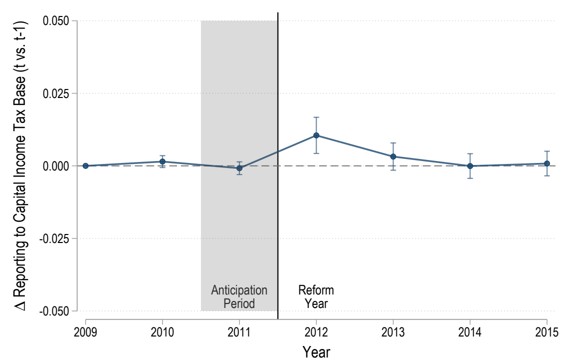



Extensive Margin: Reporting to Capital Income Base

c. Raw Trends




d. DiD: $\Delta \mathbb{1}(y^k_{i,t} > 0)$

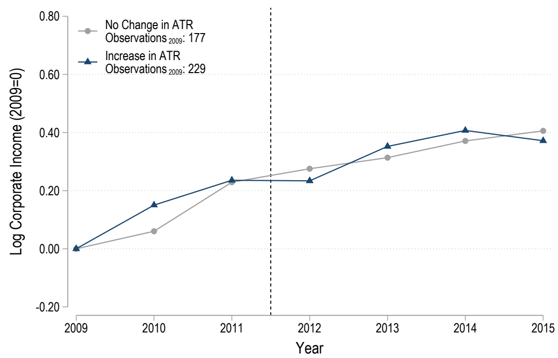



Notes: This figure illustrates the cross-tax base reduced-form effects of the 2012 tax reform on the capital income tax base. Panels (a) and (b) focus on intensive margin responses. Panel (a) depicts the raw evolution of log capital income from 2009 to 2015, normalized to 2009 values, for treated and control TIEs without further adjustments. Estimates in blue represent TIEs in the treatment group, defined in Section 5 as $treat^{MTR} = 1$, and estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (b) reports dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change in capital income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. As described in Section 5, these correspond to income-weighted estimates, where weights are computed as described in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (c) and (d) focus on extensive margin responses. Panel (c) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the capital income tax base, with the number of observations in 2009 shown in the bottom right corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (d) reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^k_{i,t} > 0)$, with $\mathbb{1}(y^k_{i,t} > 0)$ indicating whether a taxpayer reports any income to the corporate income tax base. As such, $\Delta\mathbb{1}(y^k_{i,t} > 0)$ takes values -1, 0, or 1. As described in Section 5, these are revenue-weighted estimates. Weights are described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in columns (4) and (8) of Table E.1, Appendix E.

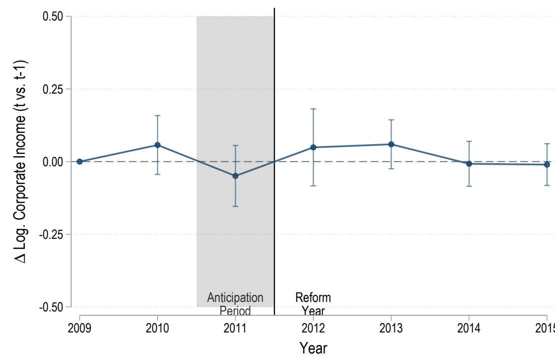
Figure 6: Cross-Tax Base Responses - Corporate Income Tax

Intensive Margin: Log. Corporate Income

a. Normalized Raw Trends (2009=0)

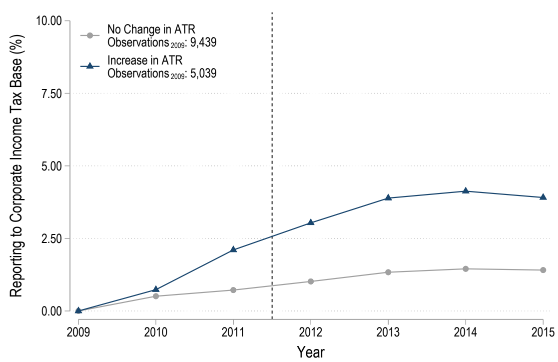



b. DiD: $\Delta\ log\ y^c_{i,t}$

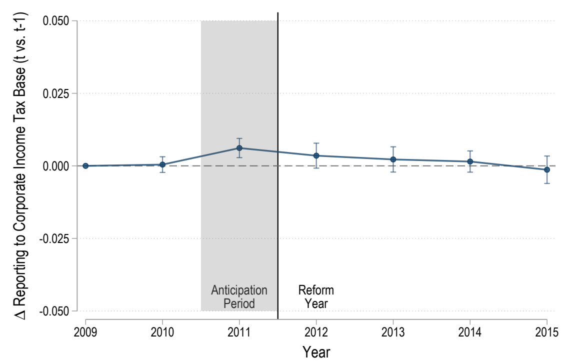



Extensive Margin: Reporting to Corporate Income Base

c. Raw Trends




d. DiD: $\Delta\mathbb{1}(y^c_{i,t} > 0)$

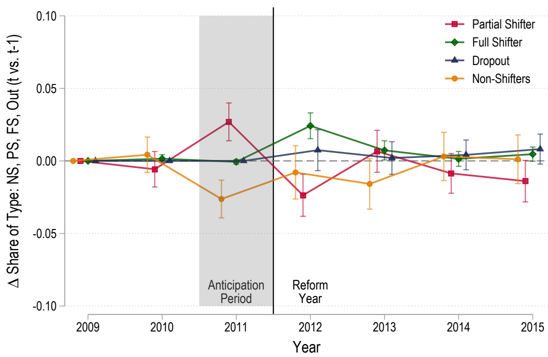



Notes: This figure illustrates the cross-tax base reduced-form effects of the 2012 tax reform on the corporate income tax base. Panels (a) and (b) focus on intensive margin responses. Panel (a) depicts the raw evolution of log corporate labor income from 2009 to 2015, normalized to 2009 values, for treated and control TIEs without further adjustments. Estimates in blue represent TIEs in the treatment group, defined in Section 5 as $treat^{MTR} = 1$, and estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (b) reports dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change in corporate income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. As described in Section 5, these correspond to income-weighted estimates, where weights are computed as described in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (c) and (d) focus on extensive margin responses. Panel (c) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the corporate income tax base, with the number of observations in 2009 shown in the bottom right corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (d) reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^c_{i,t} > 0)$, with $\mathbb{1}(y^c_{i,t} > 0)$ indicating whether a taxpayer reports any income to the corporate income tax base. As such, $\Delta\mathbb{1}(y^c_{i,t} > 0)$ takes values -1, 0, or 1. As described in Section 5, these are revenue-weighted estimates. Weights are described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in columns (4) and (8) of Table E.3, Appendix E.

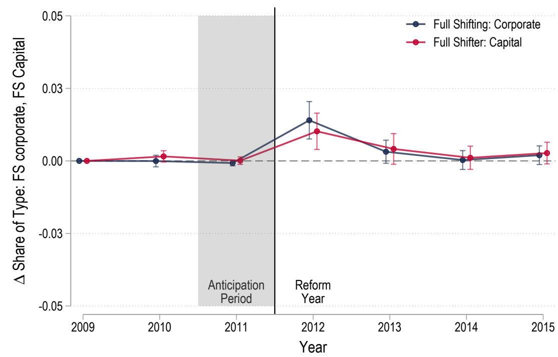
Figure 7: Types of Taxpayers and Share of Income Reported to Each Tax Base

## Transitions between Taxpayer Types

### a. Any Type

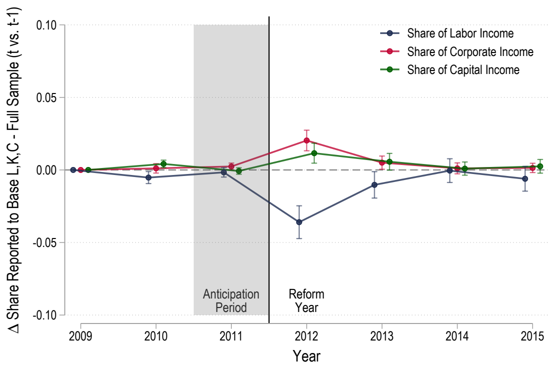



### b. Full Shifters Detailed

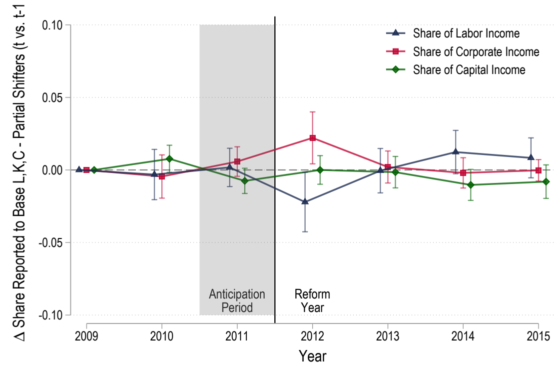



## Changes in Shares Reported to Each Tax Base

### a. Tax Mixes (All Taxpayers)




### b. Tax Mixes (Partial Shifters)




Notes: This figure illustrates how the reform affected transitions of TIEs across different types of tax mixes. In all panels, we use the dynamic DiD specification for extensive margin responses described in Equation (17), applying revenue weights as detailed in Appendix C. Panel (a) reports DiD coefficients using changes in taxpayer type as the outcome variable. Estimates are based on four separate regressions using as outcome $\Delta\mathbb{1}(type^{j}_{i,t} = 1)$, where $j$ corresponds to non-shifter, partial shifter, full shifter, or dropout (i.e., not reporting income to any of the labor, capital, or corporate tax bases). Panel (b) replicates a similar analysis for full shifters, distinguishing between corporate (blue) or capital (red) income tax bases. Panel (c) estimates separate regressions for three different outcome variables measuring the share of total income reported to each tax base: labor (blue), corporate (red), and capital (green). Panel (d) replicates the specification in panel (c) but restricts the sample to taxpayers classified as partial shifters. 99% percent confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records.

# Tables

Table 1: Descriptive Statistics

| | Universe Total Inc. > 0 (1) | Top 1% Total Income (2) | Top 1% Labor Income (3) | Analysis Sample: Pool (G1:G4) (4) | Analysis Sample: Control (G1 or G3) (5) | Analysis Sample: Low Treatment (G2) (6) | Analysis Sample: High Treatment (G4) (7) |
|---|---|---|---|---|---|---|---|
| **a. Income Variables:** | | | | | | | |
| Labor Income (BPC) | 96.29 | 810.35 | 833.38 | 852.53 | 659.74 | 957.01 | 1798.36 |
| | (0.145) | (5.789) | (5.657) | (4.192) | (1.382) | (3.138) | (24.700) |
| Labor Income (% reporting) | 96.43 | 90.50 | 95.35 | 99.75 | 99.80 | 99.69 | 99.56 |
| | (0.017) | (0.213) | (0.153) | (0.041) | (0.045) | (0.098) | (0.164) |
| PLIT Tax Liability (BPC) | 5.29 | 122.60 | 125.14 | 126.17 | 87.51 | 143.10 | 323.88 |
| | (0.024) | (1.187) | (1.174) | (0.938) | (0.338) | (0.888) | (5.820) |
| Capital Income (BPC) | 4.88 | 175.54 | 23.18 | 21.89 | 14.24 | 25.98 | 59.49 |
| | (0.405) | (26.254) | (1.967) | (2.293) | (2.434) | (5.452) | (9.605) |
| Capital Income (% reporting) | 5.40 | 21.65 | 13.25 | 13.49 | 11.48 | 16.18 | 20.11 |
| | (0.020) | (0.299) | (0.246) | (0.284) | (0.325) | (0.647) | (1.001) |
| Corporate Income (BPC) | 2.45 | 139.49 | 16.46 | 18.57 | 12.57 | 29.98 | 31.60 |
| | (0.090) | (5.742) | (1.187) | (1.268) | (1.231) | (3.300) | (5.613) |
| Corporate Income (% reporting) | 0.31 | 10.85 | 2.97 | 3.39 | 2.40 | 5.37 | 5.35 |
| | (0.005) | (0.226) | (0.123) | (0.150) | (0.156) | (0.396) | (0.562) |
| Total Income (BPC) | 103.62 | 1125.38 | 873.03 | 892.98 | 686.55 | 1012.97 | 1889.44 |
| | (0.441) | (27.087) | (6.171) | (5.064) | (3.051) | (7.044) | (27.424) |
| Only reports to PLIT (%) | 94.84 | 74.10 | 88.89 | 88.60 | 90.53 | 85.89 | 82.50 |
| | (0.020) | (0.318) | (0.228) | (0.264) | (0.298) | (0.612) | (0.948) |
| Reports to PLIT and other (%) | 1.64 | 15.97 | 10.32 | 11.14 | 9.27 | 13.80 | 17.00 |
| | (0.011) | (0.266) | (0.221) | (0.262) | (0.295) | (0.606) | (0.938) |
| Reports only to other (%) | 3.53 | 9.92 | 0.79 | 0.26 | 0.20 | 0.31 | 0.50 |
| | (0.017) | (0.217) | (0.064) | (0.042) | (0.045) | (0.098) | (0.176) |
| **b. Individual Characteristics:** | | | | | | | |
| Age | 39.98 | 49.67 | 48.98 | 49.24 | 48.51 | 50.08 | 51.88 |
| | (0.013) | (0.081) | (0.072) | (0.077) | (0.093) | (0.161) | (0.230) |
| Female (%) | 44.92 | 27.34 | 27.55 | 28.77 | 32.70 | 24.40 | 14.01 |
| | (0.045) | (0.324) | (0.325) | (0.376) | (0.478) | (0.755) | (0.866) |
| Wage-Earners (%) | 90.70 | 59.18 | 62.39 | 66.00 | 68.41 | 58.77 | 66.13 |
| | (0.026) | (0.357) | (0.352) | (0.394) | (0.474) | (0.865) | (1.181) |
| Only Self-Employed (%) | 2.32 | 7.37 | 4.79 | 3.54 | 3.80 | 3.46 | 2.18 |
| | (0.014) | (0.190) | (0.155) | (0.154) | (0.195) | (0.321) | (0.364) |
| Both W.E. and S.E. (%) | 2.15 | 24.75 | 23.73 | 25.25 | 22.75 | 32.15 | 26.34 |
| | (0.013) | (0.314) | (0.309) | (0.361) | (0.427) | (0.821) | (1.099) |
| Missing/N.A. (%) | 4.83 | 8.70 | 9.09 | 5.20 | 5.03 | 5.62 | 5.35 |
| | (0.019) | (0.205) | (0.209) | (0.185) | (0.223) | (0.405) | (0.562) |
| No employer/NA (%) | 9.05 | 14.93 | 12.18 | 7.52 | 7.21 | 8.00 | 8.41 |
| | (0.026) | (0.259) | (0.238) | (0.219) | (0.264) | (0.477) | (0.693) |
| One job (%) | 70.27 | 49.05 | 49.36 | 51.77 | 54.65 | 44.29 | 49.56 |
| | (0.041) | (0.363) | (0.363) | (0.415) | (0.507) | (0.873) | (1.248) |
| Two jobs (%) | 15.28 | 15.02 | 15.51 | 15.91 | 15.97 | 16.12 | 15.07 |
| | (0.032) | (0.260) | (0.263) | (0.304) | (0.373) | (0.646) | (0.893) |
| Three or more jobs (%) | 5.40 | 20.99 | 22.95 | 24.80 | 22.16 | 31.59 | 26.96 |
| | (0.020) | (0.296) | (0.306) | (0.359) | (0.423) | (0.817) | (1.108) |
| N | 1,229,033 | 18,930 | 18,930 | 14,478 | 9,634 | 3,238 | 1,606 |

Notes: This table reports descriptive statistics for the main samples used throughout the paper, based on 2010 *TAX* records. Column (1) includes statistics for the full universe of taxpayers in the *TAX* records, defined as individuals with positive total income in at least one year between 2009 and 2011, where total income is the sum of income reported to the PLIT, corporate, and capital income tax bases. Column (2) focuses on individuals in the top 1% of the total income distribution, while column (3) focuses on those in the top 1% of the gross labor income distribution. The 18,930 TIEs in the top 1% of the labor income distribution are defined as labor income earners with at least one year of labor income above 600 BPC between 2009 and 2011. To define the top 1% of total income, we rank all individuals by total income and select the 18,930 with the highest values. Columns (4) through (7) focus on our final analysis sample: the subset of TIEs who (a) never fell below 300 BPC in total income (roughly the 95th percentile of gross labor income), (b) did not switch income zones (G1-G4) every year between 2009 and 2011, and (c) did not experience extreme income growth in the pre-treatment period (greater than 100%). These filters yield a final sample of 14,478 TIEs. Columns (5) to (7) further split this sample using the more detailed definition of $treat^{MTR}$: column (5) includes individuals who never entered G2 or G4 between 2009 and 2011; column (6) includes those who entered G2 but never G4; and column (7) includes TIEs who entered G4 at least once. Panel (a) reports averages for income variables. Panel (b) summarizes individual characteristics, including age, gender, employment type, and number of income sources. Standard errors of the means are reported in parentheses.

Table 2: Income Taxation to Individuals (2011)

**a. Personal Labor Income Tax (PLIT)**

| Income Range | Mg. Rate | Deduction Range | Mg. Rate |
|---|---|---|---|
| 0-84 | 0% | 0-36 | 10% |
| 84-120 | 10% | 36-96 | 15% |
| 120-180 | 15% | 96-516 | 20% |
| 180-600 | 20% | 516-1,116 | 22% |
| 600-1,200 | 22% | Over 1,116 | 25% |
| Over 1,200 | 25% | | |

**b. Capital Income Tax**

| Income source | Statutory tax rate | Effective tax rate |
|---|---|---|
| Interests from deposits | 3% | 3% |
| Dividends and other financial incomes | 7% | 30% |
| Real estate rent | 12% | 12% |

**c. Corporate Income Tax**

| Annual corporate income | Statutory tax rate | Effective tax rate |
|---|---|---|
| 0-4,020 (presumptive) | 25% | 12% |
| 0-4,020 (real) | 25% | Depends on costs |
| Over 4,020 | 25% | Depends on costs |

Notes: This table reports the 2011 (i.e., pre-reform) statutory tax rates faced by taxpayers by source of income. Panel (a) shows the income/deductions brackets jointly with the marginal tax/deduction rates faced by taxpayers reporting income to the PLIT base. Panel (b) shows the statutory and effective tax rates for the capital income tax base. Corporate income effective tax rate in the presumptive regime is computed as $25\% \times 48\%$, where 25% corresponds to the statutory corporate income tax rate and 48% corresponds to the presumptive net profits established by the tax code. In the tax records, more than 80% of self-employed opting into corporate taxation do it in the presumptive regime. For dividends, we compute the effective tax rate after accounting for corporate income tax payments, since dividends and other financial income are taxed at the corporate level before being redistributed to individuals. We do this by applying the 7% capital income rate to the net income after paying 25% corporate income taxes. The assumption here is that firms distributing dividends pay corporate tax in the real regime (25% on firms' profits), and they pass through the full 25% tax rate to the income that is being shifted. For instance, if pre-tax earnings are $100 and want to be collected as dividends, then $25 ($0.25 \times 100$) correspond to corporate income tax payments, and 5.25 correspond to capital income tax payments ($0.07 \times 75$). $25 + 5.25$ is the effective amount of taxes paid if earnings are collected as dividends, which represent an effective tax rate of 30.25%. In tax records, 67% of the firms that file corporate tax returns do it in the real regime. Finally, panel (c) shows the statutory (and effective) tax rates for the corporate income tax schedule. All values are expressed in BPC, which is the monetary defined for the tax/deduction schedule. For reference purposes, the BPC/USD exchange rate in 2011 was 1 BPC = 117 USD, and in 2011 the PPP conversion factor was 0.8.

Table 3: Own- and Cross- Tax-Base Elasticity Estimates

| | Intensive Margin Elasticities | | | Extensive Margin Elasticities | | |
|---|---|---|---|---|---|---|
| | Labor (1) | Capital (2) | Corporate (3) | Labor (4) | Capital (5) | Corporate (6) |
| | **Panel a. Reduced-Form Estimates** | | | | | |
| | Dep. Var.: $\Delta$ $log$ $y_i^b$ | | | Dep. Var.: $\Delta \mathbb{1}(y_i^b > 0)$ | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.047*** | -0.096 | 0.049 | -0.038*** | 0.011*** | 0.004** |
| | (0.007) | (0.074) | (0.051) | (0.006) | (0.002) | (0.002) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.012*** | -0.155* | -0.049 | 0.001 | -0.001 | 0.006*** |
| | (0.004) | (0.082) | (0.041) | (0.001) | (0.001) | (0.001) |
| | **Panel b. First-Stage Estimates** | | | | | |
| | Dep. Var.: $\Delta$ $log$ $(1-\tau^l)$ $\Delta^+$ $log$ $(1-\tau^l)$ | | | Dep. Var.: $\Delta$ $log$ $(1-\tau^{e,l})$ $\Delta^+$ $log$ $(1-\tau^{e,l})$ | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.045*** | -0.042*** | -0.034*** | -0.014*** | -0.013*** | -0.013*** |
| | (0.000) | (0.002) | (0.002) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.044*** | -0.042*** | -0.035*** | -0.013*** | -0.012*** | -0.012*** |
| | (0.000) | (0.002) | (0.002) | (0.000) | (0.000) | (0.000) |
| | **Panel c. 2SLS Estimates** | | | | | |
| | Dep. Var.: $\Delta$ $log$ $y_i^b$ | | | Dep. Var.: $\Delta \mathbb{1}(y_i^b > 0)$ | | |
| (a): $\Delta$ $log$ $(1-\tau^l)$ | 1.054*** | 2.265 | -1.454 | 2.689*** | -0.814*** | -0.271** |
| | (0.161) | (1.738) | (1.532) | (0.454) | (0.187) | (0.129) |
| (b): $\Delta^+$ $log$ $(1-\tau^l)$ | -0.283*** | 3.658* | 1.411 | -0.047 | 0.068 | -0.518*** |
| | (0.084) | (1.946) | (1.169) | (0.056) | (0.072) | (0.108) |
| (a) + (b) | 0.770*** | 5.923** | -0.043 | 2.643*** | -0.746*** | -0.788*** |
| | (0.169) | (2.600) | (1.805) | (0.457) | (0.194) | (0.160) |
| | **Panel d. Medium-Run 2SLS Estimates** | | | | | |
| | Dep. Var.: $\Delta_{15,10}$ $log$ $y_i^b$ | | | Dep. Var.: $\Delta_{15,10} \mathbb{1}(y_i^b > 0)$ | | |
| $\Delta_{15,10}$ $log$ $(1-\tau^l)$ | 0.663** | 5.451 | -1.209 | 5.083*** | -1.096*** | -0.993*** |
| | (0.294) | (4.113) | (2.228) | (0.825) | (0.329) | (0.268) |
| Observations | 27,128 | 3,395 | 964 | 28,835 | 28,911 | 28,930 |
| Unique individuals | 14,419 | 1,946 | 533 | 14,444 | 14,466 | 14,471 |
| Weights: | Lab. Inc. | Cap. Inc. | Corp. Inc. | PIT Rev. | KIT Rev. | CIT Rev. |

Notes: This table reports our preferred intensive- and extensive-margin estimates for each tax base. Panel (a) reports reduced-form estimates, which correspond to the DiD interaction terms from Equations (16) and (17) for years 2011 and 2012. In columns (1)-(3), these coefficients are the ones associated with variables $treat^{MTR} \times \mathbb{1}(year = 2012)$ and $treat^{MTR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta \log y_i^b$ as the outcome for PLIT, capital, and corporate income, respectively. In columns (4)-(6), the coefficients are $treat^{ATR} \times \mathbb{1}(year = 2012)$ and $treat^{ATR} \times \mathbb{1}(year = 2011)$, with the outcome defined as $\Delta \mathbb{1}(y_i^b > 0)$ for the same tax bases. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable. In columns (1)-(3), these correspond to changes in the *marginal* net-of-tax rate, $\Delta \log(1-\tau_{it}^l)$ and its forward term $\Delta^+ \log(1-\tau_{it}^l)$. In columns (4) to (6), the outcomes are the change in the *effective* net-of-tax rates, $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$. Panel (c) reports the 2SLS elasticity estimates based on Equations (20) and (21). In columns (1)-(3), the endogenous variables $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$ are instrumented with the interaction terms of $treat^{MTR}$ with dummies for 2011 and 2012. In columns (4)-(6), the endogenous variables $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$ are instrumented with the interaction terms of $treat^{ATR}$ with dummies for 2011 and 2012. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). Panel (d) presents the medium-run elasticity, computed as the sum of 2SLS estimates for each year from 2011 to 2015. Intensive margin estimates are weighted by income, while extensive margin estimates are based on revenue weights. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All estimates are based on $TAX$ records.

Table 4: The Effects on Income Inequality of the 2012 Reform

| | Labor income | | | |
|---|---|---|---|---|
| | Pre-tax Gini | Pos-tax Gini | Pre-tax top 1% | Pos-tax top 1% |
| Without reform | 0.480 | 0.452 | 14.8% | 12.9% |
| (a) With reform (100% labor supply) | 0.475 | 0.448 | 14.1% | 12.3% |
| (b) With reform (100% evasion) | 0.479 | 0.452 | 14.6% | 12.8% |
| (c) With reform (mixed response) | 0.477 | 0.449 | 14.3% | 12.5% |
| | Total income | | | |
| | Pre-tax Gini | Pos-tax Gini | Pre-tax top 1% | Pos-tax top 1% |
| Without reform | 0.567 | 0.548 | 14.8% | 13.5% |
| (d) With reform (100% labor supply) | 0.565 | 0.546 | 14.4% | 13.2% |
| (e) With reform (100% evasion) | 0.567 | 0.549 | 14.8% | 13.6% |
| (f) With reform (mixed response) | 0.566 | 0.547 | 14.5% | 13.4% |

Notes: The Table presents inequality metricsâthe Gini index and top 1% shareâfor labor and total income, both pre- and post-tax, before and after the 2012 reform. The reform's effects are simulated using 2011 data. Total income comprises labor, capital, and corporate income (i.e., incomes subject to PLIT, KIT, and CIT). All oh the inequality estimates are calculated for the entire adult population, including zero-income and informal earners. In the first reform scenario (rows (a) and (c)), it is assumed that the totality of the reduction in labor income that does not shift to capital or corporate income (which amounts to 67% of the 5.7% decrease in labor income), is attributed to real labor supply responses. In the second scenario (rows (b) and (d)), it is assumed that it is entirely evaded. In the last scenario (rows (c) and (f)), a split of the 5.7% reduction in labor income as the one discussed in section 6.3 is assumed, i.e. 33% income shifting, 36% to reported hours worked, and 31% not accounted for (likely evasion).

# Online Appendix (For Online Publication Only)

**"How do Top Earners Respond to Taxation? Own- and Cross-Tax Base Responses, Efficiency, and Inequality"**

**Giaccobasso, Bergolo, Burdin, de Rosa, Leites, and Rueda**

**June 19, 2025**

# A Further Details About the Institutional Background

## Further Details About Top Income Earners in Uruguay

The group of top income earners in developed and developing countries, as well as in Uruguay, is clearly distinguishable from the rest of the population. First, TIEs capture a large share of total income. In developed countries, this was reported in seminal top income studies (Piketty, 2003; Atkinson, 2007; Atkinson et al., 2011), as well as in the re-examination of previous estimates for countries such as France and the US using Distributional National Accounts framework (Piketty et al., 2018; Garbinti et al., 2018), i.e. accounting for the totality of national income (Alvaredo et al., 2020). This is also the case in Latin America, where the rapidly expanding number of studies show even higher levels of concentration (Alvaredo, 2010; Alvaredo and Londoño Velez, 2014; Flores et al., 2020; Morgan, 2017). Recent research based on the same tax records used in this paper shows that the top 1% of Uruguayan income earners receive between 15% and 16% of all income (Burdín et al., 2022). This percentage, while among the lowest in Latin America, is relatively close to the top 1%'s share in the US. Similarly, the Distributional National Accounts' estimate for the share earned by Uruguay's top 1% in 2019 was 17.9%, vs. 18.8% for the top 1% in the U.S. These estimates are comparable across countries since they refer to national income and hence are not affected by the different tax systems. See `https://wid.world/`.

The dynamics in the evolution of top income shares are described in Figure A.1. This figure plots the evolution of total income shares held by the top 10%, 5%, 1%, 0.5%, and 0.1% of taxpayers from 2009 to 2015. It is important to note here, that the numbers reported in this figure differ from those in Burdín et al. (2022) as we do not include non-nominative capital income dividends. Each line represents a different top group, showing how their share of total income among all taxpayers changed over time. The figure documents that the top 1%, 0.5% and 0.1% income shares remained stable over the period of analysis. For instance, the top 1% share is 13.3% in 2009 and remains stable throughout the period (13.4% in 2015) and a similar stability is observed for top 0.5% and 0.1% shares. On the contrary, the top 5% and 10% income shares continuously declined during the period 2009-2015 (from 43.4 to 41.3, and from 30.5 to 29.4, respectively). On top of being illustrative about the income concentration patterns, Figure A.1 is reassuring about the validity of our empirical strategy as discussed in Section 2. In addition, the different dynamics in the evolution of the top income shares shed light on some of the issues that arise when individuals in the 90-99 or 95-99 percentiles are used as the control groups for individuals at the top 1%, and highlights one of the main strengths of our empirical setting.

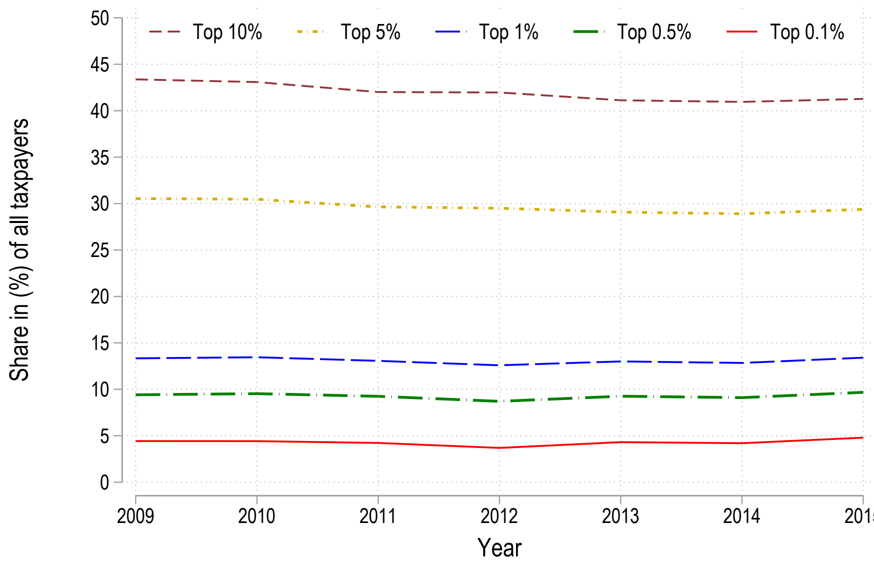
Regarding income composition at the top there remains, however, an active discussion.

Capital and business incomes have played a major role as drivers of income concentration. In the case of the U.S., for instance, the increasing use of pass-through entities for taxation purposes may alter the way individuals choose to allocate their incomes at the firm or individual level and hence may alter estimates of TIEs' income share (Kopczuk and Zwick, 2020), which is allegedly tackled by the use of estimates based on national income (Saez and Zucman, 2020). Perhaps more importantly, while it is clear that business and capital income are relevant, it is not clear whether business income can be fully attributed to capital ownership or conversely to labor. The way in which business income (especially from pass-through firms) is allocated thus entails important consequences for the composition of the top incomes (Piketty et al., 2018; Smith et al., 2019). This also holds for Latin American countries, where business income can account for over 40% of earnings in top fractiles (De Rosa et al., 2020).

In the case of Uruguayan TIEs, given the nature of the data and the tax system, income composition is far more transparent. Income sources can be categorized in terms of the taxes that individuals pay. Figure A.2 and Table 1 in the main text help illustrating the total income composition in 2010 for four groups: universe of tax records, top 1% of the total income distribution, top 1% of the gross labor income distribution, and final sample sample used in the empirical analysis. Across all samples, Figure A.2 shows that labor income is by far the largest component of total income, ranging from 72.0% in the top 1% of total income to 95.5% in the top 1% of labor income. Capital and corporate income play a much smaller role, even within the upper tail of the distribution. These two income types account for 15.6% and 12.4% of total income within the top 1% of the total income distribution, respectively. In contrast, they represent only 2.7% and 1.9% within the top 1% of labor income, and 4.7% and 2.4% when considering the full tax records. Looking at individual averages, Table 1 in the main text provides a similar characterization. For example, among taxpayers in the top 1% of gross labor income, only 13.3% report any capital income and just 3.0% report corporate income. For taxpayers in the top 1% of total income, the values are larger, but still the share of individuals reporting to other tax bases is small: 21.6% and 10.9%, respectively. Overall, 94.8% of individuals in the TAX records report only labor income. This share reaches 88.9% report for taxpayers in the top 1% of labor income distribution, and 74.1% for individuals in the top 1% of total income.

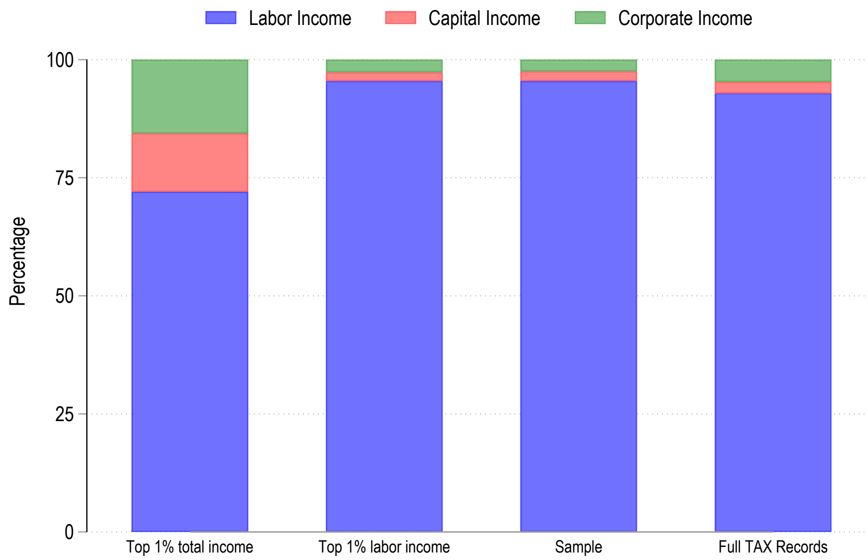
In terms of individual characteristics, Table 1 in the main text shows that individuals in the top 1% of the gross labor income distribution are, on average, around 49 years old, predominantly male (approximately 72%), with about one-third reporting some self-employment income and around 40% receiving labor income from multiple sources. Figure A.3 illustrates sectoral participation (based on ISIC 2-digit codes) across different samples. Panel (a) shows that taxpayers in the full tax records are primarily employed in manufacturing (13.5%), agriculture, forestry and fishing (11.3%), and retail (9.8%). However, among top income earners, whether defined by total income or labor income, the share working in sectors such as financial and insurance activities and human health and social work rises sharply. For instance, for the top 1% of labor income earners, these shares reach 17.2% (compared to 4.8% in the full sample) and 22.0% (versus 7.5%), respectively. This pattern aligns with previous evidence on the composition of top earners in Uruguay where liberal professionals and healthcare workers, mostly physicians, make up a substantial share of the top 1% of the income distribution (Burdín et al., 2022). These increased participation of financial and health-related activities happens mostly on the detriment of activities such as agricultural, retail, and education.

Figure A.1: Evolution of Top Income Shares in Uruguay (2009-2015)



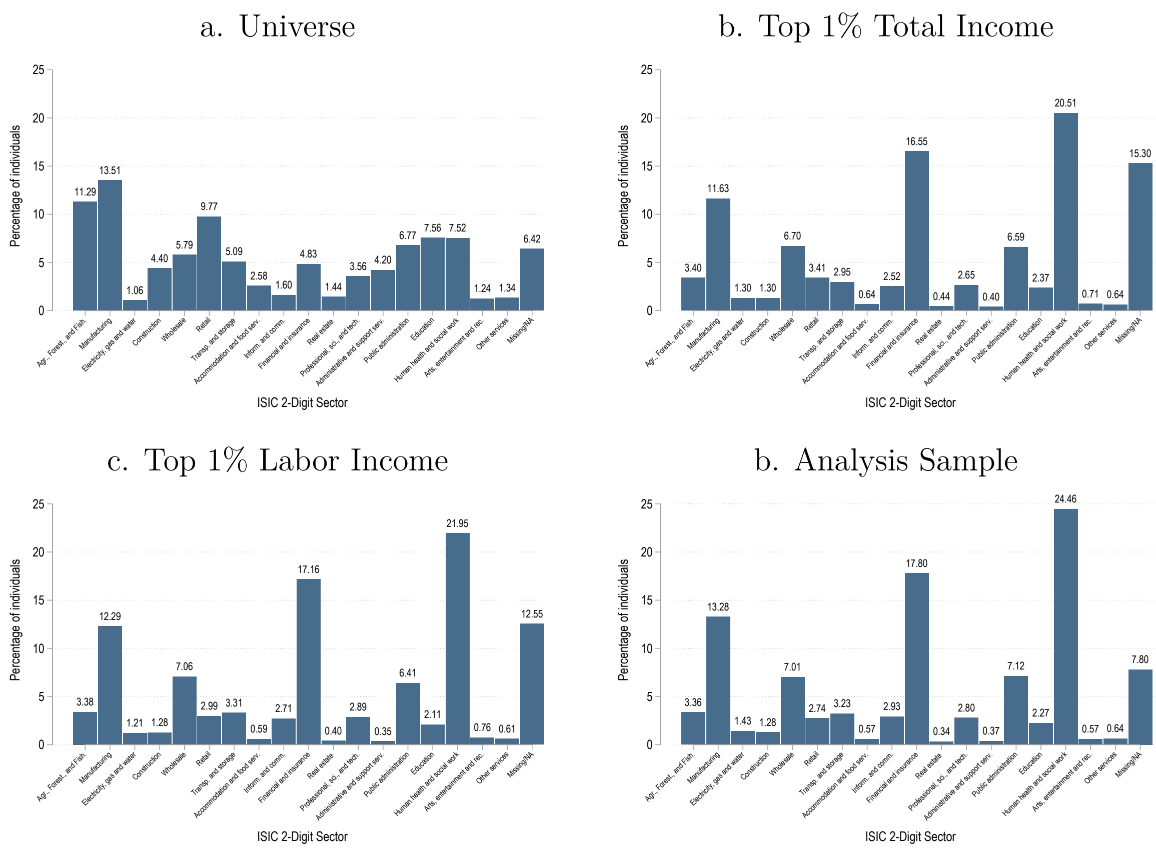

Notes: This figure shows the income shares for different top incomes threshold definitions for the 2009-2015 period based on the Tax Agency administrative records. The income shares are based on the total income that includes labor, corporate and capital income. Capital income corresponds only to nominative items, i.e., items that can be linked to specific individuals. Because of the exclusion of non-nominative capital income, the shares reported here are different from other studies (e.g., Burdín et al. 2022). To compute each threshold, we use all the regular taxpayers provided by the Uruguayan Tax Agency (DGI).

Figure A.2: Income Composition by Sample




Notes: The figure depicts the aggregate income composition based on 2010 *TAX* records for four groups. Full universe of taxpayers in the *TAX* records are defined as individuals with positive total income in at least one year between 2009 and 2011, where total income is the sum of income reported to the PLIT, corporate, and capital income tax bases. Top 1% of the gross labor income distribution, which is comprised of 18,930 TIEs with at least one year of labor income above 600 BPC between 2009 and 2011. Top 1% of total income is selected by ranking all individuals by total income and choosing the 18,930 with the highest values. Finally, analysis sample corresponds to the subset of labor TIEs used in our main analysis. These are labor TIEs who (a) never fell below 300 BPC in total income (roughly the 95th percentile of gross labor income), (b) did not switch income zones (G1-G4) every year between 2009 and 2011, and (c) did not experience extreme income growth in the pre-treatment period (greater than 100%). Capital income corresponds only to nominative items, i.e., items that can be linked to specific individuals. Because of the exclusion of non-nominative capital income, the capital income share observed for the top 1% of total income is considerable lower than in other studies (e.g., Burdín et al. 2022).

Figure A.3: ISIC 2-Digit Activity Sector by Sample




Notes: This figure reports the distribution of individuals' across 18 ISIC 2-digit industry sectors in their main occupation in year 2010. Information on activity sector comes from firm-level information in the *TAX* records. Full universe of taxpayers in the *TAX* records are defined as individuals with positive total income in at least one year between 2009 and 2011, where total income is the sum of income reported to the PLIT, corporate, and capital income tax bases. Top 1% of the gross labor income distribution is comprised of 18,930 TIEs with at least one year of labor income above 600 BPC between 2009 and 2011. Top 1% of total income is selected by ranking all individuals by total income and choosing the 18,930 with the highest values. Finally, analysis sample corresponds to the subset of labor TIEs used in our main analysis. These are labor TIEs who never fell below 300 BPC in total income (roughly the 95th percentile of gross labor income), did not switch income zones (G1-G4) every year between 2009 and 2011, and did not experience extreme income growth in the pre-treatment period (greater than 100%). Panel (a) shows the distribution for the universe of taxpayers, panel (b) for individuals in the top 1% of the total income distribution, panel (c) focuses on the top 1% of the labor income distribution, and Panel (d) in the final analysis sample used in our main estimates. Individuals with missing or unclassified sector information are grouped under "Missing/NA". Each panel shows the percentage of individuals in each sector, with values expressed as percentages of the corresponding sample. All figures are based on *TAX* records.

## Uruguay's Tax Structure

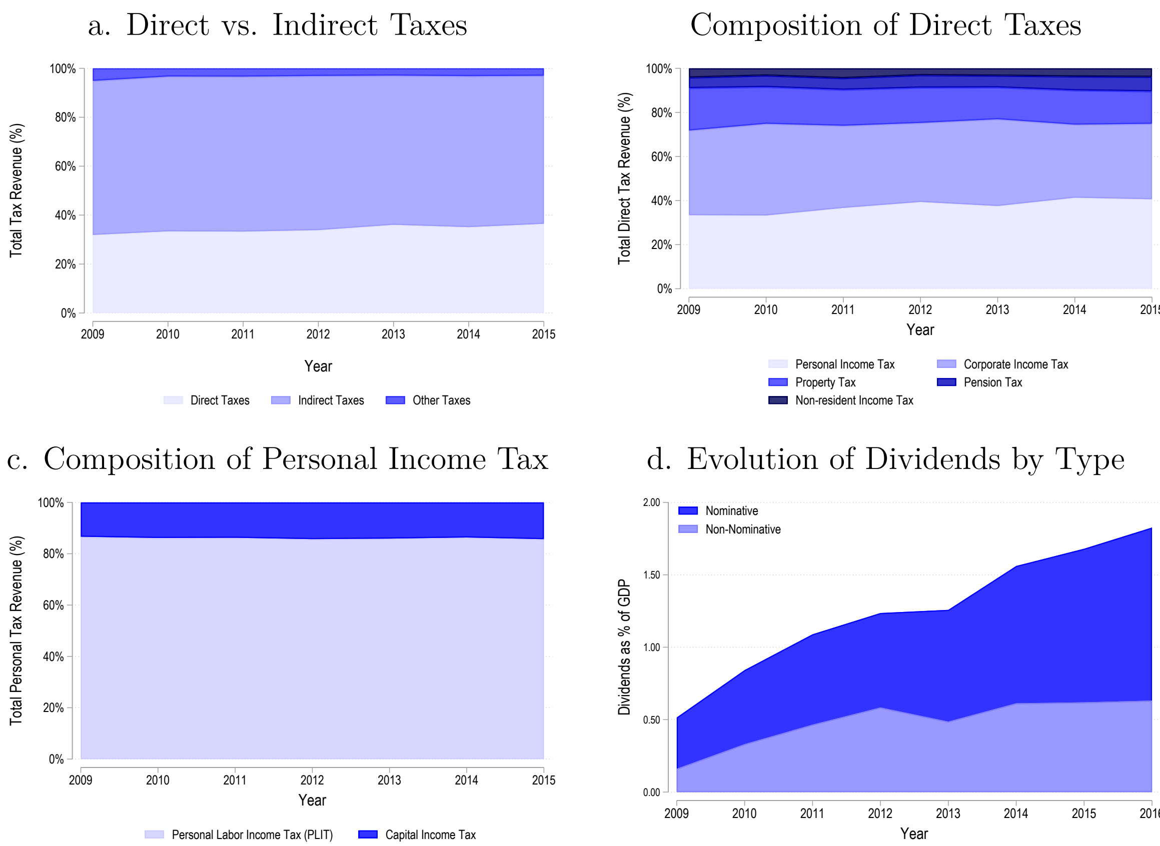
Figure A.4 describes the composition of total tax revenues between 2009 and 2015. In panel (a), we divide total revenues into direct, indirect, and other taxes. Over the period of analysis, direct taxes accounted for around 35% of total tax collection, excluding social security contributions. The remaining 65% came mostly from VAT and specific excise taxes, such as those on alcoholic beverages, cigarettes, and similar goods.

Between 2009 and 2015, revenues from direct taxation gained in relative importance. This was largely the result of a major tax reform implemented in 2007, which aimed to shift part of the tax burden from indirect to more progressive direct taxes. Among other changes, the reform introduced a Dual Personal Income Tax System (*Impuesto a la Renta de las Personas Fisicas*, in Spanish), under which labor and capital income are taxed separately. For simplicity, we refer to these components as the Personal Labor Income Tax (PLIT), which is the main focus of our study, and the capital income tax.[30]

Panel (b) of Figure A.4 breaks down revenues from direct taxation into five components: (i) the dual personal income tax, (ii) the corporate income tax, (iii) property taxes, (iv) the retirement income tax, and (v) the non-resident personal income tax. The figure shows that the dual personal income tax and the corporate income tax are the two main components of direct taxation, together accounting for about three-quarters of total direct tax revenues, in more or less equal shares. Panel (c) further decomposes revenues from the dual personal income tax into PLIT and capital income tax. It shows that nearly 90% of revenues come from the labor income portion of the dual system. This is consistent with the descriptive statistics reported in the previous section, which show that Uruguayan taxpayers earn the vast majority of their income from labor. Finally, panel (d) splits capital income tax revenues from dividends by type: nominative versus non-nominative. Nominative dividends are those that can be linked to specific individuals, while non-nominative dividends, due to legal constraints, cannot be attributed to specific individuals. The general pattern in the figure shows that taxes on nominative dividends initially accounted for about half of total dividend tax revenues, but their share increased significantly toward the end of the period.

[30] Similar income tax systems exist in Nordic countries such as Denmark, Finland, Norway, and Sweden.

Figure A.4: Tax Structure in Uruguay: 2009-2015




Notes: Panel (a) This figure describes the main features of the Uruguayan tax structure between 2009 and 2015. In panel (a), total tax revenue is split into direct and indirect taxes and a residual category that comprises several other small taxes. Indirect taxes include the two major indirect taxes in Uruguay: VAT and IMESI. The VAT is the traditional value added tax and it represents almost 80% of the total indirect tax revenue. IMESI is a tax that is collected from the first sale of a particular set of goods such as alcoholic beverages, tobacco and fuel, among others. Direct taxes include the dual personal income tax, corporate income tax, property taxes, pension income tax, and non-residents' personal income tax. Panel (b) describes how direct tax revenue is split into its five components. Panel (c) zooms in the dual personal income tax and depicts how the labor part and the capital part contribute to total revenue in this tax base. Finally, panel (d) shows the total revenue from nominative and non-nominative dividends as a share of GDP. Nominative dividends are taxed at the individual level and hence the dividend-receiver can be identified when capital income tax is paid, whilst in the case of non-nominative dividends individual the tax is withheld anonymously, even for the own tax agency, at the firm level. In panels (a) through (c) the information comes from the annual series of tax revenues that are available on the Tax Agency website. In panel (d), information on aggregate dividends by category were provided by the Tax Agency (DGI). For details, see Burdín et al. (2022)

## Additional Details on PLIT, Capital Income, and Corporate Income Taxes

### Personal Labor Income Tax (PLIT)

The PLIT is the part of the Dual Personal Income Tax focused on labor income. It progressively taxes all sources of individual labor income.[31] It comprises a *labor income tax part* and a *tax deduction part*, and the final tax liability is calculated as the difference between the two. This schedule applies to both wage earners and self-employed workers, where the latter includes individuals with professional activities, such as lawyers, public notaries, accountants, and non-professionals who provide personal services directly to customers.

The *labor income tax part* is the result of passing the total gross labor income through a set of income brackets with progressive marginal tax rates. This follows the typical structure of progressive personal income tax schedules around the world; except for the definition of the income concept, which in this case is the *gross* labor income aggregating both wage and self-employment earned income. Wage income corresponds to the total earned income as a wage earner before any taxes or deductions. This includes wages, salaries, commissions, overtime payments, vacation payments, annual leave, end-of-the-year payments, per-diem stipends, and any other payment received from one's employer. Self-employment income includes earned income from self-employment which is subject to an automatic 30% deduction on behalf of production costs and VAT payments for tax purposes. Thus, gross labor income corresponds to 70% of total earned income for self-employed individuals. Unemployment, illness and maternity subsidies, accident insurance and unemployment benefits and child allowances are excluded from the definition of gross labor income. Panel (a) in Table 2 of the main text reports the income brackets and the marginal tax rates applied to gross labor income for the pre-reform year 2011. There are six income brackets that are annually adjusted by CPI. The marginal tax rates associated with these brackets range between 0% (the exemption threshold) and 25%.

The structure of the *tax deduction part* is reported in columns (3) and (4) of Panel (a) in Table 2. Deductions include "non-itemized deductions", which are proportional to the gross labor income and are automatically considered when calculating the final tax liability, and "itemized deductions", which must be explicitly claimed by the taxpayer. Non-itemized deductions include payroll taxes and health insurance mandatory contributions. Itemized deductions include a fixed per-child tax benefit, other non-proportional social security contributions (e.g. payroll taxes made by self-employed professionals to their own pension schemes),

[31] Uruguay's tax code also allows married couples to file a joint tax return. However, this instrument is rarely used. Between 2009 and 2015 only 0.5% of taxpayers decided to pay taxes as a household unit.

and housing expenses like mortgage and rent payments. Deductions are also passed through a progressive deduction rate schedule (with rates between 10% and 25%) and this amount is subtracted from the income tax part, resulting in the final tax liability.

The Uruguayan Tax Agency collects part of the PLIT revenues by using monthly income tax withholding on wage income. Every month employers are required to remit to the Tax Agency part of their employees' wages as advance payments of annual income tax (Form 1144). Each firm computes tax liability based on the labor income and deductions related to the employee's activity in the firm. Taxpayers can achieve a more accurate monthly tax withholding by providing further information to the firm about itemized deductions (Form 3100) - e.g., non-proportional social security contributions and child deductions. In turn, self-employed workers must make bi-monthly advance payments toward their yearly tax bill.

### Capital Income Tax

The capital income tax is the other part of the Dual Personal Income Tax System. Capital income tax covers all sources of individual capital income. It is simpler compared to PLIT since it is based on a set of proportional tax rates, and deductions are not allowed. Panel (b) in Table 2 of the main text shows the structure of tax rates for the 2011 fiscal year. The tax code distinguishes twelve different capital income categories, which can be grouped into three more general categories: interest from deposits (taxed at a 3% rate), dividends and other financial income (taxed at a 7% rate), and real estate rents (taxed at a 12% rate).[32]

It is worth noting that before being distributed to individuals, dividends and other financial income are taxed at the corporate level at 25% rate. Hence, the effective tax rate for this type of income is about 30%. Banks, real estate agencies and institutions in charge of payments act as withholding agents in most cases. Withheld amounts are treated as advance payments of annual capital income tax. Thus, the tax code does not require that capital income earners file a tax return at the end of the fiscal year (Form 1101), unless their tax capital income tax has not been withheld.

More importantly, the law does not require individuals to file a tax return in the case of capital gains from bank-deposit interests (due to bank secrecy rules) and distributed (non-nominative) dividends from anonymous companies. Dividends could be either nominative or non-nominative, depending on what type of legal entity the firm is. In the case of nominative dividends, employees receiving them from an anonymous company are responsible for paying the income tax. The process involves filing a tax return and therefore they are identified by the Tax Agency. Conversely, for non-nominative interests, the tax is withheld by the firm

[32] Interest from deposits includes all cash or in-kind rents coming from bank deposits and other financial assets. Other financial income includes dividends and royalties, among others.

and the Tax Agency does not require the dividend-receiver to file a tax return (Form 1101). Hence, it is impossible for the Tax Agency to know the dividend-receiver's identity. This generates some data limitations for the empirical analysis.

### Corporate Income Tax

Instead of PLIT, self-employed workers can opt to pay corporate income tax. To have the option, individuals must organize their activity as unincorporated firms (sole proprietorships or partnerships), and must have an annual earned income below a certain threshold (about 4,000 BPC). Panel (c) in Table 2 of the main text shows the structure of the corporate income tax. It consists of a statutory 25% tax rate applied to business profits. In the case of self-employed workers who opted for corporate taxation, the 25% tax rate is applied to 48% of their (gross) earned income. This results in an effective tax rate of 12%.[33]

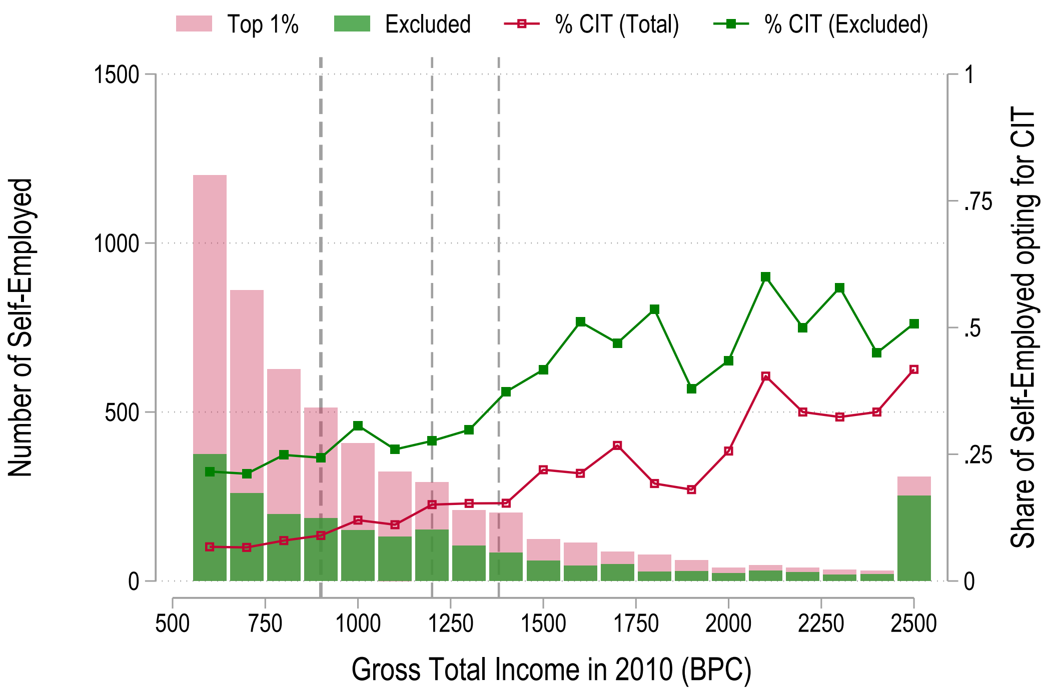
Exercising the option between PLIT and corporate income tax has no major administrative costs other than filing an application form. However, once a self-employed worker opts for corporate taxation, they are prevented from switching back to PLIT for three years. Figure A.5 describes the 2010 choices for self-employed TIEs defined by total income (in red). In this case, we use total income to define the top 1% since, by construction, our sample of top 1% labor income earners does not include taxpayers who reported all their income to the corporate income tax base. On average, around 12.8% of these self-employed workers opted to pay corporate income tax instead of PLIT. However, there are clear differences by income-level, reaching more than 40% in the upper part of the total income distribution. This pattern reflect that the flat corporate tax rate is much more beneficial compared to progressive PLIT rates, the larger the income.

In this paper, we examine whether and how TIEs responded to a reform that affected marginal tax rates under the PLIT. Our analysis sample therefore focuses on TIEs who reported income to the PLIT base, excluding self-employed individuals in the top 1% of total income who had opted for corporate taxation prior to 2010. Hence, Figure A.5 presents the same statistics for self-employed individuals in the top 1% of total income, who are not included in our top 1% gross labor income sample (shown in green). By construction, a larger share of these individuals have opted for the corporate tax base over the labor income tax base (on average, 31.6%). Interestingly, those who chose corporate income taxation before the reform tend to have higher incomes than self-employed individuals who remained in the PLIT base. This pattern is relevant for understanding potential selection biases into our top 1% labor income sample. We discuss the implications of this selection in Section 6 of the

[33] This imputed tax base assumes that the costs incurred by the firm account for 52% of the gross business income.

main text when presenting our baseline results by employment type.

Figure A.5: Self-Employed and Choices between PITL and Corporate Income Tax




Notes: This figure examines the choice between the PLIT and the corporate income tax base among self-employed individuals in the top 1% of total income in 2010. The x-axis corresponds to total gross income (in BPC), top-coded at 2,500 BPC, and grouped in 100 BPC bins. The left y-axis shows the number of self-employed individuals in each bin. The red bars represent self-employed individuals in our final analysis sample (i.e., top 1% of the labor income distribution meeting all sample restrictions), while the green bars show those excluded from the analysis sample (i.e., self-employed in the top 1% total income who are not in the top 1% of labor income or do not meet the additional sample restrictions). The sum of both, represent the total number of self-employed individuals in the top 1% of total income distribution in 2010. The right y-axis reports the share of self-employed individuals in each income bin who report income to the corporate income tax base but not to the PLIT base, interpreted as opting for the corporate income tax regime. The solid red line corresponds to the share of self-employed workers in the top 1% of total income opting into corporate income tax instead of PLIT. The solid green line replicates this for self-employed workers in the top 1% of total income who are not included in our analysis sample. Vertical dashed lines at 900, 1200, and 1380 BPC correspond to the income zones generates by the 2012 tax reform, as explained in Section 2. All estimates are based on *TAX* records. See Section 2 and Appendix A for further discussion.

## Salience of the Tax Reform

The salience of the tax reform is key to assessing how credible the estimates of taxpayers' responses to changes in the tax rates are. The political debate concerning the tax reform we analyze, the level of media coverage, and the "type" of workers the tax changes affected support the claim that the tax reform in our setting is indeed salient.

First, the question of how to increase income tax progressivity has always been a topic of discussion for the left-wing party *Frente Amplio*, which was in office throughout the whole period under analysis and was the party that designed and carried out the tax reform we analyze. By the end of the 1990s, years before they won the 2005 election and introduced the PLIT for the first time, the left-center coalition already was promoting a progressive labor income tax schedule, which did not exist at that time. *Frente Amplio* won the elections (again) in 2009, two years after the PLIT was first implemented, and its party's political platform included multiple references and proposals to change the current tax schedule, and in particular to the need to enhance the re-distributive potential of the tax system.[34] When the public discussion began in 2011, the idea of a tax reform was already salient for the general public, because it had been part of a public debate during presidential and legislative elections about taxation of top earners and top wealth holders. Furthermore, the debates about the tax reform generated a political confrontation between the president and the vice-president that drew the attention of the general public because they represented the two major wings within the party in office.

Second, the public discussion between members of the incumbent party and the official announcement by the president with his cabinet ministers was widely covered by nationwide TV channels and newspapers.[35] As an example, Figure A.6 depicts the front cover of the largest Uruguayan newspaper, which announces the 2012 tax reform.

Figure A.6: The reform covered by centre-right newspaper

a. *El Pais* - web version  b. El Pais - printed edition


Notes: This picture shows the front page edition of the largest national newspaper (*El Pais*) when the tax reform was announced. Panel (a) shows the web version and panel (b) shows the front page in their paper edition. The headline in both cases read "[the government] will increase the bracket taxed at 25%". Both figures were found using the Wayback machine site web.

[34] Program available in: `https://www.frenteamplio.uy/documento/item/135-programa-2010-2015` (in Spanish).

[35] See for instance, *La Diaria* newspaper (In Spanish) on `https://ladiaria.com.uy/articulo/2011/11/el-valor-de-la-confianza/`

# B Further Details on the Conceptual Framework

## Sorting conditions

To determine individuals' choices between Non-Shifting (NS), Partial Shifting (PS), and Full Shifting (FS), we compare their indirect utilities under each option. First, recall:

$$V_{ns} = \omega\, h^*_{ns} - T_l \;\; \omega\, h^*_{ns} \;\; - \gamma_l - v \;\; h^*_{ns} \;\; . \tag{B.1}$$

$$V_{fs} = (1-\tau_k)\, \omega\, h^*_{fs} - \gamma_k - R_k \;\; \omega\, h^*_{fs} \;\; - v \;\; h^*_{fs} \;\; . \tag{B.2}$$

$$\begin{aligned} V_{ps} = {} & (\omega\, h^*_{ps} - a^*_{ps}) - T_l \;\; \omega\, h^*_{ps} - a^*_{ps} \;\; - \gamma_l \\ & + (1-\tau_k)\, a^*_{ps} - \gamma_k - R_k \;\; a^*_{ps} \;\; - v \;\; h^*_{ps} \;\; . \end{aligned} \tag{B.3}$$

where $h^*$ and $a^*$ are solutions to equations (1):(4), accordingly. These indirect utility functions define a series of sorting conditions:

$$V_{ns} \geq V_{fs} \quad \text{and} \quad V_{ns} \geq V_{ps} \quad \Rightarrow \quad \text{Non-shifter.} \tag{B.4}$$

$$V_{fs} > V_{ns} \quad \text{and} \quad V_{fs} \geq V_{ps} \quad \Rightarrow \quad \text{Full shifter.} \tag{B.5}$$

$$V_{ps} > V_{ns} \quad \text{and} \quad V_{ps} > V_{fs} \quad \Rightarrow \quad \text{Partial shifter.} \tag{B.6}$$

**Non-shifting conditions:**

$V_{ns} \geq V_{fs}$

$$\omega\, h^*_{ns} - T_l \;\; \omega\, h^*_{ns} \;\; - \gamma_l - v \;\; h^*_{ns} \;\; \geq (1-\tau_k)\, \omega\, h^*_{fs} - \gamma_k - R_k \;\; \omega\, h^*_{fs} \;\; - v \;\; h^*_{fs} \;\; .$$

which can be re-written as:

$$\begin{aligned} \underbrace{\gamma_k - \gamma_l}_{\text{Fixed cost differential}} \quad \geq \quad & \underbrace{(1-\tau_k)\, \omega\, h^*_{fs} - (\omega\, h^*_{ns} - T_l(\omega\, h^*_{ns}))}_{\text{Net of tax income differential}} \\ & - \underbrace{[v(h^*_{fs}) - v(h^*_{ns})]}_{\text{Cost of effort differential}} \\ & - \underbrace{R_k(\omega\, h^*_{fs})}_{\text{Var. cost of full shifting}} \end{aligned} \tag{B.7}$$

$V_{ns} \geq V_{ps}$

$$\omega\, h^*_{ns} - T_l \;\; \omega\, h^*_{ns} \;\; - \gamma_l - v \;\; h^*_{ns} \;\; \geq (\omega\, h^*_{ps} - a^*_{ps}) - T_l \;\; \omega\, h^*_{ps} - a^*_{ps} \;\; - \gamma_l + (1-\tau_k)\, a^*_{ps} - \gamma_k - R_k \;\; a^*_{ps} \;\; - v \;\; h^*_{ps} \;\; .$$

which can be re-written as:

$$\gamma_k \geq \quad (\omega\, h^*_{ps} - a^*_{ps}) - T_l(\omega\, h^*_{ps} - a^*_{ps}) + (1-\tau_k)\, a^*_{ps} \quad - \quad \omega\, h^*_{ns} - T_l(\omega\, h^*_{ns})$$
$$- [v(h^*_{ps}) - v(h^*_{ns})]$$
$$- R_k(a^*_{ps}) \tag{B.8}$$

Intuitively, these conditions can be interpreted as fixed cost thresholds that depend on three factors: (1) differences in net-of-tax earnings across tax bases, (2) differences in the cost of effort due to differences in labor supply decisions, and (3) additional variable costs associated with income shifting. In terms of comparative statics, an increase in the labor income marginal tax rate ($T'_l(y)$) makes non-shifting less attractive. Formally, this implies $\frac{\partial \tilde{\gamma}_k}{\partial \tau_l} > 0$, which leads to fewer individuals choosing the non-shifting type. Here, $\tilde{\gamma}_k$ denotes the value of $\gamma_k$ makes TIEs indifferent between alternatives.

**Full-shifting conditions:**

$V_{fs} > V_{ns}$: which corresponds to Equation (B.7), with the inverted inequality sign

$$\gamma_k - \gamma_l < \quad (1-\tau_k)\, \omega\, h^*_{fs} - (\omega\, h^*_{ns} - T_l(\omega\, h^*_{ns}))$$
$$- \quad v(h^*_{fs}) - v(h^*_{ns})$$
$$- R_k(\omega\, h^*_{fs}) \tag{B.9}$$

$V_{fs} \geq V_{ps}$

$$(1-\tau_k)\, \omega\, h^*_{fs} - \gamma_k - R_k \;\; \omega\, h^*_{fs} \;\; - v \;\; h^*_{fs} \;\; \geq$$
$$(\omega\, h^*_{ps} - a^*_{ps}) - T_l \;\; \omega\, h^*_{ps} - a^*_{ps} \;\; - \gamma_l + (1-\tau_k)\, a^*_{ps} - \gamma_k - R_k \;\; a^*_{ps} \;\; - v \;\; h^*_{ps} \;\; .$$

$$\gamma_l \geq \quad (\omega\, h^*_{ps} - a^*_{ps}) - T_l \;\; \omega\, h^*_{ps} - a^*_{ps} \;\; + (1-\tau_k)\, a^*_{ps} - (1-\tau_k)\, \omega\, h^*_{fs}$$
$$- [v(h^*_{ps}) - v(h^*_{fs})]$$
$$- \quad R_k(a^*_{ps}) - R_k(\omega\, h^*_{fs}) \tag{B.10}$$

In this case, the increase in the marginal tax rate makes full shifting relatively more attractive. On the one hand, the rise in $\tilde{\gamma}_k$ relaxes the threshold for full shifting, leading more TIEs to choose this option over non-shifting. On the other hand, partial shifting becomes relatively less attractive compared to full shifting. Formally, this corresponds to a lower value of $\tilde{\gamma}_l$, which increases the share of TIEs choosing full shifting over partial shifting.

**Partial-shifting conditions:**

$V_{ns} < V_{ps}$: Corresponds to Equation (B.8) with the inverted inequality sign:

$$\begin{aligned}\gamma_k < \quad & (\omega\, h^*_{ps} - a^*_{ps}) - T_l(\omega\, h^*_{ps} - a^*_{ps}) + (1-\tau_k)\, a^*_{ps} \quad - \quad \omega\, h^*_{ns} - T_l(\omega\, h^*_{ns}) \\ & - [v(h^*_{ps}) - v(h^*_{ns})] \\ & - R_k(a^*_{ps})\end{aligned} \tag{B.11}$$

$V_{fs} < V_{ps}$: Corresponds to Equation B.10 with the inverted inequality sign:

$$\begin{aligned}\gamma_l < \quad & (\omega\, h^*_{ps} - a^*_{ps}) - T_l \;\; \omega\, h^*_{ps} - a^*_{ps} \;\; + (1-\tau_k)\, a^*_{ps} - (1-\tau_k)\,\omega\, h^*_{fs} \\ & - [v(h^*_{ps}) - v(h^*_{fs})] \\ & - \;\; R_k(a^*_{ps}) - R_k(\omega\, h^*_{fs})\end{aligned} \tag{B.12}$$

In this case, the effect of an increase in the marginal tax rate is ambiguous. On the one hand, some taxpayers will shift from non-shifting to partial shifting. On the other hand, others will shift from partial shifting to full shifting. The net effect on the share of partial shifters will depend on the relative size of these transitions.

## Derivation of the Welfare Loss Expression

By the envelope theorem, small changes in marginal tax rates should not affect taxpayers' indirect utilities at the margin, as they were already optimizing. Hence, a typical result in the public finance literature is that the deadweight loss from the tax reform can be expressed as the sum of revenue changes in each tax base due to behavioral responses.[36] We can write this as the sum of changes in revenues across tax bases:

$$\begin{aligned}\frac{dW}{d\bar{\tau}_l} = \quad & \underbrace{{}_{\Lambda} \quad I_l(\lambda)\,\tau_l\,\tfrac{\partial y_l(\lambda)}{\partial \tau_l(\lambda)} + y_l(\lambda)\,\tau^e_l(\lambda)\,\tfrac{\partial I_l(\lambda)}{\partial \tau_l(\lambda)} \quad dF(\lambda)}_{\text{Own-tax base response}} \\ & + \underbrace{{}_{\Lambda} \quad I_k(\lambda)\,\tau_k\,\tfrac{\partial y_k(\lambda)}{\partial \tau_l(\lambda)} + y_k(\lambda)\,\tau_k\,\tfrac{\partial I_k(\lambda)}{\partial \tau_l(\lambda)} \quad dF(\lambda)}_{\text{Cross-tax base response}}\end{aligned} \tag{B.13}$$

For simplicity, for own base responses, we assume $T'_l(y) = \tau_l$. By doing this, we are implicitly assuming that revenue losses are proportional to changes in income. Under a progressive tax schedule, this need not be the case, since marginal tax rates increase with income. However, because we are interested in the deadweight loss associated with the tax change,

[36] For responses in the intensive margin, this is the typical result for smooth problems. For switchers, the interpretation is that those who switch due to a small change in $\tau_l$ are the taxpayers who are on the margin. Hence, there are no first order effects on indirect utilities of swhitchers either.

this assumption is conservative: if individuals respond so strongly that they move into lower tax brackets, we will overestimate the revenue loss from the reform. Furthermore, to move $\tau_l$ outside the integral, we must assume that $\tau_l$ does not vary across individuals. In practice, however, $\tau_l$ differs depending on income levels. For the welfare analysis, we nonetheless choose to use a single rate, i.e., the pre-reform top marginal tax rate of 25%, so that every unit of income lost is valued at the highest marginal rate possible. While this assumption does not reflect the actual variation in tax rates, it simplifies the analysis considerably. This choice is consistent with other conservative assumptions, which, if anything, lead us to overestimate the deadweight loss.

We begin by examining the own-tax base response term:

$$\int_\Lambda \left( I_l(\lambda)\, \tau_l\, \tfrac{\partial y_l(\lambda)}{\partial \tau_l} + y_l(\lambda)\, \tau_l^e(\lambda)\, \tfrac{\partial I_l(\lambda)}{\partial \tau_l} \right) dF(\lambda) = \int_\Lambda I_l(\lambda)\, \tau_l\, \tfrac{\partial y_l(\lambda)}{\partial \tau_l}\, dF(\lambda) + \int_\Lambda y_l(\lambda)\, \tau_l^e(\lambda)\, \tfrac{\partial I_l(\lambda)}{\partial \tau_l}\, dF(\lambda)$$

Note that while $I_l(\lambda)$ is a binary variable, we can take its derivative with respect to $\tau_l$ by interpreting the expression in terms of smooth changes in the distribution of individuals across types. In particular, we treat $I_l(\lambda)$ as representing the share of type-$\lambda$ TIEs who report to the tax base $l$, which is smooth under the assumptions in our model.

Then, we express the partial derivatives in terms of the net-of-tax rate $(1 - \tau_l)$:

$$= -\int_\Lambda I_l(\lambda)\, \tau_l\, \tfrac{\partial y_l(\lambda)}{\partial (1-\tau_l)}\, dF(\lambda) - \int_\Lambda y_l(\lambda)\, \tau_l^e(\lambda)\, \tfrac{\partial I_l(\lambda)}{\partial (1-\tau_l)}\, dF(\lambda)$$

Multiplying and dividing by $(1 - \tau_l)$, and factoring out $\frac{\tau_l}{1-\tau_l}$:

$$= -\frac{\tau_l}{1-\tau_l} \int_\Lambda I_l(\lambda)\, (1-\tau_l)\, \tfrac{\partial y_l(\lambda)}{\partial (1-\tau_l)}\, dF(\lambda) - \frac{\tau_l}{1-\tau_l} \int_\Lambda y_l(\lambda)\, \tfrac{\tau_l^e(\lambda)}{\tau_l} (1-\tau_l)\, \tfrac{\partial I_l(\lambda)}{\partial (1-\tau_l)}\, dF(\lambda)$$

Multiplying and dividing by $y_l$ allows us to define individual-level elasticities:

$$= -\frac{\tau_l}{1-\tau_l} \int_\Lambda I_l(\lambda)\, y_l(\lambda)\, \epsilon_{l,l}(\lambda)\, dF(\lambda) - \frac{\tau_l}{1-\tau_l} \int_\Lambda y_l(\lambda)\, \tfrac{\tau_l^e(\lambda)}{\tau_l}\, \eta_{l,l}(\lambda)\, dF(\lambda)$$

where $\eta_{l,l}(\lambda) = \mu_{l,l}(\lambda) \cdot \varsigma_{l,l}(\lambda)$, with

$$\mu_{l,l}(\lambda) = (1 - \tau_l^e(\lambda)) \cdot \frac{\partial I_l(\lambda)}{\partial (1 - \tau_l^e(\lambda))}$$

$$\varsigma_{l,l}(\lambda) = \frac{\partial(1-\tau_l^e(\lambda))}{\partial(1-\tau_l)} \cdot \frac{1-\tau_l}{1-\tau_l^e(\lambda)}$$

In practice, we will estimate the extensive margin elasticity with respect to the effective net-of-tax rate ($\mu_{l,l}$) and rescale it by how the effective rate responds to changes in the marginal net-of-tax rate ($\varsigma_{l,l}$). We do this because extensive margin decisions are driven by effective tax rates, but for welfare purposes we are interested in how they change when the marginal tax rate changes.

Finally, multiplying and dividing by $Y_l = \int_{\Lambda} y_l \, dF(\lambda)$, we obtain:

$$\begin{aligned} = & -\frac{\tau_l}{1-\tau_l} Y_l \int_{\Lambda} I_l(\lambda) \, \tfrac{y_l(\lambda)}{Y_l} \, \epsilon_{l,l}(\lambda) \, dF(\lambda) \\ & - \frac{\tau_l}{1-\tau_l} Y_l \cdot \frac{\bar{\tau}_l^e Y_l}{\tau_l Y_l} \int_{\Lambda} \tfrac{\tau_l^e(\lambda) \, y_l(\lambda)}{\bar{\tau}_l^e Y_l} \, \eta_{l,l}(\lambda) \, dF(\lambda) \end{aligned}$$

This expression is essentially a sum of weighted average aggregate elasticities:

$$\int_{\Lambda} I_l(\lambda) \, \tau_l \, \tfrac{\partial y_l(\lambda)}{\partial \tau_l} + y_l(\lambda) \, \tau_l^e(\lambda) \, \tfrac{\partial I_l(\lambda)}{\partial \tau_l} \;\; dF(\lambda) = -\frac{\tau_l}{1-\tau_l} Y_l \;\; \bar{\epsilon}_{l,l} + \frac{\bar{\tau}_l^e Y_l}{\tau_l Y_l} \bar{\eta}_{l,l} \tag{B.14}$$

where the aggregate intensive margin elasticity is weighted by income $\frac{y_l}{Y_l}$:

$$\bar{\epsilon}_{l,l} = \int_{\Lambda} I_l(\lambda) \, \tfrac{y_l(\lambda)}{Y_l} \, \epsilon_{l,l}(\lambda) \, dF(\lambda) \qquad \text{with} \qquad \epsilon_{l,l}(\lambda) = \tfrac{1-\tau_l}{y_l(\lambda)} \tfrac{\partial y_l(\lambda)}{\partial(1-\tau_l)}$$

and the aggregate extensive margin elasticity is weighted by revenue $\frac{\tau_l^e(\lambda) \, y_l(\lambda)}{\bar{\tau}_l^e Y_l}$.

$$\bar{\eta}_{l,l} = \int_{\Lambda} \tfrac{\tau_l^e(\lambda) \, y_l(\lambda)}{\bar{\tau}_l^e Y_l} \cdot \eta_{l,l}(\lambda) \, dF(\lambda) \qquad \text{with} \qquad \eta_{l,l}(\lambda) = \mu_{l,l}(\lambda) \cdot \varsigma_{l,l}(\lambda)$$

$$\text{and} \qquad \mu_{l,l}(\lambda) := (1-\tau_l^e(\lambda)) \cdot \tfrac{\partial I_l(\lambda)}{\partial(1-\tau_l^e(\lambda))}, \quad \varsigma_{l,l}(\lambda) := \tfrac{\partial(1-\tau_l^e(\lambda))}{\partial(1-\tau_l)} \cdot \tfrac{1-\tau_l}{1-\tau_l^e(\lambda)}.$$

Note that the term $\dfrac{\bar{\tau}_l^e Y_l}{\tau_l Y_l}$ multiplying this revenue-weighted elasticity ensures that all terms in the equation are expressed in the same metric (i.e., mechanical change in revenues if there is a marginal increase in the tax rate $\tau_l$).

Next, following similar steps to what we do for own-base responses, we can obtain an analogous expression for cross-base responses:

$$\begin{aligned} \int_{\Lambda} I_k(\lambda) \, \tau_k \, \tfrac{\partial y_k(\lambda)}{\partial \tau_l(\lambda)} + y_k(\lambda) \, \tau_k \, \tfrac{\partial I_k(\lambda)}{\partial \tau_l(\lambda)} \;\; dF(\lambda) = & \int_{\Lambda} I_k(\lambda) \, \tau_k \, \tfrac{\partial y_k(\lambda)}{\partial \tau_l(\lambda)} \, dF(\lambda) \\ & + \int_{\Lambda} y_k(\lambda) \, \tau_k \, \tfrac{\partial I_k(\lambda)}{\partial \tau_l(\lambda)} \, dF(\lambda) \end{aligned}$$

$$\int_{\Lambda} I_k(\lambda) \, \tau_k \, \tfrac{\partial y_k(\lambda)}{\partial \tau_l(\lambda)} + y_k(\lambda) \, \tau_k \, \tfrac{\partial I_k(\lambda)}{\partial \tau_l(\lambda)} \;\; dF(\lambda) = -\frac{\tau_l}{1-\tau_l} Y_l \;\; \frac{\tau_k Y_k}{\tau_l Y_l} (\bar{\epsilon}_{l,k} + \bar{\eta}_{l,k}) \tag{B.15}$$

where we have used the fact that the alternative tax base $k$ is based on a flat tax rate $\tau_k$, such that the effective and marginal tax rates are the same.

By plugging Equations (B.14) and (B.15) into Equation (B.13), we can re-write the deadweight loss associated with increasing the marginal tax rate as a sum of weighted elasticities expressed in revenue-equivalent terms:

$$\frac{dW}{d\tau_l} = -\frac{\tau_l}{1-\tau_l} Y_l \ \ \bar{\epsilon}_{l,l} + \frac{\bar{\tau}_l^e Y_l}{\tau_l Y_l} \bar{\eta}_{l,l} + \frac{\tau_k Y_k}{\tau_l Y_l} (\bar{\epsilon}_{l,k} + \bar{\eta}_{l,k}) \tag{B.16}$$

# C Further Details on Data, Descriptive Statistics, and Methodological Decisions

## Data Structure, Income Definitions, and Weights

**Structure of the raw TAX data.** The structure of the administrative tax records varies over time. From 2009 to 2012, the Uruguayan tax authority (DGI) provided a combined file that included personal income tax records on labor, capital, and pensions, while corporate income tax returns were delivered in separate files. Beginning in 2013, DGI provided separate datasets for each tax base. Until 2014, the combined labor income data include two sources: tax return filings for individuals who submitted them, and third-party reports from the Social Security Administration (SSA) for those who did not. Starting in 2015, individual tax returns and third-party reports from the DGI were received separately. To ensure comparability with earlier years, we construct a unified dataset by retaining the tax return if available, and the third-party report otherwise.

It is worth noting that only a small fraction of workers are required to file an annual tax return. According to DGI regulations, filing is mandatory only for individuals who (1) earn income from multiple sources, (2) receive any income from self-employment, or (3) intend to claim itemized deductions. In contrast, employees with a single wage-paying job and no itemized deductions are exempt, as their monthly withholdings fully offset their annual tax liability. Despite these requirements, any taxpayer may choose to file a return. This is most common among those seeking to claim itemized deductions not included in the standard Form 3100, or deductions related to housing expenses. In practice, in 2011, for instance, only 111,011 taxpayers filed a PLIT annual return (around 9% of formal workers). While this share is higher in the sample of TIEs, it is still only about 41% of our sample.

Corporate income tax returns are provided as separate files, corresponding to tax forms 2148 and 2149. In these forms, self-employment income is labelled as business revenues. These returns do not indicate whether the filer is a self-employed individual subject to the personal labor income tax (PLIT) who has opted into the CIT regime. To approximate this population, DGI provided a list of natural persons reporting to corporate income tax and their corresponding ISIC codes. From this list, we keep the list of codes who most likely correspond to self-employed workers.

Table C.1: List of Selected Activity Codes

| | | | |
|---|---|---|---|
| 41000 | 42100 | 42200 | 42900 |
| 45104 | 46101 | 46102 | 46103 |
| 46104 | 46105 | 46106 | 46109 |
| 52219 | 52220 | 52230 | 52291 |
| 52293 | 52294 | 52295 | 62010 |
| 62020 | 62090 | 63110 | 63120 |
| 63910 | 63990 | 66122 | 66190 |
| 66210 | 66220 | 68101 | 68109 |
| 68201 | 68203 | 68209 | 69101 |
| 69109 | 69201 | 70201 | 70202 |
| 70209 | 71101 | 71103 | 71109 |
| 71200 | 72100 | 72200 | 73100 |
| 73200 | 74101 | 74902 | 74909 |
| 75000 | 82110 | 82910 | 86201 |
| 86202 | 86203 | 86209 | 86909 |
| 96099 | | | |

**Income Definitions.** We maintain consistent definitions of gross labor income over time. Earnings from wages are included in full, while for self-employment income, we use 70% of gross revenues to account for an automatic 30% deduction established in the tax code for self-employment income. Due to a legal change in 2015, mandatory 13th salary and vacation bonuses (*aguinaldo* and *salario vacacional*) were excluded from the definition of gross labor income used to compute marginal tax rates. However, to ensure comparability across years, we continue to include them in our baseline measure of gross labor income.

Capital income is defined as the sum of dividends, real estate income, and other financial income such as interest, royalties, and rights. We exclude non-resident capital income, which only begins to be reported in 2011, coinciding with the reform anticipation period. We also exclude capital gains, since they are difficult to interpret due to the inclusion of previous year losses and other various components (e.g. capital gains due to real estate and vehicle sales).

For corporate income, additional adjustments are required to ensure comparability with the self-employment income reported in the PLIT base. As discussed in Section 2, self-employed individuals may opt into the corporate income tax system through either the presumptive or the real regime. For presumptive regime filers, we define a pre-standard-deduction income as total business revenues reported in the corresponding tax form. For real regime filers, we define it as the sum of operative income (sales plus other operating income) and *other income* reported in a residual line of the tax return. In both cases, we apply an automatic 30% deduction to align the definition with the one used for self-employed income in the PLIT base. This adjustment avoids mechanical changes in gross income when individuals shift between tax bases.

**Weights construction.** Following Weber (2014b), we avoid using reform year weights since they are endogenous. Instead, we construct weights using base-year information on income/revenues (i.e., year $t-1$). This ensures that weights are not influenced by behavioral responses to the policy change.

For the personal labor income tax (PLIT) base, we construct both income and revenue weights. Income weights are used to analyze intensive margin responses and are assigned to individuals who reported income to the PLIT base both in years $t$ and $t-1$. Our preferred definition of income weights is lagged gross labor income winsorized at the 95th percentile, to prevent cases where estimates are heavily driven by very extreme outliers. It is important to note that this 95% threshold corresponds to the 99.95th percentile of the gross labor income distribution, as our sample comprises the top 1% of gross labor income earners. Revenue weights are used for extensive margin responses. We constructed these using lagged tax payments, also winsorized at the 95th percentile. Observations with zero lagged income/PLIT tax payments are assigned missing values in both cases.

Analogously, intensive margin weights for the analysis of cross-tax base responses in the capital income base are defined as lagged capital income winsorized at the 95th percentile. In this case, the 95th percentile is computed conditional on reporting because only 13% of our sample reported some capital income in 2010. As for own-base intensive margin responses, these weights are assigned only to individuals who reported income to capital tax base both in $t-1$ and $t$. Extensive margin responses on the capital income base require to make some assumptions about the share of income previously reported to the PLIT base that is now reported to the capital income tax base. To do this, we split the sample into non-shifters, partial-shifters, and full shifters. For TIEs who become full shifters, we assume that all income reported to PLIT in $t-1$ is shifted to the capital income tax base. In other words, we use income reported to the PLIT base in $t-1$, winsored at the 95%. For those who stay as non-shifters, we apply the same definition. For partial shifters we assume they shift the average share shifted by partial shifters in 2009-2010 (i.e., 12%). If anything, the tax reform should increase the share shifted, hence this approach may be seen as conservative since it potentially understates actual income shifting in the post-reform period. In any case, when estimating the efficiency costs of the reform, we conduct additional sensitivity analysis to this decision. It is also important to note that for taxes based on flat rates, such as capital and corporate taxes, the distinction between income- and revenue-weights is irrelevant since tax payments are proportional to income.

Weights for the corporate income tax base are constructed using the same logic as for capital income. Individuals already reporting corporate income are weighted according to their lagged reported corporate income. Full shifters are assumed to shift all of their prior labor income to the corporate base, while partial shifters are assumed to shift 31% of their prior labor income, based on observed patterns from the pre-reform period.

**SSA Data and Sample.** We complement our analysis with employer-employee administrative records from Uruguay's Social Security Agency (SSA), which provide individual-level labor histories for the universe of workers, both wage earners and self-employed, registered with the SSA for at least one month during the analysis period. These records are generally based on third-party reported income, i.e., reported by employers. Self-employed individuals in the SSA records may have reported hours under two scenarios. First, if they are both wage earners and self-employed, the hours may reflect their work as employees. Second, in Uruguay, the tax and social security systems allow owners of sole proprietorships to be registered as workers. In these cases, SSA records include hours worked by the owner in their own firm. Beyond complying with labor regulations, incentives to participate in the formal sector include access to social insurance benefits such as old-age pensions, unemployment insurance, health coverage, and maternity leave, among others. It is worth noting that the earnings and worker characteristics in SSA records should align with those reported in TAX records, as both rely on the same underlying third-party reported data.

SSA records can be matched only to a subset of the TAX dataset at the individual level. This matching uses a masked version of the identification number, created jointly by the SSA and Tax Agency for this purpose. Since the masked identifiers differ across datasets, the match was performed using a supplementary database of individuals covered by the National Health System. This dataset includes individuals, their spouses, and their children, effectively excluding single individuals without dependents. The resulting matched SSA-TAX dataset covers approximately 75% of the TAX sample.

Table C.2 compares observable characteristics between the full TAX sample and the subsample matched to SSA records. The two groups are similar along key dimensions such as average labor income and several demographic and employment indicators, including the share female, the proportion self-employed, and the incidence of multi-employment. However, there are two main differences. First, non-matched individuals report slightly higher capital and corporate income, resulting in a higher total income on average. This may be related to the second difference that is the average age. While individuals in the matched sample are on average 47 years old, individuals in the non-matched sample are 55. This likely reflects that the matched SSA sample is drawn from national health insurance records, which predominantly cover individuals with dependent underage children. Despite these minor differences, as shown in Table D.2, the SSA subsample shows very similar responses to the tax reform as the full TAX sample, suggesting that selection into the matched sample does not present a major concern.

Table C.2: Descriptive Statistics by SSA Sample

| | Analysis Sample<br>(1) | SSA Sample<br>(matched)<br>(2) | Non-SSA sample<br>(unmatched)<br>(3) |
|---|---|---|---|
| **a. Income Variables:** | | | |
| Labor Income (BPC) | 852.53 | 836.24 | 892.34 |
| | (4.192) | (4.683) | (8.772) |
| Labor Income (% reporting) | 99.75 | 99.82 | 99.57 |
| | (0.041) | (0.041) | (0.101) |
| PLIT Tax Liability (BPC) | 126.17 | 121.86 | 136.71 |
| | (0.938) | (1.027) | (2.023) |
| Capital Income (BPC) | 21.89 | 13.12 | 43.33 |
| | (2.293) | (1.861) | (6.447) |
| Capital Income (% reporting) | 13.49 | 12.72 | 15.37 |
| | (0.284) | (0.329) | (0.556) |
| Corporate Income (BPC) | 18.57 | 16.27 | 24.21 |
| | (1.268) | (1.308) | (2.976) |
| Corporate Income (% reporting) | 3.39 | 3.15 | 3.97 |
| | (0.150) | (0.172) | (0.301) |
| Total Income (BPC) | 892.98 | 865.63 | 959.89 |
| | (5.064) | (5.260) | (11.724) |
| Only reports to PLIT (%) | 88.60 | 89.79 | 85.70 |
| | (0.264) | (0.299) | (0.540) |
| Reports to PLIT and other (%) | 11.14 | 10.02 | 13.87 |
| | (0.262) | (0.296) | (0.533) |
| Reports only to other (%) | 0.26 | 0.18 | 0.43 |
| | (0.042) | (0.042) | (0.101) |
| **b. Individual Characteristics:** | | | |
| Age | 49.22 | 46.71 | 55.39 |
| | (0.076) | (0.079) | (0.137) |
| Female (%) | 28.77 | 29.36 | 27.32 |
| | (0.376) | (0.449) | (0.688) |
| Wage-Earners (%) | 66.00 | 67.11 | 63.30 |
| | (0.394) | (0.463) | (0.744) |
| Only Self-Employed (%) | 3.54 | 3.05 | 4.76 |
| | (0.154) | (0.170) | (0.328) |
| Both W.E. and S.E. (%) | 25.25 | 24.48 | 27.13 |
| | (0.361) | (0.424) | (0.686) |
| Missing/N.A. (%) | 5.20 | 5.36 | 4.81 |
| | (0.185) | (0.222) | (0.330) |
| No employer/NA (%) | 7.52 | 7.59 | 7.35 |
| | (0.219) | (0.261) | (0.403) |
| One job (%) | 51.77 | 53.21 | 48.24 |
| | (0.415) | (0.492) | (0.771) |
| Two jobs (%) | 15.91 | 15.60 | 16.66 |
| | (0.304) | (0.358) | (0.575) |
| Three or more jobs (%) | 24.80 | 23.60 | 27.75 |
| | (0.359) | (0.419) | (0.691) |
| N | 14,478 | 10,276 | 4,202 |

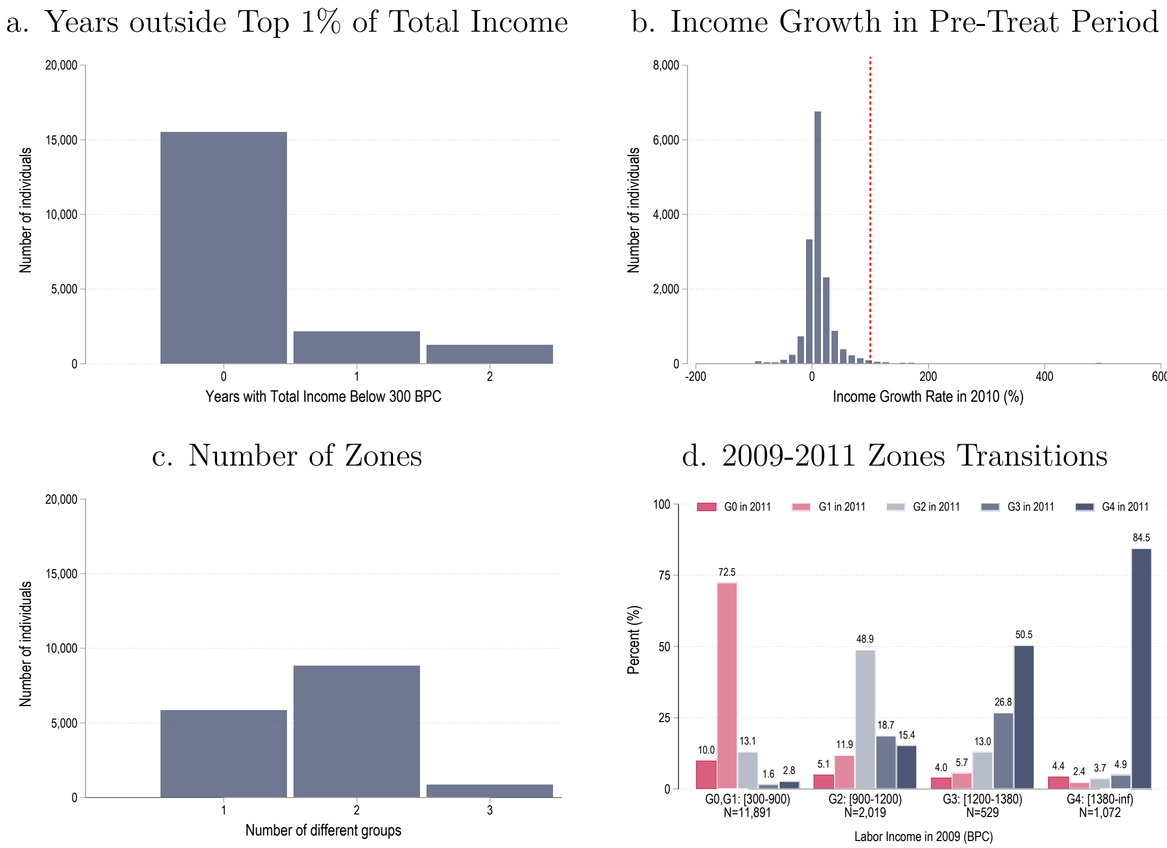
Notes: This table reports descriptive statistics for the main samples used throughout the paper, based on 2010 *TAX* records. Column (1) includes statistics for our final analysis sample: the subset of TIEs who (a) never fell below 300 BPC in total income (roughly the 95th percentile of gross labor income), (b) did not switch income zones (G1âG4) every year between 2009 and 2011, and (c) did not experience extreme income growth in the pre-treatment period (greater than 100%). Columns (2) and (3) split this final analysis sample based on whether the individual could be matched to the *SSA* sample using the crosswalk between taxpayer and social security identifiers. Column (2) includes TIEs who were successfully matched, i.e., individuals for whom we observe third-party-reported income in *SSA* records. Column (3) includes those not matched to the *SSA* data. Panel (a) reports averages for income variables. Panel (b) summarizes individual characteristics, including age, gender, employment type, and number of income sources. Standard errors of the means are reported in parentheses. All statistics are based on *TAX* records.

## Income Dynamics and Mobility in the Pre-Reform Period

Figures C.1 and C.2 help understanding pre-treatment income dynamics among individuals in our sample, in particular around the relevant thresholds for our analysis, i.e., top 5% and income zones G1:G4. Panel (a) of Figure C.1 shows that the vast majority of individuals remain within the top 5% of the income distribution throughout the 2009-2011 period. Out of 18,930 TIEs defined by gross labor income, only 3,419 (18%) ever fall below the top 5% in any of those years. Panel (b) presents the distribution of individual income growth rates during this period, while Panel (c) shows the distribution of the number of different income zones a TIE belonged to between 2009 and 2011. As described in Section 4, we exclude individuals with extreme patterns of mobility based on these two variables. Specifically, we drop TIEs who switch between three different income zones over the three-year period (859 individuals, or 5.6% of the sample), and those whose income growth exceeds 100% (174 individuals, or 1.2%). We use these filters for statistical precision purposes. In Appendix D, we show that our main results are robust to including these individuals, though the estimates become less precise.

Panel (d) of Figure C.1 tracks transitions across income zones, confirming the overall income zone stability for most TIEs. Among individuals in zone G4 in 2009, 84.5% remain in the same zone by 2011. Similarly, 72.5% of TIEs in the lower zones (G0, i.e. out of top 1% but within top 5%, and G1) remain in their initial zone over the same period. For zone G2, the persistence rate is somewhat lower, though still close to 50%. The main exception is zone G3, where a substantial share of individuals transition upward, particularly into zone G4. This pattern reflects the narrow width of the G3 bracket combined with secular income growth trends, which cause many 2009-G3 individuals to move above the upper bound of their initial zone by 2011. This is illustrated in Figure C.2, which shows the evolution of median gross labor income by 2009 income zone. While median income in most groups remains within the same zone throughout the period, the median for G3 exceeds the upper bound of the zone by 2011, explaining the lower persistence rate observed in panel (d).

Figure C.1: Sample Selection Criteria and Zone Mobility




Notes: This figure summarizes the three main sample selection filters used to construct the analysis sample, along with pre-reform zone mobility patterns. Panel (a) displays the distribution of individuals by the number of pre-reform years (2009-2011) in which their total income fell below 300 BPC, roughly the 95th percentile of the gross labor income distribution. Panel (b) shows the distribution of pre-reform labor income growth rates. The red vertical dashed line represents the cutoff used for identifying highly volatile earners, defined as individuals whose income grew by more than 100% between 2009 and 2010. Panel (c) reports the distribution of the number of distinct income zones (G0-G4) to which each individual belonged during 2009â2011. Panel (d) presents zone transition probabilities between 2009 and 2011: each bar shows the distribution of 2011 income zones conditional on the 2009 zone. Labels on the x-axis indicate the number of TIEs in each 2009 zone, and numbers above the bars indicate the share of TIEs transitioning into each 2011 group. All estimates are based on *TAX* records. See Section 4 and Appendix C for details.

Figure C.2: Evolution of Median Income by 2009 Zone


Notes: This figure shows the evolution of median gross labor income (in BPC) between 2009 and 2015 by initial income zone. Zones are defined using labor income in 2009, where G1 includes TIEs with income between 600 and 900 BPC, G2 from 900 to 1200 BPC, G3 from 1200 to 1380 BPC, and G4 above 1380 BPC. Horizontal solid lines at 900, 1200, and 1380 BPC denote the cutoffs between income zones. The vertical dashed line at 2011.5 marks the timing of the tax reform. Median values are computed within bins defined over year-by-group cells.

# D Further Results on Own-Tax Base Responses

In this appendix, we present additional results on own-tax-base responses. We first reproduce our main reduced-form figures for own-base responses using unweighted specifications. Then, we continue by testing how our results look like if we use a specification in levels and individual fixed effects, rather than the first-difference baseline approach. Third, we report estimates that use the observed change in net-of-tax rates rather than predicted changes, moving away from ITT estimates. Fourth, we move to a series of tests that address three critical challenges in our setting: 1) short-pre reform period, 2) endogeneous selection into treatment, and 3) mean reversion. Finally, we also conduct a range of additional robustness checks, including specifications with sector and sector-year fixed effects, alternative weighting schemes, winsorizing thresholds, and sample selection criteria (e.g., sample restrictions, exclusion of G3, among others).

**Unweighted Reduced-Form Evidence.** Figure D.1 replicates the difference-in-differences (DiD) estimates for both intensive and extensive margin responses using the same specification as Figure 3 in the main text, but without applying income or revenue weights. The results remain qualitatively similar: the tax reform leads to a decline in reported gross labor income, driven by both intensive and extensive margin responses. In terms of timing, we observe both anticipatory and concurrent responses, with the latter being larger in magnitude and thus driving the overall negative net effect. Intensive margin responses appear to materialize quickly, concentrating in the year before and the year after the reform. Extensive margin responses are also negative but take longer to build up, likely reflecting higher adjustment frictions along this dimension. Table D.1, columns (3) and (7), reports the year-specific reduced form point estimates for the intensive and extensive margins, respectively. This table helps clarify the magnitude of the reduced-form effects. Comparing columns (3) and (7) with their weighted counterparts in columns (4) and (8), we observe larger responses when applying weights. For example, the 2012 DiD coefficient for intensive margin responses is -0.038 in the unweighted specification, versus -0.047 in the weighted one. For the extensive margin, the corresponding estimates are -0.024 and -0.038, respectively. As discussed in the main text when analyzing heterogeneity by treatment intensity, this pattern suggests that higher-income or higher-revenue individuals exhibit stronger behavioral responses.

**Outcomes Measured in Levels Rather than Changes.** Figure D.2 replicates the main DiD estimates presented in the graphical analysis in Section 6, but uses outcome variables expressed in levels rather than changes. This corresponds to a two-way fixed effects (TWFE) specification based on Equations (16) and (17), with outcomes measured in levels rather than first differences. The results are consistent with those obtained using the change-based specification. The corresponding point estimates are reported in Table D.1, columns (1), (2), (5), and (6). As in previous figures, we present both unweighted (green) and weighted (blue) estimates. Unlike in the change-based specifications, the TWFE models using log-level outcome variables show little difference between weighted and unweighted results. However, it is important to note that estimates from the two approaches are not directly comparable in magnitude. While the change-based specification captures year-on-year changes (i.e., relative to $t-1$), the TWFE specification measures changes relative to a fixed baseline year , i.e., 2009, the omitted year. Consequently, the weights in the TWFE specification are constructed using 2009 income and revenue data. These differences make direct comparisons more subtle, specially when discussing the magnitude of the effects.

**Estimates Using *Observed* Change in Marginal Net of Tax Rates.** As noted in Section 5, our baseline estimates correspond to ITT estimates. In column (2) of Table D.2, we present results using *observed* marginal net-of-tax rates as the endogenous variable. Despite the conceptual differences between the two approaches, the results are extremely similar. Using *observed* marginal net-of-tax rates we obtain an intensive margin elasticity is 0.739 ($p$-value = 0.007), only slightly smaller than the 0.770 ITT estimate reported in column (1) . The fact that these two estimates are so close suggests that bracket mobility is limited in this setting, as discussed in detail in Appendix C making the distinction between ITT and TOT less relevant for this context. As discussed in Section 5.3, tax rates are not defined for taxpayers who drop out of the tax base. As a result, we cannot estimate TOT-like elasticities on the extensive margin.

**Anticipation and Endogenous Selection into Treatment.** Although our baseline specification uses data from 2009 to 2011 to define treatment and control groups, the presence of anticipatory behavior raises concerns about endogenous selection into treatment and, consequently, the validity of our identification strategy. To address this, we implement three additional robustness checks. First, we replicate our baseline analysis excluding the 34% of taxpayers who switched income brackets between 2010 and 2011. This approach ensures that bracket movements in 2011, potentially driven by anticipatory responses, do not affect treatment assignment. At the same time, it still allows us to capture anticipatory responses among individuals who remained in the same bracket. Importantly, this exclusion removes precisely those taxpayers who exhibit the strongest anticipatory responses, i.e., those who reacted so strongly to the reform announcement that they changed income brackets before the policy was implemented. It also avoids assigning treatment based on bracket positions in 2011, arguably the most relevant pre-reform year for measuring potential policy exposure. Hence, this strategy considerably reduces the sample size, restricts the analysis to individuals with more stable incomes, and uses a less precise measure of intention to treament. As a result, the estimated effects may be attenuated and should be interpreted as reflecting the direction, rather than the full magnitude, of the behavioral response. In this sense, the estimates can be viewed as a lower bound on the overall effect of the reform.[37] Second, we impose an even stricter criterion, requiring taxpayers to remain in the same bracket from 2009 to 2011. Finally, instead of removing actual switchers, we exclude taxpayers who were sufficiently close to bracket thresholds, where even small income changes can lead to switching brackets.

Elasticity estimates for these three tests are reported in columns (3) through (5) of Table

[37] We choose to drop 2010-2011 bracket switchers rather than define treatment solely using 2009-2010 data in order to avoid including taxpayers who are clearly misclassified. For instance, if we retained switchers and defined treatment based only on 2009-2010 information, some individuals who we know were in G2 or G4 in 2011 would have been incorrectly classified as controls.

D.2.[38] For the intensive margin, concurrent elasticities are 0.41, 0.47, and 1.10, all statistically significant at the 1% level ($p$ = 0.003, 0.004, and <0.001, respectively). Anticipatory elasticities are -0.18 ($p < 0.001$ ), -0.16 ($p = 0.032$), and -0.30 ($p < 0.001$). For the extensive margin, concurrent elasticities are 0.78, 0.99, and 2.67, all significant at the 5% level ($p$ = 0.097, 0.048, and <0.001, respectively), while anticipatory responses remain negligible. There are two main takeaways from these results. First, as expected, the first two tests yield results that are qualitatively similar to our baseline analysis but smaller in magnitude, consistent with the fact that we are excluding individuals who are more likely to respond to the reform. When we relax this restriction and drop only those close to bracket thresholds, the estimated elasticities return to levels similar to our baseline specification. Second, the fact that anticipatory elasticities remain closer to our baseline estimate suggests that taxpayers are not anticipating the reform by shifting brackets, and that endogenous selection into treatment is not the main driver of anticipation effects. Instead, anticipation appears to happen within brackets, which is entirely consistent with the progressive nature of the tax schedule: taxpayers have incentives to shift income across time but large amounts would be taxed with higher marginal rates. Overall, these results reinforce the credibility of our baseline estimates and provide further reassurance that they are not driven by endogenous selection into treatment.

Figure D.3 and Tables D.3 and D.4, complement these results. Figure D.3 presents graphical reduced-form evidence from the three alternative specifications. Panel (a) shows results for the intensive margin, while panel (b) focuses on the extensive margin. Tables D.3 and D.4 report full regression estimates - including reduced form, first stage, and 2SLS elasticities following the structure of Table 3 in the main text - for each of the robustness specifications.

**Extending pre-reform period.** A limitation of the TAX records is their short pre-reform window, which restricts how far back we can test the parallel trends in the pre-treatment period. To address this, we use SSA data starting in 2000, which provide nine additional years of pre-treatment information. While SSA data works well for analyzing pre-trends, there are a few caveats. First, the PLIT was introduced in 2007, so changes in trends around that year should be interpreted with caution. Second, SSA data only capture certain components of gross labor income, i.e., those subject to third-party reporting. While this means SSA income is only a subset of gross labor income, it still represents a substantial share: 65% for the average taxpayer and 70% for the median. This makes it a reasonable dataset for analyzing

[38] Additional details, including reduced-form and first-stage estimates, as well as graphical evidence, are reported in Appendix D.

pre-trends and rule out concerns about mean reversion or secular trends in inequality, though less suitable for studying in detail how TIES respond to taxation. Third, as explained in Section 4, SSA data can be matched to tax records for only 75% of the sample.

Figure D.4 replicates our baseline graphical analysis using SSA data from 2000 to 2015. Most importantly, the figure strongly supports the parallel trends assumption both at the intensive and extensive margins. In particular, the dynamics of gross labor income, and the reporting to PLIT binary variable are very similar between treatment and control groups until 2010. Moreover, the post-treatment evolution closely resembles the one observed in the TAX data, providing additional support for our main finding of strong negative responses to the 2012 tax reform at both margins. It is also worth noting that the intensive margin anticipatory response goes in the opposite direction to the one observed in the TAX data. We attribute this difference to the nature of third-party reporting. If TIEs want to avoid higher future tax rates through inter-temporal shifting, they may prefer using income sources that are less visible to the tax authority on the detriment of third-party reported income. In the data, this behavior would show as a negative anticipatory response in the SSA records, which relies on third-party reported income, and a positive anticipatory effect in the TAX data, which captures both third-party and non-third-party reported gross labor income.[39] Column (6) of Table D.2 report summary elasticity estimates for the SSA sample using TAX data, while Tables D.3 and D.4, report full results for intensive and extensive margin responses, respectively. While there are some differences in the magnitudes of the estimates, responses are mostly similar to those obtained in the TAX sample, thus indicating that selection to the SSA sample is not a major concern. For completeness, column (7) reports elasticity estimates obtained in the SSA sample using SSA data. However, as discussed before, elasticity estimates obtained from SSA data are not particularly informative of the full set of behavioral responses.

**Mean reverion.** Mean reversion and long-term trends in inequality are important concerns when evaluating tax reforms that change tax rates across the income distribution (see Saez et al. 2012 for a detailed discussion). To address these concerns, we follow the approach proposed by Jakobsen and Søgaard (2022). Based on earlier work that addresses mean reversion by controlling for pre-treatment income (Auten and Carroll, 1999; Gruber and Saez, 2002; Saez et al., 2012; Weber, 2014b), they develop a non-parametric and visually intuitive

[39] It is also worth noting that while extensive margin responses also show a negative statistically significant coefficient in year 2011, this appears to be driven by an unusual difference in 2010 that goes back to normal in 2011, as suggested by the raw data depicted in panel (a). We therefore do not interpret this as evidence of a negative anticipatory response on the extensive margin, particularly given that such a response would be unlikely in light of third-party reporting dynamics.

test that compares changes in income trends across the income distribution before and after the reform. We present these results in panels (a) and (b) of Figure D.5. The figure shows that, when comparing the pre-reform and post-reform periods, income trend differentials remained stable in regions that were not affected by the reform (G1), while income growth declined in regions that were directly affected either by changes in the marginal or effective net-of-tax rates (G2:G4). Our estimates are imprecise given the reduced number of TIEs in each bin. However, the graphical evidence strongly supports the assumption that income trend differentials across the distribution would have remained constant in the absence of the reform, reinforcing the validity of our research design.

We complement this visual validation test with estimates based on the more traditional approach. One key concern with controlling for base-year income is that, while it helps account for non-tax-related income changes, it may also absorb much of the exogenous variation in tax rates that is essential for identification (Giertz, 2010; Saez et al., 2012). This issue is particularly relevant in settings with a single tax reform and short panel data, as precisely our context (Saez et al., 2012). Nonetheless, by using SSA data, we are able to include additional pre-TAX income controls in our specification without compromising the variation used for identification. Column (8) of Table D.2 reports the main results from this exercise, while Tables D.3 and D.4, report full results for intensive and extensive margin responses, respectively. The key finding is that including pre-TAX labor income decile fixed effects does not change our conclusions significantly, although it slightly reduces the estimated intensive margin elasticity and increases the extensive margin estimate. Finally, specifications that restrict the sample to taxpayers with more stable income, such as those reported when discussing endogenous selection into treatment, also suggest that our results are not driven by mean reversion. These specifications mechanically limit mean reversion by restricting the extent of changes in TIEs' income, although at the cost of reduced external validity due to the focus on stable-income TIEs. Taken together, these tests suggest that our main results are not driven by mean reversion.

**Other robustness tests.** Finally, we discuss a series of additional specification and robustness tests. We report estimates that include additional control variables as well as sector- and sector-year fixed effects to rule out that responses are driven by sector-specific trends. We also conduct sensitivity analysis on the definition and inclusion of weights, as well as for the winsoring levels of the log-change variables. We also test the robustness of our results to different criteria for sample selection, including the filters used to select our main sample of analysis, different definitions for the donut regressions, exclusion of taxpayers in G3, among others.

Tables D.5 and D.6 focus on a series of specification tests without changing the sample definition. All specifications use the baseline analysis sample and explore the sensitivity of our results to alternative specifications. For reference, column (1) in both tables replicates our preferred specification. Column (2) adds a set of pre-treatment control variables, including industry-specific time trends (interacting year dummies with ISIC-2 sector indicators), age and age-squared, an indicator for being in prime working age, and indicators for having reported capital or corporate income in the pre-treatment period (2009-2010). The elasticity estimates remain nearly identical, both in sign and magnitude, showing that our baseline results are not sensitive to the inclusion of these additional controls. Columns (3) through (5) in Table D.5 assess the impact of alternative winsorizing choices for the outcome variable $\Delta \log y_i^l$. While our baseline results are based on winsoring at the 1st and 99th percentiles, these alternative specifications use no winsoring, winsoring at the 0.1st and 99.9th percentiles, and at the 0.5th and 99.5th percentiles. For all cases, estimates remain in line with our main conclusions, with slightly larger effect sizes when the winsoring window is narrower.

Finally, columns (6) and (7) in Table D.5, and columns (3) and (4) in Table D.6, present results from alternative weighting strategies. Columns (6) and (3) report unweighted elasticity estimates, while columns (7) and (4) use alternative weights where the winsoring threshold is set at 1% (corresponding to the 99.99th percentile of the full distribution), instead of the 5% threshold (99.95th percentile) used in the baseline. Qualitatively, results remain consistent across specifications. However, elasticities are smaller when no weights are applied. For instance, the short-term elasticity on the intensive margin falls from 0.770 in the baseline to 0.401, and from 2.64 to 2.26 on the extensive margin. In contrast, allowing for more extreme weights (i.e., reducing the winsoring window) yields larger estimates, i.e., 1.433 and 3.12, respectively. This result is a first hint of stronger behavioral responses among TIEs at the very top of the income distribution.

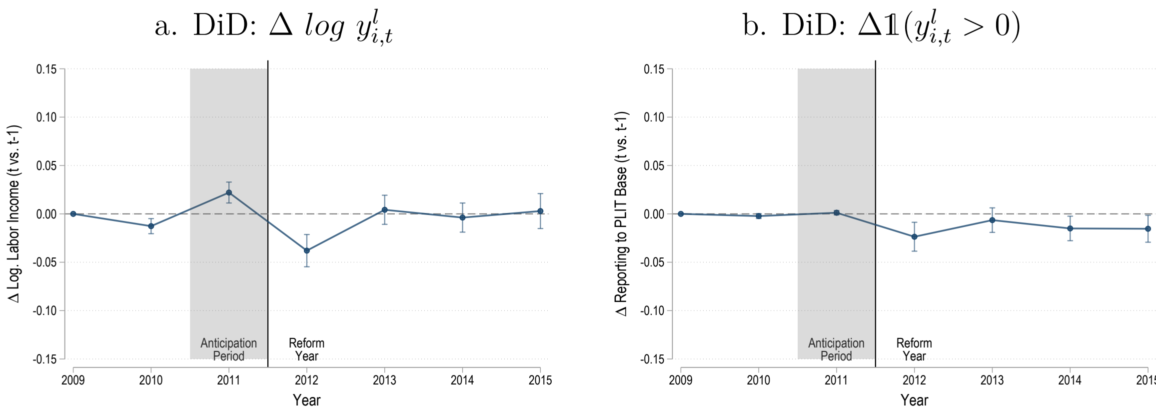
Figure D.6 explores this further by showing how elasticity estimates change with additional winsoring thresholds. Panel (a) focuses on the intensive margin and panel (b) on the extensive margin. Across both margins, responses are consistently larger when higher-income individuals are allowed to have more extreme weights, supporting the idea that TIEs with very high income responded more strongly to the reform. This interpretation aligns with the heterogeneity analysis reported in Section 6, where we show that most of the response is driven by taxpayers who are more intensively treated, i.e., those experiencing larger changes in marginal and effective tax rates, who are also in the upper end of the income distribution. One exception is the anticipatory response, which appears stronger when no weights or heavier winsoring is used. Together, these results suggest that anticipatory behavior may be driven more by lower-income individuals within the high-intensity treatment group, since the heterogeneity analysis still shows that extensive margin responses are driven by the high-intensity of treatment group.

Tables D.7 and D.8 report additional robustness checks based on alternative sample selection criteria. As before, column (1) replicates our baseline estimate. Columns (2) through (4) focus on the two sample restrictions aimed at excluding taxpayers with extreme pre-treatment mobility. Column (2) includes back in the sample individuals who were in three different G1-G4 groups during the pre-treatment period, as well as TIEs with income growth above 100% in that same period. The results remain qualitatively the same, although point estimates are slightly larger. This is probably reflecting that the excluded taxpayers are more sensitive to the reform. However, as discussed earlier, our preferred sample provides more precisely estimated parameters, particularly for anticipatory and extensive margin responses, where standard errors increase notably in this less restricted sample. Columns (3) and (4) apply these two filters separately, and results are similar to the baseline.

Columns (5) through (8) present specifications that exclude individuals for whom treatment assignment is more fuzzy. Column (5) drops taxpayers who ever fell into G3, a group where marginal net-of-tax rates did not change, although effective rates did. Column (6) excludes TIEs classified as controls but whose income was at least once within 25 BPC of a treatment threshold (G2 or G4). Columns (7) and (8) implement similar restrictions based on the average or minimum distance to other income zones, excluding those in the bottom 5th percentile of the distance distribution. Across all these specifications, elasticity estimates, both at the intensive and extensive margins, remain qualitatively and quantitatively consistent with our main results.

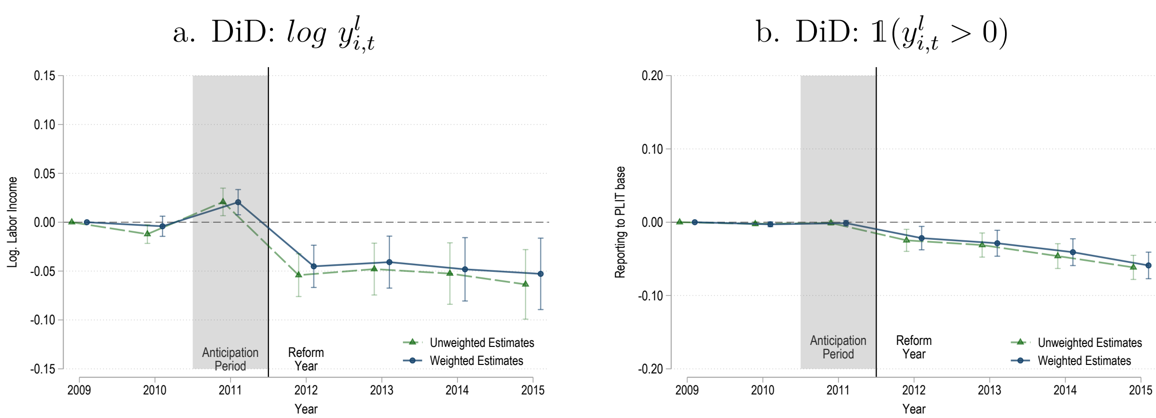
Finally, columns (9) through (12) impose increasingly strict criteria for remaining in the analysis sample based on total income. In our baseline specification, we allow individuals to drop as low as the 95th percentile of the total income distribution. This decision reflects the trade-off between accounting for normal income dynamics or mobility and restricting our analysis to taxpayers who are consistently in the top of the income distribution. The alternative specifications raise this lower bound in 1 percentage point increments from the 96th to the 99th percentile. The results remain broadly similar across specifications, although point estimates tend to decrease as the restriction becomes more stringent. This pattern is consistent with previous findings: restricting the sample to more income-stable TIEs results in lower estimated elasticities, possibly because more responsive individuals are also those with more volatile earnings patterns.

Figure D.1: Own-Tax-Base Responses: Unweighted Figures




Notes: This figure illustrates the own-tax base reduced-form effects of the 2012 tax reform using unweighted regressions. Estimates depicted here correspond to the exact same strategy used in panels (b) and (d) in Figure 3 of the main text, but estimated without applying any weights. Panels (a) focuses on intensive margin responses and reports dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change in gross labor income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. Panel (b) focuses on extensive margin responses and reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^l_{i,t} > 0)$, with $\mathbb{1}(y^l_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT base. As such, $\Delta\mathbb{1}(y^l_{i,t} > 0)$ takes values -1, 0, or 1. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area corresponds to 2011, the anticipation period. All figures are based on *TAX* records. 99% confidence intervals are based on standard errors clustered at the individual level. Full estimates, standard errors, and sample sizes are reported in columns (3) and (7) of Table D.1, Appendix D.

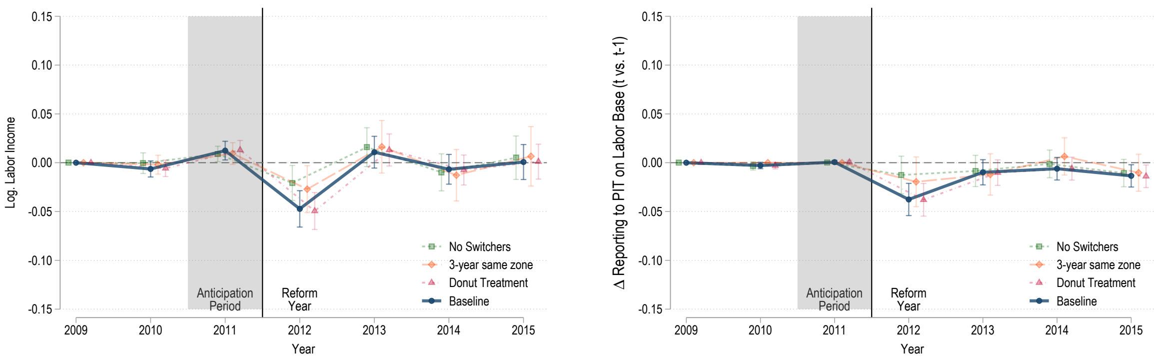
Figure D.2: Own-Tax-Base Responses: Variables in Levels, Reference Year = 2009




Notes: This figure reports dynamic DiD estimates of the own-tax base reduced-form effects of the 2012 tax reform using outcome variables in levels rather than changes. Panel (a) focuses on intensive margin responses and reports coefficients from a specification similar to Equation (16) but using log gross labor income as the outcome. Panel (b) focuses on extensive margin responses and reports coefficients from Equation (17) using the indicator $\mathbb{1}(y^l_{i,t} > 0)$ for reporting any income to the PLIT base, i.e., $\mathbb{1}(y^l_{i,t} > 0)$. Since outcomes are no longer first-differenced, these specifications include individual-level fixed effects and use 2009 as the reference year. In both panels, estimates in blue correspond to our preferred specifications using income- and revenue-based weights, respectively, while estimates in green depict unweighted regressions. Since all estimates are relative to 2009, weighted estimates use 2009 income and revenue weights. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area corresponds to 2011, the anticipation period. All figures are based on *TAX* records. 99% confidence intervals are based on standard errors clustered at the individual level. Full estimates, standard errors, and sample sizes are reported in columns (1), (2), (5), and (6) of Table D.1, Appendix D

Figure D.3: Own-Tax Base Responses: Endogenous Selection into Treatment

a. DiD: $log\ y^l_{i,t}$ b. DiD: $\mathbb{1}(y^l_{i,t} > 0)$

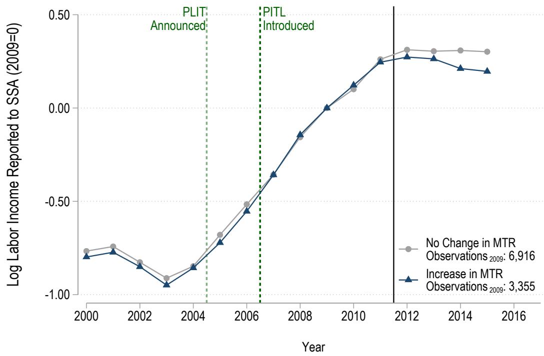



Notes: This figure reports dynamic DiD estimates of own-tax base responses across three robustness specifications discussed in Section 6. Estimates correspond to the same specification used in panels (b) and (d) of Figure 3, but are based on the sub-samples used in columns (3), (4), and (5) of Table D.2. For comparison purposes, we also depict estimates from our baseline specification from column (1). Panel (a) focuses on intensive margin responses and depicts dynamic DiD coefficients from Equation (16), using the log change in gross labor income as the outcome variable, winsorized at the 1st and 99th percentiles. Panel (b) focuses on extensive margin responses and reports coefficients from Equation (17), using $\Delta\mathbb{1}(y^l_{i,t} > 0)$ as the outcome variable, where $\mathbb{1}(y^l_{i,t} > 0)$ is an indicator for reporting any income to the PLIT base. Estimates in blue correspond to the baseline specification. Estimates in green, labeled as "no switchers", correspond to the sample to the 9,382 TIEs who did not change tax brackets between 2010 and 2011. Estimates in orange, labeled as "3-years same zone" correspond to the 5,833 TIEs who remained in the same income zone (G1:G4) throughout 2009-2011. Finally, estimates in red, labeled "Donut Treatment" excludes TIEs whose average absolute distance to a bracket threshold was less than 25 BPC in the pre-treatment period (bottom 5% of the distribution), yielding a sample of 14,228 TIEs. As described in Section 5, these are weighted estimates, using income weights for intensive margin responses and revenue weights for extensive margin responses. Details on the construction of these weights are provided in Appendix C. All estimates are based on *TAX* records. 99% confidence intervals are based on standard errors clustered at the individual level. The vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area corresponds to 2011, the anticipation period.

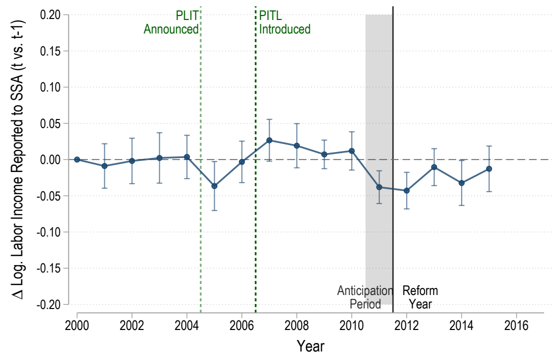
Figure D.4: Own-Tax-Base Responses: Graphical Evidence based on SSA Records

Intensive Margin: Log. Gross SSA Labor Income

a. Normalized Raw Trends (2009=0)

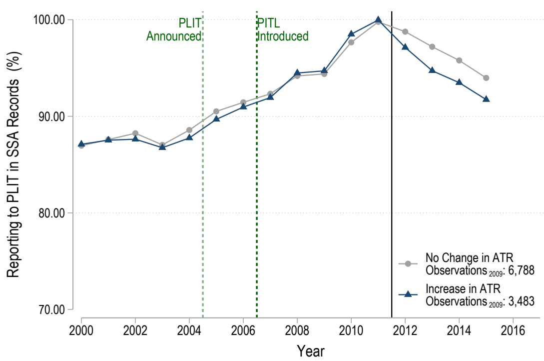



b. DiD: $\Delta\ log\ y^l_{i,t}$

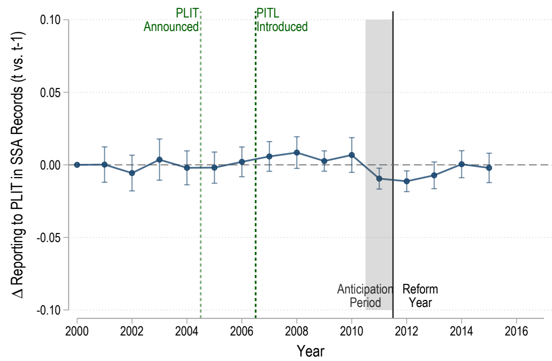



Extensive Margin: Reporting to PLIT base in SSA Records

a. Raw Trends



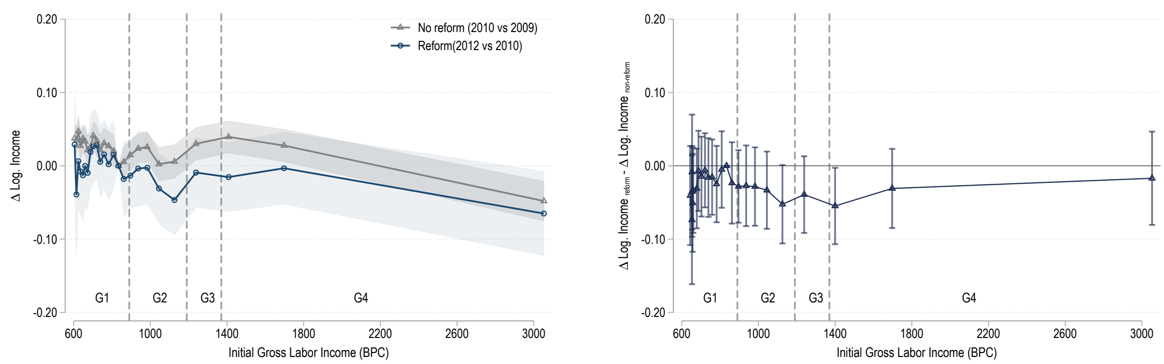

b. DiD: $\Delta\mathbb{1}(y^l_{i,t} > 0)$




Notes: This figure illustrates the evolution of income reported to the SSA for an extended pre-treatment period: 2000-2015. Three things are worth noting. First, income reported to the SSA cover only a subset of gross labor income components, i.e., those subject to third-party reporting. Second, SSA data can be matched to 75% of TIEs in our analysis sample based on TAX records. Third, due to changes in how SSA data were recorded, 1,212 TIEs in our sample entered the SSA records in bulk in March 2008 and January 2010. For these individuals, we set all variables to missing for the year they entered the SSA records and for all prior years. See Section 4 and Appendix C for further details. Panels (a) and (b) focus on intensive margin responses. Panel (a) depicts the raw evolution of log gross labor income reported to the SSA from 2000 to 2015, normalized to 2009 values, for treated and control TIEs without further adjustments. Estimates in blue represent TIEs in the treatment group, defined in Section 5 as $treat^{MTR} = 1$, and estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (b) reports dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change in gross labor income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. As described in Section 5, these correspond to income-weighted estimates. In this case, since estimates based on SSA records are not intended to reflect changes in tax revenues, and weights based on a subset of gross labor income would be misleading, we weight the estimates using average pre-treatment income weights derived from the *TAX* records. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (c) and (d) focus on extensive margin responses. Panel (c) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the PLIT base in the SSA records, with the number of observations in 2009 shown in the bottom right corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (d) reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^l_{i,t} > 0)$, with $\mathbb{1}(y^l_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT base in the SSA records. As such, $\Delta\mathbb{1}(y^l_{i,t} > 0)$ takes values -1, 0, or 1. As described in Section 5, these are revenue-weighted estimates. As for intensive margin responses, we use pre-treatment average weights, in this case based on revenue weights from the *TAX* data. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical black line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. In addition, green vertical lines indicate when the introduction of PLIT was announced and when it was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *SSA* records. Additional estimates are reported in column (7) of Tables D.3 and D.4, Appendix D.

Figure D.5: Log-Change across the Labor Income Distribution: Reform vs. Non-Reform Periods

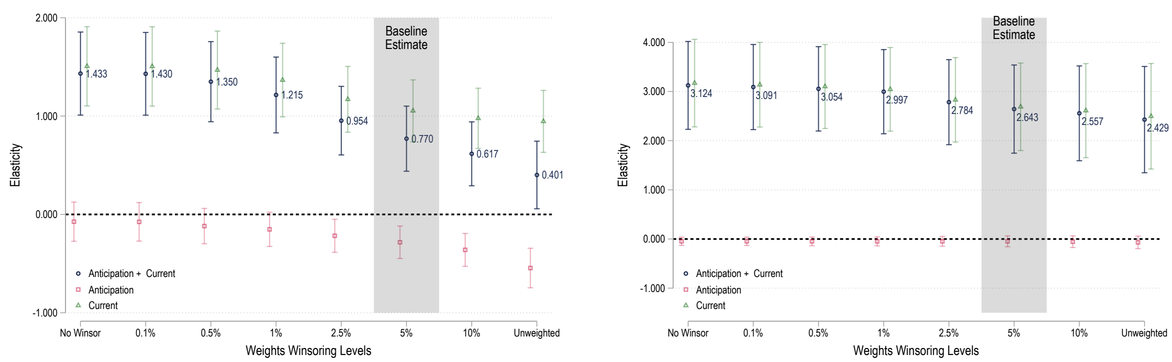
a. Log-Change | b. Difference in Log Change




Notes: This figure illustrates changes in income growth patterns across the income distribution. The sample includes TIEs already in the top 1% in 2009 (i.e., gross labor income above 600 BPC), since including individuals with gross labor income between 300-600 BPC in 2009 who cross the 600 BPC threshold later introduces substantial mechanical variation in income growth. Following Jakobsen and Søgaard (2022), we split the sample into 25 bins with equal numbers of individuals. Panel (a) shows the estimated average log-change in income by bin and period. We define the no-reform period as 2009-2010 and the reform period as 2010-2012, including the anticipation period as part of the reform period. Estimates for the no-reform period are shown in gray; reform-period estimates are shown in blue. Because the anticipation period must be included in the reform period and we lack pre-2009 data, the two periods differ in length. However, this does not affect comparisons across the distribution, as all estimates are expressed relative to a reference point which we set at bin #15. In this bin, the median income is 825 BPC, just below the first income threshold affected by the reform. Log-change in income is winsorized at the 1st and 99th percentiles. Panel (b) shows the difference in average income growth between the reform and no-reform periods, again relative to the same reference bin. Initial income always refers to gross labor income measured in 2009 and 2010. Ninety-five percent confidence intervals are based on robust standard errors.

Figure D.6: Sensitivity Analysis to Weights

a. Intensive Margin | b. Extensive Margin




Notes: This figure presents a sensitivity analysis of our short-run elasticity estimates to alternative weighting and winsoring strategies. Panel (a) focuses on intensive margin responses, and panel (b) on extensive margin responses. Each panel reports elasticity estimates under eight specifications: no winsoring, winsoring at the 0.1%, 0.5%, 1%, 2.5%, 5%, and 10% tails of the log income change distribution, and an unweighted specification. For each specification, we report the short-run elasticity (sum of anticipation and current response), the anticipation elasticity, and the current response elasticity. Estimates are obtained from 2SLS regressions based on Equations (20) and (21), using the corresponding set of weights (income or revenue weights, depending on the margin) and winsoring thresholds. 95% confidence intervals are based on standard errors clustered at the individual level. Labels above the markers correspond to the point estimate of the short-run elasticity. Shaded boxes highlight our baseline specification (5% winsoring).

Table D.1: Own-Tax-Base: Dynamic DiD Estimates

| | Intensive Margin | | | | Extensive Margin | | | |
|---|---|---|---|---|---|---|---|---|
| | $log\ y^l_{i,t}$ | $log\ y^l_{i,t}$ | $\Delta\ log\ y^l_{i,t}$ | $\Delta\ log\ y^l_{i,t}$ | $\mathbb{1}(y^l_{i,t} > 0)$ | $\mathbb{1}(y^l_{i,t} > 0)$ | $\Delta\mathbb{1}(y^l_{i,t} > 0)$ | $\Delta\mathbb{1}(y^l_{i,t} > 0)$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **a. Pre Reform Years** | | | | | | | | |
| Any Treat × 2010 | -0.012*** | -0.004 | -0.013*** | -0.006** | -0.002** | -0.003** | -0.002** | -0.003** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.001) | (0.001) | (0.001) | (0.001) |
| Any Treat × 2011 | 0.021*** | 0.020*** | 0.022*** | 0.012*** | -0.001 | -0.001 | 0.001 | 0.001 |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.001) | (0.001) | (0.001) | (0.001) |
| **b. Post Reform Years** | | | | | | | | |
| Any Treat × 2012 | -0.054*** | -0.045*** | -0.038*** | -0.047*** | -0.025*** | -0.022*** | -0.024*** | -0.038*** |
| | (0.008) | (0.008) | (0.006) | (0.007) | (0.006) | (0.006) | (0.006) | (0.006) |
| Any Treat × 2013 | -0.048*** | -0.041*** | 0.004 | 0.011* | -0.031*** | -0.029*** | -0.006 | -0.010* |
| | (0.010) | (0.010) | (0.006) | (0.006) | (0.006) | (0.007) | (0.005) | (0.005) |
| Any Treat × 2014 | -0.053*** | -0.048*** | -0.004 | -0.007 | -0.046*** | -0.041*** | -0.015*** | -0.006 |
| | (0.012) | (0.013) | (0.006) | (0.006) | (0.007) | (0.007) | (0.005) | (0.004) |
| Any Treat × 2015 | -0.064*** | -0.053*** | 0.003 | 0.001 | -0.062*** | -0.059*** | -0.015*** | -0.013*** |
| | (0.014) | (0.014) | (0.007) | (0.007) | (0.006) | (0.007) | (0.005) | (0.004) |
| Observations | 92,979 | 92,916 | 76,843 | 76,843 | 101,346 | 101,311 | 86,868 | 80,157 |
| Unique individuals | 14,458 | 14,442 | 14,451 | 14,451 | 14,478 | 14,473 | 14,478 | 14,478 |
| Weights: | No | Lab. Inc. | No | Lab. Inc. | No | PIT Rev. | No | PIT Rev. |

Notes: This table reports year-by-year dynamic DiD reduced-form estimates for both intensive and extensive margin outcomes in the PLIT tax base. Panel (a) includes estimates for pre-reform years (2010-2011), and Panel (b) includes estimates for post-reform years (2012-2015). Columns (1) through (4) focus on intensive margin responses, while columns (5) through (8) focus on extensive margin responses. Estimates in columns (1), (2), (5), and (6) are based on specifications similar to Equations (16) and (17), but use dependent variables in levels: $\log y^l_{i,t}$ for intensive and $\mathbb{1}(y^l_{i,t} > 0)$ for extensive margins. Since outcomes are no longer first-differenced, these specifications include individual-level fixed effects and use 2009 as the reference year. Accordingly, columns (2) and (6), which report weighted estimates, use 2009 income and revenue weights. Columns (3), (4), (7), and (8) replicate the baseline specifications from Equations (16) and (17), where the dependent variables are measured as changes: $\Delta \log y^l_{i,t}$ for intensive and $\Delta\mathbb{1}(y^l_{i,t} > 0)$ for extensive margins, where $\Delta\mathbb{1}(y^l_{i,t} > 0)$ takes values in $\{-1, 0, 1\}$. Columns (3) and (7) report unweighted estimates, while columns (4) and (8) use income and revenue weights, respectively. Details on the construction of these weights are provided in Appendix C. Columns (4) and (8) correspond to our preferred specifications and are depicted graphically in panels (b) and (d) of Figure 3 in the main text. All regressions are based on *TAX* records, and standard errors are clustered at the individual level. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

## Table D.2: Own-Tax-Base Elasticity Estimates: Main Robustness Test

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | **Panel a. Intensive Margin Elasticities** | | | | | | | |
| | Dep. Var.: $\Delta\ log\ y_i^l$ | | | | | | | |
| (a): $\Delta\ log\ (1-\tau^l)$ | 1.054*** | 1.211*** | 0.405*** | 0.470*** | 1.093*** | 1.201*** | 1.007*** | 0.960*** |
| | (0.161) | (0.217) | (0.136) | (0.162) | (0.162) | (0.197) | (0.229) | (0.171) |
| (b): $\Delta^+\ log\ (1-\tau^l)$ | -0.283*** | -0.471*** | -0.175*** | -0.163** | -0.295*** | -0.329*** | 0.887*** | -0.381*** |
| | (0.084) | (0.164) | (0.060) | (0.075) | (0.085) | (0.099) | (0.205) | (0.092) |
| $(a)+(b)$ | 0.770*** | 0.739*** | 0.229 | 0.307* | 0.797*** | 0.873*** | 1.894*** | 0.579*** |
| | (0.169) | (0.273) | (0.143) | (0.171) | (0.170) | (0.202) | (0.339) | (0.196) |
| Observations | 27,128 | 27,128 | 17,737 | 11,005 | 26,722 | 19,322 | 20,128 | 27,128 |
| Unique individuals | 14,419 | 14,419 | 9,378 | 5,834 | 14,203 | 10,240 | 10,235 | 14,419 |
| | **Panel b. Extensive Margin Elasticities** | | | | | | | |
| | Dep. Var.: $\Delta\mathbb{1}(y_i^l > 0)$ | | | | | | | |
| (a): $\Delta\ log\ (1-\tau^{e,l})$ | 2.689*** | | 0.776* | 0.991** | 2.671*** | 1.950*** | 0.943*** | 2.853*** |
| | (0.454) | | (0.460) | (0.499) | (0.453) | (0.538) | (0.231) | (0.493) |
| (b): $\Delta^+\ log\ (1-\tau^{e,l})$ | -0.047 | | -0.015 | 0.000 | -0.045 | -0.118* | 0.822*** | 0.211 |
| | (0.056) | | (0.012) | (0.000) | (0.056) | (0.060) | (0.241) | (0.154) |
| $(a)+(b)$ | 2.643*** | | 0.761* | 0.991** | 2.626*** | 1.832*** | 1.765*** | 3.064*** |
| | (0.457) | | (0.460) | (0.499) | (0.456) | (0.542) | (0.334) | (0.573) |
| Observations | 28,835 | | 18,748 | 11,666 | 28,403 | 20,473 | 19,576 | 28,835 |
| Unique individuals | 14,444 | | 9,382 | 5,833 | 14,228 | 10,256 | 10,266 | 14,444 |
| Specification | | | | | | | | |
| Baseline | Yes | No | No | No | No | No | No | No |
| Estimate | ITT | TOT | ITT | ITT | ITT | ITT | ITT | ITT |
| Sample | Baseline | Baseline | No Switch. | Stable G. | Donut | SSA | SSA | SSA |
| Data | TAX | TAX | TAX | TAX | TAX | TAX | SSA | SSA + TAX |
| pre-TAX controls | No | No | No | No | No | No | No | Yes |

Notes: This table reports a series of robustness checks for our baseline intensive- and extensive-margin elasticity estimates corresponding to the PLIT base. Panel (a) presents intensive margin elasticities; panel (b) presents extensive margin elasticities. For conciseness, we report only the short-run 2SLS estimates corresponding to Equations (20) and (21), analogous to panel (c) in Table 3. Full estimates for each specification are reported in Appendix D. In panel (a), the dependent variable is the log-change in gross labor income, $\Delta \log y_i^l$, while the endogenous variables are $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$ and are instrumented with the interaction terms of $treat^{MTR}$ with dummies for 2011 and 2012. In panel (b), the outcome variable is the change in PLIT reporting status, $\Delta\mathbb{1}(y_i^l > 0)$, while the endogenous variables $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$ and are instrumented with the interaction terms of $treat^{ATR}$ with dummies for 2011 and 2012. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). Column (1) replicates our preferred specification. Column (2) uses observed rather than predicted changes in marginal net-of-tax rates, yielding treatment-on-the-treated effects as discussed in Section 5; since observed rates are undefined for TIEs who drop out of PLIT, we only report intensive margin estimates. Column (3) restricts the sample to TIEs who did not change tax brackets between 2010 and 2011. Column (4) further restricts to TIEs who remained in the same income zone (G1:G4) throughout 2009-2011. Column (5) excludes TIEs whose average absolute distance to a bracket threshold was less than 25 BPC in the pre-treatment period (bottom 5% of the distribution). Column (6) replicates our baseline using the sub-sample of TIEs matched to *SSA* records, as described in Section 4 and Appendix C. Column (7) replaces *TAX* outcomes with *SSA*-based outcomes; since SSA weights are not appropriate to represent tax revenue impacts, we weight by pre-reform income or revenue from *TAX* records. Column (8) adds decile dummies for cumulative labor income over 2000-2008 from *SSA* records as controls; to use the full *TAX* sample, TIEs who do not match with *SSA* records are all included in a same category of missing pre-reform income. Except for column (7), as already mentioned, all other elasticity estimates for intensive margin responses are weighted by income, while extensive margin estimates are based on revenue weights. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates in columns (1) through (6) are based on *TAX* records, estimates in column (7) are based on *SSA* records, and estimates in column (8) use both *TAX* records and *SSA* records.

Table D.3: Own-Tax-Base Intensive Margin: Main Robustness Tests - Full Table

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | | **Panel a. Reduced-Form Estimates** | | | | |
| | | | | Dep. Var.: $\Delta$ $log$ $y_i^l$ | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.047*** | -0.047*** | -0.021*** | -0.027*** | -0.049*** | -0.053*** | -0.043*** | -0.005 |
| | (0.007) | (0.007) | (0.007) | (0.009) | (0.007) | (0.009) | (0.010) | (0.010) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.012*** | 0.012*** | 0.009*** | 0.009** | 0.013*** | 0.014*** | -0.038*** | 0.004 |
| | (0.004) | (0.004) | (0.003) | (0.004) | (0.004) | (0.004) | (0.009) | (0.009) |
| | | | | **Panel b. First-Stage Estimates** | | | | |
| | | | | Dep. Var.: $\Delta$ $log$ $(1-\tau^l)$<br>$\Delta^+$ $log$ $(1-\tau^l)$ | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.045*** | -0.036*** | -0.051*** | -0.058*** | -0.045*** | -0.044*** | -0.043*** | -0.043*** |
| | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.044*** | -0.024*** | -0.051*** | -0.058*** | -0.044*** | -0.043*** | -0.043*** | -0.042*** |
| | (0.000) | (0.002) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| | | | | **Panel c. 2SLS Estimates** | | | | |
| | | | | Dep. Var.: $\Delta$ $log$ $y_i^l$ | | | | |
| (a): $\Delta$ $log$ $(1-\tau^l)$ | 1.054*** | 1.211*** | 0.405*** | 0.470*** | 1.093*** | 1.201*** | 1.007*** | 0.956*** |
| | (0.161) | (0.217) | (0.136) | (0.162) | (0.162) | (0.197) | (0.229) | (0.170) |
| (b): $\Delta^+$ $log$ $(1-\tau^l)$ | -0.283*** | -0.471*** | -0.175*** | -0.163** | -0.295*** | -0.329*** | 0.887*** | -0.391*** |
| | (0.084) | (0.164) | (0.060) | (0.075) | (0.085) | (0.099) | (0.205) | (0.091) |
| (a) + (b) | 0.770*** | 0.739*** | 0.229 | 0.307* | 0.797*** | 0.873*** | 1.894*** | 0.565*** |
| | (0.169) | (0.273) | (0.143) | (0.171) | (0.170) | (0.202) | (0.339) | (0.193) |
| Observations | 27,128 | 27,128 | 17,737 | 11,005 | 26,722 | 19,322 | 20,128 | 27,128 |
| Unique individuals | 14,419 | 14,419 | 9,378 | 5,834 | 14,203 | 10,240 | 10,235 | 14,419 |
| Specification | | | | | | | | |
| Baseline | Yes | No | No | No | No | No | No | No |
| Estimate | ITT | TOT | ITT | ITT | ITT | ITT | ITT | ITT |
| Sample | Baseline | Baseline | No Switch. | Stable G. | Donut | SSA | SSA | SSA |
| Data | TAX | TAX | TAX | TAX | TAX | TAX | SSA | SSA + TAX |
| pre-TAX controls | No | No | No | No | No | No | No | Yes |

Notes: This table reports the full set of estimates corresponding to the robustness checks included in panel (a) of Table D.2 in the main text. Panel (a) reports reduced-form coefficients from Equation (16) for years 2011 and 2012. These are the coefficients associated with variables $treat^{MTR} \times \mathbb{1}(year = 2012)$ and $treat^{MTR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta \log y_i^l$ as the outcome variable. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable: $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$. Panel (c) reports the 2SLS elasticity estimates based on Equation (20), where endogenous variables are instrumented using the reduced-form interactions from panel (a). Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and row (c) reports their sum, the short-run elasticity. Columns (1)-(8) correspond to the robustness specifications described in the main text. Column (1) replicates our preferred specification. Column (2) uses observed rather than predicted changes in marginal net-of-tax rates, yielding treatment-on-the-treated effects as discussed in Section 5. Column (3) restricts the sample to TIEs who did not change tax brackets between 2010 and 2011. Column (4) further restricts to TIEs who remained in the same income zone (G1:G4) throughout 2009-2011. Column (5) excludes TIEs whose average absolute distance to a bracket threshold was less than 25 BPC in the pre-treatment period (bottom 5% of the distribution). Column (6) replicates our baseline using the sub-sample of TIEs matched to *SSA* records, as described in Section 4 and Appendix C. Column (7) replaces *TAX* outcomes with *SSA*-based outcomes; since SSA weights are not appropriate to represent tax revenue impacts, we weight by pre-reform income from *TAX* records. Column (8) adds decile dummies for cumulative labor income over 2000-2008 from *SSA* records as controls; to use the full *TAX* sample, TIEs who do not match with *SSA* records are all included in a same category of missing pre-reform income. Except for column (7), as already mentioned, all other estimates are income weighted. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates in columns (1) through (6) are based on *TAX* records, estimates in column (7) are based on *SSA* records, and estimates in column (8) use both *TAX* records and *SSA* records.

Table D.4: Own-Tax-Base Extensive Margin: Main Robustness Tests - Full Table

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | | **Panel a. Reduced-Form Estimates** | | | | |
| | | | | Dep. Var.: $\Delta \mathbb{1}(y_i^l > 0)$ | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.038*** | | -0.013* | -0.020** | -0.038*** | -0.027*** | -0.009 | -0.026** |
| | (0.006) | | (0.007) | (0.010) | (0.006) | (0.007) | (0.007) | (0.012) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.001 | | 0.000 | 0.000*** | 0.001 | 0.001* | 0.003** | -0.001 |
| | (0.001) | | (0.000) | (0.000) | (0.001) | (0.001) | (0.001) | (0.010) |
| | | | | **Panel b. First-Stage Estimates** | | | | |
| | | | | Dep. Var.: $\Delta\ log\ (1-\tau^l)$ $\Delta^+\ log\ (1-\tau^l)$ | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.014*** | | -0.016*** | -0.020*** | -0.014*** | -0.014*** | -0.012*** | -0.013*** |
| | (0.000) | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.013*** | | -0.015*** | -0.019*** | -0.013*** | -0.012*** | -0.012*** | -0.012*** |
| | (0.000) | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| | | | | **Panel c. 2SLS Estimates** | | | | |
| | | | | Dep. Var.: $\Delta \mathbb{1}(y_i^l > 0)$ | | | | |
| (a): $\Delta\ log\ (1-\tau^l)$ | 2.689*** | | 0.776* | 0.991** | 2.671*** | 1.950*** | 0.943*** | 2.926*** |
| | (0.454) | | (0.460) | (0.499) | (0.453) | (0.538) | (0.231) | (0.487) |
| (b): $\Delta^+\ log\ (1-\tau^l)$ | -0.047 | | -0.015 | 0.000 | -0.045 | -0.118* | 0.822*** | 0.253* |
| | (0.056) | | (0.012) | (0.000) | (0.056) | (0.060) | (0.241) | (0.138) |
| (a) + (b) | 2.643*** | | 0.761* | 0.991** | 2.626*** | 1.832*** | 1.765*** | 3.179*** |
| | (0.457) | | (0.460) | (0.499) | (0.456) | (0.542) | (0.334) | (0.553) |
| Observations | 28,835 | | 18,748 | 11,666 | 28,403 | 20,473 | 19,576 | 28,835 |
| Unique individuals | 14,444 | | 9,382 | 5,833 | 14,228 | 10,256 | 10,266 | 14,444 |
| Specification | | | | | | | | |
| Baseline | Yes | No | No | No | No | No | No | No |
| Estimate | ITT | TOT | ITT | ITT | ITT | ITT | ITT | ITT |
| Sample | Baseline | Baseline | No Switch. | Stable G. | Donut | SSA | SSA | SSA |
| Data | TAX | TAX | TAX | TAX | TAX | TAX | SSA | SSA + TAX |
| pre-TAX controls | No | No | No | No | No | No | No | Yes |

Notes: This table reports the full set of estimates corresponding to the robustness checks included in panel (b) of Table D.2 in the main text. Panel (a) reports reduced-form coefficients from Equation (17) for years 2011 and 2012. In all cases, the outcome is the change in PLIT reporting status, $\Delta \mathbb{1}(y_i^l > 0)$, which takes values in $\{-1, 0, 1\}$, while the treatment variables are the interactions of $treat^{ATR}$ with year dummies. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable: $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$. Panel (c) reports the 2SLS elasticity estimates based on Equation (21), where endogenous variables are instrumented using the reduced-form interactions from panel (a). Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and row (c) reports their sum, the short-run elasticity. Columns (1)-(8) correspond to the robustness specifications described in the main text. Column (1) replicates our preferred specification. Column (2) is omitted for extensive margin responses, as observed effective tax rates are undefined for TIEs who drop out of PLIT. Column (3) restricts the sample to TIEs who did not change tax brackets between 2010 and 2011. Column (4) further restricts to TIEs who remained in the same income zone (G1:G4) throughout 2009-2011. Column (5) excludes TIEs whose average absolute distance to a bracket threshold was less than 25 BPC in the pre-treatment period (bottom 5% of the distribution). Column (6) replicates our baseline using the sub-sample of TIEs matched to *SSA* records, as described in Section 4 and Appendix C. Column (7) replaces *TAX* outcomes with *SSA*-based outcomes; since SSA weights are not appropriate to represent tax revenue impacts, we weight by pre-reform revenue from *TAX* records. Column (8) adds decile dummies for cumulative labor income over 2000-2008 from *SSA* records as controls; to use the full *TAX* sample, TIEs who do not match with *SSA* records are all included in a same category of missing pre-reform income. Except for column (7), as already mentioned, all other estimates are revenue weighted. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates in columns (1) through (6) are based on *TAX* records, estimates in column (7) are based on *SSA* records, and estimates in column (8) use both *TAX* records and *SSA* records.

Table D.5: Own-Tax-Base: Different Specifications for Intensive Margin Elasticities

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | | | **Panel a. Reduced-Form Estimates** | | | |
| | | | | Dep. Var.: $\Delta$ $log$ $y_i^l$ | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.047*** | -0.048*** | -0.059*** | -0.058*** | -0.054*** | -0.038*** | -0.071*** |
| | (0.007) | (0.008) | (0.009) | (0.009) | (0.008) | (0.006) | (0.010) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.012*** | 0.013*** | 0.016*** | 0.016*** | 0.016*** | 0.022*** | 0.003 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.005) |
| | | | | **Panel b. First-Stage Estimates** | | | |
| | | | | Dep. Var.: $\Delta$ $log$ $(1-\tau^l)$ $\Delta^+$ $log$ $(1-\tau^l)$ | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.045*** | -0.045*** | -0.045*** | -0.045*** | -0.045*** | -0.040*** | -0.047*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.044*** | -0.044*** | -0.044*** | -0.044*** | -0.044*** | -0.040*** | -0.046*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| | | | | **Panel c. 2SLS Estimates** | | | |
| | | | | Dep. Var.: $\Delta$ $log$ $y_i^l$ | | | |
| (a): $\Delta$ $log$ $(1-\tau^l)$ | 1.054*** | 1.063*** | 1.313*** | 1.299*** | 1.213*** | 0.946*** | 1.506*** |
| | (0.161) | (0.168) | (0.205) | (0.199) | (0.182) | (0.161) | (0.206) |
| (b): $\Delta^+$ $log$ $(1-\tau^l)$ | -0.283*** | -0.307*** | -0.375*** | -0.374*** | -0.362*** | -0.545*** | -0.074 |
| | (0.084) | (0.090) | (0.101) | (0.099) | (0.093) | (0.103) | (0.102) |
| (a) + (b) | 0.770*** | 0.756*** | 0.939*** | 0.925*** | 0.851*** | 0.401** | 1.433*** |
| | (0.169) | (0.175) | (0.211) | (0.205) | (0.187) | (0.175) | (0.215) |
| Observations | 27,128 | 25,923 | 27,128 | 27,128 | 27,128 | 27,128 | 27,128 |
| Unique individuals | 14,419 | 13,610 | 14,419 | 14,419 | 14,419 | 14,419 | 14,419 |
| Specification | | | | | | | |
| Baseline Estimate | Yes | No | No | No | No | No | No |
| Controls | No | Yes | No | No | No | No | No |
| Winsoring (Log) | 1% | 1% | No | 0.1% | 0.5% | 1% | 1% |
| Winsoring (Weights) | 5% | 5% | 5% | 5% | 5% | NA | No |
| Weights | Yes | Yes | Yes | Yes | Yes | No | Yes |

Notes: This table reports results for intensive-margin responses in the PLIT base under alternative sample selection criteria. Panel (a) reports reduced-form coefficients from Equation (16) for years 2011 and 2012. These are the coefficients associated with variables $treat^{MTR} \times \mathbb{1}(year = 2012)$ and $treat^{MTR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta \log y_i^l$ as the outcome variable. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable: $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$. Panel (c) reports the 2SLS elasticity estimates based on Equation (20), where endogenous variables are instrumented using the reduced-form interactions from panel (a). Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and row (c) reports their sum, the short-run elasticity. Column (1) reports our preferred baseline specification. Column (2) adds individual-level controls (age, age squared, an indicator for being younger than 55 years old, wage-earner status, and indicators for reporting any corporate and capital income in 2009-2010) and industry-by-year fixed effects. Column (3) uses an alternative outcome variable with no winsoring of log income changes. Columns (4) and (5) apply winsoring to the dependent variable at the 0.1% and 0.5% levels, respectively. Column (6) presents unweighted estimates. Column (7) replicates the baseline but replaces income weights winsored at the 5th percentile with non-winsored weights. All estimates are based on $TAX$ records, and standard errors are clustered at the individual level. Elasticity estimates are income weighted except in column (6), which is unweighted. Appendix C provides details on how weights are constructed. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table D.6: Own-Tax-Base: Different Specifications for Extensive Margin Elasticities

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Panel a. Reduced-Form Estimates** | | | |
| | Dep. Var.: $\Delta\mathbb{1}(y_i^l > 0)$ | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.038*** | -0.032*** | -0.024*** | -0.056*** |
| | (0.006) | (0.006) | (0.006) | (0.008) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.001 | 0.003*** | 0.001 | 0.001 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| | **Panel b. First-Stage Estimates** | | | |
| | Dep. Var.: $\Delta\ log\ (1-\tau^{e,l})$ $\Delta^+\ log\ (1-\tau^{e,l})$ | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.014*** | -0.014*** | -0.010*** | -0.018*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.013*** | -0.013*** | -0.039*** | -0.016*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| | **Panel c. 2SLS Estimates** | | | |
| | Dep. Var.: $\Delta\mathbb{1}(y_i^l > 0)$ | | | |
| (a): $\Delta\ log\ (1-\tau^{e,l})$ | 2.689*** | 2.349*** | 2.375*** | 3.171*** |
| | (0.454) | (0.436) | (0.581) | (0.454) |
| (b): $\Delta^+\ log\ (1-\tau^{e,l})$ | -0.047 | -0.229*** | -0.120 | -0.047 |
| | (0.056) | (0.072) | (0.090) | (0.043) |
| (a) + (b) | 2.643*** | 2.120*** | 2.255*** | 3.124*** |
| | (0.457) | (0.444) | (0.583) | (0.456) |
| Observations | 28,835 | 27,215 | 28,956 | 28,835 |
| Unique individuals | 14,444 | 13,629 | 14,478 | 14,444 |
| Specification | | | | |
| Baseline Estimate | Yes | No | No | No |
| Controls | No | Yes | No | No |
| Winsoring (Weights) | 5% | 5% | NA | No |
| Weights | Yes | Yes | No | Yes |

Notes: This table reports results for extensive-margin responses in the PLIT base under alternative sample selection criteria. Panel (a) reports reduced-form coefficients from Equation (17) for years 2011 and 2012. In all cases, the outcome is the change in PLIT reporting status, $\Delta\mathbb{1}(y_i^l > 0)$, which takes values in $\{-1, 0, 1\}$, while the treatment variables are the interactions of $treat^{ATR}$ with year dummies. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable: $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$. Panel (c) reports the 2SLS elasticity estimates based on Equation (21), where endogenous variables are instrumented using the reduced-form interactions from panel (a). Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and row (c) reports their sum, the short-run elasticity. Column (1) reports our preferred baseline specification. Column (2) adds individual-level controls (age, age squared, an indicator for being younger than 55 years old, wage-earner status, and indicators for reporting any corporate and capital income in 2009-2010) and industry-by-year fixed effects. Column (3) presents unweighted estimates. Column (4) replicates the baseline but replaces revenue weights winsored at the 5th percentile with non-winsored weights. All estimates are based on *TAX* records, and standard errors are clustered at the individual level. All estimates are revenue weighted except in column (3), which is unweighted. Appendix C provides details on how weights are constructed. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table D.7: Own-Tax-Base: Alternative Sample Selection Criteria for Intensive Margin Elasticities

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Panel a. Reduced-Form Estimates** | | | | | | | | | | | | |
| | Dep. Var.: $\Delta$ *log* $y_i^l$ | | | | | | | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.047*** | -0.062*** | -0.047*** | -0.061*** | -0.050*** | -0.049*** | -0.054*** | -0.054*** | -0.048*** | -0.044*** | -0.038*** | -0.039*** |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.008) | (0.008) | (0.007) | (0.007) | (0.007) | (0.007) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.012*** | 0.016*** | 0.010** | 0.019*** | 0.013*** | 0.009** | 0.015*** | 0.014*** | 0.014*** | 0.014*** | 0.015*** | 0.029*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.004) |
| **Panel b. First-Stage Estimates** | | | | | | | | | | | | |
| | Dep. Var.: $\Delta$ *log* $(1-\tau^l)$<br>$\Delta^+$ *log* $(1-\tau^l)$ | | | | | | | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.045*** | -0.045*** | -0.045*** | -0.045*** | -0.045*** | -0.045*** | -0.047*** | -0.046*** | -0.045*** | -0.045*** | -0.045*** | -0.045*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.044*** | -0.043*** | -0.044*** | -0.043*** | -0.044*** | -0.044*** | -0.045*** | -0.045*** | -0.044*** | -0.044*** | -0.044*** | -0.044*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| **Panel c. 2SLS Estimates** | | | | | | | | | | | | |
| | Dep. Var.: $\Delta$ *log* $y_i^l$ | | | | | | | | | | | |
| (a): $\Delta$ *log* $(1-\tau^l)$ | 1.054*** | 1.376*** | 1.037*** | 1.358*** | 1.112*** | 1.112*** | 1.157*** | 1.179*** | 1.075*** | 0.976*** | 0.846*** | 0.861*** |
| | (0.161) | (0.159) | (0.160) | (0.161) | (0.160) | (0.161) | (0.166) | (0.167) | (0.159) | (0.155) | (0.149) | (0.163) |
| (b): $\Delta^+$ *log* $(1-\tau^l)$ | -0.283*** | -0.370*** | -0.220** | -0.438*** | -0.292*** | -0.269*** | -0.328*** | -0.315*** | -0.312*** | -0.312*** | -0.349*** | -0.646*** |
| | (0.084) | (0.094) | (0.088) | (0.090) | (0.084) | (0.085) | (0.088) | (0.087) | (0.083) | (0.081) | (0.078) | (0.080) |
| (a) + (b) | 0.770*** | 1.005*** | 0.817*** | 0.920*** | 0.820*** | 0.843*** | 0.829*** | 0.864*** | 0.764*** | 0.664*** | 0.497*** | 0.215 |
| | (0.169) | (0.173) | (0.171) | (0.171) | (0.168) | (0.169) | (0.174) | (0.175) | (0.168) | (0.164) | (0.159) | (0.173) |
| Observations | 27,128 | 28,998 | 27,426 | 28,571 | 26,771 | 26,075 | 25,781 | 25,843 | 26,700 | 25,907 | 23,888 | 15,016 |
| Unique individuals | 14,419 | 15,435 | 14,586 | 15,196 | 14,227 | 13,859 | 13,704 | 13,739 | 14,185 | 13,754 | 12,675 | 7,994 |
| Specification | | | | | | | | | | | | |
| Baseline | Yes | No | No | No | No | No | No | No | No | No | No | No |
| Drop 3 diff. groups | Yes | No | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Drop growth >100% | Yes | No | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Exclude G3 | No | No | No | No | Yes | No | No | No | No | No | No | No |
| Potential T in C | No | No | No | No | No | Yes | No | No | No | No | No | No |
| Below p5. avg. dist | No | No | No | No | No | No | Yes | No | No | No | No | No |
| Below p5. min. dist | No | No | No | No | No | No | No | Yes | No | No | No | No |
| Drop fall out of perc. | 95% | 95% | 95% | 95% | 95% | 95% | 95% | 95% | 96% | 97% | 98% | 99% |

Notes: This table reports results for intensive-margin responses in the PLIT base under different sample selection criteria. Panel (a) reports reduced-form coefficients from Equation (16) for years 2011 and 2012. These are the coefficients associated with variables $treat^{MTR} \times \mathbb{1}(year = 2012)$ and $treat^{MTR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta \log y_i^l$ as the outcome variable. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable: $\Delta \log(1 - \tau_{it}^l)$ and $\Delta^+ \log(1 - \tau_{it}^l)$. Panel (c) reports the 2SLS elasticity estimates based on Equation (20), where endogenous variables are instrumented using the reduced-form interactions from panel (a). Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and row (c) reports their sum, the short-run elasticity. Column (1) reports our preferred baseline specification. Column (2) adds back individuals with highly volatile pre-treatment income, specifically, those who belonged to three different income zones between 2009 and 2011, or experienced income changes greater than 100% in 2010. Columns (3) and (4) add back each of these groups separately. Column (5) drops individuals who belonged to G3 in any pre-reform year. Column (6) excludes individuals in the control group who were ever within 25 BPC of the treatment thresholds during 2009-2011. Columns (7) and (8) exclude individuals whose average or minimum distance to the treatment thresholds in the pre-reform period fell below the 5th percentile of the distribution. In our baseline sample, we exclude individuals who were ever in the top 1% of labor income during 2009-2011 but whose total income fell below 300 BPC (roughly the 95th percentile of the labor income distribution). Columns (9) through (12) test the sensitivity of our estimates to this cutoff by using higher thresholds: 340, 391, 470, and 627 BPC, which correspond approximately to the 96th, 97th, 98th, and 99th percentiles of the gross labor income distribution. All estimates are based on *TAX* records, and standard errors are clustered at the individual level. All estimates are income weighted. Appendix C provides details on how weights and alternative variables are constructed. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table D.8: Own-Tax-Base: Alternative Sample Selection Criteria for Extensive Margin Elasticities

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Panel a. Reduced-Form Estimates** | | | | | | | | | | | | |
| Dep. Var.: $\Delta\mathbb{1}(y_i^l > 0)$ | | | | | | | | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.038*** | -0.040*** | -0.039*** | -0.038*** | -0.040*** | -0.039*** | -0.066*** | -0.041*** | -0.037*** | -0.034*** | -0.030*** | -0.016** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) | (0.006) | (0.009) | (0.007) | (0.006) | (0.006) | (0.006) | (0.008) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.001 | -0.001 | -0.000 | -0.001 | 0.000 | 0.001 | 0.001 | 0.000 | 0.001 | 0.001 | 0.001 | 0.002** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| **Panel b. First-Stage Estimates** | | | | | | | | | | | | |
| Dep. Var.: $\Delta\ log\ (1-\tau^{e,l})$, $\Delta^+\ log\ (1-\tau^{e,l})$ | | | | | | | | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** | -0.020*** | -0.015*** | -0.014*** | -0.014*** | -0.014*** | -0.014*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.013*** | -0.012*** | -0.013*** | -0.012*** | -0.013*** | -0.013*** | -0.018*** | -0.013*** | -0.013*** | -0.013*** | -0.013*** | -0.013*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| **Panel c. 2SLS Estimates** | | | | | | | | | | | | |
| Dep. Var.: $\Delta\mathbb{1}(y_i^l > 0)$ | | | | | | | | | | | | |
| (a): $\Delta\ log\ (1-\tau^{e,l})$ | 2.689*** | 2.865*** | 2.758*** | 2.755*** | 2.690*** | 2.792*** | 3.309*** | 2.781*** | 2.632*** | 2.424*** | 2.202*** | 1.146** |
| | (0.454) | (0.433) | (0.446) | (0.440) | (0.454) | (0.457) | (0.461) | (0.453) | (0.455) | (0.460) | (0.470) | (0.562) |
| (b): $\Delta^+\ log\ (1-\tau^{e,l})$ | -0.047 | 0.115 | 0.003 | 0.063 | -0.061 | -0.050 | -0.054 | -0.037 | -0.065 | -0.057 | -0.063 | -0.183** |
| | (0.056) | (0.080) | (0.069) | (0.071) | (0.054) | (0.058) | (0.048) | (0.057) | (0.054) | (0.054) | (0.056) | (0.078) |
| (a) + (b) | 2.643*** | 2.979*** | 2.761*** | 2.818*** | 2.629*** | 2.742*** | 3.254*** | 2.745*** | 2.567*** | 2.367*** | 2.139*** | 0.964* |
| | (0.457) | (0.440) | (0.451) | (0.446) | (0.458) | (0.460) | (0.464) | (0.456) | (0.458) | (0.463) | (0.473) | (0.567) |
| Observations | 28,835 | 30,869 | 29,165 | 30,395 | 28,452 | 27,715 | 21,723 | 27,477 | 28,367 | 27,508 | 25,351 | 15,986 |
| Unique individuals | 14,444 | 15,477 | 14,618 | 15,230 | 14,251 | 13,883 | 10,881 | 13,764 | 14,208 | 13,775 | 12,695 | 8,007 |
| Specification | | | | | | | | | | | | |
| Baseline | Yes | No | No | No | No | No | No | No | No | No | No | No |
| Drop 3 diff. groups | Yes | No | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Drop growth >100% | Yes | No | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Exclude G3 | No | No | No | No | Yes | No | No | No | No | No | No | No |
| Potential T in C | No | No | No | No | No | Yes | No | No | No | No | No | No |
| Below p5. avg. dist | No | No | No | No | No | No | Yes | No | No | No | No | No |
| Below p5. min. dist | No | No | No | No | No | No | No | Yes | No | No | No | No |
| Drop fall out of perc. | 95% | 95% | 95% | 95% | 95% | 95% | 95% | 95% | 96% | 97% | 98% | 99% |

Notes: This table reports results for extensive-margin responses in the PLIT base under alternative specifications. Panel (a) reports reduced-form coefficients from Equation (17) for years 2011 and 2012. In all cases, the outcome is the change in PLIT reporting status, $\Delta\mathbb{1}(y_i^l > 0)$, which takes values in $\{-1, 0, 1\}$, while the treatment variables are the interactions of $treat^{ATR}$ with year dummies. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable: $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$. Panel (c) reports the 2SLS elasticity estimates based on Equation (21), where endogenous variables are instrumented using the reduced-form interactions from panel (a). Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and row (c) reports their sum, the short-run elasticity. Column (1) reports our preferred baseline specification. Column (2) adds back individuals with highly volatile pre-treatment income, specifically, those who belonged to three different income zones between 2009 and 2011, or experienced income changes greater than 100% in 2010. Columns (3) and (4) add back each of these groups separately. Column (5) drops individuals who belonged to G3 in any pre-reform year. Column (6) excludes individuals in the control group who were ever within 25 BPC of the treatment thresholds during 2009-2011. Columns (7) and (8) exclude individuals whose average or minimum distance to the treatment thresholds in the pre-reform period fell below the 5th percentile of the distribution. In our baseline sample, we exclude individuals who were ever in the top 1% of labor income during 2009-2011 but whose total income fell below 300 BPC (roughly the 95th percentile of the labor income distribution). Columns (9) through (12) test the sensitivity of our estimates to this cutoff by using higher thresholds: 340, 391, 470, and 627 BPC, which correspond approximately to the 96th, 97th, 98th, and 99th percentiles of the gross labor income distribution. All estimates are based on *TAX* records, and standard errors are clustered at the individual level. All estimates are revenue weighted. Appendix C provides details on how weights and alternative variables are constructed. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

# E Further Results on Cross-Base Responses

In this appendix, we provide additional estimates on cross-tax base responses. For conciseness, we present the reduced-form DiD estimates and the main robustness table for each margin within each tax base.

Table E.1 reports the reduced-form DiD coefficients for the capital income cross-base analysis. This table is analogous to Table D.1 in Appendix D, but focused on outcomes related to the capital income tax base. Columns (1) through (4) cover intensive margin responses, using log capital income in both levels and changes, estimated under weighted and unweighted specifications. It is important to note that coefficients reported in column (4) correspond to our preferred specification and are exactly those depicted in panel (b) in Figure 5 in the main text. As discussed when presenting the graphical evidence, the reduced-form results show no clear signs of cross-tax base responses in the intensive margin. Most coefficients are statistically insignificant, with the exception of the 2010 coefficient in column (4). As shown in the raw data, this appears to be a circumstantial result that quickly reverts in the following year.

Columns (5) through (8) repeat the analysis for extensive margin responses, using either an indicator for reporting to the capital income base or its change. Again, note that coefficients reported in column (8) correspond exactly to those depicted in panel (d) in Figure 5. The reduced-form estimates reported in this table confirm the intuition from the graphical evidence: the reform led to a significant increase in the share of taxpayers reporting to the capital income tax base in the year the reform was enacted. In terms of timing, Table E.1 shows that most of the response occurs in 2012 (estimate = 0.011, $p$-value <0.001), with no sign of anticipatory behavior (estimate = -0.001, $p$-value = 0.342), a small increase in 2013 (estimate = 0.003, $p$-value = 0.080), and no additional effects in 2014 or 2015. Comparing the preferred specification in column (8) to its unweighted counterpart in column (7), we again find that giving more weight to higher-income TIEs leads to larger estimated responses. This is consistent with the patterns observed in own-tax base results and the heterogeneity analysis discussed throughout the paper and in Appendix F.

To assess robustness, Table E.2 presents 2SLS elasticity estimates: panel (a) focuses on intensive margin responses, and panel (b) on the extensive margin. This table replicates the main robustness checks used in Table D.2 for own-base responses. Overall, the results from alternative specifications are very similar to those from the baseline. As discussed in Section 6, intensive margin responses are harder to interpret given the circumstantial differences in pre-treatment growth rates, likely driven by irregular capital income dynamics in 2010. However, all specifications provide similar estimates in direciton and order of magnitude, although very imprecisely estimated due to the reduced sample size. Regarding extensive margin outcomes, the results remain almost identical both in direction and magnitude. Across all seven specifications, estimated elasticities range from -0.625 ($p-value$ = 0.002) to -0.803 ($p-value$ <0.001) and are statistically significant at the 1% level. As with own-base results, effect sizes decline slightly when the sample is restricted to individuals with more stable earnings, though the changes are less pronounced. Estimates are also stable when focusing on the SSA sample or when including pre-TAX income controls.

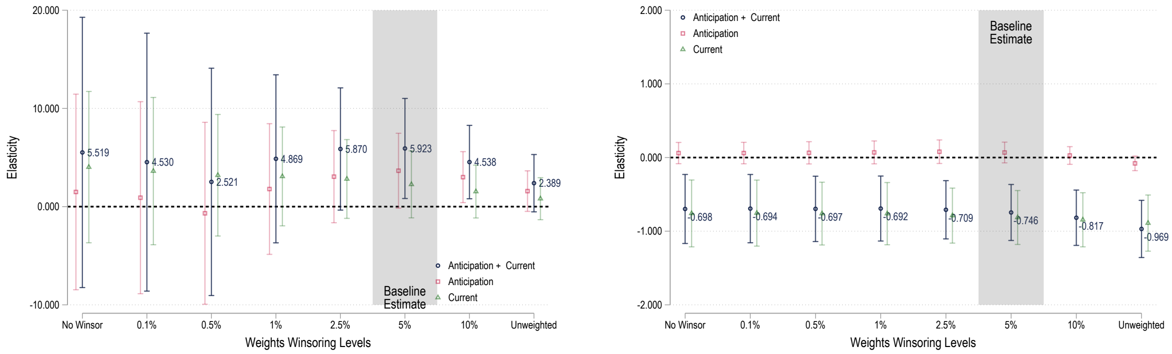
Table E.3 presents the reduced-form DiD estimates for outcomes related to the corporate income tax base. Overall, the results confirm the intuition from the graphical evidence shown in Figure 6 in the main text. On the one hand, the reform does not appear to affect the intensive margin. On the other hand, there is a strong response along the extensive margin, indicating that some TIEs reacted to the increase in PLIT marginal rates by starting to report income to the corporate tax base. In terms of timing, the extensive margin response begins in 2011 and is followed by a similarly sized response in 2012. Table E.4 confirms the robustness of these findings. None of the elasticity estimates for the intensive margin are statistically significant. However, as noted earlier, the sample size for this analysis is very small: only about 500 TIEs were reporting to the corporate income tax base prior to the reform, which limits statistical power. The extensive margin results are much more precisely estimated and highly robust. Across all specifications, the estimated elasticities range from -0.60 ($p-value = 0.002$) to -0.93 ($p-value < 0.001$) and are statistically significant at the 1% level. Overall, these findings reinforce the main conclusions from our preferred specification.

Finally, Figures E.1 and E.2 report estimates using different winsorizing options for the weights used to estimate cross-base elasticities. This is analogous to Figure D.6 in Appendix D for own-base elasticities. Overall, our results remain consistent regardless of the winsorizing definition applied to the weights.

Figure E.1: Sensitivity Analysis to Weights: Cross Tax Base Capital Income

a. Intensive Margin

b. Extensive Margin

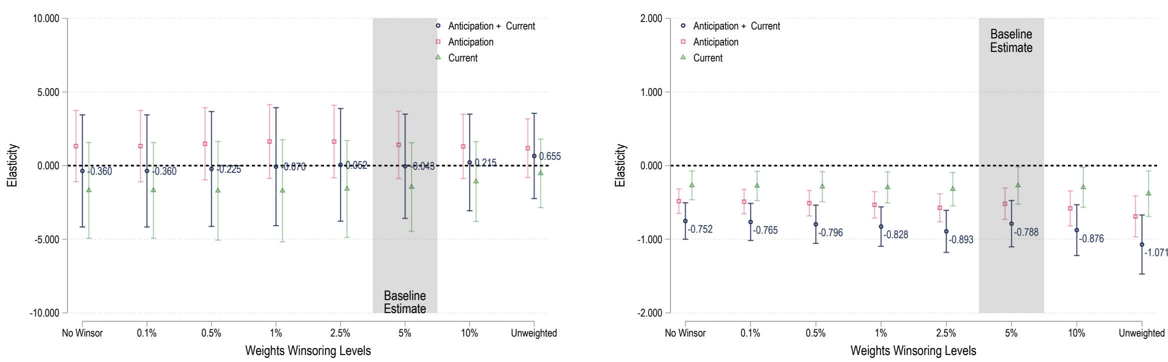



Notes: This figure presents a sensitivity analysis of our short-run elasticity estimates to alternative weighting and winsoring strategies for cross-tax base responses in the capital income tax base. Panel (a) focuses on intensive margin responses, and panel (b) on extensive margin responses. Each panel reports elasticity estimates under eight specifications: no winsoring, winsoring at the 0.1%, 0.5%, 1%, 2.5%, 5%, and 10% tails of the log income change distribution, and an unweighted specification. For each specification, we report the short-run elasticity (sum of anticipation and current response), the anticipation elasticity, and the current response elasticity. Estimates are obtained from 2SLS regressions based on Equations (20) and (21), using the corresponding set of weights (income or revenue weights, depending on the margin) and winsoring thresholds. 95% confidence intervals are based on standard errors clustered at the individual level. Labels above the markers correspond to the point estimate of the short-run elasticity. Shaded boxes highlight our baseline specification (5% winsoring).

Figure E.2: Sensitivity Analysis to Weights: Cross Tax Base Corporate Income

a. Intensive Margin

b. Extensive Margin




Notes: This figure presents a sensitivity analysis of our short-run elasticity estimates to alternative weighting and winsoring strategies for cross-tax base responses in the corporate income tax base. Panel (a) focuses on intensive margin responses, and panel (b) on extensive margin responses. Each panel reports elasticity estimates under eight specifications: no winsoring, winsoring at the 0.1%, 0.5%, 1%, 2.5%, 5%, and 10% tails of the log income change distribution, and an unweighted specification. For each specification, we report the short-run elasticity (sum of anticipation and current response), the anticipation elasticity, and the current response elasticity. Estimates are obtained from 2SLS regressions based on Equations (20) and (21), using the corresponding set of weights (income or revenue weights, depending on the margin) and winsoring thresholds. 95% confidence intervals are based on standard errors clustered at the individual level. Labels above the markers correspond to the point estimate of the short-run elasticity. Shaded boxes highlight our baseline specification (5% winsoring).

Table E.1: Cross-Tax-Base Capital Income: Dynamic DiD Estimates

| | Intensive Margin | | | | Extensive Margin | | | |
|---|---|---|---|---|---|---|---|---|
| | *log* $y^k_{i,t}$ | *log* $y^k_{i,t}$ | $\Delta$ *log* $y^k_{i,t}$ | $\Delta$ *log* $y^k_{i,t}$ | $\mathbb{1}(y^k_{i,t} > 0)$ | $\mathbb{1}(y^k_{i,t} > 0)$ | $\Delta\mathbb{1}(y^k_{i,t} > 0)$ | $\Delta\mathbb{1}(y^k_{i,t} > 0)$ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **a. Pre Reform Years** | | | | | | | | |
| Any Treat × 2010 | 0.111** | 0.119 | 0.045 | 0.299*** | 0.001 | 0.001* | 0.001 | 0.001* |
| | (0.054) | (0.083) | (0.044) | (0.087) | (0.003) | (0.001) | (0.003) | (0.001) |
| Any Treat × 2011 | 0.032 | 0.088 | -0.073* | -0.155* | 0.001 | 0.009*** | -0.000 | -0.001 |
| | (0.060) | (0.073) | (0.038) | (0.082) | (0.004) | (0.003) | (0.004) | (0.001) |
| **b. Post Reform Years** | | | | | | | | |
| Any Treat × 2012 | 0.115 | -0.172 | 0.047 | -0.096 | 0.009** | 0.020*** | 0.009** | 0.011*** |
| | (0.070) | (0.124) | (0.039) | (0.074) | (0.005) | (0.004) | (0.004) | (0.002) |
| Any Treat × 2013 | 0.045 | -0.245* | -0.050 | 0.076 | 0.012** | 0.024*** | 0.003 | 0.003* |
| | (0.072) | (0.133) | (0.037) | (0.093) | (0.005) | (0.005) | (0.004) | (0.002) |
| Any Treat × 2014 | -0.034 | -0.339* | -0.067* | -0.019 | 0.011* | 0.024*** | -0.001 | -0.000 |
| | (0.078) | (0.179) | (0.040) | (0.073) | (0.006) | (0.005) | (0.004) | (0.002) |
| Any Treat × 2015 | 0.008 | -0.154 | 0.039 | -0.040 | 0.011* | 0.026*** | 0.000 | 0.001 |
| | (0.078) | (0.119) | (0.042) | (0.083) | (0.006) | (0.006) | (0.004) | (0.002) |
| Observations | 14,389 | 9,152 | 10,791 | 10,791 | 101,346 | 101,346 | 86,868 | 81,581 |
| Unique individuals | 3,068 | 1,530 | 2,931 | 2,931 | 14,478 | 14,478 | 14,478 | 14,478 |
| Weights: | No | Cap. Inc. | No | Cap. Inc. | No | KIT Rev. | No | KIT Rev. |

Notes: This table reports year-by-year dynamic DiD reduced-form estimates for both intensive and extensive margin outcomes in the capital income tax base. Panel (a) includes estimates for pre-reform years (2010-2011), and Panel (b) includes estimates for post-reform years (2012-2015). Columns (1) through (4) focus on intensive margin responses, while columns (5) through (8) focus on extensive margin responses. Estimates in columns (1), (2), (5), and (6) are based on specifications similar to Equations (16) and (17), but use dependent variables in levels: $\log y^l_{i,t}$ for intensive and $\mathbb{1}(y^l_{i,t} > 0)$ for extensive margins. Since outcomes are no longer first-differenced, these specifications include individual-level fixed effects and use 2009 as the reference year. Accordingly, columns (2) and (6), which report weighted estimates, use 2009 income and revenue weights. Columns (3), (4), (7), and (8) replicate the baseline specifications from Equations (16) and (17), where the dependent variables are measured as changes: $\Delta \log y^k_{i,t}$ for intensive and $\Delta\mathbb{1}(y^k_{i,t} > 0)$ for extensive margins, where $\Delta\mathbb{1}(y^k_{i,t} > 0)$ takes values in $\{-1, 0, 1\}$. Columns (3) and (7) report unweighted estimates, while columns (4) and (8) use income and revenue weights, respectively. Details on the construction of these weights are provided in Appendix C. Columns (4) and (8) correspond to our preferred specifications and are depicted graphically in panels (b) and (d) of Figure 5 in the main text. All regressions are based on *TAX* records, and standard errors are clustered at the individual level. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E.2: Cross-Tax-Base Capital Income: Main Robustness Table

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | | | **Panel a. Intensive Margin Elasticities** | | | |
| | | | | Dep. Var.: $\Delta$ *log* $y_i^k$ | | | |
| (a): $\Delta$ *log* $(1-\tau^l)$ | 2.265 | -0.858 | 2.635 | 3.796* | 2.427 | -0.311 | 1.940 |
| | (1.738) | (6.396) | (1.803) | (2.146) | (1.751) | (1.495) | (1.829) |
| (b): $\Delta^+$ *log* $(1-\tau^l)$ | 3.658* | 7.949 | 0.034 | -0.648 | 3.615* | 5.983** | 3.300* |
| | (1.946) | (7.625) | (2.185) | (2.304) | (1.960) | (2.389) | (1.981) |
| (a) + (b) | 5.923** | 7.091 | 2.669 | 3.148 | 6.042** | 5.672** | 5.240* |
| | (2.600) | (4.637) | (2.885) | (3.194) | (2.639) | (2.787) | (2.792) |
| Observations | 3,395 | 3,395 | 2,316 | 1,480 | 3,349 | 2,241 | 3,395 |
| Unique individuals | 1,946 | 1,946 | 1,313 | 840 | 1,920 | 1,283 | 1,946 |
| | | | | **Panel b. Extensive Margin Elasticities** | | | |
| | | | | Dep. Var.: $\Delta\mathbb{1}(y_i^k > 0)$ | | | |
| $\Delta$ *log* $(1-\tau^{e,l}$ | -0.814*** | | -0.700*** | -0.731*** | -0.806*** | -0.854*** | -0.840*** |
| | (0.187) | | (0.197) | (0.226) | (0.186) | (0.210) | (0.198) |
| $\Delta^+$ *log* $(1-\tau^{e,l}$ | 0.068 | | 0.076 | 0.030 | 0.067 | 0.110 | 0.037 |
| | (0.072) | | (0.072) | (0.072) | (0.072) | (0.077) | (0.092) |
| (a) + (b) | -0.746*** | | -0.625*** | -0.701*** | -0.739*** | -0.744*** | -0.803*** |
| | (0.194) | | (0.205) | (0.234) | (0.194) | (0.215) | (0.231) |
| Observations | 28,911 | | 18,798 | 11,668 | 28,479 | 20,524 | 28,911 |
| Unique individuals | 14,466 | | 9,404 | 5,834 | 14,250 | 10,270 | 14,466 |
| Specification | | | | | | | |
| Baseline | Yes | No | No | No | No | No | No |
| Estimate | ITT | TOT | ITT | ITT | ITT | ITT | ITT |
| Sample | Baseline | Baseline | No Switch. | Stable G. | Donut | SSA | SSA |
| Data | TAX | TAX | TAX | TAX | TAX | TAX | SSA + TAX |
| pre-TAX controls | No | No | No | No | No | No | Yes |

Notes: This table reports a series of robustness checks for our baseline intensive- and extensive-margin elasticity estimates in the capital income tax base. Panel (a) presents intensive margin elasticities; panel (b) presents extensive margin elasticities. For conciseness, we report only the short-run 2SLS estimates corresponding to Equations (20) and (21), analogous to panel (c) in Table 3. In panel (a), the dependent variable is the log-change in capital income, $\Delta \log y_i^k$, while the endogenous variables are $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$ and are instrumented with the interaction terms of $treat^{MTR}$ with dummies for 2011 and 2012. In panel (b), the outcome variable is the change in capital income reporting status, $\Delta\mathbb{1}(y_i^k > 0)$, while the endogenous variables $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$ and are instrumented with the interaction terms of $treat^{ATR}$ with dummies for 2011 and 2012. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). Column (1) replicates our preferred specification. Column (2) uses observed rather than predicted changes in marginal net-of-tax rates, yielding treatment-on-the-treated effects as discussed in Section 5; since observed rates are undefined for TIEs who drop out of PLIT, we only report intensive margin estimates. Column (3) restricts the sample to TIEs who did not change tax brackets between 2010 and 2011. Column (4) further restricts to TIEs who remained in the same income zone (G1:G4) throughout 2009-2011. Column (5) excludes TIEs whose average absolute distance to a bracket threshold was less than 25 BPC in the pre-treatment period (bottom 5% of the distribution). Column (6) replicates our baseline using the sub-sample of TIEs matched to *SSA* records, as described in Section 4 and Appendix C. Column (7) replaces *TAX* outcomes with *SSA*-based outcomes; since SSA weights are not appropriate to represent tax revenue impacts, we weight by pre-reform income or revenue from *TAX* records. Column (8) adds decile dummies for cumulative labor income over 2000-2008 from *SSA* records as controls; to use the full *TAX* sample, TIEs who do not match with *SSA* records are all included in a same category of missing pre-reform income. Except for column (7), as already mentioned, all other elasticity estimates for intensive margin responses are weighted by income, while extensive margin estimates are based on revenue weights. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates in columns (1) through (6) are based on *TAX* records, estimates in column (7) are based on *SSA* records, and estimates in column (8) use both *TAX* records and *SSA* records.

Table E.3: Cross-Tax-Base Corporate Income: Dynamic DiD Estimates

| | Intensive Margin | | | | Extensive Margin | | | |
|---|---|---|---|---|---|---|---|---|
| | $log\ y^c_{i,t}$ (1) | $log\ y^c_{i,t}$ (2) | $\Delta\ log\ y^c_{i,t}$ (3) | $\Delta\ log\ y^c_{i,t}$ (4) | $\mathbb{1}(y^c_{i,t} > 0)$ (5) | $\mathbb{1}(y^c_{i,t} > 0)$ (6) | $\Delta\mathbb{1}(y^c_{i,t} > 0)$ (7) | $\Delta\mathbb{1}(y^c_{i,t} > 0)$ (8) |
| **a. Pre Reform Years** | | | | | | | | |
| Any Treat × 2010 | 0.106** | 0.046 | 0.066* | 0.057 | 0.002 | 0.000 | 0.002 | 0.000 |
| | (0.052) | (0.029) | (0.038) | (0.039) | (0.002) | (0.001) | (0.002) | (0.001) |
| Any Treat × 2011 | -0.073 | -0.040 | -0.040 | -0.052 | 0.014*** | 0.016*** | 0.012*** | 0.006*** |
| | (0.070) | (0.074) | (0.039) | (0.041) | (0.003) | (0.003) | (0.002) | (0.001) |
| **b. Post Reform Years** | | | | | | | | |
| Any Treat × 2012 | 0.053 | 0.010 | 0.140*** | 0.144*** | 0.020*** | 0.024*** | 0.006*** | 0.004** |
| | (0.074) | (0.071) | (0.052) | (0.054) | (0.003) | (0.003) | (0.002) | (0.002) |
| Any Treat × 2013 | 0.052 | 0.010 | 0.052 | 0.063* | 0.026*** | 0.029*** | 0.005*** | 0.002 |
| | (0.073) | (0.078) | (0.031) | (0.033) | (0.004) | (0.004) | (0.002) | (0.002) |
| Any Treat × 2014 | 0.064 | -0.039 | 0.023 | 0.009 | 0.027*** | 0.032*** | 0.001 | 0.001 |
| | (0.073) | (0.073) | (0.027) | (0.033) | (0.004) | (0.004) | (0.002) | (0.001) |
| Any Treat × 2015 | 0.013 | -0.113 | -0.023 | 0.008 | 0.025*** | 0.030*** | -0.002 | -0.001 |
| | (0.077) | (0.078) | (0.026) | (0.028) | (0.004) | (0.004) | (0.002) | (0.002) |
| Observations | 4,257 | 2,379 | 3,361 | 3,361 | 101,346 | 101,346 | 86,868 | 81,571 |
| Unique individuals | 866 | 387 | 862 | 862 | 14,478 | 14,478 | 14,478 | 14,478 |
| Weights: | No | Corp. Inc. | No | Corp. Inc. | No | CIT Rev. | No | CIT Rev. |

Notes: This table reports year-by-year dynamic DiD reduced-form estimates for both intensive and extensive margin outcomes in the corporate income tax base. Panel (a) includes estimates for pre-reform years (2010-2011), and Panel (b) includes estimates for post-reform years (2012-2015). Columns (1) through (4) focus on intensive margin responses, while columns (5) through (8) focus on extensive margin responses. Estimates in columns (1), (2), (5), and (6) are based on specifications similar to Equations (16) and (17), but use dependent variables in levels: $\log y^l_{i,t}$ for intensive and $\mathbb{1}(y^l_{i,t} > 0)$ for extensive margins. Since outcomes are no longer first-differenced, these specifications include individual-level fixed effects and use 2009 as the reference year. Accordingly, columns (2) and (6), which report weighted estimates, use 2009 income and revenue weights. Columns (3), (4), (7), and (8) replicate the baseline specifications from Equations (16) and (17), where the dependent variables are measured as changes: $\Delta \log y^c_{i,t}$ for intensive and $\Delta\mathbb{1}(y^k_{i,t} > 0)$ for extensive margins, where $\Delta\mathbb{1}(y^c_{i,t} > 0)$ takes values in $\{-1, 0, 1\}$. Columns (3) and (7) report unweighted estimates, while columns (4) and (8) use income and revenue weights, respectively. Details on the construction of these weights are provided in Appendix C. Columns (4) and (8) correspond to our preferred specifications and are depicted graphically in panels (b) and (d) of Figure 6 in the main text. All regressions are based on *TAX* records, and standard errors are clustered at the individual level. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E.4: Cross-Tax-Base Corporate Income: Main Robustness Table

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | | | **Panel a. Intensive Margin Elasticities** | | | | |
| | | | Dep. Var.: $\Delta$ *log* $y_i^c$ | | | | |
| (a): $\Delta$ *log* $(1-\tau^l)$ | -1.454 | -1.392 | -2.379 | -2.185 | -1.385 | 0.503 | -1.739 |
| | (1.532) | (1.592) | (1.905) | (2.323) | (1.530) | (1.835) | (1.610) |
| (b): $\Delta^+$ *log* $(1-\tau^l)$ | 1.411 | 1.262 | 0.798 | -0.714 | 1.405 | 1.229 | 1.135 |
| | (1.169) | (1.154) | (1.353) | (1.165) | (1.167) | (1.637) | (1.170) |
| (a) + (b) | -0.043 | -0.130 | -1.580 | -2.899 | 0.020 | 1.732 | -0.604 |
| | (1.805) | (1.733) | (2.177) | (2.372) | (1.804) | (2.353) | (1.921) |
| Observations | 964 | 964 | 672 | 345 | 955 | 639 | 964 |
| Unique individuals | 533 | 533 | 359 | 186 | 528 | 350 | 533 |
| | | | **Panel b. Extensive Margin Elasticities** | | | | |
| | | | Dep. Var.: $\Delta\mathbb{1}(y_i^c > 0)$ | | | | |
| $\Delta$ *log* $(1-\tau^{e,l}$ | -0.271** | | -0.284** | -0.188 | -0.251* | -0.228 | -0.338** |
| | (0.129) | | (0.137) | (0.130) | (0.129) | (0.159) | (0.144) |
| $\Delta^+$ *log* $(1-\tau^{e,l}$ | -0.518*** | | -0.545*** | -0.653*** | -0.519*** | -0.361*** | -0.590*** |
| | (0.108) | | (0.106) | (0.128) | (0.109) | (0.116) | (0.114) |
| (a) + (b) | -0.788*** | | -0.829*** | -0.841*** | -0.770*** | -0.589*** | -0.927*** |
| | (0.160) | | (0.165) | (0.172) | (0.160) | (0.186) | (0.191) |
| Observations | 28,930 | | 18,817 | 11,668 | 28,498 | 20,540 | 28,930 |
| Unique individuals | 14,471 | | 9,409 | 5,834 | 14,255 | 10,274 | 14,471 |
| Specification | | | | | | | |
| Baseline | Yes | No | No | No | No | No | No |
| Estimate | ITT | TOT | ITT | ITT | ITT | ITT | ITT |
| Sample | Baseline | Baseline | No Switch. | Stable G. | Donut | SSA | SSA |
| Data | TAX | TAX | TAX | TAX | TAX | TAX | SSA + TAX |
| pre-TAX controls | No | No | No | No | No | No | Yes |

Notes: This table reports a series of robustness checks for our baseline intensive- and extensive-margin elasticity estimates in the corporate income tax base. Panel (a) presents intensive margin elasticities; panel (b) presents extensive margin elasticities. For conciseness, we report only the short-run 2SLS estimates corresponding to Equations (20) and (21), analogous to panel (c) in Table 3. In panel (a), the dependent variable is the log-change in corporate income, $\Delta \log y_i^c$, while the endogenous variables are $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$ and are instrumented with the interaction terms of $treat^{MTR}$ with dummies for 2011 and 2012. In panel (b), the outcome variable is the change in corporate income reporting status, $\Delta\mathbb{1}(y_i^c > 0)$, while the endogenous variables $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$ and are instrumented with the interaction terms of $treat^{ATR}$ with dummies for 2011 and 2012. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). Column (1) replicates our preferred specification. Column (2) uses observed rather than predicted changes in marginal net-of-tax rates, yielding treatment-on-the-treated effects as discussed in Section 5; since observed rates are undefined for TIEs who drop out of PLIT, we only report intensive margin estimates. Column (3) restricts the sample to TIEs who did not change tax brackets between 2010 and 2011. Column (4) further restricts to the TIEs who remained in the same income zone (G1:G4) throughout 2009-2011. Column (5) excludes TIEs whose average absolute distance to a bracket threshold was less than 25 BPC in the pre-treatment period (bottom 5% of the distribution). Column (6) replicates our baseline using the sub-sample of TIEs matched to *SSA* records, as described in Section 4 and Appendix C. Column (7) replaces *TAX* outcomes with *SSA*-based outcomes; since SSA weights are not appropriate to represent tax revenue impacts, we weight by pre-reform income or revenue from *TAX* records. Column (8) adds decile dummies for cumulative labor income over 2000-2008 from *SSA* records as controls; to use the full *TAX* sample, TIEs who do not match with *SSA* records are all included in a same category of missing pre-reform income. Except for column (7), as already mentioned, all other elasticity estimates for intensive margin responses are weighted by income, while extensive margin estimates are based on revenue weights. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates in columns (1) through (6) are based on *TAX* records, estimates in column (7) are based on *SSA* records, and estimates in column (8) use both *TAX* records and *SSA* records.

# F Further Heterogeneity Analysis

In this appendix we provide additional estimates for the two dimensions of heterogeneity considered in our analysis. Heterogeneity by employment type is defined based on whether TIEs were wage earners or self-employed during the pre-treatment period (2009-2010). Wage earners are defined as individuals who reported no self-employment income in either year, while self-employed individuals are those who reported at least some self-employment income in that period. Heterogeneity by treatment intensity is defined according to TIEs' position relative to the income zones G1-G4 created by the reform. As discussed in Section 2, the 2012 tax reform introduced four income zones, each associated with different changes in marginal and effective net-of-tax rates. Panels (a) and (b) of Figure 2 in the main text provide a visual summary of these zones. Zones G2 and G4 experienced increases in marginal tax rates, from 22% to 25%, and from 25% to 30%, respectively. Zones G1 and G3 did not experience changes in marginal tax rates. In terms of effective tax rates, only G1 remained unaffected; G2, G3, and G4 all saw increases of up to 3 percentage points, increasing with income. Based on this structure, we define two treatment intensity groups splitting the treatment variables $treat^{MTR}$ and $treat^{ATR}$ into finer categories. The lower-intensity group includes treated TIEs who never entered G4 in the pre-treatment period, while the higher-intensity group includes those who were in G4 at least once. It is important to note that while we refer to these as "lower" and "higher" treatment intensity groups, intensity is directly correlated with income. As a result, we cannot disentangle whether the heterogeneous responses are driven by differences in the size of the tax change (e.g., Chetty et al. 2011) or by higher-income taxpayers being more sophisticated or facing more or easier margins of adjustment.

Figure F.1 replicates our baseline analysis for own-base responses included in Figure 3, but using the more detailed definition of treatment intensity described above. Note that panels (b) and (d) in these figures correspond to panels (a) and (b) in Figure 4, and are repeated here for illustration purposes. Overall, all four panels point in the same direction: the bulk of the response, both in the intensive and extensive margins, is driven by TIEs in the higher-intensity treatment group. The lower-intensity group shows smaller responses that move in the same direction but are only marginally statistically significant. This can be seen more clearly in Tables F.1 and F.2, which report the full estimates. For example, column (1) in Table F.1 shows that the intensive margin response for the lower-intensity group is -0.011 ($p$-value = 0.087), while column (4) shows that for the higher-intensity group the estimate increases to -0.096 ($p$-value < 0.001). Even after accounting for the differences in the average change in the marginal net-of-tax rate between the groups (-0.029 vs. -0.066), the implied elasticities are notoriously different: 0.48 ($p-value = 0.048$) for the lower-intensity group and

0.95 ($p$-value $< 0.001$) for the higher-intensity group. A similar pattern holds for extensive margin responses reported in Table F.2. The 2012 reduced-form estimate is -0.008 ($p-value$ = 0.198) for the lower-intensity group, compared to -0.070 ($p$-value $< 0.001$) for the higher-intensity group, almost ten times larger. Here, it is also important to note differences in the change in effective net-of-tax rate changes between the lower- and higher-intensity groups are larger than for the marginal net of tax rates. In this case, the average change in the effective net of tax rate for the lower-intensity group is -0.005, whereas for the higher-intensity group is -0.024.

Figure F.2 presents similar results on own-base responses, this time splitting TIEs based on their employment type. As before, note that panels (c) and (f) in these figures correspond to panels (c) and (d) in Figure 4, and are repeated here for illustration purposes. Overall, we observe some evidence of stronger responses among wage earners compared to self-employed TIEs, although the magnitude of the differences is substantially less pronounced than that observed when comparing higher- and lower-intensity treatment groups. For example, the 2012 reduced-form estimates reported in columns (1) and (4) of Table F.3 are statistically significant at a 5% level for both groups. Furthermore, considering both 2011 and 2012 together, the differences between groups are even smaller. The estimated short-run elasticities follow the same pattern. Regarding extensive margin responses, Table F.4 we observe similar results. The 2012 reduced-form estimate is -0.048 ($p$-value $< 0.001$) for wage earners and -0.024 ($p$-value $= 0.010$) for self-employed TIEs. In this case, the corresponding 2SLS elasticity estimates are statistically significant at the 5% level for both groups.

Overall, these estimates suggest that wage earners may be responding more strongly than self-employed TIEs, although the differences between groups are more nuanced than when comparing lower- vs. higher-intensity treated TIEs. As discussed in Sections 2 and 6, and in Appendix A, this pattern is likely driven by our sample selection criteria. In particular, the most responsive self-employed TIEs may have already exited the PLIT base prior to the reform, shifting to other tax bases, such as corporate taxation, which already offered more favorable treatment. As a result, the self-employed individuals who remain in the PLIT base are most likely those who face higher frictions to exit, or for whom the financial incentives to switch are weaker.

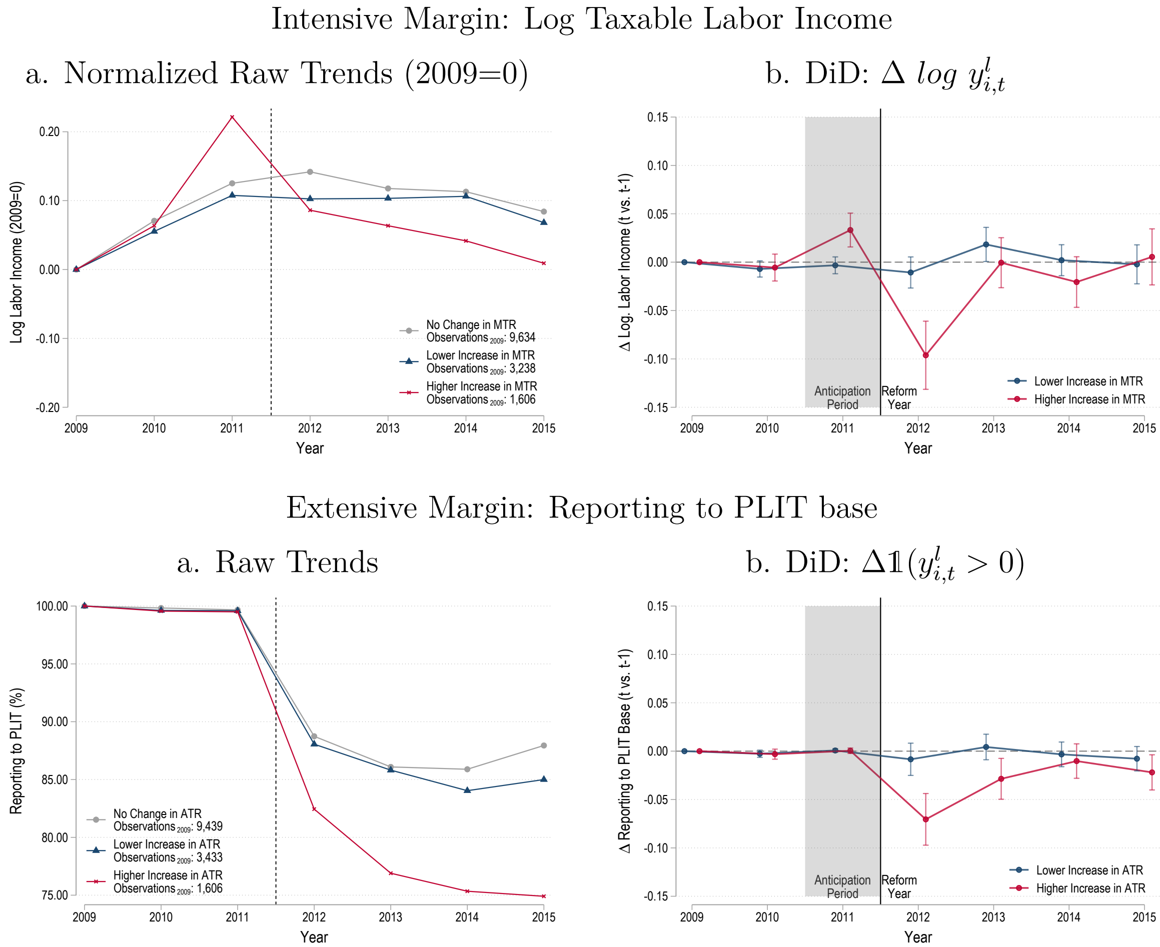
When looking at cross-tax base responses by intensity of treatment, we find the same broad patterns as in the own-base analysis. The reduced-form coefficients, depicted in Figures F.3 for the capital income tax base and in Figure F.4 for the corporate income tax base, depict two main results. First, there is no evidence of intensive margin responses for neither of the groups or tax bases. This is confirmed by the econometric evidence reported in Columns (2), (3), (5), and (6) in Table F.1. Second, in terms of extensive margin responses, it is clear that the bulk of the response is driven by higher-intensity top income earners. These patterns are supported by the econometric estimates reported in Table F.2. Combining the reduced-form effects for 2011 and 2012, we find that higher-intensity treated individuals increased their probability of reporting income to the capital and corporate tax bases by 0.018 ($p$-value <0.001) and 0.014 ($p$-value <0.001), respectively, compared to 0.004 ($p - value = 0.115$) and 0.007 ($p - value = 0.001$) for the lower-intensity group. However, in this case, when looking at the elasticity estimates, the differences in reduced-form coefficients are not as large as the differences in the first-stage estimates. This results in very similar elasticity estimates for the capital income base across the two groups, and even a larger elasticity for the lower-intensity group in the corporate tax base. As discussed in Section 6, our pooled weighted estimate already accounts for these differences, and there is no need to estimate these effects separately. Still, this breakdown helps illustrate the anatomy of the response.

When looking at capital income cross-tax base responses by type of employment, we find no evidence of intensive margin responses for self-employed individuals or wage earners. We report these results in Figure F.5 and columns (2) and (4) in Table F.3. When looking at the extensive margin, Figure F.5 shows that the reduced-form effect of the reform on reporting to the capital income base is very similar across employment types. Table F.4 confirms this preliminary visual analysis and shows a combined 2011-2012 effect of 0.010 ($p$-value = 0.005) for wage earners and 0.011 ($p$-value = 0.005) for the self-employed. Due to slight differences in the first-stage coefficients (-0.014 vs. -0.010), however, this translates into a larger estimated elasticity for the self-employed: -1.06 ($p$-value = 0.006) compared to -0.62 ($p$-value = 0.006) for wage earners.

Figure F.6 and columns (3) and (6) of Tables F.3 and F.4 report estimates of cross-base responses in the corporate income tax base. Once again, we find no evidence of intensive margin responses. It is worth noting that cross-base intensive margin estimates for the corporate income tax base are restricted to self-employed individuals, as identifying intensive margin responses requires taxpayers to report income to the corporate tax base in the base year, a condition that also defines being a self-employed. More interestingly, when looking at the extensive margin, the figure suggests that self-employed workers may respond more strongly, though estimates are more imprecise due to a smaller sample size. Table F.4 shows that the combined 2011-2012 reduced-form response is 0.005 ($p$-value < 0.001) for wage earners and 0.017 ($p$-value = 0.002) for the self-employed. This results in estimated elasticities of -0.42 ($p - value < 0.001$) and -1.70 ($p - value = 0.002$), respectively. While, as discussed earlier, the self-employed in our sample may be somewhat selected toward a less responsive group, the fact that extensive margin responses in the corporate tax base are larger for this group is reasonable. This may be explained by lower costs of shifting for self-employed individuals compared to wage earners who would also need to restructure their employment status, transitioning from wage employment to self-employment, before (or simultaneously) being able to shift tax bases.

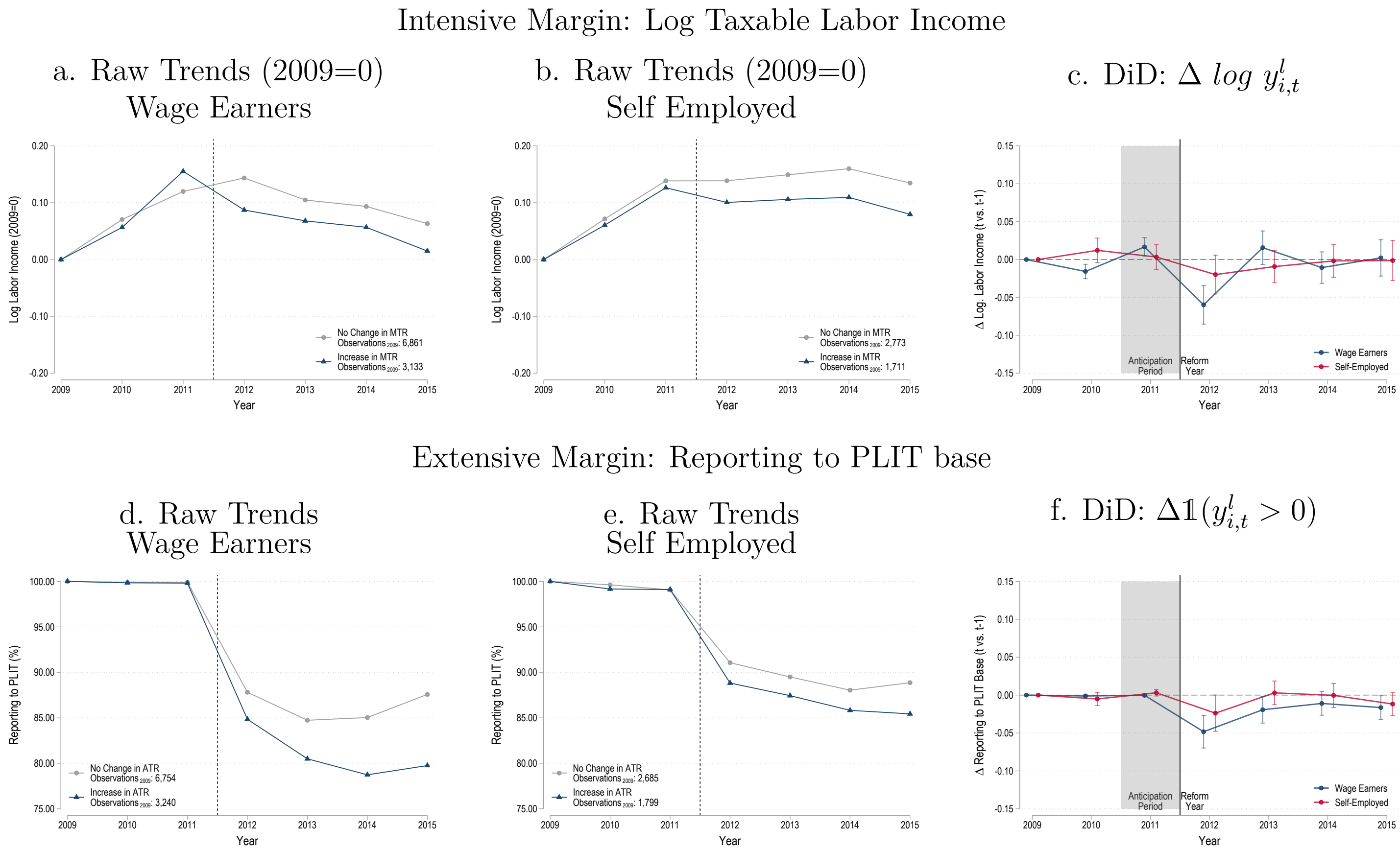
Two broad lessons constitute the main takeaway of our heterogeneity analysis. First, the strongest differences are observed across treatment intensity, with larger responses among TIEs facing greater tax increases and typically null responses among low-intensity treated TIEs. Second, differences by employment type are more nuanced. In general, both wage earners and self-employed seem to respond to the tax reform. However, there are some differences in the magnitude and direction of the heterogeneity, which makes it difficult to draw a clear conclusion. For instance, self-employed TIEs respond less on own-base margins, but show similar or even stronger responses in cross-base (extensive) shifting, particularly toward the corporate tax base, likely due to lower costs of switching to this specific tax base.

Figure F.1: Own-Tax-Base Heterogeneous Responses by Treatment Type: Graphical Evidence



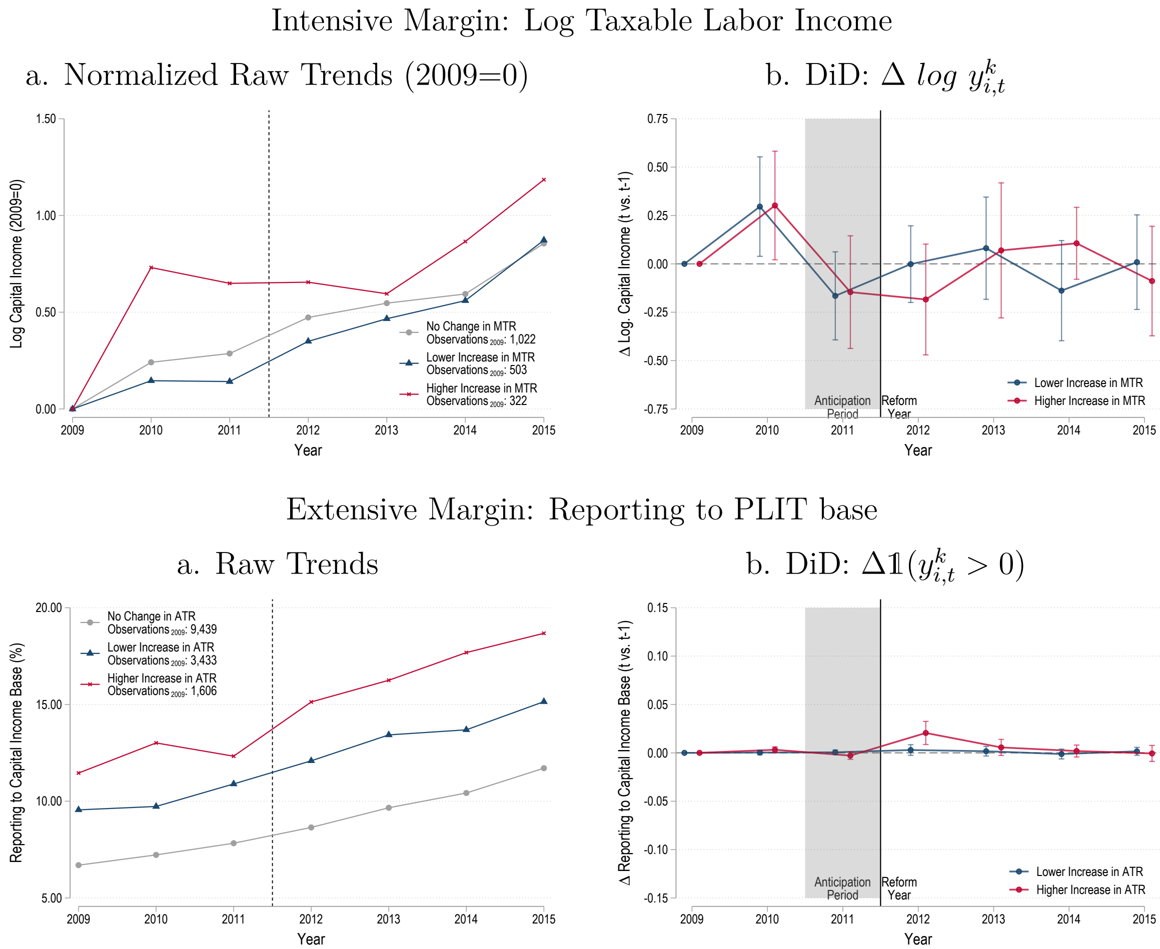

Notes: This figure illustrates the own-tax base reduced-form effects of the 2012 tax reform as in Figure 3, but splitting the the treatment group into two groups with different treatment intensity. Panels (a) and (b) focus on intensive margin responses. Panel (a) depicts the raw evolution of log gross labor income from 2009 to 2015, normalized to 2009 values, for treated and control TIEs without further adjustments. In the lower-intensity group (depicted in blue), we include treated taxpayers ($treat^{MTR} = 1$) who never stepped into G4. The higher-intensity group (depicted in red) includes all treated TIEs who entered G4 at least once. Estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (b) reports dynamic DiD coefficients based on Equation (16), adding year-by-higher-intensity interactions to capture differential effects by group. Estimates in blue correspond to the lower-intensity group, and those in red to the higher-intensity group (i.e., the sum of the baseline and interacted effects). The outcome variable in this specification is the log change in gross labor income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. As described in Section 5, these correspond to income-weighted estimates, described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (c) and (d) focus on extensive margin responses, again breaking down the treatment group into lower- and higher-intensity of treatment. Panel (c) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the PLIT base, with the number of observations in 2009 shown in the bottom right corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), who never stepped into G4. Estimates in red correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$) who were in G4 at least once in 2009-2011. Estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (d) reports dynamic DiD coefficients based on Equation (17) adding year-by-intensity interactions to capture differential effects by group. The outcome variable in this specification is $\Delta\mathbb{1}(y^l_{i,t} > 0)$, with $\mathbb{1}(y^l_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT base. As such, $\Delta\mathbb{1}(y^l_{i,t} > 0)$ takes values -1, 0, or 1. Estimates in blue correspond to the lower-intensity group, and those in red to the higher-intensity group (i.e., the sum of the baseline and interacted effects). As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in Tables F.1 and F.2.

Figure F.2: Own-Tax-Base Heterogeneous Responses by Employment Type: Graphical Evidence




Notes: This figure illustrates the own-tax base reduced-form effects of the 2012 tax reform as in Figure 3, but splitting the sample by employment type. Wage earners are defined as individuals with no self-employment income in 2009-2010, while self-employed workers report any self-employment income in that period. Panels (a), (b), and (c) focus on intensive margin responses. Panel (a) depicts the raw evolution of log gross labor income from 2009 to 2015, normalized to 2009 values, for treated and control *wage* top income earners without further adjustments. Panel (b) does the same for *self-employed* top income earners. Estimates in blue represent TIEs in the treatment group, defined in Section 5 as $treat^{MTR} = 1$, and estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (c) reports dynamic DiD coefficients based on Equation (16), estimated separately by group. Estimates in blue correspond to wage earners, and estimates in red correspond to self-employed. The outcome variable in this specification is the log change in gross labor income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. As described in Section 5, these correspond to income-weighted estimates, described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (d), (e), and (f) focus on extensive margin responses, again breaking down the analysis by wage and self-employed top income earners. Panel (d) depicts the raw evolution of the share of wage top income earners in our analysis sample who report income to the PLIT base, with the number of observations in 2009 shown in the bottom right corner. Panel (e) does the same for self-employed top income earners. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (f) reports dynamic DiD coefficients based on Equation (17) estimated separately by group. The outcome variable in this specification is $\Delta\mathbb{1}(y^l_{i,t} > 0)$, with $\mathbb{1}(y^l_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT base. As such, $\Delta\mathbb{1}(y^l_{i,t} > 0)$ takes values -1, 0, or 1. Estimates in blue correspond to wage earners, and estimates in red correspond to self-employed. As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in Tables F.3 and F.4.

Figure F.3: Cross-Tax Capital Income Base: Heterogeneous Responses by Treatment Type - Graphical Evidence

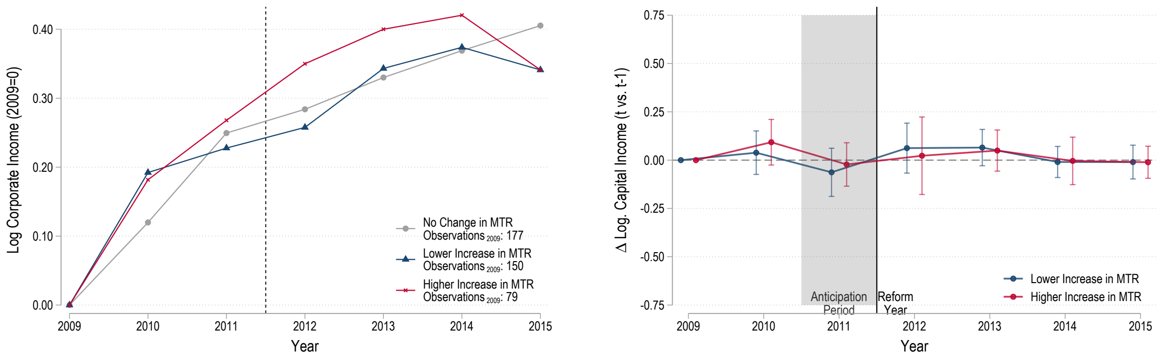



Notes: This figure illustrates the cross-tax base reduced-form effects of the 2012 tax reform in the capital income base as in Figure 5, but splitting the the treatment group into two groups with different treatment intensity. Panels (a) and (b) focus on intensive margin responses. Panel (a) depicts the raw evolution of log capital income from 2009 to 2015, normalized to 2009 values, for treated and control TIEs without further adjustments. In the lower-intensity group (depicted in blue), we include treated taxpayers ($treat^{MTR} = 1$) who never stepped into G4. The higher-intensity group (depicted in red) includes all treated TIEs who entered G4 at least once. Estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (b) reports dynamic DiD coefficients based on Equation (16), adding year-by-higher-intensity interactions to capture differential effects by group. Estimates in blue correspond to the lower-intensity group, and those in red to the higher-intensity group (i.e., the sum of the baseline and interacted effects). The outcome variable in this specification is the log change in capital income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles, conditional on reporting positive values. As described in Section 5, these correspond to income-weighted estimates, described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (c) and (d) focus on extensive margin responses, again breaking down the treatment group into lower- and higher-intensity of treatment. Panel (c) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the capital income tax base, with the number of observations in 2009 shown in the bottom right corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), who never stepped into G4. Estimates in red correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$) who were in G4 at least once in 2009-2011. Estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (d) reports dynamic DiD coefficients based on Equation (17) adding year-by-intensity interactions to capture differential effects by group. The outcome variable in this specification is $\Delta\mathbb{1}(y^k_{i,t} > 0)$, with $\mathbb{1}(y^k_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT base. As such, $\Delta\mathbb{1}(y^k_{i,t} > 0)$ takes values -1, 0, or 1. Estimates in blue correspond to the lower-intensity group, and those in red to the higher-intensity group (i.e., the sum of the baseline and interacted effects). As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in Tables F.1 and F.2.

Figure F.4: Cross-Tax Corporate Income Base: Heterogeneous Responses by Treatment Type - Graphical Evidence

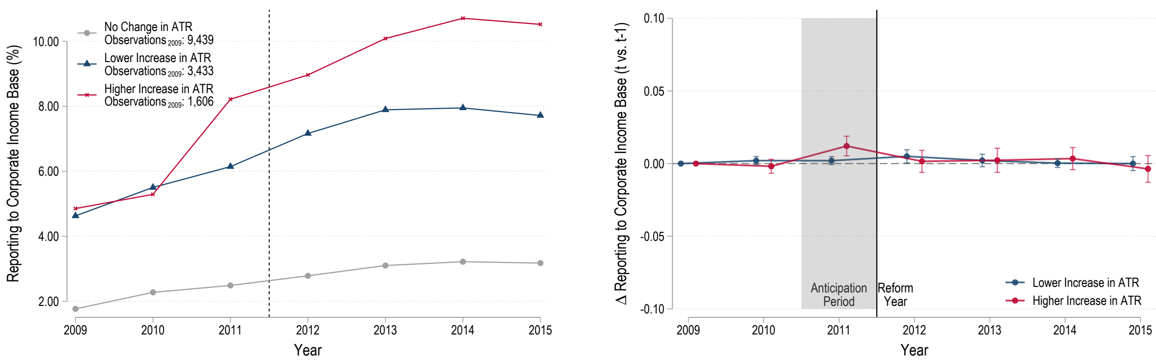
Intensive Margin: Log Corporate Income

a. Normalized Raw Trends (2009=0) b. DiD: $\Delta\ log\ y^c_{i,t}$




Extensive Margin: Reporting to Corporate Income Base

a. Raw Trends b. DiD: $\Delta\mathbb{1}(y^c_{i,t} > 0)$

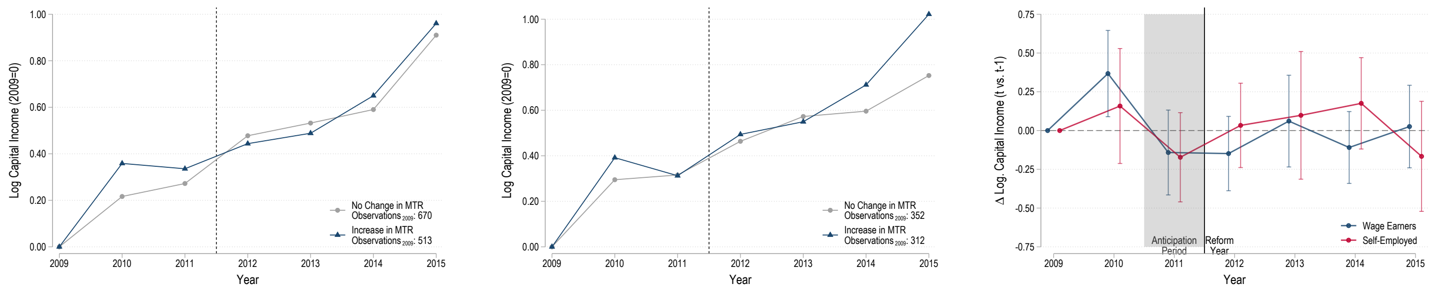



Notes: This figure illustrates the cross-tax base reduced-form effects of the 2012 tax reform in the corporate income base as in Figure 6, but splitting the the treatment group into two groups with different treatment intensity. Panels (a) and (b) focus on intensive margin responses. Panel (a) depicts the raw evolution of log corporate income from 2009 to 2015, normalized to 2009 values, for treated and control TIEs without further adjustments. In the lower-intensity group (depicted in blue), we include treated taxpayers ($treat^{MTR} = 1$) who never stepped into G4. The higher-intensity group (depicted in red) includes all treated TIEs who entered G4 at least once. Estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (b) reports dynamic DiD coefficients based on Equation (16), adding year-by-higher-intensity interactions to capture differential effects by group. Estimates in blue correspond to the lower-intensity group, and those in red to the higher-intensity group (i.e., the sum of the baseline and interacted effects). The outcome variable in this specification is the log change in corporate income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles, conditional on reporting positive values. As described in Section 5, these correspond to income-weighted estimates, described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (c) and (d) focus on extensive margin responses, again breaking down the treatment group into lower- and higher-intensity of treatment. Panel (c) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the corporate income tax base, with the number of observations in 2009 shown in the bottom right corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), who never stepped into G4. Estimates in red correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$) who were in G4 at least once in 2009-2011. Estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (d) reports dynamic DiD coefficients based on Equation (17) adding year-by-intensity interactions to capture differential effects by group. The outcome variable in this specification is $\Delta\mathbb{1}(y^c_{i,t} > 0)$, with $\mathbb{1}(y^c_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT base. As such, $\Delta\mathbb{1}(y^c_{i,t} > 0)$ takes values -1, 0, or 1. Estimates in blue correspond to the lower-intensity group, and those in red to the higher-intensity group (i.e., the sum of the baseline and interacted effects). As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in Tables F.1 and F.2.

Figure F.5: Cross-Tax Capital Income Base Heterogeneous Responses by Employment Type: Graphical Evidence

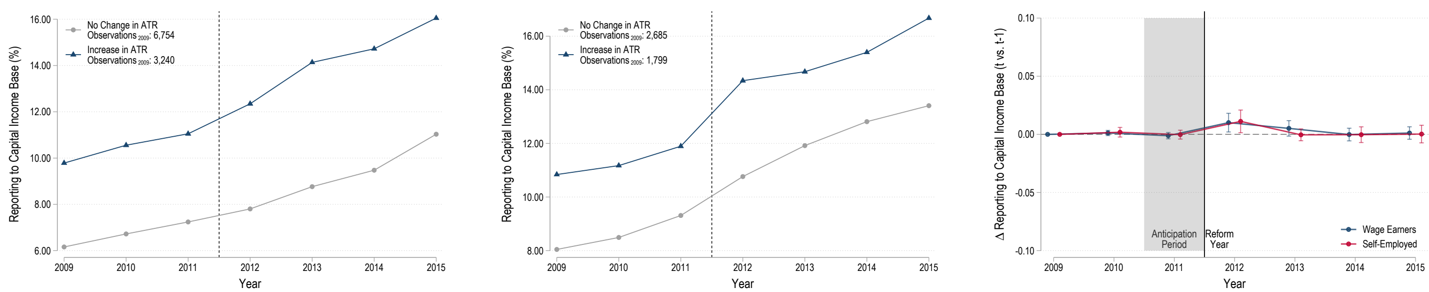
Intensive Margin: Log Taxable Capital Income

a. Raw Trends (2009=0) Wage Earners | b. Raw Trends (2009=0) Self Employed | c. DiD: $\Delta\ log\ y_{i,t}^{k}$




Extensive Margin: Reporting to Capital Income Base

d. Raw Trends Wage Earners | e. Raw Trends Self Employed | f. DiD: $\Delta\mathbb{1}(y_{i,t}^{k} > 0)$

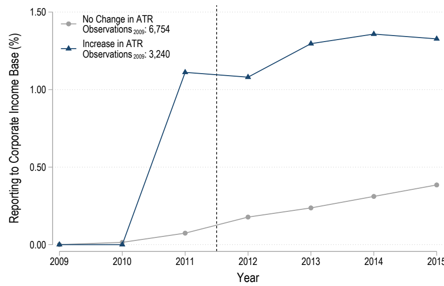



Notes: This figure illustrates the cross-tax base reduced-form effects of the 2012 tax reform in the capital income base as in Figure 5, but splitting the sample by employment type. Wage earners are defined as individuals with no self-employment income in 2009-2010, while self-employed workers report any self-employment income in that period. Panels (a), (b), and (c) focus on intensive margin responses. Panel (a) depicts the raw evolution of log capital income from 2009 to 2015, normalized to 2009 values, for treated and control *wage* top income earners without further adjustments. Panel (b) does the same for *self-employed* top income earners. Estimates in blue represent TIEs in the treatment group, defined in Section 5 as $treat^{MTR} = 1$, and estimates in gray correspond to control TIEs ($treat^{MTR} = 0$). The number of observations in 2009 for each group is reported in the bottom right corner. Panel (c) reports dynamic DiD coefficients based on Equation (16), estimated separately by group. Estimates in blue correspond to wage earners, and estimates in red correspond to self-employed. The outcome variable in this specification is the log change in capital income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. As described in Section 5, these correspond to income-weighted estimates, described in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. Panels (d), (e), and (f) focus on extensive margin responses, again breaking down the analysis by wage and self-employed top income earners. Panel (d) depicts the raw evolution of the share of wage top income earners in our analysis sample who report income to the capital income tax base, with the number of observations in 2009 shown in the bottom right corner. Panel (e) does the same for self-employed top income earners. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (f) reports dynamic DiD coefficients based on Equation (17) estimated separately by group. The outcome variable in this specification is $\Delta\mathbb{1}(y_{i,t}^{k} > 0)$, with $\mathbb{1}(y_{i,t}^{k} > 0)$ indicating whether a taxpayer reports any income to the capital income tax base. As such, $\Delta\mathbb{1}(y_{i,t}^{k} > 0)$ takes values -1, 0, or 1. Estimates in blue correspond to wage earners, and estimates in red correspond to self-employed. As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area in panels (b) and (d) corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in Tables F.3 and F.4.

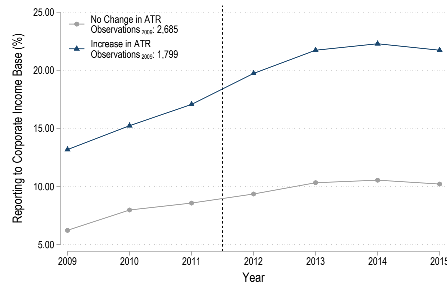
Figure F.6: Cross-Tax Corporate Base Heterogeneous Responses by Employment Type: Graphical Evidence

Extensive Margin: Reporting to Corporate Income Base

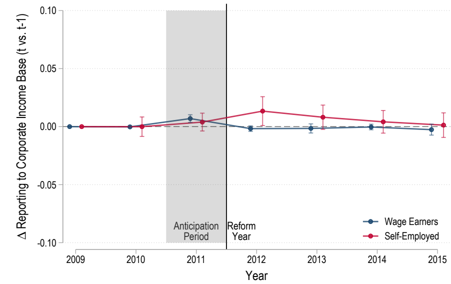
a. Raw Trends
Wage Earners




b. Raw Trends
Self Employed




c. DiD: $\Delta \mathbb{1}(y^c_{i,t} > 0)$




Notes: This figure illustrates the cross-tax base reduced-form effects of the 2012 tax reform in the corporate income base as in Figure 6, but splitting the sample by employment type. Wage earners are defined as individuals with no self-employment income in 2009-2010, while self-employed workers report any self-employment income in that period. Because all TIEs reporting income to the corporate tax base in the pre-treatment period are, by definition, classified as self-employed, there are no intensive margin estimates for wage earners. All baseline specifications shown so far therefore correspond to self-employed TIEs. Hence, in this figure we only report extensive margin results separately by employment type. Panels (a), (b), and (c) focus on extensive margin responses, breaking down the analysis in wage and self-employed top income earners. Panel (a) depicts the raw evolution of the share of wage top income earners in our analysis sample who report income to the corporate income tax base, with the number of observations in 2009 shown in the bottom right corner. Panel (b) does the same for self-employed top income earners. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (c) reports dynamic DiD coefficients based on Equation (17) estimated separately by group. The outcome variable in this specification is $\Delta \mathbb{1}(y^c_{i,t} > 0)$, with $\mathbb{1}(y^c_{i,t} > 0)$ indicating whether a taxpayer reports any income to the capital income tax base. As such, $\Delta \mathbb{1}(y^c_{i,t} > 0)$ takes values -1, 0, or 1. Estimates in blue correspond to wage earners, and estimates in red correspond to self-employed. As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area corresponds to 2011, the anticipation period. All figures are based on *TAX* records. Full estimates, standard errors, and sample sizes for panels (b) and (d) are reported in Tables F.3 and F.4.

Table F.1: Intensive Margin Elasticity Estimates by Change in Marginal Net of Tax Rate - Full Table

| | Lower Increase in MTR | | | Larger Increase in MTR | | |
|---|---|---|---|---|---|---|
| | Labor (1) | Capital (2) | Corporate (3) | Labor (4) | Capital (5) | Corporate (6) |
| | **Panel a. Reduced-Form Estimates** | | | | | |
| | Dep. Var.: $\Delta$ *log* $y_i^b$ | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.011* | -0.001 | 0.062 | -0.096*** | -0.184* | 0.023 |
| | (0.006) | (0.077) | (0.050) | (0.014) | (0.111) | (0.078) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.003 | -0.165* | -0.063 | 0.033*** | -0.146 | -0.022 |
| | (0.003) | (0.088) | (0.048) | (0.007) | (0.113) | (0.044) |
| | **Panel b. First-Stage Estimates** | | | | | |
| | Dep. Var.: $\Delta$ *log* $(1-\tau^l)$ <br> $\Delta^+$ *log* $(1-\tau^l)$ | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.029*** | -0.023*** | -0.021*** | -0.066*** | -0.060*** | -0.058*** |
| | (0.000) | (0.002) | (0.002) | (0.000) | (0.002) | (0.004) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.028*** | -0.022*** | -0.023*** | -0.064*** | -0.060*** | -0.059*** |
| | (0.000) | (0.002) | (0.002) | (0.000) | (0.003) | (0.004) |
| | **Panel c. 2SLS Estimates** | | | | | |
| | Dep. Var.: $\Delta$ *log* $y_i^b$ | | | | | |
| (a): $\Delta$ *log* $(1-\tau^l)$ | 0.363* | 0.062 | -2.898 | 1.468*** | 3.045* | -0.394 |
| | (0.212) | (3.352) | (2.349) | (0.208) | (1.851) | (1.344) |
| (b): $\Delta^+$ *log* $(1-\tau^l)$ | 0.115 | 7.478* | 2.750 | -0.517*** | 2.450 | 0.382 |
| | (0.119) | (4.170) | (2.131) | (0.104) | (1.880) | (0.741) |
| (a) + (b) | 0.478** | 7.539 | -0.148 | 0.950*** | 5.494** | -0.012 |
| | (0.241) | (5.268) | (2.960) | (0.210) | (2.690) | (1.503) |
| Observations | 27,128 | 3,395 | 964 | 27,128 | 3,395 | 964 |
| Unique individuals | 14,419 | 1,946 | 533 | 14,419 | 1,946 | 533 |
| Weights: | Lab. Inc. | Cap. Inc. | Corp. Inc. | PIT Rev. | KIT Rev. | CIT Rev. |

Notes: This table reports our preferred intensive-margin estimates for each tax base but splitting the sample by treatment intensity. We classify TIEs into three groups. In the lower-intensity group, we include treated taxpayers ($treat^{MTR} = 1$) who never stepped into G4. The higher-intensity group includes all treated TIEs who entered G4 at least once. The reference group corresponds to control TIEs ($treat^{MTR} = 0$). Panel (a) reports reduced-form estimates, based on Equation (16) augmented with year-by-higher-intensity interactions to capture differential effects by group. In columns (1)-(3), we report the coefficients associated with variables $treat^{MTR} \times \mathbb{1}(year = 2012)$ and $treat^{MTR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta \log y_i^b$ as the outcome for PLIT, capital, and corporate income, respectively. In columns (4)-(6) we report the sum of the coefficients reported in columns (1)-(3) and the corresponding interaction term, respectively. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable. In columns (1)-(3), these correspond to changes in the *marginal* net-of-tax rate, $\Delta \log(1-\tau_{it}^l)$ and its forward term $\Delta^+ \log(1-\tau_{it}^l)$. In columns (4) to (6), we report the sum of the baseline and interacted terms. Panel (c) reports the 2SLS elasticity estimates based on Equations (20), again augmented with year-by-higher-intensity interactions. In columns (1)-(3), the endogenous variables $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$ are instrumented with the interaction terms of $treat^{MTR}$ with dummies for 2011 and 2012. In columns (4)-(6), we report the sum of the baseline and interacted terms. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). All estimates are weighted by income. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All estimates are based on *TAX* records.

Table F.2: Extensive Margin Elasticity Estimates by Change in Net of Effective Tax Rate - Full Table

| | Lower Increase in ATR | | | Larger Increase in ATR | | |
|---|---|---|---|---|---|---|
| | Labor (1) | Capital (2) | Corporate (3) | Labor (4) | Capital (5) | Corporate (6) |
| | **Panel a. Reduced-Form Estimates** | | | | | |
| | Dep. Var.: $\Delta\mathbb{1}(y_i^b > 0)$ | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.008 | 0.003 | 0.005*** | -0.070*** | 0.021*** | 0.002 |
| | (0.006) | (0.002) | (0.002) | (0.010) | (0.005) | (0.003) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.001 | 0.001 | 0.002* | 0.000 | -0.003* | 0.012*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.003) |
| | **Panel b. First-Stage Estimates** | | | | | |
| | Dep. Var.: $\Delta\ log\ (1-\tau^{e,l})$ <br> $\Delta^+\ log\ (1-\tau^{e,l})$ | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.005*** | -0.005*** | -0.005*** | -0.024*** | -0.024*** | -0.024*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.005*** | -0.004*** | -0.004*** | -0.023*** | -0.023*** | -0.022*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| | **Panel c. 2SLS Estimates** | | | | | |
| | Dep. Var.: $\Delta\mathbb{1}(y_i^b > 0)$ | | | | | |
| (a): $\Delta\ log\ (1-\tau^{e,l})$ | 1.740 | -0.628 | -1.081*** | 2.901*** | -0.863*** | -0.065 |
| | (1.338) | (0.457) | (0.382) | (0.423) | (0.194) | (0.123) |
| (b): $\Delta^+\ log\ (1-\tau^{e,l})$ | -0.157 | -0.131 | -0.446* | -0.019 | 0.125* | -0.538*** |
| | (0.163) | (0.204) | (0.240) | (0.048) | (0.067) | (0.117) |
| (a) + (b) | 1.584 | -0.758 | -1.527*** | 2.882*** | -0.738*** | -0.603*** |
| | (1.348) | (0.480) | (0.449) | (0.426) | (0.200) | (0.158) |
| Observations | 28,835 | 28,911 | 28,930 | 28,835 | 28,911 | 28,930 |
| Unique individuals | 14,444 | 14,466 | 14,471 | 14,444 | 14,466 | 14,471 |
| Weights: | Lab. Inc. | Cap. Inc. | Corp. Inc. | PIT Rev. | KIT Rev. | CIT Rev. |

Notes: This table reports our preferred extensive-margin estimates for each tax base but splitting the sample by treatment intensity. We classify TIEs into three groups. In the lower-intensity group, we include treated taxpayers ($treat^{ATR} = 1$) who never stepped into G4. The higher-intensity group includes all treated TIEs who entered G4 at least once. The reference group corresponds to control TIEs ($treat^{ATR} = 0$). Panel (a) reports reduced-form estimates, based on Equation (17) augmented with year-by-higher-intensity interactions to capture differential effects by group. In columns (1)-(3), we report the coefficients associated with variables $treat^{ATR} \times \mathbb{1}(year = 2012)$ and $treat^{ATR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta\mathbb{1}(y_i^b > 0)$ as the outcome for PLIT, capital, and corporate income, respectively. In columns (4)-(6) we report the sum of the coefficients reported in columns (1)-(3) and the corresponding interaction term, respectively. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable. In columns (1)-(3), these correspond to changes in the *effective* net-of-tax rate, $\Delta \log(1-\tau_{it}^{e,l})$ and its forward term $\Delta^+ \log(1-\tau_{it}^{e,l})$. In columns (4) to (6), we report the sum of the baseline and interacted terms. Panel (c) reports the 2SLS elasticity estimates based on Equations (21), again augmented with year-by-higher-intensity interactions. In columns (1)-(3), the endogenous variables $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$ are instrumented with the interaction terms of $treat^{ATR}$ with dummies for 2011 and 2012. In columns (4)-(6), we report the sum of the baseline and interacted terms. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). All estimates are weighted by revenue. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All estimates are based on *TAX* records.

Table F.3: Intensive Margin Elasticity Estimates by Employment Type - Full Table

| | Wage Earners | | | Self-Employed | | |
|---|---|---|---|---|---|---|
| | Labor (1) | Capital (2) | Corporate (3) | Labor (4) | Capital (5) | Corporate (6) |
| | **Panel a. Reduced-Form Estimates** | | | | | |
| | Dep. Var.: $\Delta$ *log* $y_i^b$ | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.060*** | -0.148 | | -0.020** | 0.033 | 0.049 |
| | (0.010) | (0.093) | | (0.010) | (0.105) | (0.051) |
| $Treat \times \mathbb{1}(year = 2011)$ | 0.017*** | -0.142 | | 0.003 | -0.172 | -0.049 |
| | (0.005) | (0.106) | | (0.006) | (0.112) | (0.041) |
| | **Panel b. First-Stage Estimates** | | | | | |
| | Dep. Var.: $\Delta$ *log* $(1-\tau^l)$ $\Delta^+$ *log* $(1-\tau^l)$ | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.046*** | -0.043*** | | -0.042*** | -0.042*** | -0.034*** |
| | (0.000) | (0.003) | | (0.001) | (0.004) | (0.002) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.045*** | -0.044*** | | -0.041*** | -0.039*** | -0.035*** |
| | (0.000) | (0.003) | | (0.001) | (0.004) | (0.002) |
| | **Panel c. 2SLS Estimates** | | | | | |
| | Dep. Var.: $\Delta$ *log* $y_i^b$ | | | | | |
| (a): $\Delta$ *log* $(1-\tau^l)$ | 1.295*** | 3.482 | | 0.468** | -0.799 | -1.454 |
| | (0.212) | (2.161) | | (0.233) | (2.516) | (1.532) |
| (b): $\Delta^+$ *log* $(1-\tau^l)$ | -0.368*** | 3.228 | | -0.082 | 4.476 | 1.411 |
| | (0.103) | (2.418) | | (0.154) | (2.964) | (1.169) |
| (a) + (b) | 0.926*** | 6.710** | | 0.386 | 3.677 | -0.043 |
| | (0.218) | (3.295) | | (0.266) | (3.588) | (1.805) |
| Observations | 18,651 | 2,184 | | 8,477 | 1,211 | 964 |
| Unique individuals | 9,981 | 1,242 | | 4,438 | 704 | 533 |
| Weights: | Lab. Inc. | Cap. Inc. | | PIT Rev. | KIT Rev. | CIT Rev. |

Notes: This table reports our preferred intensive-margin estimates for each tax base but estimating the results separately by employment type. Wage earners are defined as individuals with no self-employment income in 2009-2010, while self-employed workers report any self-employment income in that period. Panel (a) reports reduced-form estimates, based on Equation (16). In columns (1)-(3), we focus on wage top income earners, and we report the coefficients associated with variables $treat^{MTR} \times \mathbb{1}(year = 2012)$ and $treat^{MTR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta \log y_i^b$ as the outcome for PLIT, capital, and corporate income, respectively. In columns (4)-(6) we report the same estimates but for self-employed TIEs. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable. These correspond to changes in the *marginal* net-of-tax rate, $\Delta \log(1-\tau_{it}^l)$ and its forward term $\Delta^+ \log(1-\tau_{it}^l)$. Panel (c) reports the 2SLS elasticity estimates based on Equation (20). , where the endogenous variables $\Delta \log(1-\tau_{it}^l)$ and $\Delta^+ \log(1-\tau_{it}^l)$ are instrumented with the interaction terms of $treat^{MTR}$ with dummies for 2011 and 2012. In columns (1)-(3), we report estimates for wage earners, whereas in columns (4)-(6) for self-employed TIEs. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). All estimates are weighted by income. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All estimates are based on *TAX* records.

Table F.4: Extensive Margin Elasticity Estimates by Employment Type - Full Table

| | Wage Earners | | | Self-Employed | | |
|---|---|---|---|---|---|---|
| | Labor (1) | Capital (2) | Corporate (3) | Labor (4) | Capital (5) | Corporate (6) |
| **Panel a. Reduced-Form Estimates** | | | | | | |
| Dep. Var.: $\Delta\mathbb{1}(y_i^b > 0)$ | | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.048*** | 0.010*** | -0.002* | -0.024** | 0.011*** | 0.013*** |
| | (0.008) | (0.003) | (0.001) | (0.009) | (0.004) | (0.005) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.000 | -0.001 | 0.007*** | 0.003* | -0.000 | 0.004 |
| | (0.001) | (0.001) | (0.001) | (0.002) | (0.001) | (0.003) |
| **Panel b. First-Stage Estimates** | | | | | | |
| Dep. Var.: $\Delta\ log\ (1-\tau^{e,l})$ / $\Delta^+\ log\ (1-\tau^{e,l})$ | | | | | | |
| $Treat \times \mathbb{1}(year = 2012)$ | -0.015*** | -0.014*** | -0.014*** | -0.011*** | -0.010*** | -0.010*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| $Treat \times \mathbb{1}(year = 2011)$ | -0.014*** | -0.013*** | -0.013*** | -0.011*** | -0.010*** | -0.010*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| **Panel c. 2SLS Estimates** | | | | | | |
| Dep. Var.: $\Delta\mathbb{1}(y_i^b > 0)$ | | | | | | |
| (a): $\Delta\ log\ (1-\tau^{e,l})$ | 3.139*** | -0.708*** | 0.120* | 2.119** | -1.088*** | -1.297*** |
| | (0.537) | (0.218) | (0.064) | (0.824) | (0.367) | (0.469) |
| (b): $\Delta^+\ log\ (1-\tau^{e,l})$ | 0.019 | 0.088 | -0.538*** | -0.292* | 0.023 | -0.400 |
| | (0.055) | (0.082) | (0.099) | (0.159) | (0.151) | (0.303) |
| (a) + (b) | 3.158*** | -0.620*** | -0.417*** | 1.827** | -1.065*** | -1.697*** |
| | (0.539) | (0.225) | (0.103) | (0.839) | (0.384) | (0.548) |
| Observations | 19,963 | 19,985 | 19,968 | 8,872 | 8,926 | 8,962 |
| Unique individuals | 9,988 | 9,993 | 9,989 | 4,456 | 4,473 | 4,482 |
| Weights: | Lab. Inc. | Cap. Inc. | Corp. Inc. | PIT Rev. | KIT Rev. | CIT Rev. |

Notes: This table reports our preferred extensive-margin estimates for each tax base but estimating the results separately by employment type. Wage earners are defined as individuals with no self-employment income in 2009-2010, while self-employed workers report any self-employment income in that period. Panel (a) reports reduced-form estimates, based on Equation (17). In columns (1)-(3), we focus on wage top income earners, and we report the coefficients associated with variables $treat^{ATR} \times \mathbb{1}(year = 2012)$ and $treat^{ATR} \times \mathbb{1}(year = 2011)$, in a regression that uses $\Delta\mathbb{1}(y_i^b > 0)$ as the outcome for PLIT, capital, and corporate income, respectively. In columns (4)-(6) we report the same estimates but for self-employed TIEs. Panel (b) reports first-stage estimates, obtained analogously to panel (a) but using changes in the net-of-tax rate as the outcome variable. These correspond to changes in the *effective* net-of-tax rate, $\Delta \log(1-\tau_{it}^{e,l})$ and its forward term $\Delta^+ \log(1-\tau_{it}^{e,l})$.. Panel (c) reports the 2SLS elasticity estimates based on Equation (21), where the endogenous variables $\Delta \log(1-\tau_{it}^{e,l})$ and $\Delta^+ \log(1-\tau_{it}^{e,l})$ are instrumented with the interaction terms of $treat^{ATR}$ with dummies for 2011 and 2012. In columns (1)-(3), we report estimates for wage earners, whereas in columns (4)-(6) for self-employed TIEs. Row (a) presents the concurrent elasticity, row (b) the anticipation elasticity, and their sum is reported as the short-run elasticity, (a)+(b). All estimates are weighted by revenue. Appendix C provides details on how these weights are constructed. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All estimates are based on *TAX* records.

# G Other Margins of Response

In this appendix, we present the main results from our exploratory analysis of other margins of response. These results are summarized in Section 6 of the main text, but here we provide additional discussion along with the corresponding supporting evidence.

Figure G.1 presents a series of figures that show how TIEs responded to the reform in terms of their total income, defined as the sum of labor, capital, and corporate income. While total income is not the ideal measure for analyzing efficiency costs due to the different tax treatment of income from different tax bases, it helps summarizing all margins of response in a single outcome. In addition, comparing changes in total income with changes in labor income offers a first approximation of how much of the overall response corresponds to shifting behavior to other margins not explicitly included in our analysis. Panel (a) reports estimates from a Poisson TWFE specification. This approach captures both intensive and extensive margin responses in a single estimate. We provide estimates for two outcome variables: total and gross labor income. Estimates depicted in red correspond to reduced-form effects of the 2012 tax reform on income reported to the PLIT base, while those depicted in blue refer to total income. Because total income is mostly comprised of labor income, their dynamics closely resemble to each other, reflecting the negative effect of the 2012 tax reform on reported gross labor income. However, what is interesting here is the gap between the two effects. As expected, given the income shifting patterns documented throughout the paper, changes in total income are about 25% smaller than changes in labor income.

Panels (b) and (c) aim to better disentangle whether changes in total income are driven by intensive or extensive margin responses. In panel (b), we replicate our baseline specification for intensive margin responses but use the log change in total income as the outcome variable. In panel (c), we replicate our baseline extensive margin specification but using changes in the decision to report income to any of the three tax bases (labor, capital, or corporate). Point estimates as well as standard errors for all panels in Figure G.1 are reported in columns (1) through (4) in Table G.1. For instance, column (3) shows a 2012 reduced-form effect of about 11% ($p-value < 0.001$) on log total income, while column (4) shows no effect on the extensive margin (estimate = -0.004, $p-value$= 0.387). Taken together, the patterns shown in the figure suggest that, at least in the short run, most of the response is concentrated on the intensive margin, with little evidence of large-scale exits from under the DGI radar. That said, by the end of the period we do observe a small response on the extensive margin (e.g., for 2015 the estimated effect is -0.01.p.p., $p-value$-0.018), corresponding to less than 150 TIEs.

These patterns of response in total income suggest that, at least in the short run, full shifters do not consider other tax bases as substitutes for PLIT. Hence, it is important to understand better what drives the documented responses in the intensive margin responses in total income. As discussed in our model, changes in PLIT reporting can result from income shifting, real labor supply adjustments, or tax evasion. We have already documented in detail how income shifting operates in our sample. In what follows, we provide exploratory evidence on real labor supply responses along the intensive margin.

Figure G.2 presents additional evidence aimed at exploring potential responses along the real labor supply margin. As discussed in our theoretical framework, our model and empirical strategy do not distinguish between changes in real labor supply and changes due to evasion or avoidance. For welfare purposes, what matters is reported income, regardless of whether observed changes reflect actual labor supply decisions or shifting, avoidance, or evasion. However, because these mechanisms are conceptually different, it is still interesting to provide some discussion on the possible mechanisms behind the observed changes in reported PLIT income. Hence, panel (a) uses data from the SSA to examine responses in reported hours worked. Our measure of hours is simply the sum of monthly hours reported by each firm in which an individual was employed over the course of the year.[40] To estimate intensive margin effects on hours, we replicate our baseline specification using the SSA sample and SSA hours as the outcome variable. The figure provides some suggestive evidence of behavioral responses along this real labor supply margin. For example, column (5) in Table G.1 shows a 2% decline ($p - value = 0.004$) in reported hours worked among treated TIEs relative to the control group.

A common concern when working with data on reported hours worked is the quality of the information. In this regard, it is important to note that the information on reported hours we use come from SSA records, which are reported by employers and subject to periodic controls by the SSA. Firms found to be reporting inaccurate information face penalties and fines. As a result, unless they are actively engaged in evasion or avoidance practices, firms have a clear monetary incentive to report hours accurately.[41] Another potential concern is that hours worked may not be a meaningful margin of adjustment for top-income earners. However, in the case of Uruguay, a significant share of the top 1% of earners are liberal professionals and workers in the health sector as described in Section A and already documented in Burdín et al. (2022). For these type of workers, hours worked can indeed be a relevant margin of response. Finally, it is possible that some of the observed changes in reported hours reflect collusive arrangements between employers and employees. Given Uruguay's labor market regulations that do not allow reductions in workers' compensation without changes in hours worked, collusive employer-employee agreements should mechanically translate into changes in reported hours.

[40] The raw data on hours worked displays a multimodal distribution, with spikes at 20, 25, 40, 44, 48, and 60 hours. However, there are some other more extreme values. For this reason, we winsorize monthly hours for each job at the 84 hours.

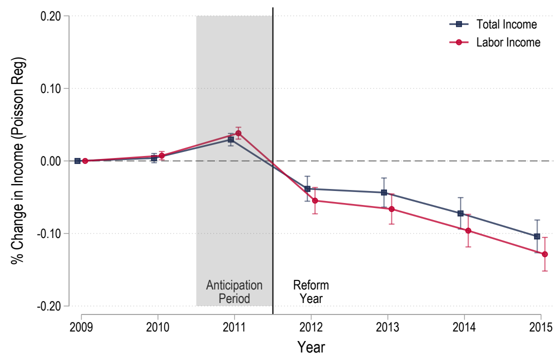
[41] This was confirmed through informal interviews with accountants at various organizations, which helped us understand how labor histories are constructed and how firms report each variable.

Given these concerns, we complement the analysis of reported hours worked with additional estimates using an alternative outcome variable that may also capture real labor supply responses but is presumably more reliable: the number of different employers or sources of third-party reported income in the SSA records. While this variable is still only a proxy for total income sources, changes in it can reflect behavioral responses, such as physicians taking shifts at fewer hospitals or consultants working with fewer clients. Even if hours worked are not a relevant margin of response for these TIEs due to flexible work arrangements, a reduction in the number of employers would still signal a lower overall work commitment. Furthermore, responses along this margin are less likely to reflect collusive employer-employee arrangements as they would require full under-the-table payments. Panel (b) in Figure G.2 presents evidence consistent with this interpretation. As shown in column (6) in Table G.1, the number of employers in 2012 declined by 0.017 more among treated TIEs compared to the control group ($p - value = 0.053$). For reference, the average number of employers among control group TIEs in 2011 was 1.42. This effect appears not only in the short run but also continues to accumulate over time, further supporting the idea of a real labor supply adjustment in response to the reform.

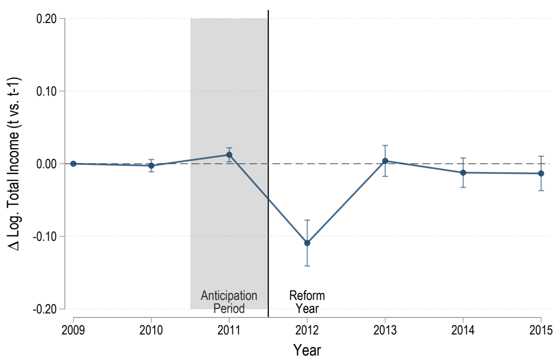
Finally, an alternative margin of response that could capture real labor supply responses is anticipated retirement, which might be relevant for TIEs close to retirement age. Figure G.3 shows estimates for extensive margin responses using pension tax records. As reported in column (7) of Table G.1, we find no clear signs of behavioral responses in this margin, except for the last year where there seems to be a very modest increase in the share of TIEs in the treatment group who are reporting income to the pension tax relative to TIEs in the control group (estimate = 0.7p.p., $p - value$=0.087). While still suggestive, this may be the counterpart of the negative extensive margin responses observed for total income that also show up in the last years of the period of analysis, although this mechanism remains speculative.

Figure G.1: Changes in Total Income

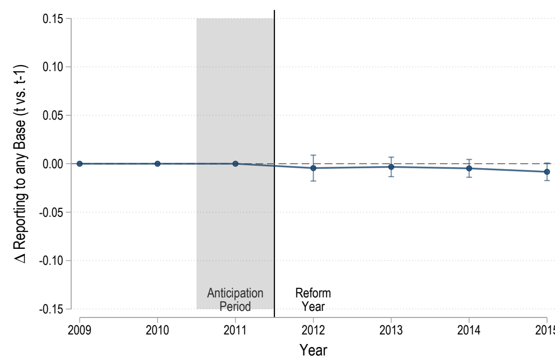
a. Total Income (Poisson Regression)




c. Total Income: Intensive Margin

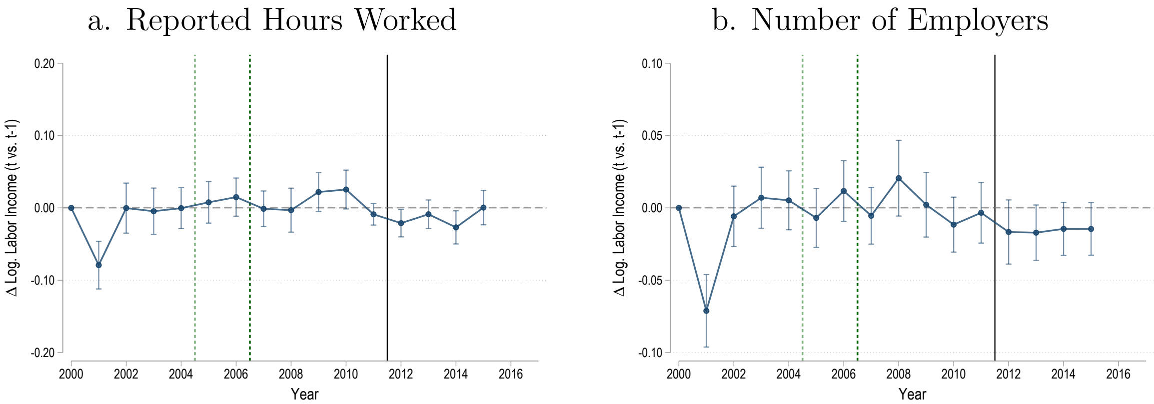



b. Total Income: Extensive Margin




Notes: This table reports additional estimates about total income responses. Total income is defined as the sum of income reported to the PLIT, corporate, and capital income tax bases. Panel (a) reports estimates from a Poisson TWFE regression that uses total income (depicted in blue) and labor income (depicted in red) as the dependent variables. Both outcomes are winsorized at the 95th percentile. This specification is similar to Equation (16) using log *total* income as the outcome variable. Since outcomes are no longer first-differenced, these specifications include individual-level fixed effects and use 2009 as the reference year. Accordingly, we use 2009 total income and weights. Because the poisson specification allow us to include 0s in the outcome variables, estimates reported in the figure are based on the full analysis sample, and combine intensive and extensive margin responses. Panels (b) and (c) report dynamic DiD estimates of the reduced-form effects on total income, following our baseline specification for all other tax bases. Panels (a) focuses on intensive margin responses and reports dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change in *total* income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. Panel (b) focuses on extensive margin responses and reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^T_{i,t} > 0)$, with $\mathbb{1}(y^T_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT, corporate, or capital income tax base. As such, $\Delta\mathbb{1}(y^T_{i,t} > 0)$ takes values -1, 0, or 1. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area corresponds to 2011, the anticipation period. All figures are based on *TAX* records. 99% confidence intervals are based on standard errors clustered at the individual level. Estimates are weighted by total income. Full estimates, standard errors, and sample sizes are reported in columns (3) and (4) of Table G.1.

Figure G.2: Real Labor Supply Responses

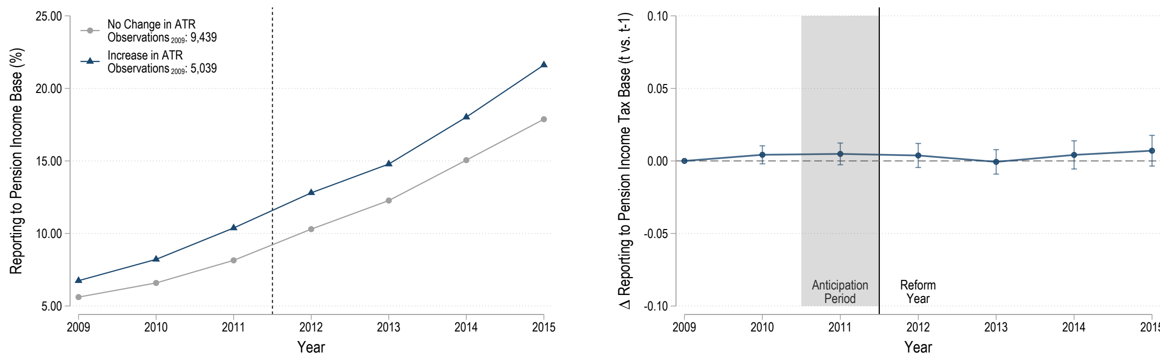



Notes: This figure illustrates the evolution of hours worked and number of employers reported to the SSA for the period 2000-2015. Three things are worth noting. Two things are worth noting. First, SSA data can be matched to 75% of TIEs in our analysis sample based on TAX records. Hence, data in this figure correspond to the *SSA* sample. Second, due to changes in how SSA data were recorded, 1,212 TIEs in our sample entered the SSA records in bulk in March 2008 and January 2010. For these individuals, we set all variables to missing for the year they entered the SSA records and for all prior years. See Section 4 and Appendix C for further details. Panels (a) focus on hours worked measured as the sum of monthly hours reported by each firm in which an individual was employed over the course of the year. We winsorize monthly hours for each job at 84 hours. To estimate intensive margin effects on hours, we replicate our baseline specification using the SSA sample and SSA hours as the outcome variable. Hence, estimates depicted in panal (a) correspond to dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change between $t-1$ and $t$ in total hours worked. As described in Section 5, these correspond to income-weighted estimates. As in other estimates using *SSA* data, we weight the estimates using average pre-treatment income weights derived from the *TAX* records. Panel (b) reports analogous estimates but using the change in the number of employers or sources of third-party reported income in the SSA records as the outcome variable. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical black line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. In addition, green vertical lines indicate when the introduction of PLIT was announced and when it was enacted. Additional estimates, standard errors, and sample sizes are reported in columns (5) and (6) of Table G.1.

Figure G.3: Cross-Base Responses: Reporting to Pension Income Tax

a. Raw Trends

b. DiD: $\Delta \mathbb{1}(Y_t > 0)$




Notes: This figure illustrates the reduced-form effects of the 2012 tax reform on changes in reporting behavior to the pension income tax. Panel (a) depicts the raw evolution of the share of TIEs in our analysis sample who report income to the pension tax base, with the number of observations in 2009 shown in the upper left corner. Estimates in blue correspond to TIEs exposed to changes in the *effective* tax rate (i.e., $treat^{ATR} = 1$), and estimates in gray correspond to TIEs not exposed to such changes (i.e., $treat^{ATR} = 0$). Panel (b) reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta \mathbb{1}(y^p_{i,t} > 0)$, with $\mathbb{1}(y^p_{i,t} > 0)$ indicating whether a taxpayer reports any income to the pension tax base. As such, $\Delta \mathbb{1}(y^p_{i,t} > 0)$ takes values -1, 0, or 1. As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. 99% confidence intervals are based on standard errors clustered at the individual level. In all panels, the vertical line marks the midpoint between 2011 and 2012, the year in which the reform was enacted. The gray shaded area corresponds to 2011, the anticipation period. All figures are based on pension tax records. Additional estimates, standard errors, and sample sizes are reported in column (7) of Table G.1

Table G.1: Other Margins of Response: Dynamic DiD Estimates

| | | Total Income | | | Real Labor Supply | | |
|---|---|---|---|---|---|---|---|
| | $y^l_{i,t}$ (1) | $y^T_{i,t}$ (2) | $\Delta$ $log$ $y^T_{i,t}$ (3) | $\Delta\mathbb{1}(y^T_{i,t} > 0)$ (4) | $\Delta$ $log$ $hs^T_{i,t}$ (5) | $\Delta$ $\#emp_{i,t}$ (6) | $\Delta\mathbb{1}(y^P_{i,t} > 0)$ (7) |
| **a. Pre Reform Years** | | | | | | | |
| Any Treat × 2010 | 0.006* | 0.004 | -0.003 | 0.000 | 0.025** | -0.012 | 0.004* |
| | (0.003) | (0.003) | (0.003) | (0.000) | (0.010) | (0.007) | (0.002) |
| Any Treat × 2011 | 0.037*** | 0.029*** | 0.012*** | 0.000 | -0.009 | -0.003 | 0.005* |
| | (0.005) | (0.004) | (0.004) | (0.000) | (0.006) | (0.008) | (0.003) |
| **b. Post Reform Years** | | | | | | | |
| Any Treat × 2012 | -0.057*** | -0.038*** | -0.109*** | -0.004 | -0.021*** | -0.017* | 0.004 |
| | (0.010) | (0.009) | (0.012) | (0.005) | (0.007) | (0.009) | (0.003) |
| Any Treat × 2013 | -0.066*** | -0.044*** | 0.004 | -0.003 | -0.009 | -0.017** | -0.001 |
| | (0.011) | (0.010) | (0.008) | (0.004) | (0.008) | (0.007) | (0.003) |
| Any Treat × 2014 | -0.096*** | -0.072*** | -0.012 | -0.005 | -0.027*** | -0.014** | 0.004 |
| | (0.012) | (0.011) | (0.008) | (0.004) | (0.009) | (0.007) | (0.004) |
| Any Treat × 2015 | -0.128*** | -0.104*** | -0.013 | -0.008** | 0.000 | -0.015** | 0.007* |
| | (0.012) | (0.011) | (0.009) | (0.004) | (0.009) | (0.007) | (0.004) |
| Observations | 101,346 | 101,346 | 79,416 | 82,342 | 117,719 | 128,712 | 80,157 |
| Unique individuals | 14,478 | 14,478 | 14,478 | 14,478 | 10,138 | 10,206 | 14,478 |
| Weights: | Lab. Inc. | Tot. Inc. | Tot. Inc. | Tot. Inc. | Lab. Inc. | Lab. Inc. | PIT Rev. |

Notes: This table reports year-by-year dynamic DiD reduced-form estimates for both intensive and extensive margin outcomes in the corporate income tax base. Panel (a) includes estimates for pre-reform years (2010-2011), and Panel (b) includes estimates for post-reform years (2012-2015). Columns (1) through (4) focus on total income responses, while columns (5) through (7) focus on potential labor supply responses.Columns (1) and (2) report estimates from a Poisson TWFE regression that uses labor and total income, respectively, as the dependent variables. Both outcomes are winsorized at the 95th percentile. This specification is similar to Equation (16) using log *total* income as the outcome variable. Since the outcomes are expressed in levels and are no longer first-differenced, estimates include individual-level fixed effects and use 2009 as the reference year. Accordingly, weighted estimates use 2009 total income and weights. Columns (3) and (4) report dynamic DiD estimates of the reduced-form effects on total income, following our baseline specification for all other tax bases. Column (3) focuses on intensive margin responses and reports dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change in *total* income between $t-1$ and $t$, winsorized at the 1st and 99th percentiles. Column (4) focuses on extensive margin responses and reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^T_{i,t} > 0)$, with $\mathbb{1}(y^T_{i,t} > 0)$ indicating whether a taxpayer reports any income to the PLIT, corporate, or capital income tax base. As such, $\Delta\mathbb{1}(y^T_{i,t} > 0)$ takes values -1, 0, or 1. Colums (5) through (7) report additional estimates for labor-supply related variables. Column (5) focuses on hours worked measured as the sum of monthly hours reported by each firm in which an individual was employed over the course of the year. We winsorize monthly hours for each job at 84 hours. Estimates reported in this column correspond to dynamic DiD coefficients based on Equation (16), where the outcome variable is the log change between $t-1$ and $t$ in total hours worked. As described in Section 5, these correspond to income-weighted estimates. Panel Column (6) reports analogous estimates but using the change in the number of employers or sources of third-party reported income in the SSA records as the outcome variable. As in other estimates using *SSA* data, we weight the estimates using average pre-treatment income weights derived from the *TAX* records. Finally, Column (7) focus on reporting behavior to the pension income tax base. It reports dynamic DiD coefficients based on Equation (17), where the outcome is $\Delta\mathbb{1}(y^p_{i,t} > 0)$, with $\mathbb{1}(y^p_{i,t} > 0)$ indicating whether a taxpayer reports any income to the pension tax base. As such, $\Delta\mathbb{1}(y^p_{i,t} > 0)$ takes values -1, 0, or 1. As described in Section 5, these are revenue-weighted estimates. Weights are explained in detail in Appendix C. All standard errors are clustered at the individual level and reported in parentheses. Statistical significance is indicated by asterisks: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates in columns (1) through (4) are based on *TAX* records, estimates in columns (5) and (6) are based on *SSA* records, and estimates in column (7) are based on pension tax records.

# H Further Details on Inequality Analysis

In this appendix, we describe the specific details behind inequality simulation presented in Section 7. The simulation proceeds in five steps. First, we estimate aggregate 2012 post-reform labor, corporate, and personal capital income by applying the reduced-form estimates reported in Table G.1 to 2011 (pre-reform) aggregate income. To do this, we consider three alternative scenarios. In the first scenario, we assume that all of the reduction in labor income that does not shift to capital or corporate income, which represents 67% of the total decrease in labor income (i.e., $\frac{3.8\%}{5.7\%}$), corresponds to real labor supply responses. In the second scenario, we attribute this to tax evasion instead. This implies that the aggregate labor income, excluding the amount shifted to other tax bases, remains the same, as none of that response correspond to actual changes in labor supply. In the final scenario, we split the total 5.7% reduction in labor income following our back-of-the-envelope calculations discussed in Section 6.3: 33% is attributed to income shifting, 36% to reductions in hours worked, and the remaining 31% to income that cannot be accounted for in any of the tax bases in our data. This residual response is conservatively attributed to other forms of tax evasion or avoidance. Regardless of the specific mechanism, the key assumption is that this residual response remains untaxed.

Second, once we estimate the counterfactual labor, capital and corporate income aggregates, we estimate the probability of individuals responding in the intensive margin or fully shifting to corporate or personal capital income. To do this, we use their observed behavior in 2012 and 2013. In particular, we use a simple probit model, with labor income and individual characteristics as independent variables.

In the third step, we use these probabilities to assign individuals to each margin of response until the aggregate reported incomes match the targets derived in Step 1. In the third scenario, we also need to distinguish between TIEs who respond along the real labor supply margin and those whose do it in the residual (or tax evasion) margin. For this, we proceed by assigning non-shifters to each group, such that individuals with higher income are more likely to be classified as responding in the real labor supply margin.

Fourth, for each individual in 2011, we compute a counterfactual reported income (labor, corporate, and capital), yielding both actual and counterfactual income distributions. We use these counterfactual individual-level income vectors to simulate total tax liabilities under each scenario.

Finally, we merge our dataset with the adult population data from Burdín et al. (2022), which includes informal incomes, to estimate top income shares before and after the reform. Burdín et al. (2022) combine the same administrative records used in this study with household survey data to capture the entire adult population, including informal income earners and individuals with zero income. By merging our simulated incomes with their dataset, we extend the inequality analysis beyond the top of the distribution, the primary focus of the reform, to the full income distribution. This allows us to compute Gini indices and top income shares for the entire adult population.

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