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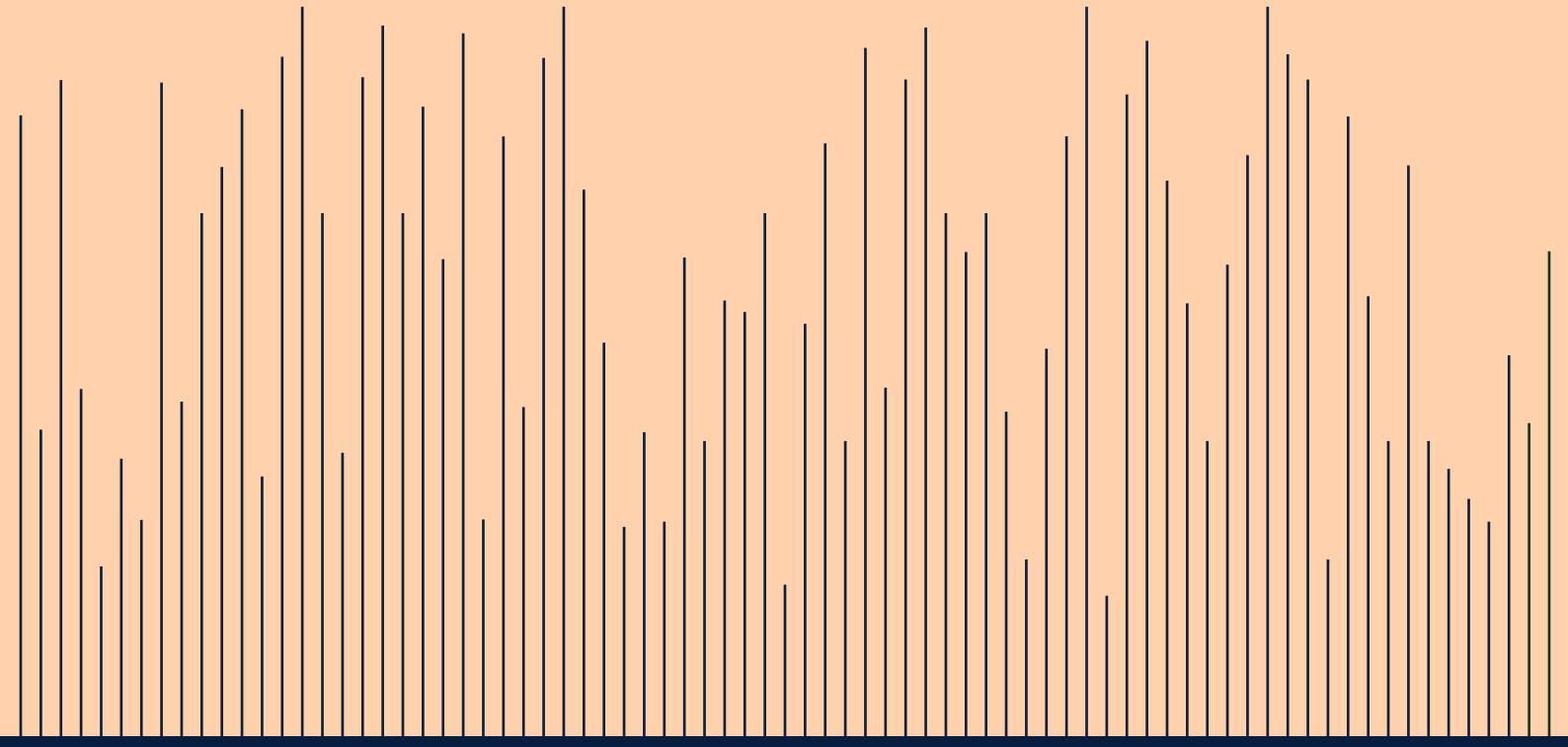

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The Transparency Tax: Discouraging Firm Ownership via Tax Havens

Jakob Brounstein ⓡ Pierre Bachas ⓡ Alex Bajaña *

Abstract

Tax havens weaken tax enforcement by obscuring firm ownership and facilitating offshore evasion. This paper studies a reform in Ecuador that imposed a corporate tax surcharge on firms with owners in tax havens, using a newly available ownership registry. Comparing haven-owned firms to other foreign-owned firms, the analysis finds that the reform reduced haven ownership by 12 percentage points, largely through disclosure of non-haven individual owners. Exposed firms reduced transactions with tax havens and increased corporate tax payments, with no detectable effects on investment or firm size. The results suggest that ownership transparency can meaningfully improve tax compliance at limited efficiency cost, even through unilateral action.

JEL codes: H26, H25, H32, F38

Key words: Tax havens, Beneficial Ownership Registry, Corporate Income Taxation

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1 Introduction

Tax havens weaken tax enforcement by obscuring firm ownership and facilitating offshore tax evasion and avoidance (Alstadsæter, Johannesen and Zucman, 2019; Tørsløv, Wier and Zucman, 2022). A central challenge for governments is identifying the individuals behind offshore entities and layered ownership structures (Laffitte, 2024). In response, many countries have established beneficial ownership registries (BORs), which aim to record the natural persons with ultimate control (Figure A.1). Yet, ownership reporting often remains incomplete, especially when ownership chains involve non-cooperative jurisdictions. As a result, BORs are viewed as transparency tools with limited direct enforcement consequences.

This paper studies whether governments can use ownership registries to attach fiscal costs to opaque offshore ownership structures. We analyze a reform in Ecuador that imposed a corporate income tax surcharge on firms whose terminal owners were located in tax havens. We provide causal evidence on how firms respond to such incentives. Comparing firms with pre-reform tax haven exposure to other foreign-owned firms, we show that the reform reduced terminal haven ownership by 12 percentage points (20 percent relative to the counterfactual), primarily through increased disclosure of foreign individual owners outside tax havens. Exposed firms also reduced financial ties to havens and increased reported corporate tax payments, with no detectable decline in investment or firm scale. The results suggest that governments can leverage ownership registries not only to collect information, but also to influence offshore ownership structures through targeted fiscal incentives, even in settings with imperfect enforcement and incomplete international coordination.

Ecuador provides a unique setting because it combined a beneficial ownership registry with fiscal costs tied to offshore ownership. Established in 2012, the registry aimed to identify firms' ultimate owners, but reporting remained incomplete, especially when ownership chains involved tax havens. The 2015 surcharge increased the cost of haven-based ownership by taxing firms with terminal owners in tax havens. Firms could avoid the surcharge by disclosing non-haven owners or restructuring away from havens. The reform therefore had the potential to expand the corporate tax base through reduced evasion and, where beneficial

owners became identifiable, strengthen personal taxation of dividend income.

To guide the empirical analysis, we adapt the canonical Allingham and Sandmo (1972) tax evasion model to incorporate fixed costs of tax haven use. Firms choose whether to maintain haven ownership and how much income to conceal, trading off tax savings against detection risk. The reform raises both the pecuniary cost of haven ownership and the salience of enforcement for firms linked to tax havens. The model yields three testable predictions. First, some firms exit haven ownership by disclosing non-haven beneficial owners or restructuring away from havens. Second, tax revenues increase mechanically through the surcharge and behaviorally through reduced evasion, particularly among firms exiting haven structures. Third, if offshore ownership serves productive purposes, the reform may increase financing costs and reduce firm activity.

We test these predictions using an exposure design that compares firms with pre-reform tax haven ownership to other foreign-owned firms not subject to the surcharge. The data consist of a 2012–2019 firm-level panel linking ownership registry records, corporate tax returns, and cross-border transactions. Firms are classified as domestic, foreign non-haven, or haven-owned based on the country of their reported terminal owner—the last observable ownership link. Tax havens follow Ecuador's official classification, with most exposed firms linked to Panama. Reporting of beneficial owners (natural persons), rather than corporate intermediaries, is high among domestic firms but low among haven-owned firms (43 percent at baseline), highlighting the offshore opacity targeted by the reform.

Our first set of results examines ownership reporting. Despite imperfect implementation—with tax rates rising by 1.5–2 points rather than the full 3-points—the reform reduced terminal tax haven ownership among exposed firms by 12 percentage points over the following five years, corresponding to a 20 percent decline relative to the estimated counterfactual post-reform level. The response occurs primarily along the extensive margin with firms exiting haven ownership, and most adjustment occurs upon impact in 2015. Newly reported terminal owners are predominantly individuals rather than corporations, and foreign non-haven residents rather than Ecuadorians. The reform thus increased offshore ownership

transparency, prompting firms to disclose natural-person owners in cooperating jurisdictions. Responses are stronger among larger firms and those with more opaque baseline ownership, and weaker among firms that frequently transact with tax havens. Results account for mean reversion and remain robust to alternative exposure definitions, and controls for the 2016 Panama Paper leaks.

We find evidence that changes in terminal ownership reflect genuine disengagement from tax havens rather than just relabeling. Consistent with this interpretation, transaction data show that exposed firms, initially with haven outflows more than twice those of controls, reduce dividend payments to haven entities after the reform. Moreover, the probability that a tax haven entity appears anywhere in the ownership chain falls sharply, with similar patterns observed in independent Orbis data.

Our second set of results examines corporate tax payments and reported activity. CIT liabilities of exposed firms increase by 17 percent relative to controls. This reflects both the mechanical effect of the higher tax rate on firms retaining haven ownership and a behavioral response, as exposed firms report higher taxable profits. Firms exiting haven ownership exhibit particularly large increases in reported profits, which we conjecture reflects the loss of tax evasion technology. While return on assets was about 40 percent lower for exposed firms at baseline, the reform closes roughly half of this gap.

Taxing and restricting tax haven use may reduce real activity if offshore structures facilitate investment (Suárez Serrato 2019). We find little evidence of such effects in Ecuador. The reform does not affect firms' probability of remaining active, scale, or investment. We can reject negative effects larger than 3 percentage points on the probability of paying zero CIT and any negative effects on investment. We also find null effects on payroll, though less precisely estimated. The results are more consistent with tax havens facilitating tax avoidance rather than productive activity, with much of the incidence borne by firm owners.

Overall, the reform increased ownership transparency and tax revenue with limited detectable efficiency costs. We estimate that Ecuador's CIT revenue rose by 2–3 percent. Personal income tax gains in Ecuador were likely limited, since newly disclosed owners were for-

eigners. However, the reform may have generated positive fiscal externalities abroad by helping partner countries identify their taxpayers through information exchange with Ecuador. At the same time, most incumbent haven-owned firms remained opaque and haven-owned, highlighting the limits of the policy's scope and enforcement. The final section therefore considers alternative and more ambitious pro-transparency policies, including broader targeting of opaque ownership structures and alternative tax bases.

Ecuador's unilateral policy illustrates how countries can use BORs not only for transparency, but also to attach fiscal costs to opaque offshore ownership. While high-income countries rely on instruments such as controlled foreign company (CFC) rules to curb tax erosion by their own multinationals (Clifford, 2019), many countries lack comparable tools despite substantial exposure to offshore tax avoidance. As BORs become widespread, policies tied to haven-based ownership structures may provide countries with a feasible unilateral mechanism to discourage offshore opacity, particularly where multilateral coordination and information exchange remain incomplete. By increasing the cost of opaque offshore ownership, such policies may also reinforce international information exchange and cooperation, illustrated by Panama's 2025 information-exchange agreement with Ecuador (CIAT, 2025).

The paper is organized as follows. Section 2 introduces the institutional context and data. Section 3 develops a simple model that provides intuition and empirical predictions. Section 4 presents the empirical strategy. Section 5 estimates the effects of the reform on ownership and transparency, while Section 6 examines tax payments and firm activity. Section 7 discusses alternative policy designs and concludes.

1.1 Contributions to the Literature

We make two main contributions. First, we provide causal evidence on the effectiveness of a unilateral, incentive-based policy to curb tax haven use, complementing a literature focused largely on multilateral tools. Second, we contribute to the limited economic literature on corporate ownership transparency by showing how ownership registries, when paired with fiscal incentives, can improve tax compliance and raise revenue.

A growing literature studies multilateral efforts aimed at curbing offshore tax evasion, including information exchanges, reporting requirements, and anti-haven rules (Casi, Spengel and Stage, 2020; Menkhoff and Miethe, 2019). Their effectiveness depends on governments' enforcement capacity and ability to use new information (Alstadsæter et al., 2023*a*; Fejerskov Boas et al., 2024). International coordination is often slow, consensus-driven, and diluted by exemptions, potentially limiting its effectiveness (Bomare and Collin, 2025; Alstadsaeter et al., 2023*b*). We complement this work by studying instead a unilateral policy.

A smaller literature studies unilateral policies aimed at curbing offshore tax avoidance and opacity, primarily through reporting mandates and transaction-based enforcement (e.g., Wier, 2020; Liu, Schmidt-Eisenlohr and Guo, 2020). While these measures can reduce haven use, they may also impose substantial compliance costs and dampen economic activity (Suárez Serrato, 2019; Bustos et al., 2022). We study instead a corporate policy that attaches fiscal costs to haven-based ownership structures using a beneficial ownership registry (BOR).[1] More broadly, the paper relates to the emerging literature on ownership transparency and BORs.[2] Existing work shows that opaque ownership structures facilitate tax evasion, money laundering, and governance distortions, while transparency reforms affect outcomes such as public procurement, investment, and corporate governance (Bennedsen and Zeume, 2017; Seregni, 2024; Amberger, Wilde and Wu, 2025). Our results show that countries can use BORs not only to collect information, but also to reshape offshore ownership structures and improve tax compliance through fiscal incentives, consistent with evidence that even moderate frictions can substantially alter profit shifting (Ferrari et al., 2024).

Finally, we contribute to the literature on tax haven use, which shows that it is concentrated among the wealthy and that flows with havens follow systematic patterns (Londoño Vélez and Ávila Mahecha, 2024; Rose and Spiegel, 2007; Laffitte, 2024). Using administrative data, we document firms' ownership structures and haven linked transactions in Ecuador.

[1] Relatedly, Brounstein (2025) shows that taxing transfers reduced haven use among wealthy taxpayers.
[2] One strand studies real estate, showing that opacity facilitates tax evasion and money laundering, and that transparency reforms are often circumvented (Alstadsæter et al., 2022; Bomare and Le Guern Herry, 2022; Johannesen, Miethe and Weishaar, 2022; Collin, Hollenbach and Szakonyi, 2022).

2 Data, Context, and Sample Construction

Ecuador is an upper-middle income country of 18 million inhabitants, which has been dollarized since 2001. We collaborated with the Internal Revenue Service (SRI) to combine three primary data sources: the Annex of Participants, Shareholders, Administrators, and Board Members (APS), the corporate income tax (CIT), and the International Currency Movement Annex (MID). We describe each dataset, the context, and sample construction.

2.1 Data Sources

Annex of Participants, Shareholders, Administrators, and Board Members (APS). In 2012, Ecuador became one of the first countries to establish a beneficial ownership registry. The APS aims to record each year the full ownership structure of Ecuadorian firms up to their ultimate individual owner. When filed as intended, it provides annual firm–shareholder linkages, including direct and indirect ownership shares, the tier (number of ownership layers), and the domicile country of every shareholder.

The tax administration maintains the APS by requiring all firms to hire a government-approved auditor who, each April, must report the firm's shareholders as of December 31 of the previous year.[3] Firms must disclose the full ownership chain—identifying every person and entity up to the ultimate beneficial owner—even when intermediaries are foreign. The administration verifies beneficial owners using an Ecuadorian ID or an international passport.

APS compliance with ownership reporting is high, likely because firms that fail to report 100% of their direct owners face a 3 percentage point CIT surcharge—a sanction in place since the APS was introduced in 2012. However, this penalty applies only to incomplete ownership reporting, not to incomplete beneficial ownership reporting. As long as a firm reports 100% terminal ownership, it is considered compliant and avoids the sanction. This gap reflects a practical constraint: the administration recognizes that Ecuadorian subsidiaries may not be able to obtain information on their ultimate individual owners. In ownership

[3]The APS also requires within-year reporting of changes in shareholders, partners, board members, or administrators to prevent temporary reshuffling that conceals true ownership.

chains with multiple foreign layers, each entity would need to transmit its own shareholder information down the chain, which may be beyond the control of the Ecuadorian firm.

Corporate income tax declarations (Form F101). We combine the ownership registry with the annual corporate income tax declarations, over the period 2012 to 2019. Ecuador's tax structure only distinguishes between self-employed/single-person firms versus all other corporate entities. The CIT applies to the latter. By definition, the registry only reports information on the approximately 70,000 formal firms filing CIT.[4] The CIT contains the firm's full balance sheet information in addition to tax related variables.

Annex of International Currency Movements (MID). The MID records all cross-border transactions involving Ecuador from 2012 to 2019. Ecuador introduced the system in 2008 to administer its foreign transaction tax. The data are reported at the transaction level and include the amount, date, direction of the flow (into or out of Ecuador), the identity of the Ecuadorian party, and coarse transaction categories (e.g., dividends, imports, credit-card payments). Because the tax applies only to currency exits, outflows are considered more reliably captured than inflows. We therefore focus on outflows in our main analysis.

We express all monetary values in units real 2014 USD. All monetary variables from the F101 and the MID are winsorized above the 99% percentile; all share variables (e.g. tax haven outflows share of revenue) are winsorized above 1.

2.2 Beneficial versus Terminal Ownership

We distinguish between two ownership concepts used in the paper. *Beneficial ownership* refers to the ultimate natural person who ultimately owns or controls a firm (it can also include not-for-profit and public entities; these non-person beneficial owners are rarely observed in our data). When a person-owner is recorded, we cannot tell whether they are

[4]Informality is at roughly 30 percent of GDP and two-thirds of jobs are outside the formal sector (Elgin et al., 2021; Canelas, 2019). Informal firms are absent from our analysis, but prior studies have shown that tax haven usage is concentrated among high-earners and firms with access to formal banking (Brounstein (2025); Londoño Vélez and Ávila Mahecha (2024); Alstadsæter, Johannesen and Zucman (2019)).

the actual beneficial owner or a nominee acting on someone else's behalf. But, once the tax authority identifies a nominee owner it can at least start litigation. As a result, we use *beneficial owner* to refer to a person-owner of a business. We contrast this with the concept of *terminal owner*, which corresponds to the last reported owner of a firm beyond which no further ownership linkages are reported. This distinction is needed, since in the absence of full compliance with beneficial ownership reporting standards, some *terminal* owners are persons (thus *beneficial owners*) and others are firms (thus *not* beneficial owners).

All APS ownership links fall into three categories: (1) a firm—domestic or foreign—that reports its own owners, allowing us to trace the chain further; (2) a firm that does not report its ownership to the Ecuadorian tax authority; (3) an individual. Cases (2) and (3) are terminal links. For each terminal link, the authority multiplies all ownership shares along the chain back to the Ecuadorian firm. The tax haven terminal ownership is then the sum of these indirect ownership shares for all terminal owners located in havens.

2.3 The Corporate Tax Surcharge on Tax Haven Ownership

The 2015 CIT surcharge. Ecuador taxes corporate income on a territorial basis, at a flat 22% rate between 2012-2017, raised to 25% in 2018.[5] Using the ownership registry (APS), the tax authority built a measure of terminal ownership share by domicile, based on the tax residence of the terminal owners. In June 2015—three years after the APS was introduced—it announced a 3 percentage point CIT surcharge on firms with terminal owners in tax havens, based on APS filings as of December 31, 2015.

The reform imposes a 3 percentage point CIT surcharge on firms with majority terminal ownership in tax havens (50% or more). Firms with minority terminal haven ownership face lower surcharge rates that increase proportionally with the haven ownership share: the surcharge rises linearly from zero to 1.5 percentage points for firms with just under 50 percent terminal haven ownership (i.e., 0.03 percentage points of additional CIT per percentage

[5]The CIT rate is higher for extractive industries, while some preferred industries see a reduction in their statutory rate. The CIT schedule also gives a 10 percentage point reduction for re-investment out of profit. Additionally, Ecuador taxes personal income on a worldwide basis with a top marginal rate of 35%.

point of haven ownership; see Figure A.2). At the 50 percent threshold, the surcharge jumps discontinuously to 3 percentage points and remains constant thereafter.

The reform therefore increased the cost of maintaining ownership structures linked to non-cooperative jurisdictions, while allowing firms to avoid the surcharge by restructuring ownership chains or disclosing terminal owners outside tax havens.

Tax haven classification. The tax administration classifies a country as a tax haven if at least two of three criteria hold: (1) A lack of cooperation on tax matters and no exchange of ownership information; (2) a personal or corporate tax rate below 60% of Ecuador's (i.e., 13% or less for the CIT); and (3) attracting activity primarily to appropriate other countries' tax bases. The first criterion—non-cooperation and opacity—is central to the 2015 reform, meaning tax havens are partly defined as non-transparent jurisdictions. In practice, the resulting list is stable over our study period and closely aligns with tax haven lists used in the literature (e.g. Hines and Rice (1994), Tørsløv, Wier and Zucman (2022)).

2.4 Sample Construction and Descriptive Statistics

Sample Construction. We combine the APS, CIT, and MID data over 2012–2019. We restrict the sample to firms that (i) filed a CIT return with positive revenue in 2014 and (ii) filed at least one APS declaration between 2012 and 2014, allowing us to assign a baseline ownership profile. The resulting panel consists of firms active in the pre-reform year with attributable ownership links. In total, 62,350 firms satisfy these criteria, accounting for 90% of Ecuadorian CIT payments in 2014.[6]

The panel is well balanced, with APS and F101 filings observed in 88% and 90% of firm-years, respectively. Table A.2 shows that filing rates decline only modestly after 2014: by 2019, 80% of baseline firms still file an APS declaration and 78% an F101 return. We further examine differential attrition and transitions into and out of the sample in Section 5.2.

[6] Table A.1 compares excluded firms to the core sample. Excluded firms are roughly an order of magnitude smaller in assets, revenues, and taxes paid, implying negligible effects on aggregate outcomes.

Descriptive statistics, all firms. Table A.3 reports descriptive statistics as of 2014. The average (median) firm reports annual revenue of USD 1.9 million (140,000). Mean (median) return on assets is 6.2% (4.3%), and 77% of firms report positive profits in 2014. It also reports terminal ownership shares across five categories: Ecuadorian persons, Ecuadorian entities, foreign persons, foreign entities, and a residual category of unaccounted ownership. Ecuadorian persons account for 86% of terminal ownership in the core sample, while foreign persons account for an additional 4%. Thus, natural persons represent about 90% of total reported ownership. This indicates partial non-compliance with the APS's intended beneficial ownership reporting standard, as roughly 10% of ownership remains attributable to entities or is unreported. Moreover, while 97% of firms *nominally* account for 100% of ownership, only 83% report a complete beneficial ownership profile.

Tax haven ownership. Table A.4 reports foreign terminal ownership by country and ownership threshold. Panel (a) lists tax haven jurisdictions. We identify 584 firms majority-owned by a tax haven.[7] Panama dominates, with 377 majority-owned firms, followed by the Netherlands, the Bahamas, the British Virgin Islands, and the Cayman Islands. The right-hand side of the table reports the share of aggregate Ecuadorian corporate assets (and revenue) owned from each jurisdiction. Overall, 4.1% of all assets are haven-owned, including 1.9% through Panama. Natural persons account for 1.6 percentage points of haven-owned assets, or 39% of the total. The remaining 2.5 percentage points cannot be traced back to a natural person.

Panel (b) reports the corresponding statistics for foreign non-havens. We identify 3,397 firms majority-owned from these countries. Colombia and the United States each account for roughly 600 firms, followed by Spain, Peru, and Venezuela. Non-haven countries own 22.9% of corporate assets, of which just over half can be traced to a beneficial owner.

[7] An additional four firms are jointly majority-owned by multiple havens. These firms are excluded from the single-country counts but included in the total of 588 majority haven-owned firms used in the analysis.

3 Conceptual Framework

Offshore ownership structures may facilitate tax evasion and avoidance by obscuring control relationships and increasing enforcement frictions, particularly when ownership chains involve non-cooperative jurisdictions. Even when offshore intermediary entities are reported, incomplete information on ultimate beneficial ownership may make it harder for tax authorities to verify the economic substance of cross-border transactions and identify the individuals ultimately receiving income.

The reform weakens tax havens as a technology for reducing tax liabilities by increasing their direct fixed usage cost and potentially raising the perceived risk of detection. Firms with low benefits from offshore ownership exit haven structures, while remaining users might reduce evasion. These responses generate testable predictions on ownership restructuring, tax payments, and real activity. To organize the empirical analysis, we formalize these mechanisms in a simple extension of the Allingham and Sandmo (1972) tax evasion model. We present a streamlined version here and provide the full model and extensions in Appendix C.1.

Setup An agent with quasi-linear utility earns pre-tax income normalized to one and faces a proportional tax rate τ. She chooses whether to use a tax haven, $E \in \{0, 1\}$, at fixed cost ξ_i, and the share of income $e \in [0, 1]$ to underreport. Underreporting is possible only through haven use, so reported income equals $z = 1 - Ee$. The probability of detection, $\rho(e)$, rises in underreporting. If detected, she must repay evaded taxes and pay a proportional penalty. Haven use generates tax savings but raises expected enforcement costs. We interpret detection broadly as capturing audits, ownership disclosure, and information exchange.

Policy Reform We model the surcharge as increasing: (i) the cost of haven use (ii) the perceived detection probability conditional on haven ownership. This captures both the mechanical tax effect and the increased scrutiny associated with offshore ownership. The model highlights two margins of adjustment: the decision to maintain haven ownership (extensive margin) and underreporting conditional on haven use (intensive margin).

- Extensive margin (haven ownership): Agents use tax havens only if the net benefits exceed the fixed cost:

$$\xi_i \leq \tau e^* - \rho(e^*)e^*\tau(1+\theta).$$

  Policies that increase the cost of haven ownership or the perceived detection risk induce some firms to exit havens.

- Intensive margin (tax reporting): Conditional on haven use, the agent chooses underreporting to equate marginal tax savings with expected marginal penalties:

$$(\rho'(e^*)e^* + \rho(e^*))(1+\theta) = 1.$$

  Higher detection risk or penalties reduce underreporting. The tax rate does not affect evasion along this margin, as tax savings and expected penalties scale with τ.

Testable Predictions. The framework yields three main predictions guiding our analysis of firms' ownership structures, tax payments, and activity in Ecuador.

- **Prediction 1: Ownership Restructuring.** The surcharge increases the fixed cost ξ_i of tax haven ownership and the perceived risk of detection ρ. As a result, some firms should exit haven structures, either by disclosing beneficial owners outside havens or by restructuring ownership chains. Exit should be more likely among firms with lower benefits from haven use or lower costs of ownership disclosure.

- **Prediction 2: Tax Payments.** The reform should increase corporate tax payments through three channels. First, firms exiting haven structures may report higher profits if offshore ownership previously enabled evasion. Second, firms that remain face a mechanical rise in taxes from the surcharge. Third, they may perceive higher detection risk and reduce intensive-margin evasion. As the reform combines a pecuniary cost with greater enforcement salience, their relative importance is an empirical question. To the extent that the reform also induces the disclosure of beneficial owners, it may strengthen the

taxation of dividend income under the personal income tax. If newly disclosed owners are foreign residents, these effects may generate positive fiscal externalities abroad.

- **Prediction 3: Real Effects.** If offshore structures ease financing or organization, or if the tax falls on marginal investment, then real activity (scale, investment, labor) of exposed firms in Ecuador would decline (Curtis et al., 2021; Auerbach, 2002). However, if haven ownership mainly enables tax avoidance and the reform taxes rents, efficiency costs may be limited (Bustos et al., 2022; Yagan, 2015; Chetty and Saez, 2010).

4 Empirical Strategy

Defining exposure and control groups. Our identification strategy exploits differential expected exposure to the reform, based on firms' pre-reform ownership structure. We define exposed firms as those with at least 50% tax haven terminal ownership in 2014. The control group consists of firms with more than 50% foreign non-haven terminal ownership, and tax haven ownership below 5%. Because the surcharge targets tax haven ownership, these firms should not be affected by the reform. We focus on majority foreign-owned firms, which represent the bulk of foreign-owned firms and provide the sharpest treatment.[8]

Table A.5 compares exposure to control firms at baseline, as well as to domestically-owned firms. Our sample of interest consists of 3,939 unique foreign-owned firms that, by construction, are active in 2014. Of these, 588 are haven-owned and 3,351 are foreign non-haven-owned. These internationally owned firms exhibit strong panel balance, with 93% and 90% balance for exposure and control firms, respectively, over 2012–2019.

Panel (a) shows that exposed and control firms are similar in revenue, profits, and taxes paid. Three-quarters of firms in both groups report positive profits. Exposed firms differ in exhibiting 40% lower profit-to-assets ratios and 80% higher exports. Panel (b) also shows that exposed firms have stronger transactional links to havens: haven transactions amount to 4.2% of revenue, versus 1.7% for other foreign-owned firms.

[8]Majority-owned firms account for three quarters of foreign-owned firms. The remaining quarter are minority-owned; for these, we use a continuous exposure measure analyzed in Section 5.4.

Panel (c) reports ownership characteristics. Both groups exhibit high compliance with terminal ownership reporting: 98% of firms fully report their corporate ownership chains. Compliance with beneficial ownership disclosure is substantially lower, however. Exposed firms disclose only 43% of beneficial ownership, compared to 70% among other foreign-owned firms. Exposed firms also exhibit more complex ownership structures: 56% of haven-owned firms include an additional haven intermediary in the ownership chain. Notably, 9% of foreign non-haven-owned firms also include a haven intermediary.[9]

These patterns are consistent with greater scope for tax evasion among haven-owned firms, reflected in lower reported profitability and facilitated by more complex ownership structures, lower beneficial ownership transparency, and stronger haven linkages.

Research Design. Our empirical strategy compares tax haven-owned firms to other foreign-owned firms in a difference-in-differences framework, examining changes in ownership structures and economic activity following the reform:[10]

$$y_{it} = \alpha_i + \delta_t + \sum_{t=2012, \neq 2014}^{2019} \beta_t \cdot Exposure_i \cdot Year_t + \varepsilon_{it}, \tag{1}$$

where y_{it} is the outcome of interest for firm i in year t, α_i and δ_t are firm and year fixed effects, $Exposure_i$ indicates majority tax haven ownership in 2014, and ε_{it} is a mean-zero error term. The equation estimates coefficients $\{\hat{\beta}\}_k$ relative to 2014 values. Standard errors are clustered at the firm level, and we report estimates both unweighted and weighted by firm size, as measured by 2014 assets.

We also estimate a version of Equation (1) coarsened in the time dimension as:

$$y_{it} = \alpha_i + \delta_t + \beta_{post} \cdot Exposure_i \cdot Post_t \cdot \mathbb{1}\{Year_t \geq 2015\} + \varepsilon_{it}, \tag{2}$$

where $\hat{\beta}_{post}$ captures the average effect over 2015-2019 relative to the pre-reform average.

[9] By definition, these intermediaries reveal their ownership in non-havens.

[10] Although the 50% surcharge cutoff suggests an RD or bunching design, there is little mass just below the threshold (Figure A.3), and firms around 50% may differ also for legal reasons (e.g. effective control).

Under a parallel-trends assumption, the coefficients $\hat{\beta}_t$ identify the reduced-form Intent-to-Treat (ITT) effect of the tax haven CIT surcharge on outcome y for exposed firms in year t relative to 2014. The pre-reform coefficients, $\hat{\beta}_{2012}$ and $\hat{\beta}_{2013}$, serve to test for parallel-trends.

The ITT may differ from the Average Treatment Effect on the Treated (ATT), defined as the effect of surcharge exposure among compliers. The ATT corresponds to a 3 percentage point increase in the CIT rate for firms that both retain tax haven ownership and report positive taxable income. In practice, some exposed firms avoid the surcharge by reducing reported haven ownership or taxable profits, and enforcement may be incomplete. While one could instrument changes in effective tax rates with exposure status, the reform captures more than just a tax increase: it also signals heightened scrutiny of haven-linked firms (Section 3). Accordingly, we prefer the reduced-form specification which reflect a combination of mechanical tax effects, increased enforcement risk, and exit from havens.

Mean Reversion in Ownership. For economic outcomes (e.g., profits), identification relies on parallel trends between exposed and control firms. Yet, for ownership outcomes, mean reversion is a concern: by construction, exposed firms have majority haven ownership (>50%), whereas control firms have majority foreign non-haven ownership and negligible haven ownership. Estimating Equation (1) with tax-haven ownership as the outcome may yield biased $\hat{\beta}$: even in the absence of the reform, exposed firms are more likely to reduce haven ownership, whereas control firms have limited scope for reduction.[11]

To address this concern, we examine ownership shares in firms' *baseline domicile*—the country of majority terminal ownership at baseline (tax havens for exposed firms, foreign non-havens for controls). We then compare changes in these shares across groups, where:

$$y_{it} = \begin{cases} Tax\ haven\ ownership_{it},\ i \in Exposure, \\ Foreign\ non\text{-}haven\ ownership_{it},\ i \in Control \end{cases}$$

This strategy requires that mean reversion in *baseline domicile* ownership evolves simil-

[11] This form of mean reversion is common when exposure is defined by a threshold in a running variable that is also analyzed as an outcome, e.g. earnings responses around tax thresholds Kleven et al. (2025).

arly across groups, i.e. that absent the reform, tax-haven ownership among exposed firms would have followed the same trend as foreign non-haven ownership among control firms. We estimate Equation (1) using *baseline domicile* terminal ownership as the dependent variable and interpret $\hat{\beta}_{dd}$ as the effect of the reform on terminal ownership in tax havens. Symmetrically, we define the *inverse domicile* as the opposite foreign ownership category—foreign non-havens for exposed firms and tax havens for control firms.

We further assess the robustness of the design using alternative specifications and exposure definitions. One concern is differential mean reversion due to group size imbalance: fewer than 600 firms are majority haven-owned, compared to roughly 3,000 majority foreign non-haven firms, potentially inducing stronger reversion among exposed firms. Section 5.3 addresses this issue using alternative comparison groups, including specifications comparing similarly sized groups of Panamanian- and Colombian-owned firms.

5 Impact of the Reform on Tax Haven Connections

5.1 Surcharge Implementation

How much did effective CIT rates rise after the reform? Figure A.4 reports estimates from Equation (1) using the effective CIT rate as the outcome. Exposed firms' effective rate increases significantly, but by only about 1 percentage point relative to control firms (Panel (a)). This muted response, relative to the 3 percentage-point statutory surcharge, could reflect behavioral responses, contamination of the control group through changes to other taxes or penalties, or incomplete enforcement. To shed light on these channels, we sequentially impose sample restrictions. These exercises are not causal, but diagnostic decompositions of the gap between effective and statutory rates.

Panel (b) restricts the sample to firms that remain in their baseline ownership group throughout 2015–2019, as per the APS: exposed firms remaining haven-owned and control firms remaining foreign non-haven-owned. Among these stayers, the effective rate rises by 1.3 percentage points in 2015 and reaches 1.8 percentage points by 2017. Panel (c) further

restricts the sample to firms whose effective rate does not exceed the statutory rate applicable to their group (25% for exposed firms and 22% for control firms). This restriction addresses changes to other unobserved surcharges and penalties (e.g. fines for failing to file the APS), that can inflate measured effective rates. Under this restriction, the estimated increase rises to 1.8 percentage points in 2015 and 2.4 percentage points by 2017.

We interpret the remaining gap as reflecting incomplete application of the surcharge. Panel (d) provides direct evidence on this margin by redefining exposure using the variable actually used by the tax authority to assign the surcharge: firms' self-reported terminal haven ownership on the corporate tax return (F101). This measure, available from 2015 onward, is reported by all firms and occasionally diverges from our APS-based measure. Using it to redefine "stayers", the estimated effect reaches the full 3 percentage-point increase. This is consistent with institutional evidence indicating that, in post-reform years, enforcement relied primarily on self-reported ownership in the tax return, with APS data used selectively for verification.[12]

Taken together, Panels (b)–(d) suggest that the gap between statutory and effective rate changes reflects a combination of ownership responses, control-group rate increases from unobserved penalties, and incomplete enforcement. The effective tax increase amounts to roughly one-half to two-thirds of the statutory 3 percentage-point surcharge (Panels (b)–(c)). Combined with discussions with the tax administration, this pattern suggests that enforcement based on APS data was incomplete. Our reduced-form estimates therefore capture the effects of a partially enforced policy.

5.2 Tax Haven Ownership

Terminal Ownership in Tax Havens. Figure 1 Panel (a) plots the *baseline domicile* terminal ownership share (haven ownership for exposed firms and foreign non-haven ownership for control firms). Both series rise modestly before the reform, from just below 80% in 2012 to 85%–90% in 2014. After 2014, they decline, consistent with mean reversion, with the

[12] Figure A.5 shows that consistency between the APS registry and ownership reported on the CIT return is not systematically correlated with observable characteristics among exposed firms

control group providing a benchmark for this pattern. While pre-reform levels and trends are similar, the series diverge sharply after 2015, with tax-haven ownership among exposed firms falling faster than foreign non-haven ownership among controls. Panel (b) reports the corresponding event-study coefficients, showing a gap in baseline-domicile ownership for exposed firms that opens at about 10pp and widens to nearly 20pp by 2019.

Table 1 reports difference-in-differences estimates based on Equation (2). Odd-numbered columns present unweighted estimates; even-numbered columns weight observations by baseline firm assets. The reform significantly reduces baseline-domicile ownership among exposed firms by 12.3 percentage points over 2015-2019 (Column (1)), with larger effects when weighting by firm size (22.3 percentage points; Column 2). Consistent with extensive-margin adjustment, exposed firms are 11.2 percentage points more likely to report zero baseline-domicile ownership post-reform (23.1 with size weights; Columns (3)–(4)).[13]

We interpret these estimates as the reform-induced change in terminal ownership in tax havens. Relocation occurs primarily along the extensive margin, with many firms reporting exit from haven ownership. Relative to the counterfactual haven ownership share absent the reform, the estimates imply that the reform led to 20% lower haven ownership, estimated as $\hat{\beta}_{dd}/(\hat{\beta}_0 + \hat{\beta}_{Post})$ (Column (1) of Table 1). This calculation incorporates the post-reform coefficient to account for naturally-occurring mean reversion. These effects increase over the duration of the panel and are more pronounced when weighting by firm size.

Beneficial Ownership and Ownership Reallocation. We examine whether the decline in tax-haven terminal ownership is accompanied by greater transparency in *beneficial ownership*—that is, whether ownership can be traced to ultimate individual owners rather than remaining with intermediate corporate entities. Figure 1 Panel (c) plots the share of ownership attributable to individuals for exposed and control firms. At baseline, haven-owned firms are substantially less transparent: in 2014, only 43% of ownership is linked to individuals, compared to 70% for foreign non-haven firms.

Post-reform, the beneficial ownership share increases for exposed firms in 2015–2016,

[13] Figure A.6 shows larger effects with Poisson specification, combining intensive and extensive margins.

while remaining flat for controls, partially closing this gap. Panel (d) reports the corresponding event-study estimates. While there is a mild negative pre-trend—likely mechanical, as treated firms become more haven-owned and thus more opaque approaching 2014—the post-reform coefficients indicate an increase of about 10 percentage points relative to 2014. This implies that a substantial share of the decline in tax-haven ownership reflects real changes in ultimate ownership rather than purely nominal relabeling.[14]

We next examine the reported domicile of new terminal owners. Figure 2 Panel (a) shows that nearly all the decline in tax-haven ownership is reallocated to foreign non-haven owners, with little shift toward domestic Ecuadorian owners. The difference-in-differences estimates imply a 10.4 percentage point increase in foreign non-haven ownership among exposed firms, accounting for almost the entire reallocation (Table A.6). Note that we cannot distinguish whether these owners newly-acquired shares or were already part of the ownership structure, and started disclosing after the reform.

Taken together, the results indicate that the reform increases transparency by inducing firms to replace tax-haven entities with identifiable foreign individual owners. However, we do not observe net repatriation of ownership to Ecuador. Appendix C.2 provides intuition for this asymmetric disclosure pattern, arguing that disclosure to the Ecuadorian tax authorities is substantially more costly for Ecuadorian than for foreign individuals.

Did Firms Sever Ties with Tax Havens? Because the reform targets only terminal ownership in tax havens, it does not directly affect intermediate ownership links. We therefore examine whether firms maintain connections to tax havens. We consider three sources of evidence: (i) intermediate ownership links reported in the APS, (ii) dividend outflows to tax havens using currency tax data, and (iii) ownership structures in Orbis.

Figure 2 Panel (b) shows that exposed firms also reduce their reported intermediate haven ownership. The figure plots event study coefficients using as the outcome an indicator for whether a firm reports any terminal or intermediate presence in its baseline domicile (the

[14] Table 1 Columns (5)–(6) yield slightly smaller estimates, consistent with attenuation from the pre-trend. We also find increases in the probability of 100% beneficial ownership disclosure (Columns (7)–(8)).

dashed gray line is the coefficients for terminal ownership only, for comparison). This allows us to contrast having *any* reported presence in tax havens with terminal ownership alone. The results indicate a 7.9 percentage point decline in the probability that exposed firms report any presence in tax havens (Table A.6 Column (5)). Relative to the 11.2 percentage points reduction in terminal ownership (Table 1 Column 3), it implies that roughly three quarters of the decline in terminal haven ownership reflects firms reporting no haven presence.

This indicates that the drop in terminal tax-haven ownership does not simply reflect the reporting of a new final ownership link. Consistently, we find a similarly-sized decline (14 percentage points) in the probability that exposed firms report their 2014 largest terminal owner anywhere in the ownership chain (Table A.7 Columns (1)–(2)). Following the reform we also observe that the number of reported terminal owners of exposed firms increases and average ownership concentration drops (Columns (3)–(6)).

These findings may reflect genuine disengagement from tax havens, but could also arise from reporting responses. To help disentangle these channels, we next examine whether real financial flows are impacted, using data on cross-border currency exits. Table 2 reports difference-in-differences estimates of dividend outflows to tax-haven shareholders, applying the same mean-reversion correction by comparing exposed firms' outflows to tax havens with control firms' outflows to foreign non-havens. The results show significant declines on the probability of sending any dividends, the level of dividend outflows, and their value relative to firm assets (see also Figure A.7).[15] The magnitudes are large: the likelihood of any dividend payment falls by 23%; dividend outflows relative to assets decline by over 50% relative to baseline. Size-weighted regressions yield larger effects, consistent with the greater reduction in reported terminal ownership in tax havens.

While our measure captures only *direct* dividend outflows from Ecuadorian firms to foreign entities—and thus misses more complex channels such as "round-tripping"—the evidence nonetheless shows a clear decline in dividends sent to tax havens post reform. Moreover, because these data are based on third-party bank-reporting, the reduction in dividend pay-

[15] We also construct a measure of probability of a year on year reduction in all outflows, as in Kremer, Willis and You (2021); Edmans, Fang and Lewellen (2013), yielding similar results (Columns 7 and 8)

ments supports the interpretation that firms' exit from tax-haven ownership is in part real. Table A.8 expands on these results by examining shifts in post-tax profit allocation across dividends, retained earnings, and reinvestment. It documents an overall decline in dividend payments among exposed firms, with most of the reduction retained as earnings.

Our final piece of evidence draws on an external data source, *Orbis*, described in Appendix B. Although the sample of all foreign-owned firms covered at baseline is much smaller (500 firms in 2014, with two-thirds appearing also post-reform, Table B.1), the results corroborate our findings from administrative data. Replicating our APS-based empirical design, we estimate a marginally significant 14% decline among exposed firms relative to controls in the likelihood of presence in the baseline domicile, and 11% decline in the ownership share (Table B.2, Column 2). Despite their opaque collection process, Orbis data are likely independent of tax authority reporting.[16] This provides additional support that the observed decline in tax-haven ownership does not merely reflect changes in reporting behavior.

Entry and exit. The changes in ownership structures were conditional on continued reporting. We next examine whether the reform affected firm exit and explore aggregate trends in tax-haven ownership. Table A.9 estimates Equation (2) using several extensive-margin measures of firm activity and reporting: filing the F101 corporate income tax return with positive revenues, filing the APS ownership declaration, and reporting a complete ownership profile. Across specifications, we find precise null effects on treated firms' operation and reporting, ruling out impacts larger than 2–3 percentage points. Because the panel is constructed from firms active in 2014, these results imply no differential exit between haven- and non-haven-owned foreign firms within our main sample.

We complement this analysis with descriptive aggregate time-series: Figure A.8 plots the evolution of aggregate firm counts (Panel (a)) and assets (Panel (b)), normalized with respect to 2014 levels, for haven- and non-haven-owned foreign firms, extending beyond the firms included in our baseline sample. The two groups exhibit different pre-reform trends,

[16] Orbis data are collected and maintained by Bureau van Dijk/Moody's, a private provider that aggregates ownership information from business intelligence sources and financial reporting to shareholders.

making the evidence difficult to interpret. Still, the series diverge somewhat more after the reform, particularly for assets, which stagnate among firms with terminal haven ownership.

5.3 Robustness and Heterogeneity

Differential Mean Reversion. A potential threat to our design is differential mean reversion between exposed and control firms absent the reform. We address this by using the control group's mean reversion as a benchmark, which yields parallel pre-reform trends for most outcomes.[17] However, as the set of non-haven foreign domiciles is larger than that of tax havens, even a uniform stochastic process governing ownership reallocation can generate asymmetric transitions: ownership may be mechanically more likely to shift from the smaller to the larger group.

To address this concern, we re-estimate our main specification using alternative haven and non-haven definitions that better balance country-group size and geographic proximity. Table A.10 Panel (a) reports the results. Restricting the comparison to Panama and neighboring Colombia yields similar group sizes (437 versus 663 firms) and estimates almost identical to baseline. Comparisons of all havens to Colombia, and all havens to Latin American non-havens, produce slightly smaller but significant effects of -8.3 and -9.4 pp, respectively. As before, size-weighted estimates are larger.

Another possible source of differential mean reversion is differential churn between haven-owned and foreign non-haven-owned firms. To address this concern, we estimate an alternative specification that absorbs dynamic heterogeneity in treatment effects:

$$y_{it} = \alpha_i + \delta_t + \overbrace{\beta \cdot Exposure_i \cdot Post_t}^{\text{Intent-to-treat effect}} + \overbrace{\gamma \cdot (Year_t - 2014) \cdot Exposure_i \cdot Post_t}^{\text{Dynamic treatment effect}} + \overbrace{\xi \cdot (Year_t - 2014) \cdot Exposure_i}^{\text{Test on differential pre-trend}} + \varepsilon_{it}. \quad (3)$$

[17] To illustrate the identification challenge, Figure A.9 presents a naive difference-in-differences using tax-haven ownership, rather than baseline-domicile ownership, as the outcome. Relative to our preferred specification, it exhibits non-parallel pre-trends and yields substantially larger estimates.

Where γ represents a dynamic treatment, allowing the treatment effect to vary linearly over time, so that β estimates a treatment effect that assumes no post-reform dynamics—effectively the immediate impact in 2015, and ξ tests for differential linear pre-trends.

Table A.10 Panel (b) reports estimates for ownership shares in the baseline domicile (intensive and extensive margins, unweighted and firm-size weighted). The results show a decline in tax-haven ownership, attenuated by one-third to one-half relative to Table 1 (Columns (1)–(4)). Interpreting the coefficient β as the treatment effect yields a lower bound on the reform's impact, as it removes all dynamic adjustment netting out the subsequent decline (thus aligning with the 2015 immediate effect in Figure 1 Panel (b)). However, this approach is overly conservative. First, firms may take time to adjust their ownership structures, so dynamic responses are part of the treatment effect. Second, the reform may increase the ongoing hazard of maintaining haven ownership (e.g., through greater scrutiny or compliance costs), producing a genuinely rising treatment effect over time.

Heterogeneity. The model predicts stronger haven exit among firms with lower benefits from haven use or lower adjustment costs. We test these predictions by linking these mechanisms to pre-reform firm characteristics and estimating difference-in-differences specifications with heterogeneous treatment effects (multi-level treatment). We consider three proxies. First, we split firms by whether they transferred funds to havens in 2014, capturing their attachment to haven structures. Second, we use the extent of beneficial ownership disclosed prior to the reform as a proxy for the ease of adjusting ownership. Third, we distinguish firms by whether they reported positive taxable profits in 2014, capturing exposure to the surcharge and thus the effective cost of the reform.

Figure A.10 Panels (a)–(c) plot the dynamic coefficients (average effects in Table A.11). All groups exhibit positive and significant declines in haven ownership, indicating broad-based responses to the reform. Yet, in Panel (a), firms with prior haven outflows exhibit a smaller decline in haven ownership, indicating weaker responses among firms with stronger economic ties to havens. In Panel (b), firms with higher baseline beneficial ownership disclosure also respond less, consistent with more substantive presence in havens. In contrast, Panel

(c) shows no meaningful heterogeneity by baseline profitability. Overall, the patterns support the model's predictions: firms with lower benefits from haven use or lower adjustment costs respond more strongly.

Finally, we test for heterogeneity by whether firms' self-reported terminal haven ownership on the corporate tax return (F101), available from 2015 onward, matches terminal haven ownership in the APS registry (see Section 5.1). Point estimates of responses are larger among these "consistent" firms, but largely insignificantly different from those of "non-consistent" firms (Table A.12).

Panama Papers. The April 2016 Panama Papers revealed offshore entities and beneficial owners linked to Mossack Fonseca, often through structures designed to obscure ownership. The leak may constitute a subsequent treatment: named firms may have perceived a higher detection risk and responded by increasing transparency (O'Donovan, Wagner and Zeume, 2019). We test whether named firms reacted by raising reported beneficial ownership.

To assess this channel, we match our sample to the ICIJ leak data. We identify 14 exposed firms and 38 control firms from the Panama Papers.[18] We then estimate a triple-differences specification interacting surcharge exposure, the post-reform period, and an indicator for being named in the leaks:

$$y_{it} = \alpha_i + \delta_t + \zeta_1 \cdot Exposure_i \cdot Post_t + \zeta_2 \cdot ICIJ_i \cdot Post_t + \zeta_{DDD} \cdot Exposure_i \cdot ICIJ_i \cdot Post_t + \varepsilon_{it} \quad (4)$$

$ICIJ_i$ indicates whether firm i was named in the Panama Papers either as an entity or a beneficiary. The $ICIJ_i \times Post_t$ term captures the effect of the reform on all named firms, including control firms. The coefficient on $Exposure_i \times ICIJ_i \times Post_t$ captures whether named treatment firms exhibit differential responses.

Table A.13 reports the results. Firms named in the leaks increase reported beneficial

[18] We follow Brounstein (2025) who used the International Consortium of Investigation Journalist leak with fuzzy matching on corporation's names. He identifies a total of 151 Ecuadorian firms, for fiscal year 2010. Of those, 14 are exposure firms, 38 control firms and the remaining 99 are observed domestically-owned.

ownership by 8.6 percentage points relative to unnamed firms, indicating a direct transparency effect of the Panama Papers. However, the main effect for surcharge-exposed firms remains unchanged at 6.8 percentage points (relative to Table 1), suggesting that the leak does not contaminate our baseline estimates. Relative to this baseline effect, we find no additional impact on beneficial ownership (or terminal haven ownership) for exposed firms also named in the leaks, though estimates are imprecise given the small number of such firms.

5.4 Effects by Surcharge Rate

We now examine how the reform's effects vary with the tax rate. If firms respond more strongly to higher rates, policymakers seeking to raise ownership transparency may wish to impose larger surcharges on haven ownership. To estimate continuous treatment effects, we exploit variation in baseline exposure by including firms with minority terminal haven ownership, which face surcharge rates of 0–1.5 percentage points (Section 2.3). We estimate:

$$y_{it} = \alpha_i + \delta_t + \gamma \cdot \textit{Predicted Surcharge}_i \times \textit{Post}_t + \varepsilon_{it}, \tag{5}$$

where $\textit{Predicted Surcharge}_i$ denotes the statutory surcharge implied by the firm's 2014 terminal haven ownership, ranging from 0 to 3 percentage points. Under a homogeneous treatment effects assumption, $\hat{\gamma}$ captures the average effect of a one percentage point increase in the predicted surcharge.[19]

Table 3 reports the coefficients for terminal haven ownership and beneficial ownership disclosure. Responses significantly increase with the magnitude of the tax. Each additional percentage point of predicted surcharge reduces haven ownership by around 5 percentage points and increases the probability of extensive-margin exit from haven ownership by 2.7 percentage points (Columns (1)–(2)). We find similar patterns for beneficial ownership disclosure: each additional point of predicted surcharge increases disclosed beneficial ownership by 2.4 percentage points and raises the probability of full beneficial ownership disclosure by

[19] This specification also provides an additional check against differential mean reversion. Beyond average differential mean reversion between haven- and non-haven-owned firms, such reversion would also need to vary proportionally with the surcharge schedule implied by baseline ownership shares.

1.8 percentage points (Columns (3)–(4)).[20]

These estimates can be used to decompose the baseline effects from Table 1. For majority-exposed firms facing the full 3 percentage point surcharge, the implied continuous-treatment effect is $3 \times \hat{\gamma}$. Differences between $3 \times \hat{\gamma}$ and the baseline effects can be interpreted as a common transparency effect, independent of the surcharge rate. The comparison suggests that variation in the rate accounts for at least two-thirds of the response to the reform.

6 Effect of the Reform on Reported Economic Activity

Section 3 yields predictions for the effects of the reform on fiscal outcomes and real activity. We test them along three margins: taxes paid, reported profits, and productive inputs (assets and labor) that are harder to misreport and arguably better track economic activity. The reform raises tax liabilities through both mechanical and behavioral channels (Prediction 2). For firms that remain haven-owned, the surcharge directly increases taxes and may also raise reported taxable income through higher perceived enforcement. For exiting firms, the loss of haven-based evasion technologies should increase reported taxable income. We thus expect higher taxes paid and potentially higher reported profits, both from firms exiting haven structures and from those that remain but perceive greater detection risk. Real effects are ambiguous (Prediction 3): If haven structures facilitate financing or investment, the reform may reduce domestic activity. If they primarily enable tax avoidance, efficiency costs may be limited, and firms may even reallocate resources toward productive uses.

Results on taxes, profit, and profitability. We estimate Equation (1) using annual corporate tax return outcomes—tax liability, profits, and return on assets (ROA)—as measures of fiscal outcomes and reported profitability. Table 4 reports the difference-in-differences estimates (Figure 3 plots event-study coefficients over time). We find no differential effects

[20] Table A.14 reports analogous estimates for alternative parameterizations of terminal haven ownership and beneficial ownership disclosure, as well as tax, profit, and profitability outcomes.

on the extensive margin of paying any CIT (Column (1)). However, along the intensive margin, exposed firms increase CIT liabilities by 17% per year (Column (3)), alongside a similarly sized increase in reported profits (Column (5)). While part of the increase in tax payments is mechanical for haven stayers, the parallel rise in profits points to behavioral responses—consistent with reduced access to evasion technologies or higher perceived detection risk. Reported ROA also rises by 1.9 percentage points off an 11.5 percent base (a 17 percent increase; Column (7)). Asset-weighted specifications, reported in even-numbered columns, yield larger point estimates.

To shed light on mechanisms, we study heterogeneity in ex-post changes in terminal ownership. We partition exposed firms into three mutually exclusive groups based on post-reform ownership: (i) haven remainers (average haven ownership above 50%), (ii) full exiters (haven ownership falls to zero from 2015 onward), and (iii) partial/late exiters (average haven ownership stays positive but at a level below 50% and/or firms that exit after 2015). We estimate our baseline specification with this multi-valued treatment, comparing each group to non-haven-owned control firms.[21] These estimates are descriptive rather than causal, as exit decisions are endogenous, but help decompose the treatment effects.

Table 6 reports the results. Exiters exhibit larger increases in tax liabilities and reported profits (+24% and +28%) than remainers (+8% and +9%), with partial exiters in between (+19% and +17%), although differences across groups are not statistically significant (Columns (2)–(3)). ROA rises significantly more for exiters—by roughly five times the increase among remainers, and financial outflows to havens decline more sharply (Columns (4)–(5)). Overall, exit from terminal haven ownership is associated with higher transparency (Column (1)), reduced haven outflows, and stronger increases in taxes paid and reported profitability, consistent with havens providing a potent tax evasion technology.

Results on scale, investments, and payroll. Table 5 examines changes in firm activity—scale (Columns (1)–(3)), investment (Columns (4)–(6)), and payroll (Columns (7)–(9))—under the one-sided hypothesis that the surcharge may have discouraged economic activity

[21] Among 588 exposed firms, 229 are remainers, 70 full exiters, and 289 partial or late exiters.

among exposed firms.[22] The estimated effect on assets and revenue are instead positive but small (for assets: 0.397, s.e. 0.055), with 95% confidence interval that includes economically meaningful negative values. The coefficient on log investment is similarly positive but noisy (0.074, s.e. 0.254), as expected given the lumpiness of investment.

In contrast, the binary indicator for any investment yields a positive and more precisely estimated effect: the 95% one-sided lower bound is −0.006, allowing us to rule out economically meaningful declines in investment participation. Given a baseline investment rate of 17%, this corresponds to rejecting a drop larger than about 3.6 percentage points. We also examine investment scaled by baseline assets, which captures investment intensity relative to firms' initial capital base and avoids mechanically giving more weight to larger firms; here too, we can reject negative effects. Overall, these estimates provide little evidence that the reform reduced investment among exposed firms in Ecuador.

While we do not observe employment, we observe payroll. Estimates for an indicator for any positive payroll and log payroll are slightly negative but statistically insignificant. The one-sided 95% lower bounds (−0.049 and −0.11) imply we cannot rule out economically meaningful declines along either the intensive or extensive margins. In contrast, when we scale payroll by baseline assets, the coefficient is positive and we reject any negative effect at the 5% level (one-sided p=0.028). Table A.15 reports the 2014 asset-weighted analogue, yielding qualitatively similar results.

While standard frameworks predict that higher corporate taxes reduce investment, labor demand, and firm scale (Curtis et al. (2021); Hassett and Hubbard (2002)), the reform did not lead to detectable changes in proxies for real activity, such as scale and investment, though we cannot rule out some reductions in labor. A conjecture is that, in settings with tax haven use, the policy operated partly as a tax on rents, or attenuated these effects by inducing substitution away from tax planning and toward productive activities (Bustos et al. (2022)). Consistent with this interpretation, Table A.8 shows a decline in dividend payouts alongside a corresponding increase in retained earnings. Taken together, the evidence suggests limited effects on firm operations and indicates that part of the tax burden was borne by firm owners.

[22] Figure A.11 presents the corresponding event-study estimates.

Implication for tax revenue Given the size of exposed firms and the absence of detectable declines in economic activity, the estimated responses imply economically meaningful revenue gains. A back-of-the-envelope calculation applying the 17% average effect to baseline tax payments yields a 2.2% increase in aggregate CIT revenue; using asset-weighted estimates raises this to 3.2%.[23] The evidence is consistent with the surcharge reducing evasive haven use by inducing some firms to exit haven structures and increasing compliance: although haven-owned firms reported ROA about 40% lower than other foreign-owned firms at baseline, they close roughly half of this gap post reform.

7 Policy Design and Conclusion

Table 7 compares Ecuador's reform with alternative anti-opacity policies based on fiscal incentives, including proposals and reforms from the United Kingdom and Argentina. The policies differ along two dimensions: the firms they target and the tax base to which they apply. The comparison highlights differences in the size of affected tax bases, as well as the trade-offs between informational requirements, potential distortions, and pitfalls.

Targeting rules. A first design choice concerns which firms are targeted. Ecuador's reform applied to firms with terminal ownership in tax havens, linking taxation to observable offshore ownership structures through the registry. Relative to broader anti-opacity policies, however, its scope remained limited: around 800 firms were liable, representing 11% of aggregate assets, 7% of corporate profits and 44% of dividend flows to havens.

Alternative approaches could broaden coverage. One possibility would be to target any ownership link to a tax haven, including intermediary entities, which would roughly double the number of firms under scope and the taxable bases. Another would be to target firms failing to disclose beneficial ownership: these firms would face a surcharge on cross-border transactions regardless of their owner's location, substantially expanding the set of affected

[23] This likely understates the full fiscal effects, as it excludes spillovers to other taxes such as the VAT. PIT effects are ambiguous: in theory improved transparency should raise revenue, but newly disclosed owners are not Ecuadorians, and the lower dividend payouts may even offset any compliance gains.

firms and, in Ecuador's case, multiplying by seven the taxable profit base. However, many of the additional firms captured under such a regime are domestic firms with limited dividend flows to tax havens. A policy combining both targeting criteria—any ownership linkage to tax havens and BO non-disclosure—would yield a broadly similar number of affected firms and tax base as the actual policy. This reflects the substantial overlap with the actual policy, unsurprising given the poor BO reporting among terminally haven-owned firms.

Thus haven targeting and transparency targeting pursue related but distinct objectives. Ecuador's reform targeted terminal haven ownership, while continuing to tolerate some opacity through intermediary structures or undisclosed non-haven owners. As a result, enforcement still depended on ownership reporting quality and international cooperation.

Tax base definitions. Policies may also differ in the activity base subject to taxation. Ecuador applied the surcharge to corporate profits. Alternative policies could instead target broader and less manipulable bases, such as domestic assets, or narrower bases more directly linked to tax evasion, such as dividends and payments to tax havens. This logic underlies the *transparency levy* proposal, which would impose withholding-style taxes on payments to entities that fail to disclose beneficial ownership in the United Kingdom (Neidle, 2024).[24] Relative to ownership-based surcharges, transaction-based taxes may more directly target offshore flows. Yet they require transaction-level data, and may be easier to circumvent through rerouting or round-tripping.

Argentina implemented a related approach toward non-cooperative jurisdictions, relying on higher withholding taxes, deductibility restrictions, and presumptive income rules on all offshore transactions. This yields a very large tax base, but one that likely also reflects legitimate transactions, and in practice was struck down as not compliant with World Trade

[24]The *transparency levy* is more ambitious, and conceived as a first step toward a global BOR. Under the proposal, firms from countries lacking transparent BOR regimes—for example, without public BORs or information exchange—would face a tax on payments received from the United Kingdom unless they disclose their BO information to the UK's Companies House. The proposal creates incentives for both firms and governments to adopt BOR standards: firms can avoid the levy through disclosure, while countries can exempt all domestic firms by implementing compatible BORs. If adopted broadly, such a system could gradually create a de facto global network of interoperable BORs and sharply reduce offshore secrecy.

Organization guidelines.[25] In comparison, Ecuador relied on a narrower ownership-based trigger linked to its existing registry and applied to an intermediate tax base, corporate profits. Notably, Panel B shows that actual revenue gains from the policy substantially exceeded the mechanical estimates, reflecting improved tax compliance.

Conclusion Overall, the comparisons highlight the trade-offs in anti-opacity policy design. Broader targeting rules and less manipulable tax bases expand the enforcement perimeter, but may also increase compliance costs, administrative complexity, and real distortions. Ecuador adopted a narrow but feasible approach, reflecting administrative and political constraints. Despite imperfect implementation, the surcharge generated meaningful reductions in haven ownership, greater beneficial ownership disclosure, and higher reported profits, with limited effects on investment and firm scale. The reform also appears to have strengthened cooperation with some tax havens over time, including Panama. More broadly, the results suggest that even partial transparency-based enforcement can reshape offshore ownership structures: introducing frictions to opacity can generate substantial behavioral responses.

A feature of policies that use beneficial ownership registries (BORs) to target offshore opacity is that they can be implemented unilaterally, without waiting for slow and often incomplete international coordination (Alstadsæter et al., 2023*a*). As BORs become widespread, countries may be able to adopt more ambitious anti-opacity policies. At the same time, several questions remain open. First, the effectiveness of BORs depends on the accuracy of domestic reporting, including the ability to address nominee shareholders and other mechanisms that continue to obscure ultimate ownership. Second, the ability to request and rely on credible ownership information ultimately depends on collective action and reporting quality across countries. In this context, international coordination on information exchange, reporting standards, and sanctions for non-cooperative countries can complement unilateral action by strengthening enforcement and limiting opportunities for regulatory arbitrage.

[25] CIT deductions from transactions with offshore entities were disallowed, unless firms could demonstrate economic substance and arm's-length pricing. The policy similarly aimed at raising the cost of offshore opacity, but relied on country-based distinctions and trade measures deemed discriminatory under GATT (World Trade Organization, 2016). This episode highlights a constraint on anti–tax haven policy design: measures need to be based on objective and consistently applied criteria to be deemed non discriminatory.

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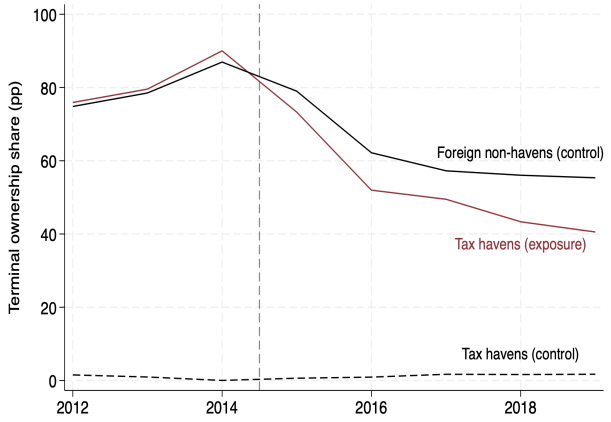
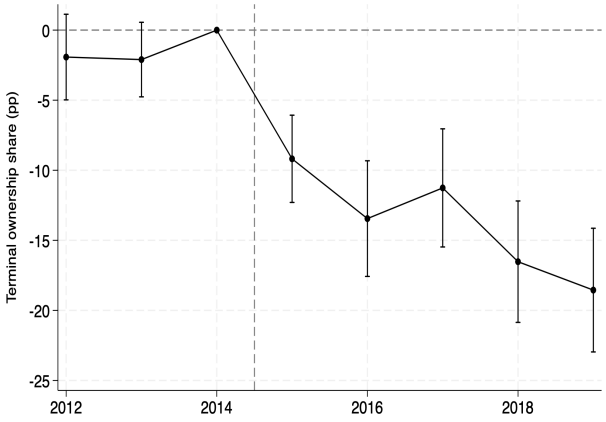
Figures and Tables

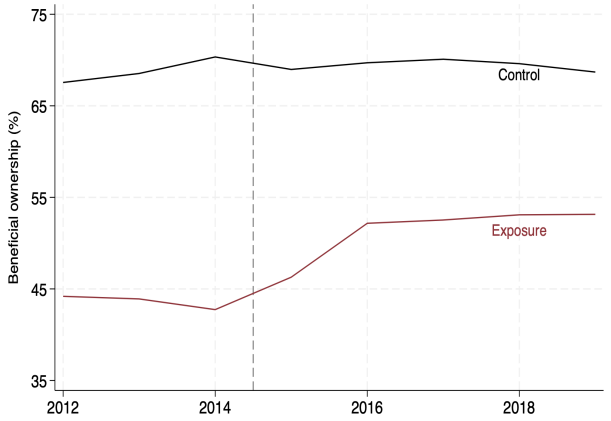
Figure 1: Reported ownership and beneficial ownership disclosure



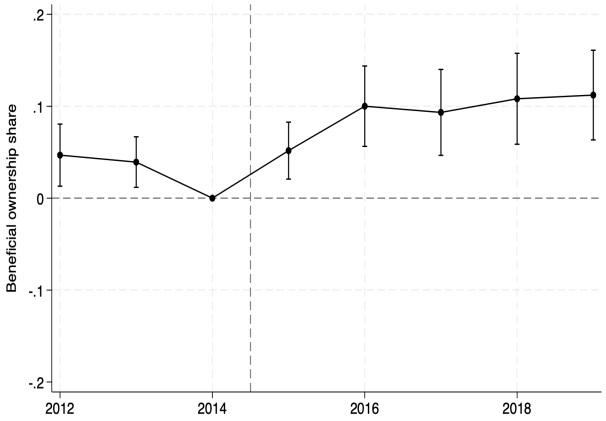
(a) Time series of terminal ownership



(b) Terminal ownership, baseline domicile (DD)



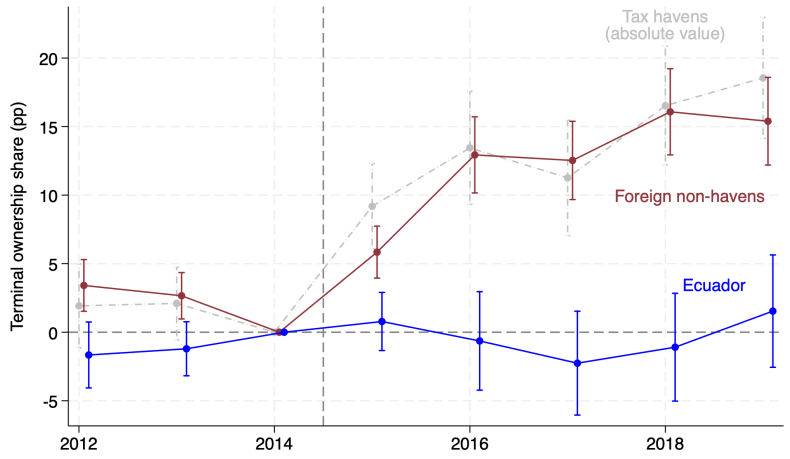
(c) Time series of beneficial ownership



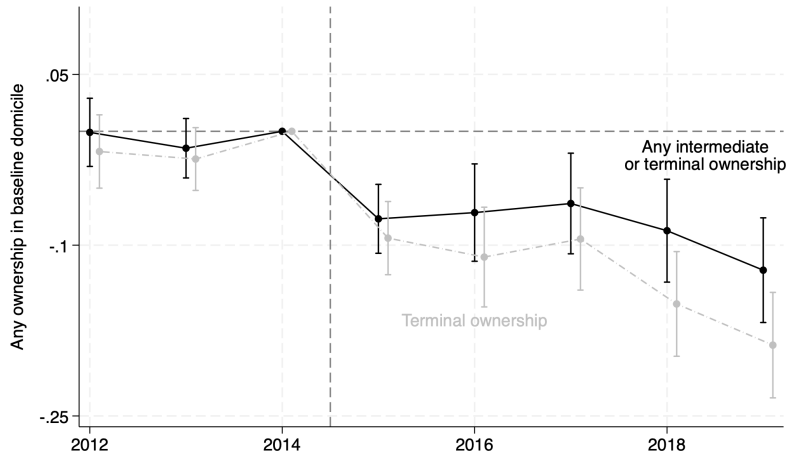
(d) Beneficial ownership (DD)

Note: These figures display the time series and the difference-in-differences results estimating the change in ownership by baseline domicile following the 2015 reform. Baseline domicile is defined as the reported location of the majority owner in 2014. Panel (a) plots the time series of outcome variables by group, including tax haven shareholdership for the control group to illustrate the utility of our approach for correcting mean reversion. Panel (b) uses this baseline domicile shareholdership variable as the dependent variable in our main difference-in-differences design; Panel (c) plots the mean share of beneficial ownership by exposure/control group and Panel (d) plots the corresponding difference-in-differences coefficients. The dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals use standard errors clustered on the firm-level. This figure is referred to in Section 5.2.

Figure 2: Difference-in-differences estimates: Reported terminal ownership by domicile and extensive margin responses



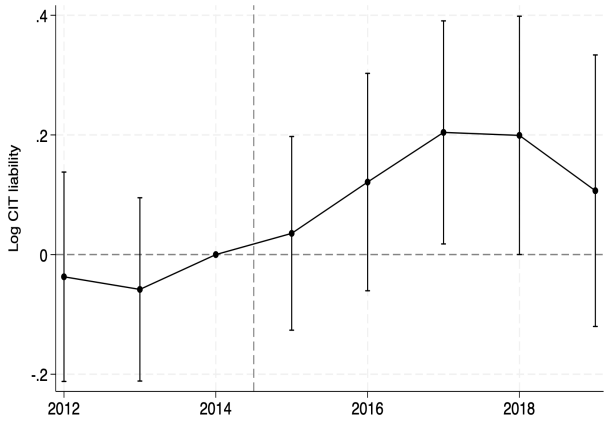
(a) Terminal Ownership Changes by Country Domicile Group (DD)



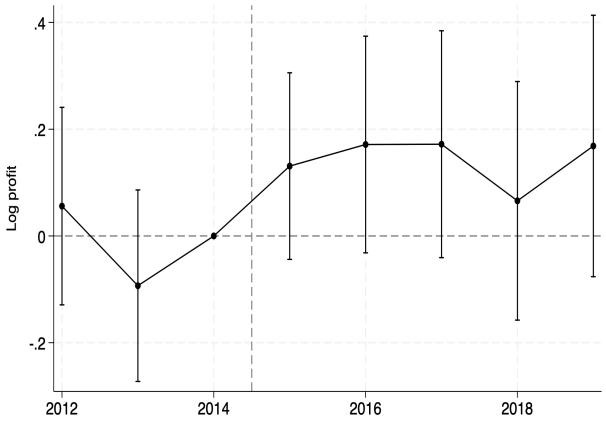
(b) Extensive Margin: Any Ownership in Baseline Domicile (DD)

Note: These figures display difference-in-differences estimates following Equation (1). Panel (a) plots coefficients for terminal ownership changes by country domicile category. The gray line labeled "Tax havens (absolute value)" plots the value of the coefficients from Figure 1 Panel (b) multiplied by minus one in order to juxtapose with the red line, which estimates the change in terminal ownership in foreign non-havens. The blue line plots the coefficients from a difference-in-differences design that uses Ecuadorian terminal ownership as the main dependent variable. In Panel (b), the solid black line represents estimates using as the dependent variable an indicator for whether a firm has either (1) any terminal owner in the baseline domicile group or (2) any wholly-owned intermediate owner in tax havens. For reference, the dashed gray line replicates the result from Figure 1 Panel (d) multiplied by minus one, which uses as the dependent variable an indicator for whether a firm has any terminal owner in the baseline domicile group. In both panels, the dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals are constructed using standard errors clustered on the firm-level. This figure is referred to in Section 5.2.

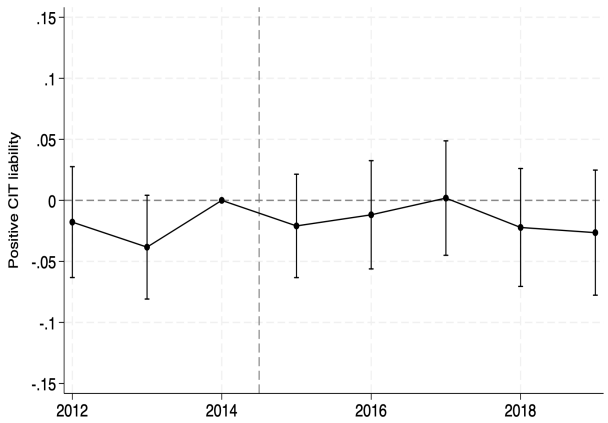
Figure 3: Difference-in-differences estimates: Taxes and profitability measures



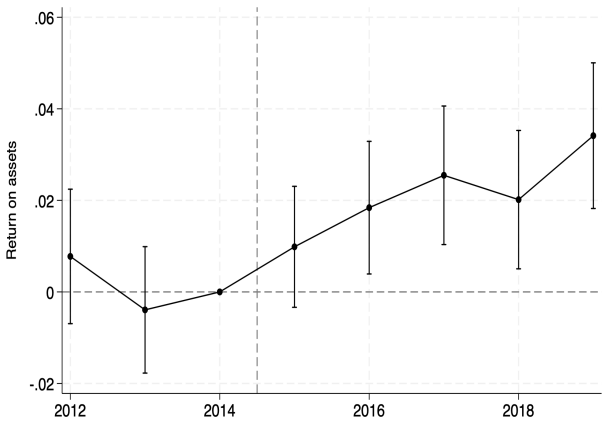
Corporate income taxes (DD)



Gross profits declared (DD)



Positive CIT liability (DD)



Return on assets (DD)

Note: These figures display difference-in-differences results pertaining to taxes, profits, and profitability (return on assets). Coefficients $\{\hat{\beta}_t\}$ are estimated from the difference-in-differences equation with two-way fixed effects: $y_{it} = \alpha_i + \delta_t + \sum_{t=\neq}^{2019} \beta_t \cdot Exposure_i \cdot Year_t + \varepsilon_{it}$. The dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals are constructed using standard errors clustered on the firm-level.

Table 1: Difference-in-differences estimates: Ownership reporting

	Terminal ownership in baseline domicile (pp)		Zero terminal ownership in baseline domicile		Beneficial ownership reported (pp)		100% reporting of beneficial ownership	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure × Post (β_{DD})	-12.30	-22.20	0.112	0.231	6.820	5.850	0.0550	0.0800
	(1.83)	(3.6)	(.019)	(.037)	(1.79)	(3.68)	(.019)	(.036)
Post (β_{Post})	-17.90	-4.860	0.202	0.0480	0.926	-2.990	0.0120	-0.0270
	(.617)	(1.75)	(.007)	(.016)	(.556)	(2.01)	(.006)	(.018)
Constant (β_0)	80.60	77.40	0.0690	0.110	64.80	47.20	0.552	0.307
	(.36)	(1.11)	(.004)	(.012)	(.331)	(1.09)	(.004)	(.011)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y	N	Y
N	28,362	28,362	28,362	28,362	28,362	28,362	28,362	28,362
Unique firms	3,928	3,928	3,928	3,928	3,928	3,928	3,928	3,928
Adjusted R2	0.456	0.451	0.356	0.379	0.654	0.619	0.667	0.610

Note: This table summarizes the difference-in-differences results for our main specification pertaining to the change in tax haven ownership and beneficial ownership reporting. Standard errors are clustered on the firm-level. This table is referred to in Section 5.2.

Table 2: Difference-in-differences estimates: Dividend outflows

	Any dividends sent to havens		Dividends sent to havens (1000s USD)		Dividends to havens (assets share)		Has year-to-year decrease in overall haven outflows	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure × Post	-0.0134	-0.0281	-29.90	-180	-0.00540	-0.0182	0.0247	0.0344
	(.006)	(.016)	(4.2)	(25.3)	(.0017)	(.0062)	(.012)	(.0313)
Constant	0.0590	0.147	32	122	0.0101	0.0222	0.108	0.233
	(.0018)	(.0088)	(2.15)	(15.9)	(.0006)	(.0024)	(.003)	(.0144)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y	N	Y
N	28,692	28,692	28,692	28,692	28,072	28,072	24,724	24,724
Unique firms	3,929	3,929	3,929	3,929	3,926	3,926	3,901	3,901
Adjusted R2	0.484	0.543	0.000500	0.000500	0.369	0.589	0.234	0.158

Note: This table presents difference-in-differences results evaluating the impact of the tax haven ownership reform on dividend/profit distribution flows and other outflows to tax havens. As in the main specification, to correct for mean reversion all dependent variables compare haven flows for the treatment group against foreign non-haven flows for the control group. Standard errors are clustered on the firm-level. This table is referred to in Section 5.2.

Table 3: Difference-in-differences estimates with continuous treatment intensity

	Terminal ownership in baseline domicile		Beneficial ownership disclosed	
	Levels	Binary	Levels	100% BO (Binary)
	(1)	(2)	(3)	(4)
Projected tax surcharge × Post (γ)	-5.020	-0.0270	2.430	0.0180
	(.59)	(.006)	(.579)	(.006)
Constant	65.70	0.920	68.70	0.560
	(.285)	(.003)	(.258)	(.003)
Average effect for average majority exposure (3γ)	-15	-0.0820	7.300	0.0530
	(1.77)	(.019)	(1.74)	(.018)
Main estimated effect	-12.30	-0.112	6.830	0.0550
	(1.83)	(.019)	(1.79)	(.019)
TWFE	Y	Y	Y	Y
N	38,472	38,472	38,472	38,472
Unique firms	5,305	5,305	5,305	5,305
Adjusted R2	0.590	0.359	0.682	0.692

Note: This table presents estimates of the model $y_{it} = \alpha_i + \delta_t + \gamma \cdot PredictedSurcharge_i Post_t + \varepsilon_{it}$. The estimation sample here consists of all majority and minority exposure and control firms in our core sample. The row beginning "Implied average effect" corresponds with the point estimate effect evaluated at the mean projected tax surcharge $3 \cdot \hat{\gamma}$, where $3 = \bar{s}_{maj.}$ by definition represents the average predicted surcharge among majority exposure firms. The row "Main estimated effect" presents analogous estimates from Table 1 for reference. Standard errors are clustered on the firm-level.

Table 4: Difference-in-differences estimates of tax and profit outcomes

	Log CIT		Positive CIT		Log profit		Return on assets	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure × Post	0.158	0.219	0.00300	-0.00700	0.153	0.245	0.0190	0.0240
	(.069)	(.118)	(.016)	(.027)	(.072)	(.115)	(.005)	(.009)
Constant	9.180	12	0.711	0.762	10.60	13.50	0.115	0.145
	(.015)	(.042)	(.004)	(.01)	(.016)	(.044)	(.001)	(.004)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y	N	Y
N	18,394	18,394	28,364	28,364	17,910	17,910	28,072	28,072
Unique firms	3,319	3,319	3,926	3,926	3,307	3,307	3,926	3,926
Adjusted R2	0.807	0.803	0.381	0.472	0.787	0.786	0.433	0.754

Note: This table summarizes the difference-in-differences results evaluating the impact of the reform on profit, taxes, and profitability. Differences in sample sizes across columns arise from either log dependent variables that exclude observations with zero-values or zero-valued denominators. Standard errors are clustered on the firm-level. This table is referred to in Section 6.

Table 5: Difference-in-differences estimates: firm activity

	Scale			Investment			Wage bill		
	Assets (log)	Positive revenue (binary)	Revenue (log)	Positive investment (binary)	Investment (log)	Investments assets share	Positive payroll (binary)	Payroll (log)	Payroll assets share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure × Post	0.0397	0.0244	0.00720	0.0238	0.0741	0.0170	-0.0225	-0.0255	0.0160
	(.0551)	(.0127)	(.0692)	(.0152)	(.254)	(.0059)	(.0161)	(.0539)	(.0083)
Constant	12.90	0.937	13	0.169	11.50	0.0333	0.880	11.60	0.315
	(.0133)	(.0029)	(.0161)	(.0031)	(.0541)	(.0011)	(.0033)	(.0114)	(.0022)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	N	N	N	N	N	N	N	N
N	28,072	28,364	25,047	28,364	3,747	28,072	28,364	22,896	28,072
Unique firms	3,926	3,926	3,844	3,926	853	3,926	3,926	3,628	3,926
Adjusted R2	0.828	0.326	0.756	0.520	0.789	0.614	0.485	0.844	0.662

Note: This table summarizes the difference-in-differences results evaluating the impact of the reform on assets, investment, and labor expense. Differences in sample sizes across columns arise from either log dependent variables that exclude observations with zero-values (Columns (1), (3), (5), and (8)) or zero-valued denominators (Column (6) and (9)). Standard errors are clustered on the firm-level. This table is referred to in Section 6.

Table 6: Difference-in-differences estimates by post-reform ownership

	Beneficial ownership disclosed (pp)	Log taxes paid	Log profit declared	Return on assets	Financial outflows to havens (1000s USD)
	(1)	(2)	(3)	(4)	(5)
Exposure × Remained in havens × Post	-2.26	.0825	.0923	.0072	5.25
	(2)	(.103)	(.116)	(.0078)	(26.5)
Exposure × Left havens × Post	36.5	.243	.274	.0377	-66.5
	(6.71)	(.1)	(.128)	(.0091)	(32.6)
Exposure × Partial haven exit × Post	6.18	.193	.164	.0245	-56.2
	(2.46)	(.107)	(.104)	(.0063)	(23.8)
Constant	65.4	9.12	10.5	.0922	389
	(.163)	(.0061)	(.0065)	(.0005)	(2.12)
$\hat{\beta}_{partial\ exit} - \hat{\beta}_{remained}$	8.44	.111	.0717	.0174	-61.5
	(3.08)	(.143)	(.15)	(.0094)	(21)
$\hat{\beta}_{left} - \hat{\beta}_{remained}$	38.8	.161	.181	.0305	-71.8
	(6.95)	(.138)	(.167)	(.0115)	(30.6)
TWFE	Y	Y	Y	Y	Y
N	28,362	18,394	17,910	28,072	28,692
Unique firms	3,928	3,319	3,307	3,926	3,929
Adjusted R2	.657	.808	.788	.434	.00072

Note: This table summarizes the results for difference-in-differences regression with a multi-leveled treatment specification, where each exposure firm is assigned to a mutually exclusive group based on its country category of terminal ownership domicile in 2015-2019. The control group consists of all majority foreign-owned control firms. Standard errors are clustered on the firm-level. This table is referred to in Section 6.

Table 7: Benchmarking Anti-Opacity Fiscal Policies

Panel A: Alternative Targeting Rules and Tax Bases for Ecuador

Firms Targeted, based on ownership registry	(1) Number of firms	Activity base (Million USD 2014) (2) Assets	(3) Profits	(4) Dividends to havens	(5) All flows to havens
A. Tax haven terminally-owned (total)	779	5,557	580	23	289
Majority haven-owned	584	2,920	295	21.5	229
Minority haven-owned	195	2,636	285	1.46	60.3
B. Any presence in tax havens (total)	1,447	8,824	1,348	40.1	630
Tax haven terminally-owned	779	5,557	580	23	289
Foreign non-haven terminally-owned	321	2,301	610	13.7	234
Domestic terminally-owned	347	967	158	3.41	106
C. Reports less than 50% BO (total)	6,943	18,135	4,116	26.5	1,052
Tax haven terminally-owned	416	2,912	332	21.4	180
Foreign non-haven haven terminally-owned	1,200	6,164	1,149	1.08	396
Domestic terminally-owned	5,327	9,060	2,636	4.01	476
D. Any presence in tax havens & reports less than 50% BO (total)	695	4,436	591	23.6	305
Tax haven terminally-owned	416	2,912	332	21.4	180
Foreign non-haven haven terminally-owned	146	917	195	.105	63.5
Domestic terminally-owned	133	607	63.9	2.11	61.4
E. All firms	62,350	49,737	8,411	52.6	2,209

Panel B: Anti-Opacity Fiscal Policy Designs, International Cases

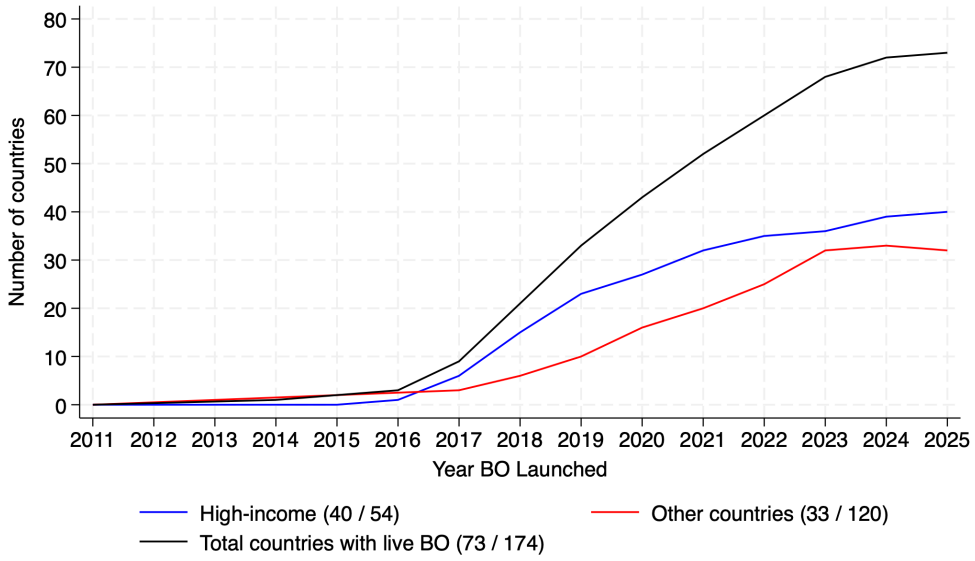
	Targeting Rule	Reliance on BOR	Tax Liability	Mechanical Revenue Estimates	Limits & Pitfalls
Ecuador's CIT surcharge. Discourage opaque ownership structures.	Terminal ownership in tax havens (Row A).	✓	3% of Corporate profits (A3).	USD 17.4 Million ≈ 0.9% of CIT revenue.	Opacity in intermediary structures, verifying non-haven owners.
U.K. transparency levy proposal. Incentivize adoption of interoperable BORs and reduce offshore secrecy.	Failure to disclose BO, firms from countries without BORs or info. exchange (≈ Row C).	✓	10% on dividends or payments to non-disclosing firms (C4, C5).	USD 2.7 or USD 105 Million, 0.1% to 5.7% of CIT revenue.	Transaction rerouting, BO verification, defining BOR compatibility, multilateral coordination.
Argentina anti-haven transaction rules. Discourage transactions with tax havens.	All firms with tax haven transactions (Row E).	✗	Non-deductibility of cross-border flows from CIT (E5).	USD 486 Million ≈ 26% of CIT revenue, using 22% CIT rate.	Applies to transactions with substance, challenged by firms and WTO.

Note: This table summarizes hypothetical tax bases given different firms of interest and different taxed activities. Calculations in Panel A consider all Ecuadorian firms that comprise our core panel of active firms in the administrative tax data environment. All firm definitions and flows correspond with 2014, the final pre-reform year. "Tax haven terminally-owned" firms consist of firms with greater than 5% terminal ownership in tax havens; "Foreign non-haven terminally-owned" firms consist of firms with greater than 5% terminal ownership in foreign non-havens and zero terminal ownership in tax havens. Firms with "Any presence in tax havens" correspond with those either terminally-owned via tax havens or having a non-terminal intermediary in a tax haven with at least 50% direct ownership. All other firms are classified as "Domestic". There are 731 firms excluded from this table who did not report any ownership structure, but they represent less than 1% of assets or profits. Panel B compares the Ecuador reform studied in this paper with alternative anti-opacity approaches. The U.K. proposal refers to the "transparency levy" proposed by (Neidle, 2024). Argentina's case refers to anti-haven rules applying higher withholding taxes, deductibility restrictions, and presumptive income rules on transactions involving non-cooperative jurisdictions discussed in World Trade Organization (2016). These rules were passed in 2012, but mainly struck down by the WTO in 2016. This table is referred to in Section 7.

SUPPLEMENTARY APPENDICES

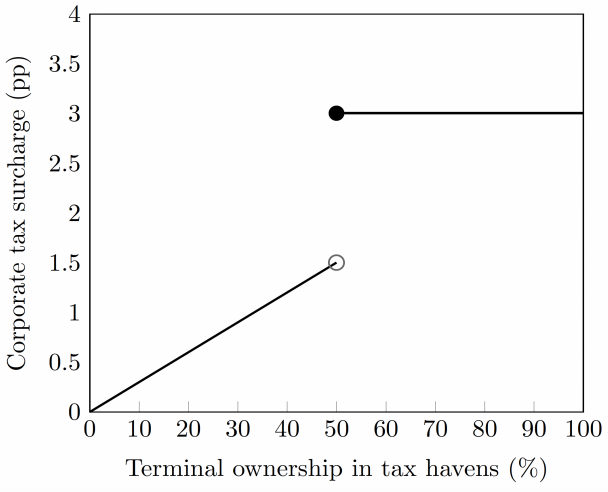
Appendix A Additional figures and tables

Figure A.1: Global adoption of beneficial ownership registries



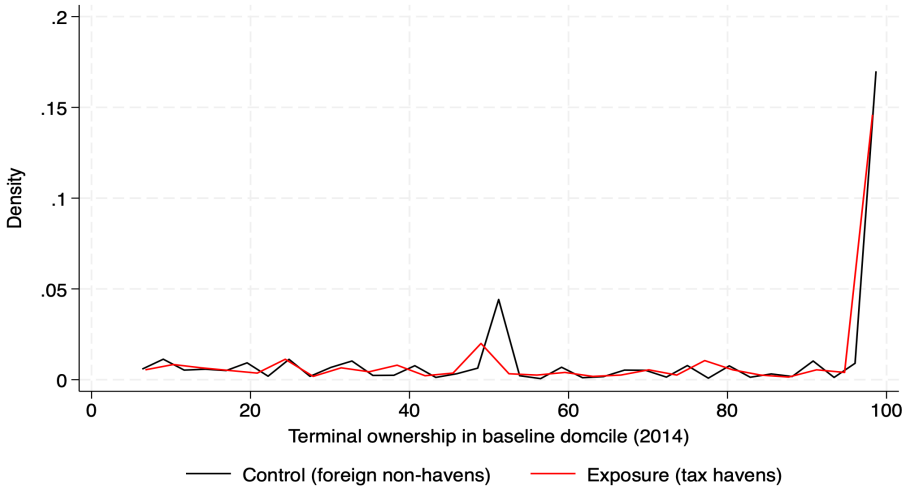
Note: This figure displays time series of the cumulative count of countries that maintain a beneficial ownership registry (BO). High-income and other countries are defined mutually exclusively according to World Bank country classifications of income level for 2024-2025. Data updated as of July 2025, 73 out of 174 (42%) countries with populations exceeding 500,000 have launched a live BO registry. Income classification: High-income (40); Upper-middle income (21); Lower-middle income (9); Low income (2)." This figure is referred to in Section 1.

Figure A.2: CIT surcharge schedule by tax haven ownership share



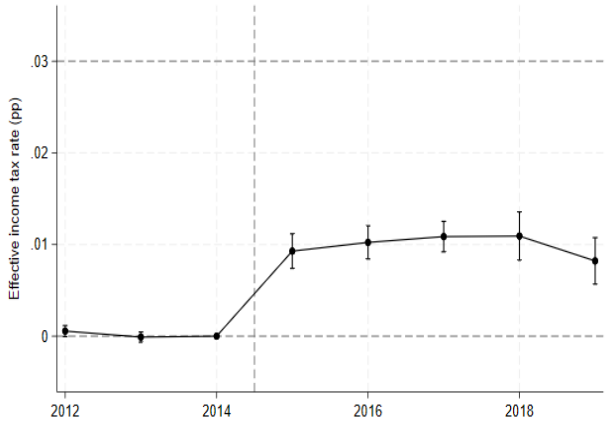
Note: This figure shows the statutory CIT surcharge based on terminal ownership attributable to tax havens. This figure is referred to in Section 2.3.

Figure A.3: Distribution of terminal ownership shares in 2014 (foreign-owned firms)

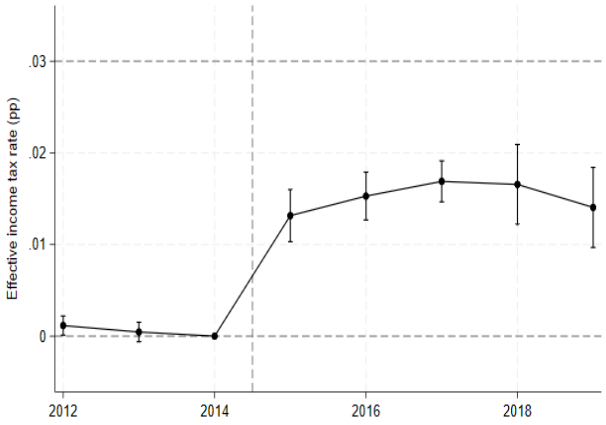


Note: This figure shows the 2014 distribution of terminal ownership attributable to owners in foreign non-havens and tax havens for our control and exposure groups respectively. Note that both spikes in density around 50% ownership indeed occur precisely at 50%; visual offset is due to discrepancies in kernel estimation. This figure is referred to in Section 4.

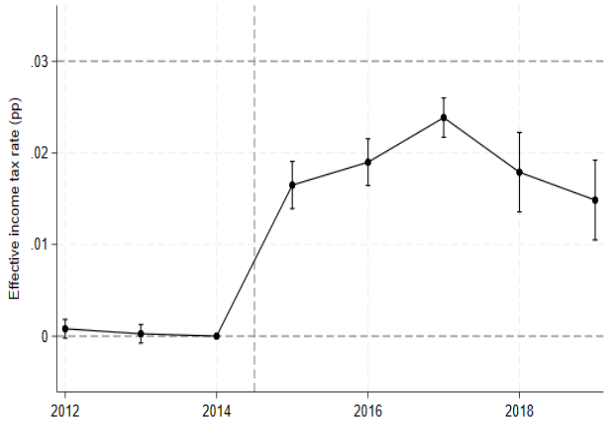
Figure A.4: Difference-in-differences estimates: Effective CIT rates



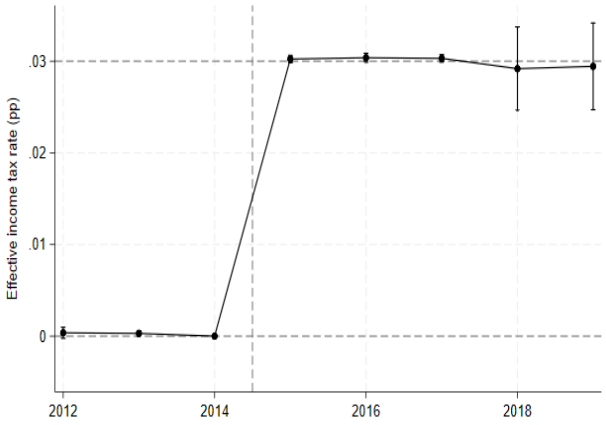
(a) No sample restriction



(b) Condition: exposure and control maintain presence/absence in tax havens per APS data



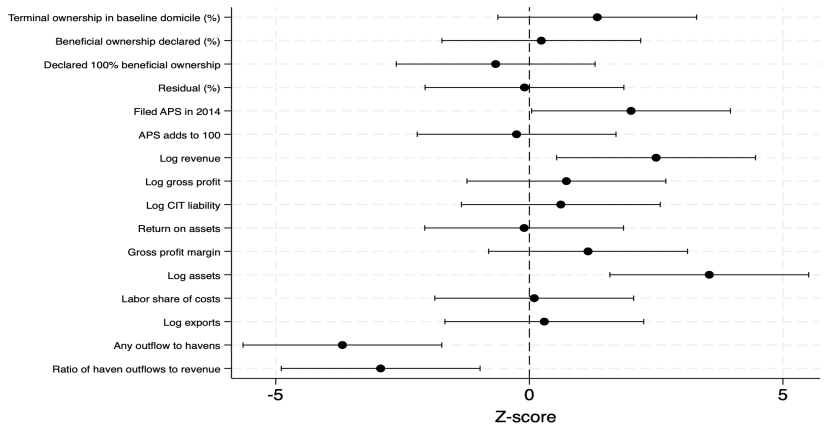
(c) Condition: same as (b) + No additional penalties



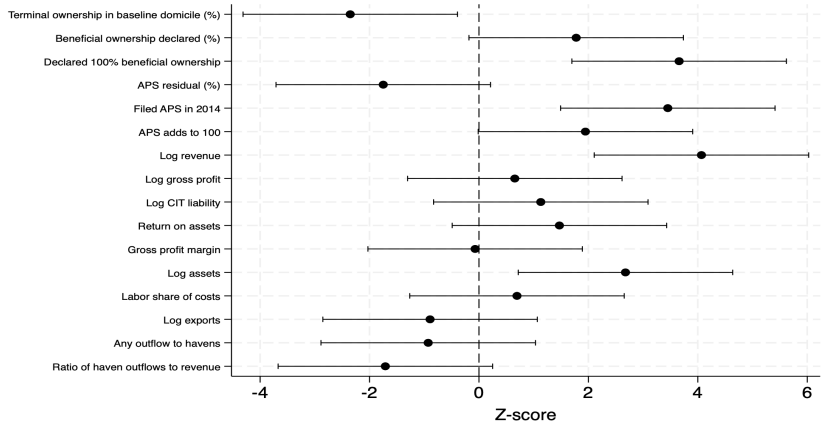
(d) Condition: exposure and control maintain presence/absence in tax havens, per F101 data No additional penalties

Note: These figures display difference-in-differences results that illustrate components of the "bindingness" of the CIT surcharge among firms that maintain majority shareholdership in tax havens and pay corporate income tax. Panel (a) estimates our standard difference-in-differences design using effective corporate income tax rate as the dependent variable. Panel (b) conditions the sample to consider treatment firms that maintained majority terminal ownership in tax havens and control firms that maintained zero terminal ownership in non-havens from 2015 to 2019 per the APS data; Panel (c) adds in the further restriction , winsorizes the dependent variable above the statutory CIT rate to mitigate the presence of alternate surcharges and fines; Panel (d) changes the data source used to condition firms presence or absence from tax havens from the APS data to the F101, which was used in practice to administer the surcharge. The increase in dispersion in estimates starting in 2018 is due to a CIT rate change applied to larger firms. Horizontal dashed lines mark the zero change and 3pp change lines; the dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals are constructed using standard errors clustered on the firm-level. This figure is referred to in Section 5.

Figure A.5: Balance on observables, Firms with and without ex-post verification



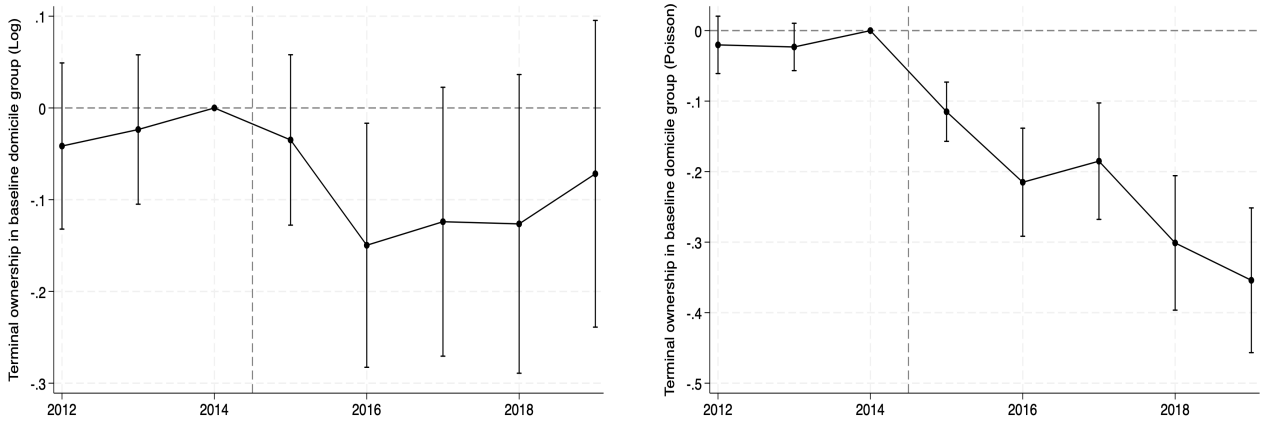
(a) Treatment



(b) Control

Note: This figure displays z-scores and confidence bounds for coefficients $\hat{\beta}$ corresponding with cross-sectional regressions of 2014 observable characteristics on indicators for whether the value of firms' reported terminal ownership in tax havens aligns (a difference of less than 1pp in absolute value) between the F101 and the APS data for years 2015-2017. Regressions are stratified based on treatment status, with Panels (a) and (b) displaying estimates for treatment and control firms respectively. Confidence intervals use heteroskedasticity-robust standard errors. This figure is referred to in Section 5.

Figure A.6: Difference-in-differences estimates: Alternative specifications of terminal ownership in baseline domicile

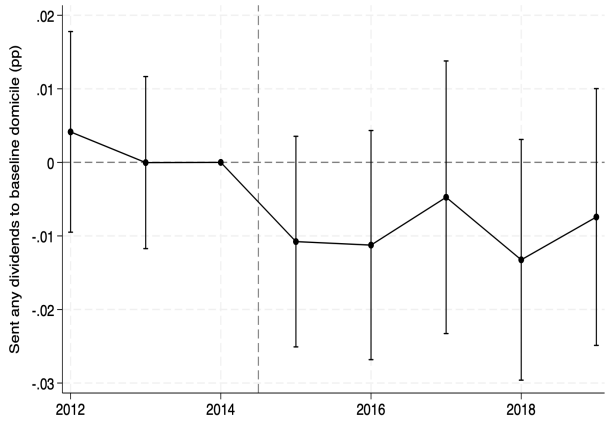


(a) Log ownership

(b) ownership, Poisson (PPML)

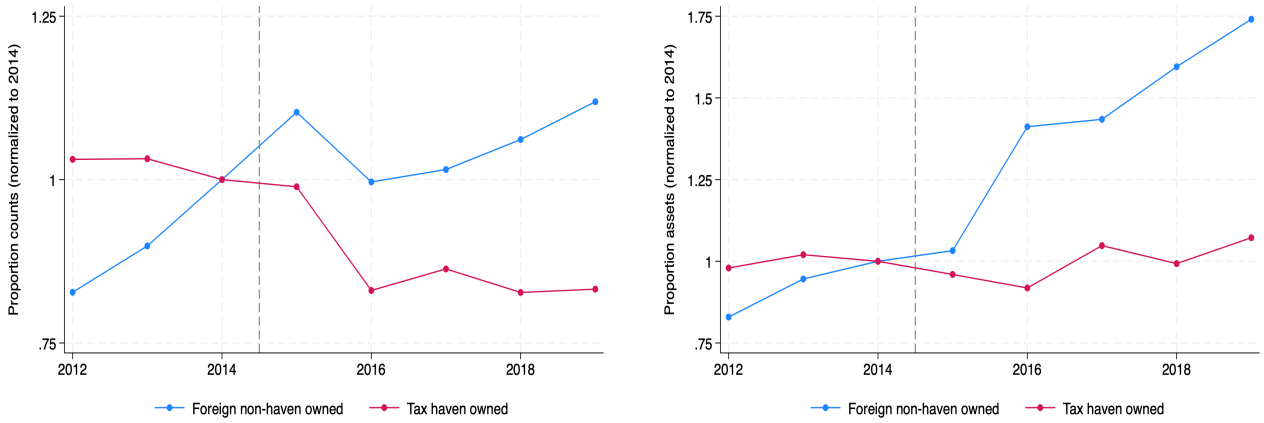
Note: These figures display difference-in-differences results for alternate parameterizations for the change in terminal ownership in baseline domicile group between the exposure and control firms in our main specification. The dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals use standard errors clustered on the firm-level. This figure is referred to in Section 5.

Figure A.7: Difference-in-differences estimates: Dividend outflows to tax havens



Note: This figure display difference-in-differences coefficients for the change in probability that firms send dividends to shareholders in the country of their baseline domicile: tax havens for treatment firms and foreign non-havens for control firms. The dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals are constructed using standard errors clustered on the firm-level. This figure is referred to in Section 5.2.

Figure A.8: Aggregate trends in haven and non-haven ownership

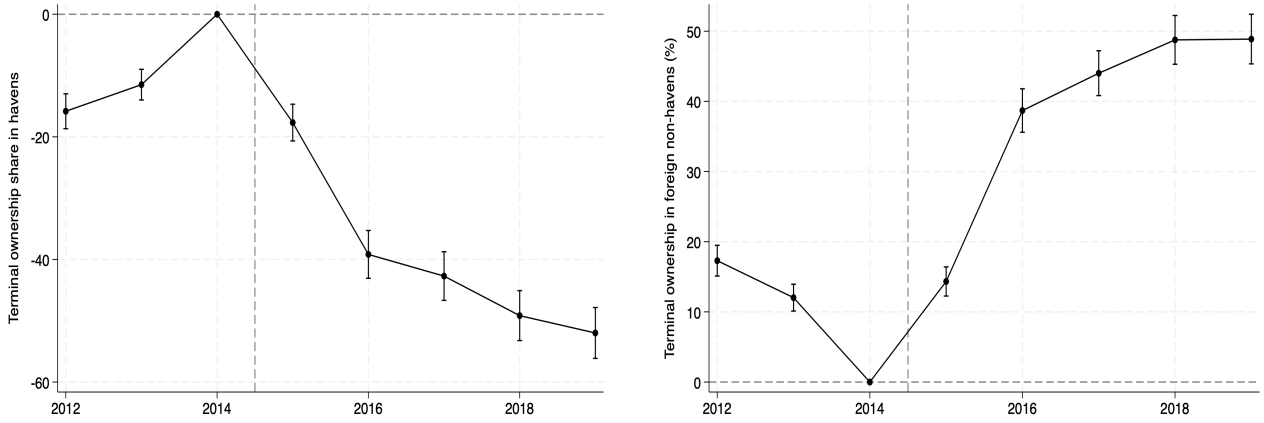


(a) Counts relative to 2014

(b) Aggregate assets relative to 2014

Note: this figure displays the timeseries evolution of counts and aggregate assets of all operating firms with majority terminal ownership either in tax havens or in foreign non-havens, relative to 2014. Note that these time series include all firms operating in Ecuador every year that file the APS ownership declaration, not only firms active in 2014 that we use to study the reform in our main causal analysis. This figure is referred to in Section 5.2.

Figure A.9: Difference-in-differences estimates of ownership: Direct comparisons without mean-reversion correction

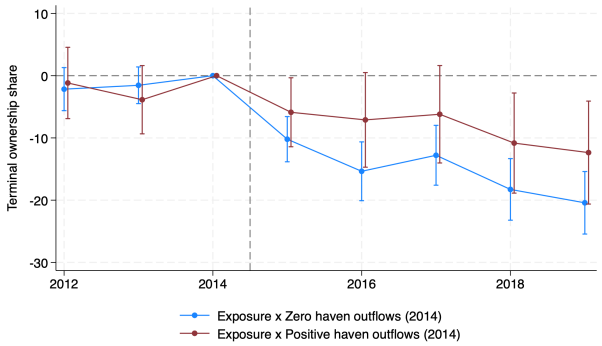


(a) Tax haven terminal ownership

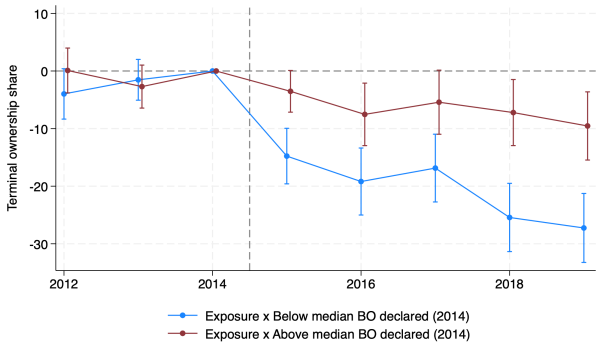
(b) Foreign non-haven terminal ownership

Note: These figures display difference-in-differences results that directly compare the relative changes in ownership domicile between the exposure and control groups. Panel (a) uses tax haven shareholdership as the dependent variable; Panel (b) uses foreign non-haven ownership as the dependent variable. The dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals are constructed using standard errors clustered on the firm-level. This figure is referred to in Section 5.2.

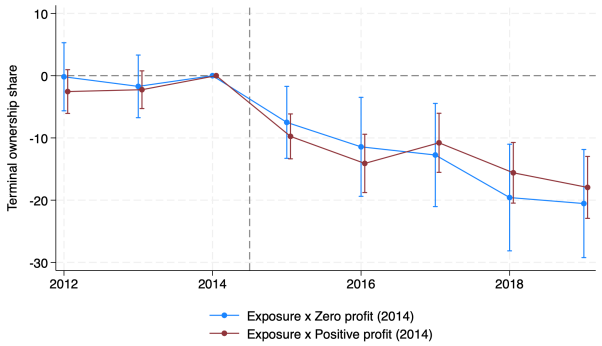
Figure A.10: Difference-in-differences estimates: Heterogeneity in terminal ownership responses



(a) Any haven outflow in 2014



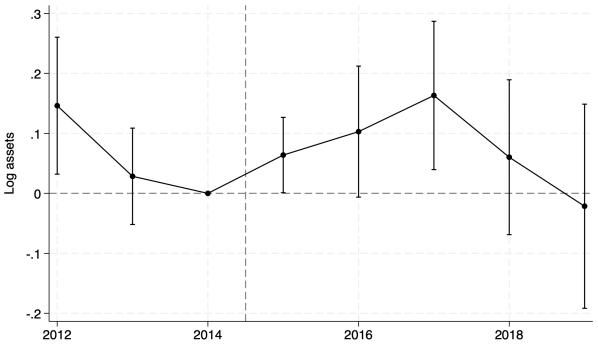
(b) Above/below median beneficial ownership declared in 2014



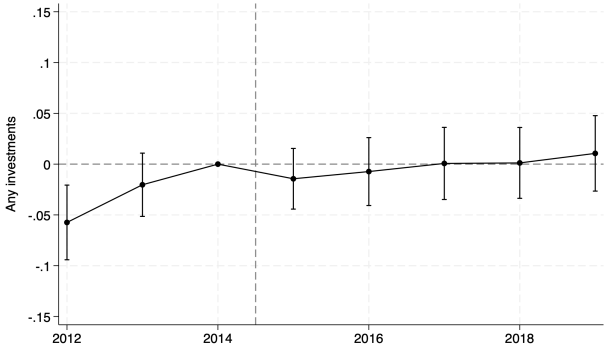
(c) Declared positive profit in 2014

Note: These figures display the impacts of the reform on firms along different margins of ex-ante heterogeneity. Panels (a), (b), and (c) plot difference-in-difference coefficients using a multi-level treatment specification, where the baseline group consists of all control firms. The dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals are constructed using standard errors clustered on the firm-level. This figure is referred to in Section 5.3.

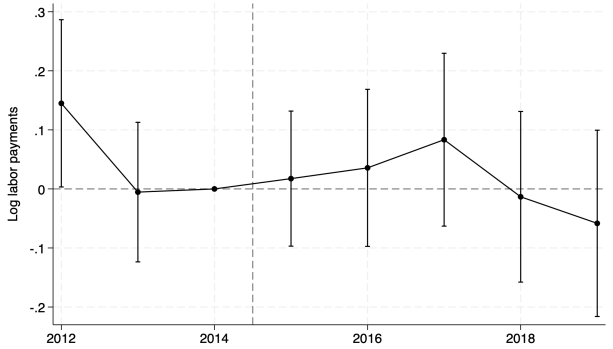
Figure A.11: Difference-in-differences estimates: Assets, investment, and labor expenses



(a) Total assets



(b) Any new investments



(c) Labor expense

Note: These figures display difference-in-differences results pertaining to firm inputs. The dashed gray vertical line marks the implementation of the CIT surcharge. Confidence intervals are constructed using standard errors clustered on the firm-level. This figure is referred to in Section 6.

Table A.1: Covariate balance of active firms
based on whether they appear in the APS business ownership data

	Firms not present in the APS	Difference relative to main sample
Log revenue	10.3	-1.28
		(.046)
Log gross profit	8.13	-.821
		(.068)
Log taxable profit	6.9	-2.08
		(.113)
Has taxable profit revenue	.071	-.7
		(.003)
Log CIT liability	5.54	-1.92
		(.106)
CIT rate	.217	-.003
		(.001)
Log assets	9.59	-1.97
		(.042)
Gross profit margin	.118	.025
		(.004)
Labor share of costs	.368	.056
		(.007)
Any outflow to havens	.142	-.105
		(.012)
Any outflow to non-havens	.526	-.312
		(.017)
Active firms never filing APS	14,830	
Core sample firms	62,350	

Note: This table displays results from a series of cross-sectional uni-variate regressions of 2014 firm business characteristics on an indicator for whether the firm is included in our core sample. Parentheses contain heteroskedasticity-robust standard errors. Firms in these cross-sectional regressions are deemed active in 2014 by having a business income tax declaration with non-zero revenue; firms within this group are further included in the core sample if they also have filed the APS at least once between 2012 and 2014. This table is discussed in Section 2.4.

Table A.2: Panel balance of the core sample

Year	Filed APS	Complete APS (Conditional on filing)	Filed F101	Filed F101 with positive revenue	Filed both APS and F101	Filed APS and F101 With positive revenue
2012	.823	.95	.836	.737	.821	.726
2013	.914	.956	.929	.839	.912	.827
2014	.988	.967	1	1	.988	.988
2015	.956	.972	.969	.873	.948	.857
2016	.897	.987	.921	.788	.881	.76
2017	.872	.986	.885	.737	.857	.721
2018	.827	.99	.868	.714	.813	.674
2019	.795	.991	.781	.632	.731	.598

Note: This table tabulates various measures of panel balance for our core sample over time relative to the full count of 62,350 firms in 2014. This table is discussed in Section 2.4.

Table A.3: Descriptive statistics and ownership of Ecuadorian Firms (2014)

	Mean	SD	p10	Median	p90	p99	Count	N
Firm Characteristics:								
Revenue (1000s USD)	1,929,413	16,843,744	4,300	137,447	2,361,029	30,412,296	62,350	62,350
Log revenue	11.6	2.68	8.37	11.8	14.7	17.2	62,350	62,350
Log taxable profit	8.98	2.56	5.63	9.16	12	14.8	48,065	62,350
Has taxable profit revenue	.771						62,350	62,350
CIT liability (1000s USD)	29,311	622,357	0	834	25,154	424,830	62,350	62,350
Log CIT liability	7.46	2.55	4.11	7.65	10.5	13.3	48,060	62,350
CIT rate	.22	.00363	.22	.22	.22	.22	48,073	62,350
Log assets	11.5	2.4	8.27	11.7	14.5	16.6	61,354	62,350
Gross profit margin	.0927	.173	0	.0335	.231	.993	62,350	62,350
Labor share of costs	.312	.922	0	.223	.743	1	61,823	62,350
Terminal ownership:								
Ecuadorian person (%)	85.8	32.1	8.92	100	100	100	62,350	62,350
Foreign person (%)	3.65	16.8	0	0	0	100	62,350	62,350
Ecuadorian non-person entity (%)	6.42	22.1	0	0	6.25	100	62,350	62,350
Foreign non-person entity (%)	1.63	11.9	0	0	0	99	62,350	62,350
Residual (%)	1.4	11.1	0	0	0	90	61,619	62,350
Ownership characteristics:								
Ownership share of plurality owner	61.3	30.2	20	52	99.8	100	62,283	62,350
Average share of terminal owner	38.4	19.7	8.33	50	50	100	62,283	62,350
Number of terminal owners	8.99	392	2	2	10	78	62,350	62,350
Avg. ult. shareholder chain	1.05	.347	1	1	1	2.98	62,283	62,350
Ownership reporting compliance:								
Filed APS	.988						62,350	62,350
Declared 100% nominal ownership	.967						61,619	62,350
Declared any beneficial ownership	.96						62,350	62,350
Beneficial ownership declared (%)	89.8	27.7	50	100	100	100	62,350	62,350
Declared 100% beneficial ownership	.829						62,350	62,350
Cross-border flows:								
Any outflow to havens	.0773						62,350	62,350
Ratio of haven outflows to revenue	.0198	1.46	0	0	0	.145	62,350	62,350
Any inflow from havens	.0097						62,350	62,350
Ratio of haven inflows to revenue	.0316	3.61	0	0	0	0	62,350	62,350
Any outflow to non-havens	.262						62,350	62,350
Ratio of non-haven outflows to revenue	29	4,608	0	0	.144	.87	62,350	62,350
Any inflow from non-havens	.122						62,350	62,350
Ratio of non-haven inflows to revenue	259	58,249	0	0	.00453	1.34	62,350	62,350

Note: This table displays descriptive statistics of firms in our core sample for 2014 from the form F101 business income tax declarations, the APS ownership data, and the MID cross-border flows data. This table is discussed in Section 2.4.

Table A.4: Foreign terminal ownership of core sample Ecuadorian companies (2014)

		Count of firms by ownership threshold				Terminal ownership of all Ecuadorian firms			
						% of Ecuadorian assets		% of Ecuadorian revenue	
	Country	$\geq 50\%$	$> 25\%$	$> 10\%$	$> 5\%$	All	By persons	All	By persons
Panel (a):	Tax havens								
1	Panama	377	449	501	513	1.93	1.12	1.66	.896
2	Netherlands	62	71	85	90	.57	.119	.693	.139
3	Bahamas	47	51	52	52	.425	.205	.256	.076
4	British Virgin Islands	16	22	27	29	.068	.032	.101	.049
5	Cayman Islands	11	11	11	11	.196	.001	.101	.001
6	Luxembourg	10	11	14	15	.215	0	.196	0
7	Belize	9	9	9	9	.062	.004	.014	.003
8+	All others	50	64	72	74	.599	.117	.532	.046
hline All	Total	584	686	759	780	4.07	1.6	3.56	1.21
hline	% of all firms in sample	.949	1.11	1.23	1.27				
	% of all assets	3.48	4.59	6.5	10.2				
	% of all revenue	3.14	3.76	5.56	6.18				
Panel (b):	Foreign non-havens								
1	Colombia	668	783	866	900	1.23	.792	1.73	1.08
2	USA	597	839	1,052	1,154	3.55	1.76	5.3	2.46
3	Spain	328	404	462	484	1.87	1.29	2.01	1.3
4	Peru	230	271	306	323	.92	.68	1.05	.674
5	Venezuela	144	172	186	190	.372	.335	.522	.407
6	Chile	139	167	197	208	.421	.166	.646	.236
7	Argentina	118	152	177	184	.234	.192	.337	.277
8+	All others	1,102	1,403	1,637	1,732	14.3	7.18	10.5	4.92
All	Total	3,397	4,056	4,534	4,703	22.9	12.4	22.1	11.4
	% of all firms in sample	5.52	6.59	7.37	7.65				
	% of all assets	24.9	28.2	31.6	33.4				
	% of all revenue	22.4	26.3	28.9	30.5				

Note: This table uses the APS to tabulate the international terminal owners of Ecuadorian companies in our core sample in 2014 by country. Panel (a) displays the tax havens, and panel (b) the non-haven countries. Firms in sample include all firms that file the APS in 2014. Country rankings are constructed by tabulating the number of firms with 100% terminal ownership within a given country by haven/non-haven status in 2014 (below the column labeled "$\geq 50\%$"). The first five columns count the number of firms with terminal ownership above a specific threshold by country, where the threshold can take the value 100%, 50%, 25%, 15% or 5%. By definition, a single firm can be counted in multiple countries except in the 100% column. The next four columns show the terminal ownership share of Ecuadorian firms for each foreign country, weighted by firms' assets or revenue. The denominator of these terms consists of aggregate assets or revenues reported in the business income tax declarations. This table is discussed in Section 2.4.

Table A.5: Descriptive statistics and ownership characteristics: Foreign-owned Ecuadorian Firms (2014)

	Haven (Exposure)	Foreign (Control)	Difference	Domestic	Difference: foreign v. Domestic
Panel (a): Firm Characteristics					
Log revenue	12.7	12.5	.185	11.5	1.06
			(.135)		(.049)
Log taxable profit	10.5	10.3	.158	8.83	1.5
			(.135)		(.052)
Has positive taxable profit	.747	.742	.005	.777	-.035
			(.019)		(.007)
Log CIT liability	8.95	8.8	.152	7.31	1.51
			(.134)		(.051)
Log exports	13.5	12.5	.963	11.8	.802
			(.328)		(.114)
Log assets	13.8	12.7	1.06	11.3	1.6
			(.097)		(.039)
Return on assets	.077	.125	-.049	.141	-.024
			(.007)		(.003)
Labor share of costs	.243	.289	-.046	.303	-.021
			(.011)		(.004)
Panel (b): Cross-border flows					
Any outflow to havens	.231	.177	.054	.067	.118
			(.019)		(.006)
Ratio of haven outflows to revenue	.042	.017	.025	.004	.017
			(.006)		(.002)
Panel (c): Ownership reporting					
Terminal ownership in baseline domicile (%)	90	87	3.05	100	-12.6
			(.761)		(.299)
Avg. ult. shareholder chain	1.92	1.54	.383	1.01	.584
			(.043)		(.016)
Has a haven strict intermediary	.561	.093	.468	.006	.158
			(.021)		(.006)
APS adds to 100	.983	.98	.003	.988	-.008
			(.006)		(.002)
Beneficial ownership declared (%)	42.7	70.4	-27.6	93.2	-27
			(2.07)		(.72)
Declared 100% beneficial ownership	.327	.612	-.286	.873	-.303
			(.021)		(.008)
Unique firms	588	3,351		55,674	

Note: This table displays descriptive statistics of firms in our sample for 2014 from the F101 business income tax declarations (Panel (a)), the MID cross-border flows data (Panel (b)), and the APS ownership data (Panel (c)). The first difference column corresponds with a cross-sectional univariate regression of the dependent variable (given by the row) on an indicator for exposure with the set of majority foreign-owned firms; the second difference column (the final column) compares the union of exposure and control firms against domestic firms. Parentheses contain heteroskedasticity-robust standard errors. This table is discussed in Section 4.

Table A.6: Difference-in-differences:
Domicile of terminal ownership and intermediate presence in tax havens

	Terminal ownership in Ecuador (pp)		Terminal ownership in inverse domicile (pp)		Has terminal or intermediary ownership in baseline domicile		Has an intermediary in havens (excl. terminal ownership)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure × Post	0.643	4.670	10.40	15.80	-0.0780	-0.170	-0.0880	-0.0680
	(1.56)	(2.45)	(1.19)	(3.04)	(.019)	(.034)	(.015)	(.023)
Constant	16.30	12.60	1.510	4.870	0.936	0.914	0.898	0.904
	(.333)	(.744)	(.143)	(.739)	(.004)	(.01)	(.003)	(.008)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y	N	Y
N	28,362	28,362	28,362	28,362	28,362	28,362	11,559	11,559
Unique firms	3,928	3,928	3,928	3,928	3,928	3,928	1,857	1,857
Adjusted R2	0.504	0.547	0.394	0.424	0.366	0.392	0.787	0.735

Note: This table summarizes the difference-in-differences results for our main specification pertaining to the change in terminal ownership in other domiciles as well as to persisting ownership presence in tax havens via intermediaries. The dependent variable in Columns (7)–(8) is only defined for firms with interior intermediaries (i.e. at least one firm separating the terminal owner from the Ecuadorian firm), and therefore has fewer observations. Standard errors are clustered on the firm-level.

Table A.7: Difference-in-differences: Additional mechanisms of ownership change

	Has its 2014 baseline group plurality shareholder		Number of terminal owners (Poisson)		Average terminal ownership concentration		Ownership share of plurality owner (pp)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure × Post	-0.140	-0.0900	0.418	0.181	-0.0970	-0.0950	-3.610	-4.800
	(.02)	(.04)	(.176)	(.082)	(.034)	(.07)	(.87)	(1.93)
Post	-0.192	-0.270	0.0160	-0.0560	-0.0340	0.00400	-0.304	-1.700
	(.008)	(.025)	(.043)	(.091)	(.011)	(.073)	(.315)	(1.24)
Constant	0.874	0.766	3.880	5.730	3.520	3.090	69.70	70.60
	(.004)	(.015)	(.027)	(.055)	(.006)	(.044)	(.181)	(.694)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y	N	Y
N	28,362	28,362	28,362	9.186e+10	28,278	28,278	28,280	28,280
Unique firms	3,928	3,928	3,928	3,928	3,928	3,928	3,928	3,928
Adjusted R2	0.369	0.430	0.870	0.947	0.662	0.655	0.738	0.695

Note: This table summarizes the impacts of the reform on additional ownership characteristics following our our main difference-in-differences specification. Average terminal ownership concentration is calculated as 100 divided by the number of terminal owners. Average ownership chain length is calculated as the average number of ownership layers separating a firm and each of its terminal owners in a given year, weighted by the ownership share of each respective terminal owner. Column (7) omits a weighted specification due to a persistent differential pre-trend. Poisson specifications use frequency weights instead of analytic weights. Standard errors are clustered on the firm-level.

Table A.8: Difference-in-differences
Post-tax profit allocation (1000s USD)

	Dividends paid		Retained earnings		Reinvested profits	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × Post	-58.4	-235	79.4	445	.704	4.33
	(30.4)	(141)	(37.3)	(217)	(.139)	(.837)
Constant	256	1,391	32.7	-61.4	.666	10.1
	(4.86)	(70.8)	(10.3)	(109)	(.0634)	(1.64)
TWFE	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y
N	24,395	24,395	24,395	24,395	24,395	24,395
Unique firms	3,883	3,883	3,883	3,883	3,883	3,883

Note: This table summarizes the difference-in-differences results evaluating the impact of the reform on post-tax profits allocated among profit distributions, retained earnings, and reinvestment. All variables here are expressed in units 1000s real USD (2014). Dividends are calculated using a dynamic imputation method based on declarations of gross profits, taxes paid, reinvestment, and year-to-year changes in accumulated profits. For this reason, the dependent variables in columns (1)–(4) cannot be constructed in year 2019; Columns (5)–(6), corresponding with results reinvested profits, are estimated on samples restricted to exclude year 2019 to preserve comparability across columns. Standard errors are clustered on the firm-level.

Table A.9: Panel balance and APS compliance

	Filed APS and active		Filed F101		Filed APS		Declared 100% of its terminal ownership		APS ownership residual	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure × Year ≥ 2015	.008	-.014	-.011	-.003	-.015	-.008	-.011	-.026	.983	.431
	(.016)	(.02)	(.011)	(.012)	(.011)	(.012)	(.008)	(.021)	(.51)	(1.48)
Constant	.861	.902	.929	.961	.921	.952	.96	.883	1.55	5.04
	(.004)	(.008)	(.003)	(.005)	(.003)	(.006)	(.002)	(.009)	(.101)	(.631)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y	N	Y	N	Y
N	31,512	31,512	31,512	31,512	31,512	31,512	28,315	28,315	28,315	28,315
Unique firms	3,939	3,939	3,939	3,939	3,939	3,939	3,925	3,925	3,925	3,925
Adjusted R2	.356	.317	.285	.274	.283	.264	.333	.371	.188	.22

Note: this table displays results from a series of difference-in-differences regressions that evaluate the change in various compliance and panel balance measures between our main exposure and control groups. Parentheses contain standard errors clustered on the firm-level.

Table A.10: Robustness of terminal ownership response
Panel (a): Robustness by sample definition

	Main specification		Caribbean havens v. Colombia		All havens v. Colombia		All havens v. Latin American non-havens	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure × Post	-12.30	-22.20	-12.30	-17.70	-8.340	-16.60	-9.400	-15.70
	(1.83)	(3.6)	(2.42)	(4.79)	(2.18)	(5.68)	(1.97)	(8.34)
Constant	80.60	77.40	67.60	72.90	67.60	75.50	67.10	70.50
	(.36)	(1.11)	(.61)	(1.9)	(.646)	(2.71)	(.37)	(2.41)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y	N	Y	N	Y
N	28,362	28,362	8,004	7,970	9,080	9,043	14,327	14,262
Unique firms in regression	3,928	3,928	1,098	1,091	1,246	1,238	1,977	1,964
Unique control firms	3,351	3,351	663	663	660	660	1,393	1,393
Unique exposure firms	588	588	437	437	588	588	588	588
Adjusted R2	0.456	0.451	0.518	0.528	0.502	0.475	0.497	0.452

Panel (b): Controlling for continuous differential linear time trends

	Levels		Binary	
	(1)	(2)	(3)	(4)
Exposure × Post	-6.910	-15.30	-0.0551	-0.141
	(1.88)	(5.65)	(.0197)	(.0514)
Exposure $\times(t-2014)\times$ Post	-2.160	-2.890	-0.0225	-0.0390
	(.563)	(1.47)	(.006)	(.0114)
Exposure $\times(t-2014)$	0.986	0.300	0.00930	0.00560
	(.778)	(2.34)	(.0082)	(.0218)
TWFE	Y	Y	Y	Y
Weight (2014 assets)	N	Y	N	Y
N	28,362	28,219	28,362	28,219
Unique firms	3,928	3,901	3,928	3,901
Adjusted R2	0.504	0.402	0.416	0.349

Note: This table tests the robustness of our main results. Panel (a) summarizes the change in firms' terminal ownership in their respective baseline domicile group following our main difference-in-differences specification, where each pair of columns features a different geographic definition of tax havens and foreign non-havens. Columns (1) and (2) reproduce the specifications from Table 1 Panel (a) Columns (1) and (2) for ease of comparison. Panel (b) summarizes the change in firms' terminal ownership in their respective baseline domicile group by estimating the equation: $y_{it} = \alpha_i + \delta_t + \beta Exposure_i Post_t + \xi(Year_t - 2014) \cdot Treat_i + \gamma(Year_t - 2014) \cdot Exposure_i \cdot Post_t + \varepsilon_{it}$. Standard errors are clustered on the firm-level. This table is referred to in Section 5.2.

Table A.11: Difference-in-differences:
Terminal ownership responses along margins of ex-ante heterogeneity

	Levels (pp)	Binary	Levels (pp)	Binary	Levels (pp)	Binary
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure × Zero haven outflows (2014) × Post	-14	-0.134				
	(2.08)	(.021)				
Exposure × Any haven outflow (2014) × Post	-6.710	-0.0410				
	(3.47)	(.038)				
Exposure × Below median BO declared (2014) × Post			-18.80	-0.143		
			(2.58)	(.027)		
Exposure × Above median BO declared (2014) × Post			-5.610	-0.0800		
			(2.38)	(.026)		
Exposure × Zero profit (2014) × Post					-12.80	-0.0960
					(3.39)	(.035)
Exposure × Positive profit (2014) × Post					-12.10	-0.117
					(2.1)	(.022)
Constant	80.60	0.931	80.60	0.931	80.60	0.931
	(.36)	(.004)	(.359)	(.004)	(.36)	(.004)
Difference (High v. Low)	7.300	0.0926	13.20	0.0626	0.712	-0.0210
	(3.95)	(.0429)	(3.4)	(.0358)	(3.89)	(.04)
TWFE	Y	Y	Y	Y	Y	Y
N	28,362	28,362	28,362	28,362	28,362	28,362
Unique firms	3,928	3,928	3,928	3,928	3,928	3,928
Adjusted R2	0.456	0.357	0.457	0.357	0.456	0.356

Note: This table summarizes the estimates from a series of regressions summarizing heterogeneous treatment effects of the reform along different margins of ex-ante heterogeneity. Odd-numbered columns use terminal ownership in baseline domicile (in percentage points) as the dependent variable. Even-numbered columns use a binarized version of this dependent variable as an outcome that indicates positive terminal ownership in baseline domicile . Each column corresponds with a different regression with a multi-level treatment specification that estimates the response of different segments of exposure firms against all control firms. Standard errors are clustered on the firm-level. This table is referred to in Section 5.3.

Table A.12: Heterogeneous responses (DDD) based on ex-post verification in tax havens
Tax haven terminal ownership and beneficial ownership

	Terminal ownership in baseline domicile			Beneficial ownership declared			
	Levels (pp)	Any positive	Poisson	Levels (pp)	Any positive	Poisson	Full BO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exposure × Consistent × Post	-6.320	-0.0670	-0.0820	8.270	-0.0250	0.157	0.113
	(4.38)	(.045)	(.079)	(4.41)	(.035)	(.093)	(.047)
Exposure × Post	-10.90	-0.116	-0.190	3.720	0.0400	0.0920	0.00800
	(2.32)	(.024)	(.039)	(2.25)	(.018)	(.045)	(.023)
Consistent × Post	-0.884	-0.0440	-0.0180	-1.530	0.00900	-0.0250	-0.0290
	(1.49)	(.016)	(.022)	(1.42)	(.013)	(.022)	(.015)
Constant	80.70	0.932	4.480	64.80	0.794	4.410	0.552
	(.361)	(.004)	(.005)	(.331)	(.003)	(.005)	(.004)
Difference	-7.200	-0.111	-0.0990	6.740	-0.0170	0.132	0.0840
	(4.12)	(.042)	(.076)	(4.18)	(.032)	(.09)	(.045)
TWFE	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	N	N	N	N	N	N
N	28,362	28,362	28,362	28,362	28,362	25,308	28,362
Unique firms	3,928	3,928	3,928	3,928	3,928	3,506	3,928
Adjusted R2	0.456	0.358	0.323	0.654	0.697	0.437	0.668

Note: This table displays triple differences estimates following our main design, testing for heterogeneous effects of the reform based on whether firms' presence in tax havens was verified during the 2015-2017 period. The variable "Consistent" corresponds with an indicator for whether value of firms' reported terminal ownership in tax havens aligns (a difference of less than 1pp in absolute value) between the F101 and the APS data for each of the three years. Note that this variable sees support among both control and treatment firms. The row "Difference" estimates the sum $\hat{\beta}_{ddd} + \hat{\beta}_{Consistent \times Post}$, the difference in treatment effects for "consistent" and "non-consistent" exposure firms. Standard errors are clustered on the firm-level. This table is referred to in Section 5.3.

Table A.13: Triple differences:
Interaction between the reform and the Panama Papers leaks

	Beneficial ownership disclosed (pp)	100% BO disclosed (binary)	Terminal ownership in baseline domicile (pp)	Has positive terminal ownership in baseline domicile (binary)
	(1)	(2)	(3)	(4)
Exposure × Post	6.770	0.0550	-12.30	-0.110
	(1.82)	(.019)	(1.84)	(.019)
ICIJ × Post	8.550	0.133	-2.190	0.0490
	(4.54)	(.056)	(6.64)	(.066)
Exposure × ICIJ × Post	-3.240	-0.0680	0.248	-0.0930
	(9.47)	(.1)	(12.7)	(.14)
Constant	65.30	0.559	69.70	0.806
	(.172)	(.002)	(.177)	(.002)
TWFE	Y	Y	Y	Y
N	28,362	28,362	28,362	28,362
Unique firms	3,928	3,928	3,928	3,928
Adjusted R2	0.655	0.668	0.503	0.416

Note: This table summarizes the estimates from a series of triple differences regressions testing the impacts and interactions of the reform and being named in the Panama Papers on terminal ownership in baseline domicile and beneficial ownership disclosed. Standard errors are clustered on the firm-level. This table is referred to in Section 5.3.

Table A.14: Difference-in-differences with continuous surcharge: Additional outcomes

	Terminal ownership in baseline domicile		Beneficial ownership disclosed		Profit, tax, profitability		
	Log	Poisson	Log	Poisson	Log CIT	Log profit	Return on assets
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Projected tax surcharge × Post (γ)	-0.0380	-0.0680	0.185	0.0530	0.0640	0.0590	0.00600
	(.02)	(.011)	(.051)	(.012)	(.022)	(.023)	(.002)
Constant	4.100	4.380	3.890	4.430	9.230	10.70	0.113
	(.007)	(.005)	(.016)	(.004)	(.013)	(.013)	(.001)
Average effect for average majority exposure ($3 \cdot \gamma$)	-0.113	-0.204	0.556	0.158	0.193	0.178	0.0190
	(.059)	(.032)	(.154)	(.037)	(.066)	(.069)	(.005)
Main estimated effect	-0.0690	-0.204	0.513	0.151	0.158	0.153	0.0190
	(.06)	(.032)	(.159)	(.038)	(.069)	(.072)	(.005)
TWFE	Y	Y	Y	Y	Y	Y	Y
N	29,522	38,472	31,973	35,220	25,296	24,634	38,206
Unique firms	5,201	5,305	4,762	4,857	4,503	4,479	5,302
Adjusted R2	0.541	0.474	0.620	0.455	0.818	0.803	0.443

Note: This table presents estimates of the model $y_{it} = \alpha_i + \delta_t + \gamma \cdot PredictedSurcharge_i Post_t + \varepsilon_{it}$. The estimation sample here consists of all majority and minority exposure and control firms in our core sample. The row beginning "Implied average effect" corresponds with the point estimate effect evaluated at the mean projected tax surcharge $3 \cdot \hat{\gamma}$, where $3 = \bar{s}_{maj.}$ by definition represents the average predicted surcharge among majority exposure firms. The row "Main estimated effect" presents analogous estimates from Table 1 for reference. Standard errors are clustered on the firm-level. This table is referred to in Section 5.4

Table A.15: Weighted difference-in-differences results: Productive inputs

	Scale			Investment			Wage bill		
	Assets (log)	Positive revenue (binary)	Revenue (log)	Positive investment (binary)	Investment (log)	Investments assets share	Positive payroll (binary)	Payroll (log)	Payroll assets share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure × Post	0.105	-0.00990	0.0288	0.0938	-0.121	0.0422	-0.0984	-0.0821	-0.0125
	(.0508)	(.0157)	(.109)	(.0422)	(.412)	(.014)	(.0333)	(.0716)	(.0071)
Constant	15.90	0.945	15.90	0.317	12.40	0.0525	0.938	13.80	0.223
	(.0284)	(.006)	(.049)	(.0166)	(.193)	(.0065)	(.0079)	(.0357)	(.0034)
TWFE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Weight (2014 assets)	N	N	N	N	N	N	N	N	N
N	28,072	28,364	25,047	28,364	3,747	28,072	28,364	22,896	28,072
Unique firms	3,926	3,926	3,844	3,926	853	3,926	3,926	3,628	3,926
Adjusted R2	0.728	0.289	0.781	0.537	0.741	0.678	0.494	0.879	0.845

Note: This table is an asset-weighted version of Table 5. The table displays the difference-in-differences results evaluating the impact of the reform on assets, investment, and labor expense. Differences in sample sizes across columns arise from either log dependent variables that exclude observations with zero-values (Columns (1), (3), (5), and (8)) or zero-valued denominators (Columns (6) and (9)). Standard errors are clustered on the firm-level. This table is referred to in Section 6.

Appendix B Analysis Using Orbis Data

This section uses ownership data from Orbis to replicate our analysis of the impact of the reform on terminal ownership via tax havens.

B.1 Data

Orbis is a subscription-based global database managed by Bureau van Dijk/Moody's that aims to provide comprehensive financial and corporate information on millions of public and private companies worldwide, including their ownership structures. In what follows we focus on direct ownership, as other definitions of ownership (e.g. intermediate or terminal beyond a single layer) are not always observable or only available for the latest cross-section, rather than on an annual basis.

Our aim is to collect a panel of foreign-owned firms operating in Ecuador. The first year with data in Orbis is 2014 (at least that we have access to). We select Ecuadorian firms that are active and that have foreign shareholders anywhere in the world, regardless of the shares of the direct owner. For each subsidiary we include information on direct ownership such as the name, location, and share of direct owners (at the end of December of each year). We exclude from the sample firms with missing direct ownership shares, and firms with 100% of direct ownership located in Ecuador or in unknown/missing countries.

Our empirical strategy using Orbis mirrors our baseline approach with the APS. We compare Ecuadorian firms with majority direct tax haven ownership in 2014—a modified version of the exposure group—versus firms with majority direct foreign non-haven ownership in 2014—the control group. We classify direct owners in three country groups:

1. Tax havens, following the list of tax haven countries established by the Ecuadorian government, as in the baseline analysis.

2. Foreign non-tax havens: any country not listed as tax haven, excluding Ecuador or any other country labeled as "unknown".

3. Ecuador & Unknown

We collapse shares of direct ownership at the firm × country group × year-level. We then build our exposure and control groups based on direct ownership type, as follows:

- **Majority haven-owned:** firm with $\geq 50\%$ direct ownership in tax haven countries.
- **Majority non-haven foreign-owned:** firm with $\geq 50\%$ direct ownership in foreign non-tax haven country & 0% direct ownership in tax havens.

Sample size and data quality. In 2014 (the final pre-reform year) Orbis lists 499 majority foreign-owned Ecuadorian firms, of which 73 are tax haven-owned and 426 are foreign non-haven-owned. This is a small (and likely selected) subset of the firms we observe in the APS, although the ratio of tax haven- to foreign-owned is similar as in the main analysis. Two cautionary notes are worth highlighting. First, economic data on Ecuadorian subsidiaries are

missing in the vast majority of cases, in contrast to ownership information, which is largely available. Second, the number of Ecuadorian firms covered in Orbis fluctuates considerably over time, showing an overall upward trend, though not a linear one.

To mirror our main estimation strategy, we track forward the 499 subsidiaries identified in 2014. Table B.1 presents the number of these firms appearing in the Orbis data for each subsequent year between 2014 and 2019. Attrition is considerable, with nearly half of the firms (46%) no longer observed by 2016.

Table B.1: Tracking foreign-owned Ecuadorian firms, active in 2014

	2014	2015	2016	2017	2018	2019
All foreign-owned firms	499	451	293	271	328	313
Mostly tax haven-owned	73	64	34	34	35	32
Non tax haven-owned	426	387	259	237	293	281

Note: This table tracks Ecuadorian firms with foreign direct ownership, in the Orbis data. We fix the sample to the 499 firms active in 2014 and count how many are present in the sample each year. The group labeled as "1(mostly tax haven-owned)" are the firms with ≥50% direct ownership in tax haven countries (exposed firms). The group labeled as "1(non-tax haven-owned)" are the foreign-owned firms, with 0% direct ownership in tax havens (control firms).

B.2 Empirical Strategy

The empirical strategy compares the impact of the CIT surcharge on firms directly owned by majority tax haven-owned firms versus majority non-haven-owned firms. Given the unbalanced nature of the panel, we estimate a simple two-period difference-in-differences model comparing outcomes in 2018 (the post-period) and 2014 (the pre-period). We select 2018 as the post period due to its relatively lower attrition rate than 2016 and 2017. The estimating equation takes the standard before-after difference-in-differences form:

$$y_{it} = \alpha_i + \beta \cdot \mathbf{1}(Treatment_i) \times \mathbf{1}(Post_t) + \gamma \cdot \mathbf{1}(Post_t) + \varepsilon_{it} \tag{6}$$

Where y_{it} is the outcome-of-interest for firm i in year t; α_i are firm fixed effects; $\mathbf{1}(Treatment_i)$ takes the value of 1 for tax haven-owned firms and zero for foreign-owned firms. $\mathbf{1}(Post)$ is a dummy equal to zero in 2014 and 1 in 2018. β measures the differential effect of the tax haven surcharge in Ecuador. Standard errors are clustered at the firm-level. As in the baseline estimates in the paper, we mitigate mean reversion in ownership by comparing shares in "baseline domicile group": for treated/exposure firms this is tax havens; for controls firms it is foreign non-havens. Thus, to estimate the impact of the reform on direct ownership, we compare the tax haven ownership share of exposed firms to the international ownership of

control firms. We generate the composite dependent variable y_{it}:

$$y_{it} = \begin{cases} \text{Tax haven ownership} & \text{if } i \in \ 1(\text{Mostly tax haven-owned}) \\ \text{Foreign non-haven ownership} & \text{if } i \in \ 1(\text{Non tax haven-owned}) \end{cases}$$

Our outcomes-of-interest are extensive and intensive margin changes in ownership (y_{it}). We measure extensive margin as having zero observable direct ownership, and the intensive margin as the change in direct ownership shares. We estimate the effects on these two outcomes for two different owner types: all direct owners and the main direct owner at baseline.

We run our estimates using two samples. The first sample includes 328 firms observed in both 2014 and 2018. The second set of estimates is based on a balanced sample by way of imputation, with 471 firms: we impute missing outcome values using the last available year post reform (e.g. missing direct ownership in 2018 is replaced with the latest non-missing value from 2015 to 2017).

B.3 Results

Table B.2 presents the difference-in-differences results. Panel A reports estimates using only the 328 firms in the balanced sample that report ownership in both 2014 and 2018. Panel B presents results based on the imputed sample, where missing ownership values in 2018 were filled using the last available year with ownership data.

Panel (A), Column (1), shows that attrition is large in the Orbis data and substantially larger for the exposure group (21 percentage points higher probability of attrition) than the control group. For firms appearing in both 2014 and 2018, we estimate a marginally significant increase in the probability of leaving their baseline domicile (extensive margin) of 14 percentage points. In addition, we observe a marginally significantly decrease in baseline domicile ownership for exposure firms compared to control firms by 11 percentage points. These results are similar in magnitude, but noisier, than those observed in the paper using the APS.

Panel (B), runs the same regression on the imputed ownership share sample, comparing again 2014 to 2018. The imputation implies that firm ownership data is imputed for 2018 based on the latest available year of data post reform. Column (1) shows that attrition in the Orbis data in any period post reform—i.e. any period from 2015 to 2018—increased by 7 percentage points for exposure firms, compared to control group. Results for changes in direct ownership are smaller in magnitudes and significance to those of Panel (A), but remain qualitatively consistent. The point estimates for changes in baseline domicile ownership (Column 2-3) show a marginally significant increase in having zero baseline domicile ownership by 8 percentage points. Ownership changes concerning the main baseline owner are, however, smaller than in Panel (A) and insignificant (Columns 4-5).

Table B.2: Tax Reform Impact in Orbis Data: DiD Estimates (2014 vs 2018)

		Baseline domicile ownership		Ownership of main baseline owner	
	(1)	(2)	(3)	(4)	(5)
	Attrition	Zero ownership	Ownership share	Zero ownership	Ownership share
Panel A: Balanced Sample					
1(Mostly tax haven owned) × 1(Post)	0.208***	0.144*	-0.106*	0.127	-0.104*
	(0.06)	(0.08)	(0.06)	(0.08)	(0.06)
1(Post)	0.312***	0.171***	-0.024**	0.188***	-0.025**
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
Observations	998	656	656	656	656
# Firms	499	328	328	328	328
R-Squared	0.61	0.56	0.86	0.56	0.87
Control Mean Dep Var 2014	0.00	0.00	0.88	0.00	0.85
Treatment Mean Dep Var 2014	0.00	0.00	0.93	0.00	0.90
Panel B: Imputed Sample Last Available Year					
1(Mostly tax haven owned) × 1(Post)	0.067*	0.083*	-0.052	0.017	-0.051
	(0.04)	(0.05)	(0.03)	(0.05)	(0.03)
1(Post)	0.042***	0.086***	-0.018**	0.153***	-0.018**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Observations	998	942	942	942	942
# Firms	499	471	471	471	471
R-Squared	0.52	0.53	0.91	0.54	0.91
Control Mean Dep Var 2014	0.00	0.00	0.86	0.00	0.83
Treatment Mean Dep Var 2014	0.00	0.00	0.95	0.00	0.91

Note: This table summarizes the difference-in-differences results on firm direct ownership using a pairwise comparison on a sample of firms that are in both years 2014 and 2018. Panel A displays results using the first observed year for the subsidiaries, hence, there are missing values for those subsidiaries that are not in sample in 2018. Panel B displays results using a balanced version of the sample by replacing missing values in 2018 with the observation in the previous (post-reform) year available. In all cases, our treatment group is composed of active Ecuadorian subsidiaries with ≥50% direct ownership in a tax haven in 2014, which we refer to as "mostly tax haven-owned". The dependent variable in column (1) is a dummy taking the value of one if the firm is not in sample in 2018 and zero otherwise. The dependent variable in even columns (2) and (4) named as "zero ownership" is the extensive margin measured by any change in any direct owner and the main direct owner, respectively. The dependent variable in odd columns (3) and (5) named as "ownership %" is the intensive margin measured by changes in the shares of any direct owner and the main direct owner, respectively. Every regression includes firm fixed effects and standard errors are clustered on the firm-level. Significance levels are denoted as * 0.10, ** 0.05, and *** 0.01, respectively.

Appendix C Full model development

C.1 Model

Setup An agent i earns pre-tax income normalized to one and faces a linear income tax at rate τ. She chooses whether to use a tax haven, $E \in \{0, 1\}$, at a fixed cost ξ_i, and if so, what share $e \in [0, 1]$ of income to conceal from the tax authority. We assume that concealment is possible only through haven use, so reported income is $z = 1 - Ee$. Thus, non-users ($E = 0$) report all income, while haven users ($E = 1$) conceal the share e.

The detection probability ρ increases with underreporting. We assume a detection function $\rho(x) := \rho(e)E$ with $\rho'(x) > 0$.[26] If detected, the agent repays the evaded tax τe and pays a proportional penalty $\theta\tau e$, with $\theta > 0$. We impose Inada-type conditions ensuring an interior solution: the marginal cost of evasion is below (above) the marginal benefit at zero (full) evasion. The agent maximizes quasi-linear utility in income:

$$\max_{E\in\{0,1\},\ e\in[0,1]} 1 - \tau + \tau Ee - \xi E - \rho(e)e\tau(1 + \theta)E \tag{7}$$

The agent chooses latent underreporting e^* to satisfy the first-order condition $(\rho'(e^*)e^* + \rho(e^*))(1+\theta) = 1$, which equates the marginal expected benefit and cost of underreporting.[27] Underreporting along this intensive margin declines with the detection probability and the penalty rate but is unaffected by the tax rate or the fixed cost of haven use.[28] However, underreporting is realized only if the agent uses a tax haven at cost ξ_i, which occurs when $U(E = 1, e^*) \geq U(E = 0, e^*)$, i.e. $\xi_i \leq \tau e^* - \rho(e^*)e^*\tau(1 + \theta)$. For a cumulative distribution $F(\cdot)$ of fixed costs, the share of haven users is

$$P(\xi_i \leq \tau e^* - \rho(e^*)e^*\tau(1 + \theta)) = F\Big(\tau e^* - \rho(e^*)e^*\tau(1 + \theta)\Big).$$

This increases with the tax rate and drops with the detection probability and penalty rate.

Modeling the tax surcharge We model the policy as an increase in the tax rate, $d\tau$, on firms based in havens, together with a potential increase in the perceived detection probability, ρ_γ, due to heightened scrutiny of haven-related activities. The agent's problem is:

$$\begin{aligned}\max_{E\in\{0,1\},\ e\in[0,1]} U &= 1 \cdot (1 - \tau + \tau Ee - d\tau \cdot E) - (\xi_i E + \tilde{\rho}(e)e\tau(1 + \theta)E) \\ &= (1 - \tau + \tau Ee) - (\tilde{\xi}_i E + \tilde{\rho}(e)e\tau(1 + \theta)E)\end{aligned} \tag{8}$$

[26] For simplicity, we assume $\rho''(x) = 0$.

[27] The interior solution is a local maximum if $\rho''(e^) + 2\rho'(e^)e^* < 0$. Under linear detection, $\rho''(e) \equiv 0$, this condition holds trivially.

[28] The invariance of evasion to the tax rate follows from quasi-linear utility, the linear scaling of both taxes and penalties in the tax rate, and the absence of convex evasion costs.

Where $\tilde{\xi} = \xi_i + d\tau$ and $\tilde{\rho}(e) = \rho(e) + \rho_\gamma$. This representation of the policy leverages the simplifying assumption of fixed corporate profits, where the three percentage point surcharge effectively combines with the fixed cost of haven usage to raise the direct cost of using havens.

The reform has two effects. Using the implicit function theorem, we can evaluate the impact of changing detection probability on underreporting:

$$\frac{\partial e^*}{\partial \rho_\gamma}|_{E=1} = \frac{-1}{2\rho'(e)} < 0. \tag{9}$$

The increase in subjective detection probability induces a decrease in intensive margin underreporting e^*. The second effect is to induce an extensive margin exit from haven usage through both the increased fixed cost γ and through the lower benefit of haven usage conditional that $E = 1$ due to the decrease in underreporting from the first effect. Section 5.4 studies to what extent we can separate out these two effects of the pecuniary surcharge $d\tau$ ("the stick") and the perceived detection effect ρ_γ ("the flashlight").

Let e_0^* and e_1^* represent agent's optimal underreporting prior to and following the reform (possibly censored by E). An agent making use of tax havens prior to the reform that ceases to use tax havens has the characteristic that $\xi \leq \tau e_0^* - \rho(e_0^*)e_0^*\tau(1+\theta)$ and that $\tilde{\xi} \geq \tau e_1^* - \tilde{\rho}(e_1^*)e^*\tau(1+\theta)$. Here, the increased cost of haven usage and the increased subjective detection probability induces a mass of individuals to exit haven usage.[29] However, individuals with sufficiently low costs or sufficiently high benefit of haven usage will maintain haven use in the post-reform environment. Based on our empirical estimates of the impact of the reform on haven usage, we will be able to calibrate $P\Big(\xi \geq \tau e_1^* - \gamma - \tilde{\rho}(e_1^*)e_1^*\tau(1+\theta)\Big|\xi \leq \tau e_0^* - \rho(e_0^*)e_0^*\tau(1+\theta)\Big)$.

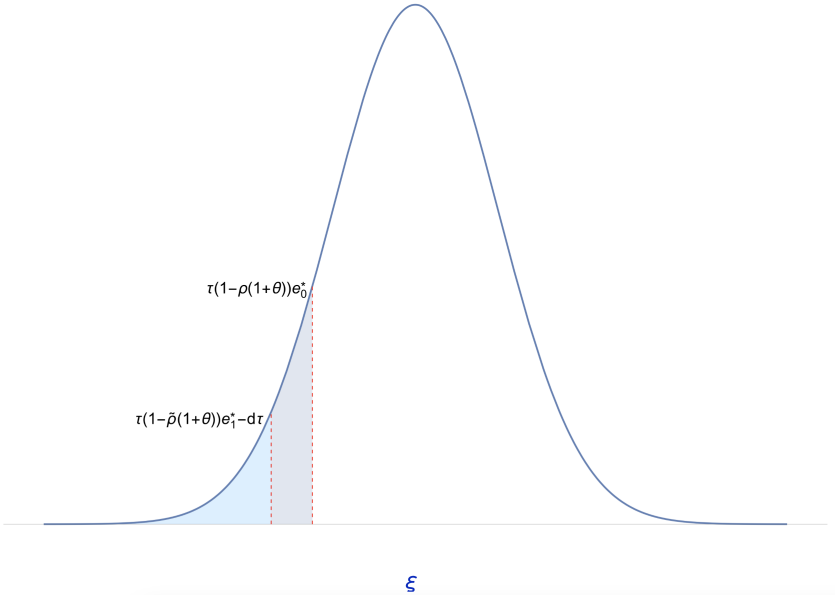
This setup also yields some insight into characterizing the agents that exit from or maintain haven usage. For one, using our transaction data, we can observe which firms were transacting with havens prior to the reform. If we construe this activity as a measure of gross benefit of haven usage, the model would predict greater exit of firms with less tax haven activity. Second, the model yields some prediction for which firms would disclose their true owners based on owner nationality. Consider an asymmetric cost of exit from and disclosure of tax haven usage to the Ecuadorian tax authorities by the agent based on whether she is Ecuadorian or a foreign national. For instance, if the tax haven user were an Ecuadorian individual, disclosure to the tax authorities may *ipso facto* reveal non-compliance and induce audit, whereas this threat is not necessarily present to the same degree for a foreign individual.[30] Moreover, the tax base subject to audit by the Ecuadorian tax authorities likely

[29] Figure C.1 graphically illustrates this mechanism for an arbitrary distribution of fixed costs.

[30] Namely, for a foreign individual disclosing to the Ecuadorian government her beneficial ownership of the Panamanian intermediary, neither the foreign nor the Ecuadorian authorities necessarily know of the agent's evasion of her home country's personal income tax or other non-compliant activity, although the agent may perceive an increased risk of audit or intergovernmental cooperation. This said, the probability of intergovernmental cooperation in the latter scenario is likely weakly less the probability of audit induced by revealed non-compliance in the former scenario.

also differs substantially for foreigners and domestic nationals. We therefore anticipate that the reform will induce revelation of foreign national owners rather than Ecuadorian owners.[31]

Figure C.1: Cutoff points for haven usage based on fixed costs ξ



Note: This figure shows a conceptual illustration for an arbitrary distribution of fixed costs of tax haven usage ξ. In the baseline scenario, for an interior optimum underreporting share e^* and induced audit rate $\rho(e^*)$, all individuals with fixed costs less than $\tau(1-\rho(1+\theta))e^*$ make use of tax havens. In the case corresponding with the policy, individuals with fixed costs in the region less than the baseline upper bound and greater than $\tau(1-\tilde{\rho}(1+\theta))e^* - d\tau$ (corresponding with the shaded darker blue region), are induced to exit tax haven usage, where all individuals with fixed costs less than this latter amount (in the shaded light blue region) maintain their tax haven usage.

The model also offers a set of predictions for the impacts of the policy on taxes paid and on previously non-disclosed profits. Observe that we can express change in tax obligation as

$$\Delta T = \overbrace{d\tau \cdot (1+\Delta E)}^{\substack{Mechanical \\ tax\ change}} + \underbrace{|\Delta(E \cdot e^*)|}_{\substack{Change\ in \\ undeclared\ profit}} \cdot \tau$$

The reform enacts 1) a mechanical effect on revenue collections for firms maintaining haven usage and 2) a behavioral effect on profits declared which also effects revenues.[32] For parsing

[31] We develop a model extension of this scenario in Appendix C.2.

[32] We document that most firms owned in tax havens declare positive profit, although we also explore extensive margin responses of profit declaration. For the purposes of illicit tax haven usage, declaring precisely zero profits (e.g. Bilicka (2019)) would likely trigger audit.

the behavioral effects, note that if E shifts from 1 to 0, tax revenues increase by $\tau \cdot e^*$, as the agent loses access to the evasion technology via tax havens. However, also recall from Equation (9) that a marginal increase in subjective detection probability induces as reported income increase by $\frac{1}{2\rho'(e)}$. Thus, even for agents that maintain haven usage, we anticipate the impact of the policy on increased probability of detection induces a decrease in evasion on the intensive margin. Note that "detection" in this case could indeed refer to detection of non-compliance or more broadly to sustained investigation into other activities or even cooperation with other national tax authorities (if relevant). An agent engages in illicit activity wants to avoid all of any of these outcomes in our setup.

Ex-ante, we anticipate that individuals entirely exiting haven usage decrease their under-reporting more than those that maintain haven usage, who also exhibit a decrease in haven usage.

Impacts of the policy on real activity. By normalizing pre-tax profits to one, we abstracted away from impacts of the policy on real activity. However, we highlight several internally conflicting potential impacts of the reform on real activity.

From the perspective that the surcharge taxes business profit, the tax may increase the cost of capital, placing downward pressure on investment and output (Hassett and Hubbard, 2002; Hall and Jorgenson, 1967). The tax also potentially discourages external financing from entities based in tax havens, further increasing the cost of capital (Auerbach, 2002). These forces suggest a negative impact on investment of exposed firms. However, while wage bill is normally tax deductible so that the surcharge wouldn't have direct effects on labor inputs, scale effects and capital substitution/complementarity effects would indirectly impact labor inputs (Curtis et al., 2021). The scale effect would presumably operate to depress labor inputs, but the sign of latter effect depends on to what extent labor and capital operates substitutes for exposed firms. Additionally, the effects of the reform on inputs will dampen to the extent that such input expenses are fully deductible (Kennedy et al., 2024).

Several forces operate in the opposite direction. The reform may have an impact on dividend outflows paid out to tax haven shareholders (Brounstein, 2025).Under the "trapped equity" view of dividend taxation, firms finance marginal investment through retained earnings, so that changes in dividend payment policy likely affect investment via substitution effects (Yagan, 2015; Chetty and Saez, 2010; Zodrow, 1991). Therefore, to the extent that the reform reduces dividend payments, we may anticipate *positive* investment effects (and then analogous scale and capital-labor substitution/complementarity effects as discussed in Curtis et al. (2021)). Lastly, consider that the fixed cost of tax haven usage represents firm expenditure on "non-productive" activity (Bustos et al., 2022; Chetty, 2009). By reducing firm expense on non-production inputs, the reform may induce a substitution of firms toward "productive" inputs. Finally, the surcharge may operate simply as a tax on firm rents, in which case the policy would have no effect on inputs. However, even in this scenario the surcharge may induce extensive-margin exit by reducing variable post-tax profits relative to possible fixed operational costs.

These forces generate conflicting predictions of the impact of the reform on real activity.

C.2 Extension: asymmetric costs for foreigners versus nationals

This section extends the model to allow for differential costs of tax haven use for foreigners and domestic nationals. The extension helps rationalize why the observed decline in tax haven ownership is driven entirely by foreigners rather than Ecuadorian owners (see Figure 2).

Two mechanisms can generate this result, both operating through the audit channel. First, audit by the Ecuadorian tax authorities likely poses a much greater threat to Ecuadorian nationals than to foreigners. Ecuadorian owners are more likely to have additional domestic tax bases that can be scrutinized following an audit, whereas effective cooperation across tax authorities remains partial.

Second, the *act* of exiting tax haven ownership may reveal information to the Ecuadorian tax authorities, and the implications of this disclosure differ across owner types. An Ecuadorian owner who replaces a haven entity with direct ownership effectively discloses prior haven use to the domestic tax authority, potentially triggering further scrutiny. By contrast, this threat is weaker for foreign owners without substantial tax presence in Ecuador.

We formalize these mechanisms in two ways. First, we allow the effective audit tax rate, $\tilde{\tau}_{m(i)}$, to differ between foreign and domestic individuals. Second, we generalize the audit probability to $\rho(eE, \mathcal{I}_{it})$, where $\mathcal{I}_{it}$ captures additional information available to the tax authority when selecting taxpayers for audit.

For the first mechanism, allow the effective tax rate applied upon audit to differ by owner nationality. Assume that all tax haven users evade personal and corporate income taxes at total rate τ, abstracting from cross-country heterogeneity in statutory rates. However, the Ecuadorian tax authorities can enforce only taxes owed on Ecuadorian-sourced income, so the audit-relevant tax rate differs across owners.

Consider an individual i with fixed nationality $m(i) \in \{\text{Ecuadorian}, \text{Foreigner}\}$. For Ecuadorian individuals, audit can uncover evaded Ecuadorian personal and corporate income taxes, as well as other domestic tax liabilities. Thus,

$$\tilde{\tau}_{Ecuadorian} = \tau = \tau^{PIT}_{Ecuador} + \tau^{CIT}_{Ecuador}.$$

For foreign individuals, by contrast, audit is limited to taxes related to Ecuadorian-source business activity. Hence,

$$\tilde{\tau}_{Foreigner} = \tau^{CIT}_{Ecuador} < \tilde{\tau}_{Ecuadorian}.$$

Second, allow audit selection to depend on a richer information set. Tax authorities observe tax haven use and—when an owner exits a haven structure—the owner's nationality. We can write the subjective audit probability as

$$\rho(eE, \mathcal{I}_{it}) = \rho\left(eE, m(i) \cdot \mathbb{1}\{\Delta E_{it} = -1\}\right).$$

Here, the tax authority either does not observe the individual's nationality, denoted by 0, or observes it when the individual exits a haven structure and discloses prior haven use.

The second argument therefore captures not only nationality, but nationality revealed together with information that the taxpayer previously used a tax haven. We restrict attention to parameterizations satisfying

$$\rho(eE, 0) < \rho(eE, \text{Foreigner}) < \rho(eE, \text{Ecuadorian}) \quad \forall\ eE.$$

Thus, conditional on evasion, disclosure raises audit risk, especially for Ecuadorian nationals. We also assume that any individual revealed to have used a tax haven faces higher audit risk than an individual whose haven affiliation remains unobserved:

$$\rho(eE, 0) < \rho(0, m(i)) \quad \forall\ eE, \quad m(i) \in \{\text{Foreigner}, \text{Ecuadorian}\}.$$

For simplicity, assume that taxpayers only make a decision on extensive margin tax haven usage, treating intensive margin latent underreporting $e > 0$ as fixed. Now, indexing terms by taxpayer i and year t, we can express the modified objective function as

$$\max_{E_{it} \in \{0,1\}} 1 - \tau + \tau E_{it} e - (\xi_i E_{it} + \rho(eE_{it}, m_i \cdot \mathbb{1}\{\Delta E_{it} = -1\}) e \tilde{\tau}_{m(i)}(1 + \theta))$$

We can similarly augment the objective function by incorporating in the policy:

$$\max_{E_{it} \in \{0,1\}} (1 - \tau + \tau E_{it} e) - (\tilde{\xi}_i E_{it} + \tilde{\rho}(eE_{it}, m_i \cdot \mathbb{1}\{\Delta E_{it} = -1\}) e \tilde{\tau}_{m(i)}.(1 + \theta))$$

Selection effects. Introducing nationality-specific effective audit tax rates and nationality-specific audit risks upon haven exit changes the composition of individuals who use tax havens through Ecuador, even before the reform. To see this, we can compare the extensive-margin decision for tax haven use between foreign and Ecuadorian individuals.

Ecuadorians that enter into tax haven usage satisfy the condition:

$$\begin{aligned} U(E_{it} = 1, e; Ecuadorian) &> U(E_{it} = 0, e; Ecuadorian) \\ (1 - \tau + \tau e) - (\xi_i + \rho(e, 0) e \tau (1 + \theta)) &> 1 - \tau - \rho(0, 0) e \tau (1 + \theta) \\ \implies \xi_i &< \tau e - \tau \cdot (1 + \theta) \cdot (\rho(e, 0) - \rho(0, 0)), \end{aligned}$$

whereas the analogous condition for foreigners requires that

$$\begin{aligned} U(E_{it} = 1, e; Foreigner) &> U(E_{it} = 0, e; Foreigner) \\ \implies \xi_i &< \tau e - \tilde{\tau}_{Foreigner} \cdot (1 + \theta) \cdot (\rho(e, 0) - \rho(0, 0)). \end{aligned}$$

Because $(\rho(e,0)-\rho(0,0)) > 0$ and $\tilde{\tau}_{Foreigner} < \tau$, it is necessarily the case, *ceteris paribus*, that Ecuadorians require lower fixed costs to use tax havens (via Ecuador) than do foreigners. I.e. it is more likely that an Ecuadorian business operated via a tax haven is owned by a foreigner rather than by an Ecuadorian.

This selection effect is further compounded when considering dynamic optimization of agents over entry into tax haven usage. In particular, Ecuadorian agents will anticipate greater effective exit costs than will foreign agents (considering the differential audit selection effects of subsequent revelation of tax haven association to the Ecuadorian tax authorities). If prospective haven users consider some fixed probability $\delta > 0$ of having to exit tax haven ownership in the following period, Ecuadorian individuals will internalize a greater discounted probability-weighted cost via the increased audit probability than would foreign individuals when considering their net fixed costs of engaging in tax haven use.

Policy implications. The second effect of this environmental modification affects how the policy incentivizes exit from tax havens for foreigners versus for Ecuadorians. Consider individuals that were using tax havens in the prior period. One such Ecuadorian individual that exits haven usage, revealing themselves to the tax authorities in response to the policy, satisfies the condition:

$$U(E_{it}=0,e;Ecuadorian,E_{it-1}=1) > U(E_{it}=1,e;Ecuadorian,E_{it-1}=1)$$

$$1-\tau-\tilde{\rho}(0,Ecuadorian)e\tau\cdot(1+\theta) > (1-\tau+\tau e)-(\tilde{\xi}_i+\tilde{\rho}(e,0)e\tau\cdot(1+\theta)).$$

We can further write that the marginal Ecuadorian incumbent tax haven user is characterized as

$$\tilde{\xi}_i^{Ec.}-\tau e = \Big(\tilde{\rho}(0,Ecuadorian)-\tilde{\rho}(e,0)\Big)\cdot e\tau\cdot(1+\theta)),$$

where individuals with equality replaced by "less than" are induced to remain in haven usage, with ownership concealed from the tax authorities. This expression shows that the mass of remaining haven users increases in the relative audit risk posed by disclosure/revelation to the tax authorities as well as the gross penalty associated with successful audit by the tax authorities (assuming that the difference term is positive).

Importantly, we can observe that mass of haven remainers (within the distribution of Ecuadorian haven users) $F_{Ec.}\left(\tau e_{it}^* + \Big(\tilde{\rho}(0,Ecuadorian)e_{it}^* - \tilde{\rho}(e_{it}^*,0)\Big)\cdot e_{it}^*\tau\cdot(1+\theta)\right)$ will be greater than that for the distribution of foreign haven users:

$$\begin{aligned}\tilde{\xi}_i^{Ec.} - \tau e &= \Big(\tilde{\rho}(0, Ecuadorian)e - \tilde{\rho}(e,0)\Big) \cdot e\tau \cdot (1+\theta)) \\ &> \Big(\tilde{\rho}(0, Foreigner)e - \tilde{\rho}(e,0)\Big) \cdot e\tau \cdot (1+\theta)) \\ &> \Big(\tilde{\rho}(0, Foreigner)e - \tilde{\rho}(e,0)\Big) \cdot e\tilde{\tau}_{Foreigner} \cdot (1+\theta)) \\ &= \tilde{\xi}_i^{Foreign} - \tau e.\end{aligned}$$

I.e. the marginal individual induced into exiting from haven ownership is located at a point of greater haven fixed costs for Ecuadorians than for foreigners. Assuming that the distribution of fixed cost of haven use is independent of nationality status, this result implies that a relatively greater share of foreigners would choose to exit tax haven ownership in this scenario than would Ecuadorians.

Another way to see this result in the lens of the policy is by identifying the mass of individuals for whom the policy induces tax haven exit, and how this mass directly varies with nationality status. For Ecuadorians, this mass can be expressed as

$$\mathbb{P}\left(\tau e - d\tau - \Big(\tilde{\rho}(e,0) - \tilde{\rho}(0, Ecuadorian)\Big)\tau e(1+\theta) < \xi_i < \tau e - \rho(e,0)\tau e(1+\theta)\right) =$$

$$\overbrace{F\Big(\tau e - \rho(e,0)\tau e(1+\theta)\Big)}^{\text{Using havens before reform}} - \overbrace{F\Big(\tau e - d\tau + \Big(\tilde{\rho}(0, Ecuadorian) - \tilde{\rho}(e,0)\Big)\tau e(1+\theta)\Big)}^{\text{Using havens after reform}},$$

whereas for foreigners, this mass can be expressed as

$$F\Big(\tau e - \rho(e,0)\tilde{\tau}_{foreigner}e(1+\theta)\Big) - F\Big(\tau e - d\tau + \Big(\tilde{\rho}(0, Foreigner) - \tilde{\rho}(e,0)\Big)\tilde{\tau}_{foreigner}e(1+\theta)\Big).$$

The first term in each of these expressions gives the upper bound fixed cost for Ecuadorians and foreigners respectively that use tax havens prior to the reform. We can observe that because $\tilde{\tau}_{foreigner} < \tau$, this upper bound is greater for foreigners than for Ecuadorians.

The lower bound also differs between foreigners and Ecuadorians, since $\tilde{\tau}_{foreigner} < \tau$ and $\tilde{\rho}(e,0) < \tilde{\rho}(0, Foreigner) < \tilde{\rho}(0, Ecuadorian)$. Namely the marginal tax haven user after the reform has lower fixed costs of haven usage for foreigners than for Ecuadorians. The range of taxpayers induced to exit haven usage by the reform is unambiguously wider for foreigners than for Ecuadorians.

Based on the relative sizes of $\tilde{\rho}(0, Ecuadorian), \tilde{\rho}(0, foreigner), \tilde{\rho}(e,0)$, and the distribution of fixed costs $F(\xi)$, this disclosure effect may entirely mitigate the disclosure/haven exit incentives of the policy for Ecuadorians. In practice, this result is precisely what we observe, where counterfactually, no Ecuadorians disclose ownership via tax havens (opting instead to pay the surcharge $d\tau$), and all the tax haven owners that reveal themselves to the tax authorities are foreign. This result follows intuitively if $\tilde{\rho}(0, Ecuadorian) >> \tilde{\rho}(0, Foreigner)$, which very likely holds if $\tilde{\rho}(0, Ecuadorian) \approx 1$ and $\tilde{\rho}(0, Foreigner)$ is very low.