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**Tax Rules and Capital Reallocation: Real Effects of
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Coûts commerciaux climatiques : événements météorologiques extrêmes, transport et chaînes d’approvisionnement¹

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Résumé : Les gouvernements du monde entier renforcent le contrôle fiscal des entreprises afin de protéger leurs recettes budgétaires, mais l’impact de ces politiques sur l’allocation interne du capital au sein des firmes multinationales reste peu exploré. Cet article examine les effets réels et fiscaux des règles de limitation des charges financières (Earnings Stripping Rules - ESR)—une réforme majeure limitant le transfert de bénéfices par l’endettement, désormais adoptée par plus de 90 juridictions. À l’aide d’un large panel de filiales et de plusieurs protocoles de recherche, je montre que l’adoption des ESR entraîne de fortes augmentations des bénéfices déclarés et des taux d’imposition effectifs, mais réduit simultanément l’investissement et l’emploi locaux. Les groupes multinationaux compensent ces baisses en réallouant leur activité vers des filiales situées dans des juridictions non adoptantes, préservant ainsi une marge d’échappatoire internationale. De manière notable, ce schéma de réallocation s’inverse lorsque l’adoption des ESR est coordonnée à l’échelle mondiale : les filiales sœurs nationales se développent, et la réallocation à l’étranger cesse. Ces résultats démontrent que les effets réels du contrôle fiscal dépendent de manière cruciale de la coordination des politiques, ce qui détermine si les règles anti-évitement faussent la géographie du capital mondial ou s’ils génèrent des recettes fiscales sans déplacer l’activité économique à l’étranger.

Mots-clés : Évasion fiscale, Investissement multinational, Transfert de bénéfices

Tax Rules and Capital Reallocation: Real Effects of Anti-Tax Avoidance Policies

Abstract: Governments worldwide are strengthening corporate tax enforcement to protect revenue, yet the impact of these policies on internal capital allocation within multinational firms remains underexplored. This paper examines the real and fiscal effects of Earnings Stripping Rules (ESR)—a major reform limiting debt-based

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profit shifting, now adopted by over 90 jurisdictions. Using a large affiliate-level panel and multiple research designs, I document that ESR adoption triggers sharp increases in reported profits and effective tax rates, but simultaneously reduces local investment and employment. Multinational groups offset these declines by reallocating activity to affiliates in non-adopting jurisdictions, effectively preserving an international escape margin. Notably, this reallocation pattern reverses when ESR adoption is coordinated globally: domestic sister affiliates expand, and foreign reallocation ceases. These findings demonstrate that the real effects of enforcement depend critically on policy coordination, determining whether anti-avoidance rules distort the geography of global capital or raise revenue without displacing economic activity abroad.

Keywords : Tax Avoidance, Multinational Investment, Profit Shifting

JEL Codes: F23, H25, H26

I Introduction

Tax avoidance by multinational enterprises (MNEs) reduces global corporate income tax revenue by USD 100–240 billion per year — equivalent to 4–10% of worldwide corporate tax receipts (OECD, 2024). To curb such practices, governments have implemented a wide range of anti-avoidance rules. Although effective in principle, these policies often raise capital costs (Bilicka *et al.*, 2022), depress investment and operations (Djankov *et al.*, 2010), and reduce employment (Serrato, 2018). Therefore, it remains an empirical question: when can anti avoidance policies effectively limit tax avoidance without stifling investment and economic growth?

This paper addresses this question by studying the causal impacts of Earnings Stripping Rule (ESR), a key anti-avoidance measure adopted by more than 90 jurisdictions within the last decade (OECD, 2025). ESR targets one of the prominent profit-shifting channels: internal debt shifting. Because interest expenses are deductible while interest income is taxable, MNEs have incentives to load debt into high-tax affiliates and shift profits to low-tax jurisdictions through interest payments (Graham, 1996; Mooij & Hebous, 2017).

The ESR policy caps a firm’s net financial expenses, above a *de minimis* threshold, at 30% of its Earnings Before Interest, Tax, Depreciation, and Amortization (EBITDA), disallowing tax benefits on interest expenses beyond this threshold (OECD, 2015). By directly restricting the tax advantage of debt, the policy creates a trade-off between tax revenue goals and economic expansion: while ESR can increase tax revenues by taxing interest payments, it may also raise the user cost of debt-financed capital and hence, discourage investments by MNEs. This tension between revenue objectives and real activity makes ESR an ideal setting to study whether anti-avoidance rules can be both effective and non-distortionary.

This study shows that the answer depends critically on the extent to which the policy is coordinated across the multinational network. While unilateral adoption allows MNEs to maintain an international escape margin by reallocating capital to non-adopting jurisdictions, I find that this margin collapses—and eventually reverses—once the rule is adopted across a group’s global footprint.

The analysis draws on affiliate-level and consolidated-group data from ORBIS spanning 2010-2021 and covering more than 170,000 MNE affiliates across forty countries. ORBIS is particularly well suited for this purpose for two reasons. First, its detailed ownership information allows precise identification of multinational group structures. Second, the availability of both consolidated and unconsolidated financial statements makes it possible to trace debt, investment, employment, and reported profits within affiliates and across entire groups.

The empirical strategy exploits the staggered adoption of ESR across countries and relies on difference-in-differences research design. I conduct the analysis at both the affiliate

and group levels to capture direct effects as well as broader intra-group adjustments. At the affiliate level, treated firms are those whose three-year pre-reform average financial expenses exceed the ESR cap. Because the reform may induce MNEs to reallocate debt and real activity away from constrained affiliates toward unconstrained ones within the same group (Wilson, 2009; Giroud & Rauh, 2019), I separately identify sister firms — affiliates belonging to the same group as treated firms but not exceeding the cap. Firms in groups without any treated affiliates serve as control firms. The empirical design therefore compares treated affiliates to control firms to estimate the direct effects of ESR, and sister firms to control firms to capture reallocation within multinational groups. At the group level, I classify MNEs with at least one treated affiliate as treated groups. Across all specifications, identification relies on the assumption that treated and control units would have followed parallel trends absent the reform. I assess this assumption using event-study estimates that trace outcome dynamics around the policy change.

Before turning to the causal analysis, I document several stylized facts that illustrate how ESR binds. First, prior to the reform, treated firms display a distinct distribution of profits relative to assets, with substantial mass at zero and negative values — patterns consistent with aggressive tax avoidance (Bilicka, 2019). After ESR adoption, this distribution shifts to the right, i.e., the share of treated firms reporting negative or zero profits declines while that reporting positive profits increases. Effective tax rates move in parallel, increasing visibly and persistently among treated firms across the size distribution. These non-parametric shifts indicate reform-induced compression of tax avoidance. Additionally, the distribution of treated firms' financial-expense-to-EBITDA ratios shows pronounced post-reform bunching just below the ESR cap, consistent with firms actively adjusting their financing behaviour to remain within the allowable limit.

The event-study results reinforce these patterns. Relative to control firms, ESR substantially reduces tax avoidance at treated affiliates but at the cost of lower investment (Mintz & Smart, 2004). By limiting interest deductibility, the reform increases the user cost of debt-financed capital (Hall & Jorgenson, 1967). Treated firms respond with sharp and persistent declines in debt and interest expenses, leading to higher reported profits and effective tax rates. At the same time, higher capital costs reduce real activity: investment falls by roughly 11 percent, with comparable declines in employment and capital.

These contractionary effects at treated affiliates do not translate into declines at the group level. Instead, MNEs reallocate resources toward unconstrained affiliates: investment at sister firms rises by about 9%, accompanied by similar increases in employment. This reallocation is strongly asymmetric. Nearly all of the expansion occurs in foreign sister affiliates, particularly in high-tax countries that have not adopted ESR and where interest remains deductible. This pattern aligns with strategic relocation of financial and operating activities to jurisdictions where the tax advantage of debt remains intact (Graham, 1996;

Giroud & Mueller, 2015). When adoption is partial, ESR therefore curbs domestic tax avoidance but induces capital reallocation toward non-adopting countries.

The central contribution of this paper is to show that this reallocation pattern fundamentally reverses when ESR adoption becomes coordinated across jurisdictions. When all affiliates within an MNE are subject to ESR—either because the group operates exclusively in adopting countries or because policy coordination expands to cover its full geographic footprint—the escape margin collapses. With debt costs rising uniformly across affiliates, reallocating activity abroad no longer yields a tax advantage. For these groups, investment at domestic sister firms increases by about 11 percent, while foreign investment remains unchanged. Firms re-optimize capital allocation entirely within domestic boundaries, channelling it toward unconstrained domestic units. Thus, under coordinated adoption, ESR raises taxable income without displacing real activity abroad.

To further isolate the role of coordination, I replace the binary treatment indicator with a continuous measure of exposure and interpret ESR as a process of expanding policy coverage within multinational groups. As a larger share of a group’s affiliates becomes subject to ESR, opportunities to exploit cross-country tax differences progressively shrink, narrowing the scope for reallocation. The treatment-intensity results confirm this mechanism. At low exposure, MNEs shift investment toward foreign affiliates. As exposure increases, this response weakens, reverses, and ultimately yields a strictly domestic reallocation. The magnitudes are economically meaningful. When two additional countries adopt ESR within a ten-country MNE, domestic investment rises by about 8 percent while foreign investment falls by 6 percent. Moving from zero to full exposure raises domestic sister-firm investment by 14 percent and reduces foreign investment by 13 percent, indicating that each additional adopter compounds the containment of capital flight.

Evidence at the consolidated group level reinforces these findings. Total group-level investment and employment remain stable after ESR adoption, indicating that the policy reallocates activity within multinational groups rather than reducing overall scale. In contrast, group-level reported profits increase once interest deductions are capped, confirming that ESR limits tax avoidance even in aggregate accounts. Although substitution toward other avoidance channels cannot be ruled out (Dharmapala, 2014), such behaviour would likely attenuate the estimated effects. Taken together, the group-level results show that coordinated ESR broadens the tax base without reducing total economic activity once internal reallocation is taken into account.

I also complement the difference-in-differences analysis with a regression discontinuity design around the de minimis threshold. The RD compares changes in outcomes before and after the reform as a function of firms’ pre-reform average financial expenses. Unlike difference-in-differences, the RD relies on the testable assumption that firms cannot precisely manipulate their pre-reform averages to sort just below the threshold (Lee & Lemieux,

2010). Graphical evidence shows no indication of manipulation, and the RD estimates reveal sharp discontinuities in debt, reported profits, investment, and capital at the cut-off. Firms farther above the threshold exhibit larger responses, consistent with the policy biting more strongly for high-interest firms. Extensive robustness checks—including alternative staggered DiD estimators, balanced samples, leave-one-out designs across industries and countries, sensitivity to trimming and winsorization, and tests for mean reversion and business cycles—confirm that the findings are stable and not driven by functional-form choices.

Taken together, these results show that the effectiveness of anti-avoidance rules depends critically on the extent of international coordination (Sørensen, 2004; Johannesen, 2022; Bilicka *et al.*, 2023; Devereux, 2023). Unilateral ESR adoption curbs avoidance but induces capital reallocation toward non-adopting jurisdictions. As coordination expands, the escape margin collapses, reallocation shifts domestically, and reported profits rise without harming real activity. In the limit, multilateral adoption transforms ESR from a policy that risks eroding domestic investment into one that both protects the tax base and improves the internal allocation of capital within multinational firms.

This paper contributes to several strands of literature. To the best of my knowledge, it is the first empirical study to show that the real effects of anti-avoidance rules depend critically on the extent of international coordination. While existing studies document how tax enforcement shapes global movements of profits, investment, and employment within MNEs (Serrato, 2018; Clifford, 2019; Liu, 2020; Gauß *et al.*, 2024; Laudage Teles *et al.*, 2023), I show that coordinated enforcement can raise revenues without reducing investment, complementing the policy objectives under BEPS Pillars 1 and 2 (OECD, 2023). Second, it contributes to the literature on intra-group reallocation (Cummins *et al.*, 1996; Desai & Dharmapala, 2011; Giroud & Rauh, 2019; Akcigit *et al.*, 2022; Link *et al.*, 2024). The staggered and heterogeneous global adoption of ESR allows me to observe the full reallocation response as a group's exposure increases from zero to complete coverage.

In addition, this paper extends the literature on thin capitalization rules. Prior studies examine limits on interest deductibility within single jurisdictions — such as Norway (Andresen & Thorvaldsen, 2023), the UK (Bilicka *et al.*, 2022), Uganda (Bashir *et al.*, 2024) and the US (Goodman *et al.*, 2025; Sanati & Beyhaghi, 2025) — where rules differ in design or scope. In contrast, I study a largely harmonized ESR framework adopted across many countries following the OECD recommendation, which allows me to isolate how a common policy design operates as its coverage expands across the multinational network. Furthermore, it enhances the understanding of tax-motivated debt-shifting practices of MNEs by offering novel evidence that, although anti-avoidance measures may reduce debt holdings and investments in MNE firms, they do not result in a complete exit from the enforcement country (Desai *et al.*, 2004; Huizinga *et al.*, 2008; Huizinga & Laeven, 2008; Mintz & Weichenrieder, 2010; Buettner *et al.*, 2012).

The remainder of this paper is organized as follows: Section II describes the institutional details of ESR. Section III presents the theoretical framework and derives testable predictions. Section IV outlines the empirical strategy, and Section V describes the data. Section VI documents a few stylized facts, Section VII presents the empirical results, and lastly, Section VIII concludes.

II Policy Background

A central concern in corporate taxation is the asymmetric treatment of debt and equity. Because interest payments are deductible from taxable income, while returns to equity are not, firms have a tax incentive to finance operations with debt. For MNEs, this asymmetry creates scope for strategic debt allocation across affiliates: internal loans can be directed to high-tax jurisdictions, where interest payments reduce taxable profits, while the corresponding income is booked in low-tax affiliates, where it faces a lighter tax burden (Hall & Jorgenson, 1967; Graham, 1996).¹ This practice, known as tax-motivated debt shifting, erodes the tax base of high-tax countries and can be achieved with limited changes in real activity in some settings (Huizinga *et al.*, 2008; De Mooij, 2012).²

To address this, the OECD’s Base Erosion and Profit Shifting (BEPS) project—through Action 4—recommended in 2015 the adoption of Earnings Stripping Rule (ESR). This rule links interest deductibility directly to earnings, tying tax relief to a firm’s capacity to generate income rather than to its balance sheet composition. Under the OECD’s recommended design, ESR comprises two central elements: a de minimis threshold (set by each jurisdiction; typically EUR 3 million) and a fixed ratio (within a 10–30% corridor of EBITDA; most jurisdictions adopt 30%). Financial expenses that exceed both of these thresholds are inadmissible for tax deduction purposes.³ These excess expenses can generally be carried forward in future years, and unused capacity can often be carried back or shared within the group under specific conditions.

From an economic perspective, this policy raises the marginal cost of debt-financed capital. For firms close to the deductibility cap, an additional dollar of debt no longer

¹ Such practice can potentially lead to negative effective marginal tax rates on debt-financed investments (Dharmapala, 2014). Notably, South Africa experienced USD 357 million in forgone tax revenue in 2011 solely due to corporate interest deductions (Readhead, 2017).

² Early attempts to contain such behaviour relied on Thin Capitalization Rules (TCR), which set maximum allowable debt-to-equity ratios. The idea was to prevent excessive leveraging in high-tax jurisdictions by restricting how much debt could be recognized for tax purposes. In practice, however, TCR proved both rigid and relatively easy to circumvent. Multinationals could redesign financial structures, reclassify hybrid instruments, or adjust internal equity injections to remain within prescribed leverage limits. Moreover, by tying the constraint to the stock of capital rather than the firm’s capacity to service debt, TCR often penalized legitimate borrowing by capital-intensive firms and left scope for manipulation through accounting conventions (Johannesen, 2014; Crivelli *et al.*, 2016).

³ In most jurisdictions, ESR applies to “financial expenses,” a category that includes contractual interest as well as payments economically equivalent to interest (e.g., guarantee fees, bank charges, etc.).

yields a full interest tax shield—reducing the post-tax return to debt relative to equity or retained earnings. This effectively narrows the tax advantage of debt, discouraging excessive leveraging in high-tax jurisdictions. However, it also means that highly leveraged firms may face a higher user cost of capital, potentially reducing investment. Thus, the ESR introduces a classic trade-off: it strengthens the integrity of the tax base but may distort financing and investment decisions (Modigliani & Miller, 1963; Auerbach & Hassett, 1992).

Following the OECD recommendation, the European Union’s Anti-Tax Avoidance Directive (ATAD) of 2016 mandated member states to implement an ESR by the end of 2018, with non-compliance subject to infringement proceedings (Deloitte, 2021). Outside the EU, countries including the United States, the United Kingdom, and Japan adopted similar provisions. Importantly, some countries—such as Germany and Italy (2008), Norway (2014), and Spain (2015)—had already implemented comparable rules prior to BEPS, but the OECD and EU coordination substantially accelerated adoption. By 2025, nearly ninety jurisdictions (including forty middle and lower income countries) had enacted ESR-type policy (OECD, 2025). Additional country-level policy details are provided in Online Appendix.

This coordinated global rollout is central to the empirical design of this paper. The country-level ESR announcement serves as a plausibly exogenous shock. For EU member states, the 2019 implementation deadline was externally mandated by ATAD, while for many other countries, adoption followed through domestic legislative processes rather than firm-level conditions. Empirically, pre-trend tests show no differential trends between treated and control firms prior to ESR adoption. In addition, the results remain robust to the inclusion of time-varying country-level controls that absorb concurrent macroeconomic and fiscal shocks. While implementation details slightly differ across jurisdictions—such as the de minimis threshold, ratio threshold, and escape clauses—the core mechanism remains consistent: ESR caps the share of earnings that can be shielded through financial expenses, curbing debt-based profit shifting while retaining flexibility for legitimate borrowing. These institutional features provide a natural quasi-experimental setting to examine how firms adjust their financial and real decisions when the tax advantage of debt declines across countries.

III Conceptual framework

This section develops a model that clarifies how earnings-stripping rules alter financing choices, the user cost of capital, and the cross-border and within-country reallocation of MNE investment. The framework builds on Mintz & Smart (2004) and Huizinga *et al.* (2008) but departs from the standard treatment in two ways. First, the rule is modelled as a cap on the deductibility of *total* net interest expense that applies at the level of the legal entity and binds only above a statutory allowance. Second, time and space are explicit:

a baseline period with full interest deductibility is followed by a policy period in which a subset of countries adopt ESR, and displaced capital can move either across borders or across co-located affiliates.

III.1 Environment

An MNE operates a fixed set of affiliates $j \in J$, each incorporated in a country $c(j) \in \{1, \dots, M\}$ with statutory corporate tax rate $\tau_c \in [0, 1)$. Let $J_c \subseteq J$ denote the affiliates located in country c . Time is discrete with two dates $t \in \{0, 1\}$. At $t = 0$ interest is fully deductible everywhere. At $t = 1$ a subset of countries $S \subseteq \{1, \dots, M\}$ adopt ESR with $\tau_c > 0$ for every $c \in S$ (a deductibility limit binds only under a positive rate).

Note that ESR do not change statutory rates and do not tax interest; they cap the amount of net interest expense an entity may deduct. In other words, at $t = 1$, entity j in an adopting country may deduct its interest expense only up to an allowance

$$(1) \quad A_j^1 = \max\{\bar{A}_c, \delta_c \bar{y}_j\}, \quad c = c(j) \in S,$$

where $\bar{A}_c$ is a de minimis threshold, δ_c is the statutory earnings share (e.g. 30% of EBITDA), and $\bar{y}_j$ is the affiliate's predetermined operating income. For $c \notin S$, and for all countries at $t = 0$, $A_j^t = +\infty$.

III.2 Technology and internal organization

Production possibilities are attached to countries, not to legal entities. Total capital deployed by the group in country c is $K_c = \sum_{j \in J_c} K_j$, and it generates operating income $F_c(K_c)$ with $F_c(0) = 0$, $F'_c > 0$, $F''_c < 0$, and $F'_c(0)$ large enough that all markets are served. Affiliate-level operating income is the pro-rata share of the country total, $y_j = (K_j/K_c) F_c(K_c)$.

Assumption 1. *Capital is perfectly substitutable across affiliates located in the same country: output, factor markets, and investment opportunities depend on K_c only, not on its division across the entities in J_c .*

Assumption 1 formalizes the observation that the legal entity is a tax and liability boundary, not a technological one: two affiliates of the same group in the same country face the same product market, the same labour market, and the same group technology, so which entity's balance sheet carries a machine is irrelevant for production. No such substitutability is assumed *across* countries: each F_c is a separate concave technology, reflecting the fact that affiliates in different countries serve different markets.

III.3 Financing and exposure

Affiliate j finances its capital K_j with debt $d_j = b_j K_j$, $b_j \in [0, 1]$, and equity $(1 - b_j)K_j$. Debt carries the market interest rate i and the opportunity cost of equity is r . Carrying

leverage entails the usual non-tax costs (expected distress, monitoring, agency costs of debt), summarized by $m(b_j)K_j$ with m strictly convex, $m(0) = m'(0) = 0$ and $m'(b) \rightarrow \infty$ as $b \rightarrow 1$. The after-tax cash flow of affiliate j at date t is

$$(2) \quad \pi_j^t = (1 - \tau_{c(j)}) y_j - r(1 - b_j)K_j - i b_j K_j + \tau_{c(j)} \min\{i b_j K_j, A_j^t\} - m(b_j)K_j,$$

where the fourth term is the value of the interest deduction: the full shield $\tau i b_j K_j$ when the cap is slack, and the capped amount τA_j^1 when it binds. The parent maximizes group profits $\Pi^t = \sum_{j \in J} \pi_j^t$ subject to a group capital budget

$$(3) \quad \sum_{j \in J} K_j = \bar{K}.$$

Constraint (3) captures the allocation of a given stock of productive capital across a fixed network of affiliates. An affiliate is *exposed* if its pre-reform interest expense exceeds its date-1 allowance,

$$(4) \quad B_j = \mathbf{1}\{i b_j^0 K_j^0 > A_j^1\},$$

where (b_j^0, K_j^0) are the date-0 optima characterized below. Note that the affiliates below their allowances retain full deductibility even in adopting countries, and the cap removes deductibility only *at the margin*—inframarginal interest up to A_j^1 remains deductible.

III.4 The pre-reform benchmark

At $t = 0$ the cap is slack everywhere and (2) separates. Define the unit financing cost and its minimized value at marginal deduction rate $s \in [0, \tau_c]$:

$$(5) \quad u(b; s) = r(1 - b) + i(1 - s)b + m(b), \quad \omega(s) = \min_{b \in [0, 1]} u(b; s),$$

with minimizer $b^*(s)$ solving $m'(b) = r - i(1 - s)$. Three properties follow immediately: $b^*(s)$ is strictly increasing; by the envelope theorem $\omega'(s) = -i b^*(s) < 0$; and the value of full deductibility,

$$(6) \quad \Delta(\tau) \equiv \omega(0) - \omega(\tau) = \int_0^\tau i b^*(s) ds > 0,$$

is strictly increasing and strictly convex in τ . $\Delta(\tau)$ is the annual tax-shield value, per unit of capital, of deducting interest at rate τ .

Lemma 1. *At $t = 0$: (i) every affiliate levers to $b_j = b^*(\tau_{c(j)})$, so the leverage ratio and interest expense per unit of capital are increasing in the statutory rate; (ii) country capital solves*

$$(7) \quad (1 - \tau_c)F'_c(K_c) = \omega(\tau_c) + \eta \quad \text{for all } c,$$

where η is the multiplier on (3); (iii) the division of K_c across co-located affiliates is indeterminate.

Proof. See Appendix A

Part (iii) notes that under full deductibility the group is indifferent as to which local entity carries capital and debt. The pre-reform division $\{K_j^0\}_{j \in J_c}$ is therefore inherited from history and exposure (4) is measured on it; because co-located affiliates share the leverage ratio $b^*(\tau_c)$, exposure within a country is a pure size cut-off, consistent with the de-minimis-based measurement.

III.5 Effects of exposure on debt, investment, and taxes

At $t = 1$, fix an exposed affiliate j ($c(j) \in S$, $B_j = 1$) and write $\tau = \tau_{c(j)}$. Interest up to A_j^1 remains deductible, but the *marginal* euro of interest is not: the affiliate's marginal deduction rate falls from τ to zero.

Proposition 1 (Direct effects). *At date 1, an exposed affiliate j :*

- (i) *Reduces its leverage from $b^*(\tau)$ to $b^*(0) < b^*(\tau)$: total debt and interest expense fall strictly and the equity share rises strictly.*
- (ii) *Faces a marginal cost of capital that rises from $\omega(\tau)$ to $\omega(0)$ —an increase of $\Delta(\tau)$ —and its capital falls relative to unexposed affiliates.*
- (iii) *Deducts exactly $A_j^1 < i b_j^0 K_j^0$, so taxable income rises relative to operating income, tax payments per unit of operating income rise, and the average effective tax rate rises.*

Proof. See Appendix A

The quantity response in (ii) inherits the monotonicity of the cost shock in the statutory rate provided technologies are comparable across countries: with similar curvature at the pre-reform allocation, $dK \approx -\Delta(\tau)/[(1 - \tau)|F''|]$, and $\Delta(\tau)/(1 - \tau)$ is increasing in τ .

Prediction 1: *The introduction of ESR reduces total debt and interest expense at exposed affiliates and increases their equity share.*

Prediction 2: *Following the enforcement of ESR, investment within exposed affiliates declines relative to comparable unexposed affiliates.*⁴

Prediction 3: *Exposed affiliates' taxable income rises relative to their operating income, and their tax payments and average effective tax rates increase.*

⁴ The model omits labour for parsimony. Curtis *et al.* (2021) establish the complementarity between capital investment and labour demand; labour can accordingly be added to the country technology as a complementary input at no additional cost, with affiliate employment co-moving with installed capital and delivering analogous predictions for employment.

III.6 Reallocation and the escape margin

Proposition 1 establishes that exposure displaces capital; this subsection characterizes its destination. Lemma 2 first ensures that marginal comparisons evaluated at the pre-reform allocation sign the equilibrium reallocation.

Lemma 2. *The date-1 group problem is globally concave, an optimum exists, and country-level capital aggregates are unique. Relative to date 0, the shadow value of group capital weakly falls, country capital weakly rises in every country whose marginal deployment can still be financed at full deductibility, and the countries whose marginal financing cost rose lose capital in aggregate.*

Proof. See Appendix A

Define the marginal group value of capital deployed through affiliate h ,

$$(8) \quad v_h = (1 - \tau_{c(h)})F'_{c(h)}(K_{c(h)}) - \omega_h, \quad \omega_h = \begin{cases} \omega(\tau_{c(h)}) & \text{if } h \text{ retains marginal deductibility,} \\ \omega(0) & \text{if } h \text{ is exposed.} \end{cases}$$

By Lemma 1(ii), $v_h = \eta$ for every affiliate at the pre-reform allocation: cross-affiliate productivity differences are arbitrated away before the reform arrives. Evaluated at the pre-reform allocation, the reform changes v_h only through ω_h :

$$(9) \quad v_h = \begin{cases} \eta & \text{if } h \text{ retains marginal deductibility,} \\ \eta - \Delta(\tau_{c(h)}) & \text{if } h \text{ is exposed.} \end{cases}$$

The marginal net benefit of redeploying a unit of capital, together with its re-optimized financing, from an exposed affiliate j to a candidate host h is therefore $v_h - v_j$.

Fix an exposed origin j and let $c^* \equiv c(j)$. Candidate hosts come in four kinds: (a) an *unexposed local* affiliate $\ell \in J_{c^*}$ (below its allowance); (b) an *unexposed foreign affiliate in a no-policy country* ($c(k) \notin S$); (c) an *unexposed foreign affiliate in an adopting country* ($B_{k'} = 0$, $c(k') \in S$); and (d) an *exposed* affiliate anywhere. The escape margin is *open* if the group's network contains a host of kind (b), and *closed* if all of the group's foreign affiliates are exposed.

Proposition 2. *Evaluated at the pre-reform allocation, the marginal gains from redeploying a unit of displaced capital from an exposed affiliate j are:*

$$(10) \quad v_h - v_j = \begin{cases} \Delta(\tau_{c^*}) > 0 & h \text{ of kind (a), up to allowance headroom,} \\ \Delta(\tau_{c^*}) > 0 & h \text{ of kind (b), unbounded,} \\ \Delta(\tau_{c^*}) > 0 & h \text{ of kind (c), up to allowance headroom,} \\ \Delta(\tau_{c^*}) - \Delta(\tau_{c(h)}) & h \text{ of kind (d).} \end{cases}$$

Consequently:

- (i) **Escape margin open.** Every unexposed margin strictly dominates every exposed destination. The margins of kinds (a) and (c) are bounded by allowance headroom, $A_h^1 - i b_h K_h \geq 0$; the no-policy margin (b) is unbounded. Displaced capital beyond aggregate headroom therefore crosses the border to no-policy hosts, and the treated country loses capital. Under comparable host technologies, re-equalization of marginal values allocates the cross-border inflow across no-policy hosts in proportion to $[(1 - \tau_{c(k)})|F''|]^{-1}$, which is increasing in the destination's statutory rate, and each absorbed unit carries leverage $b^*(\tau_{c(k)})$, also increasing in the destination rate: both reallocated investment and reallocated debt concentrate in high-tax no-policy destinations.
- (ii) **Escape margin closed.** The domestic margin strictly dominates every cross-border option, since $\Delta(\tau_{c^*}) > \Delta(\tau_{c^*}) - \Delta(\tau_{c(h)})$ for every foreign h (all adopters have $\tau_c > 0$). Coordination collapses the largest attainable cross-border gain from the level of the shield, $\Delta(\tau_{c^*})$, to the dispersion of shield values within the network,

$$D \equiv \Delta(\tau_{c^*}) - \min_{h \text{ foreign}} \Delta(\tau_{c(h)}) < \Delta(\tau_{c^*}),$$

which shrinks toward zero as adopters' statutory rates converge. Displaced capital is redeployed domestically to unexposed local affiliates.

Proof. See Appendix A

Real capital moves because debt capacity is attached to it: the leverage cost $m(\cdot)$ is convex in the ratio b_j , so an affiliate's debt capacity scales with its real capital, and recovering a lost tax shield requires moving investment together with its financing. The allowance headroom bound on unexposed hosts in adopting countries operates in both cases of the proposition; what changes between them is the cross-border margin. Under an open escape margin the no-policy margin is unbounded and carries the bulk of displaced capital; under a closed margin the bounded domestic margin is the best available destination, and the first-order tax motive for crossing a border falls from $\Delta(\tau_{c^*})$ to at most the dispersion term D . Unexposed hosts in adopting countries (kind (c)) offer the same first-order gain as the local sister but are weakly dominated for finite moves, because a within-country move preserves country output exactly while a cross-border move erodes the destination's marginal product; the sharp cross-border prediction is therefore specific to no-policy destinations. Closing the escape margin thus creates no new impediment to cross-border movement—it removes the first-order reason for it, leaving the within-country margin that exists only because the cap applies entity by entity.

Prediction 4: When the group retains unexposed affiliates in no-policy countries, displaced debt and investment reallocate across the border to those affiliates, concentrated in high-tax no-policy destinations.

Prediction 5: *When all of the group’s foreign affiliates are exposed—coordinated adoption—displaced investment is redeployed within the treated country toward unexposed local affiliates.*

The five predictions map one-to-one into the empirical design: Predictions 1–3 are tested on exposed affiliates against unexposed comparisons, with the below-allowance affiliates of adopting countries providing the placebo; Prediction 4 on the foreign no-policy sisters of treated groups; and Prediction 5 on the local sisters of groups whose entire network is exposed. I test these predictions using the data described later in the paper.

IV Research Design

The staggered introduction of ESR across jurisdictions provides a quasi-experimental setting suitable for a staggered difference-in-differences (DiD) framework. I treat the country-level policy announcement as the treatment event and identify treated firms based on a firm’s pre-reform exposure to interest expense restrictions. Specifically, firms are identified as treated if their three-year pre-reform average interest expenses exceed both the jurisdiction-specific de minimis threshold and the ratio threshold.⁵ In essence, this specification provides intent-to-treat (ITT) estimates that capture how the treatment definition affects firm outcomes.

Note that the policy may also trigger indirect effects through intra-group reallocation of capital and debt (Giroud & Rauh, 2019; Egger *et al.*, 2014). When a treated firm faces a cap on interest expenses, its parent may substitute capital flows toward sister affiliates—domestic or foreign—that remain outside the scope of the rule.⁶ Such behavioural responses may contaminate the measured treatment effects if not explicitly modelled. To account for these channels, I classify firms into three mutually exclusive categories:

- *Treated Firms:* Firms that fail the jurisdiction-specific ESR test at the baseline.
- *Sister Firms:* Other affiliates within the same multinational group that do not fail the ESR at the baseline.
- *Control Firms:* Affiliates that neither fail the ESR test nor belong to the same multinational group as any treated firm.

In essence, this classification yields two types of multinational groups in the empirical design: those containing both treated and sister firms, and those composed exclusively of control firms. To identify the direct effect of ESR, I compare outcomes of treated firms with those of control firms, excluding all sister firms to prevent contamination from intra-group adjustments. To isolate the reallocation margin, I instead compare sister firms with the same

⁵ Many ESR regimes incorporate extra escape provisions, including debt-to-equity tests, equity and group escapes, and grandfathering clauses. Because these provisions can induce behavioural responses (Andresen & Thorvaldsen, 2023), I classify an affiliate as treated only if it fails other relevant elements of the country-specific ESR design. Further details on country-specific ESR provisions are provided in Online Appendix.

⁶ Affiliates may fall outside the scope either because they do not trigger treatment in jurisdictions that enforce ESR (for example, by remaining below the relevant thresholds) or because they operate in countries that have not adopted the rule.

control group, excluding treated firms. Using a common control group across specifications ensures comparability and enables a cleaner decomposition of policy effects into direct and within-group reallocation responses.

At the group level, a multinational group is classified as treated if at least one affiliate is treated. I assign the earliest treatment year among its treated affiliates as the treatment year for all sister firms and for the consolidated entity. This timing reflects the moment at which the group first becomes exposed to the ESR environment. To illustrate the identification strategy, consider the following event-study specification:

$$(11) \quad y_{ist} = \alpha_i + \mu_t + \sum_{\substack{p=-4 \\ p \neq -1}}^4 \beta_p \text{treat}_i \times \mathbb{1}(t - t_s^* = p) + X_{st}' \delta + \varepsilon_{ist}$$

where y_{ist} denotes the outcome for firm (group) i in country s and year t , α_i and μ_t are firm (group) and year fixed effects, and $\mathbb{1}(t - t_s^* = p)$ captures the leads and lags relative to the policy year t_s^* . The coefficient β_p traces the dynamic path of outcomes around the reform, while X_{st} includes time-varying country-level controls such as the statutory corporate tax rate, log of population, and log of GDP per capita to control for macro confounds. However, standard two-way fixed effects (TWFE) estimation of this equation can produce biased estimates under staggered adoption with heterogeneous treatment effects (Goodman-Bacon, 2021). I, therefore, implement the estimator of de Chaisemartin & D'Haultfoeuille (2020), which computes the weighted average of individual treatment effects by comparing units that change treatment status ('switchers') to units whose treatment status remains stable between consecutive periods.

For average effects, I collapse the event-time interactions into a single post-policy indicator. Because variables such as reported profits and equity can take negative values, I report balance sheet and financial variables in levels (USD millions) and express ratios as decimals.⁷ For interpretability, I normalize these measures by the treatment group mean in the pre-reform years. I also verify the robustness of the results by log-transforming the level variables and scaling them by lagged total assets. Standard errors are bootstrapped and clustered at the firm (group) level.

The empirical design relies on the standard parallel-trends assumption: in the absence of ESR, treated and control firms would have evolved similarly. I assess this assumption by examining the pre-treatment coefficients in the event-study specification. Across all outcomes, the estimates display no systematic pre-trends, providing direct support for the identification assumption. Online Appendix further shows that alternative staggered difference-in-differences estimators, such as Borusyak *et al.* (2024), Sun & Abraham (2021),

⁷ Please see Chen & Roth (2024) for a discussion of why log-transformations are problematic when variables include zeros or negative values.

and [Cengiz et al. \(2019\)](#), yield nearly identical dynamic paths, reinforcing the credibility of the identification strategy. Additionally, placebo tests with treatment identified in different years also confirm the timing exogeneity assumption.

A key potential threat to identification is that ESR adoption could correlate with macroeconomic shocks if countries introduce the reform during periods of changing financial conditions. To address this concern, the specification includes year fixed effects and time-varying country-level controls. I also verify the robustness of the baseline estimates by substituting these macro controls with country-by-year fixed effects. Another threat is that because treatment status depends mechanically on pre-reform interest expenses, firms could attempt to manipulate these expenses in anticipation of the rule. This manipulation is unlikely for two reasons: ESR thresholds differ across jurisdictions, making anticipation difficult, and treatment classification relies solely on three pre-announcement year average data. As further validation, section [VII.7.1](#) presents an RD analysis of the pre-reform distribution of interest expenses, confirming no evidence of manipulation around the relevant thresholds.

V Data & Summary Statistics

I extract detailed firm-level and group-level financial data from ORBIS for the years 2010-2021. Supplied by Moody's, ORBIS provides entity-level information for over 489 million companies worldwide, including historical data on location, industry classification, and ownership structures dating back to 1989 ([Dharmapala, 2014](#); [Liu, 2020](#)).

I restrict the sample to MNE groups, defined as networks of affiliates under the same global ultimate owner (GUO) with at least one affiliate located outside the GUO's country.⁸ When identifying consolidated financial statements, I exclude intermediate holding entities to prevent double counting, retaining only the highest corporate unit in each ownership chain. To address potential biases from duplicate entries, I retain the most recent financial record for each entity ([Kalemli-Özcan et al. , 2024](#)).⁹

The main accounting variables are debt, equity, capital, employment, interest expenses, and reported profits. Investment is defined as the ratio between current-year gross investment spending and beginning-of-year net fixed assets, i.e., $[K_t - K_{t-1} + depreciation]/K_{t-1}$, where K_t denotes the book value of fixed assets in year t . Effective Tax Rate (ETR) is defined as the ratio between taxation and EBITDA.¹⁰ Additional details on data cleaning steps are

⁸ The de minimis allowance embedded in most ESR regimes exempts small borrowers, making the rule bind almost exclusively on multinationals. In the estimation sample, roughly 5% of MNE affiliates exceed the threshold, compared to under 0.7% of domestic firms.

⁹ While ORBIS has limitations like uneven cross-country coverage driven by reporting requirements and potential measurement error in ownership links ([Torslov et al. , 2023](#)), the data is well-suited for this study: tracing cross-border reallocation necessitates harmonized, affiliate-level unconsolidated financials, which ORBIS largely reports. To the extent that coverage gaps obscure activity in unobserved jurisdictions, the estimates presented here represent a conservative lower bound.

¹⁰ To ensure the measure is economically meaningful, I bound the ETR between zero and the statutory

provided in the Online Appendix.

The final dataset is an unbalanced panel of 1,186,799 firm-year observations from 172,549 affiliates (belonging to 73,038 multinational groups) across 39 countries. At the group level, the sample contains consolidated financial statements for 8,584 MNEs operating in 78 countries. Summary statistics in nominal USD are reported in Table I. For all variables, means exceed medians, reflecting the right-skewed distribution characteristic of firm size; accordingly, I winsorize all outcomes at the 5th and 95th percentiles. The Online Appendix provides jurisdiction- and industry-level summary statistics. The treatment is balanced across industries, with control firms outnumbering treated and sister affiliates in all categories.

Data on global statutory corporate tax rates are drawn from [Enache \(2023\)](#), annual exchange rates from [Treasury \(2024\)](#), and population and GDP per capita from the World Bank’s World Development Indicator (WDI) Database.

VI Stylized Facts

Before turning to the causal estimates from the empirical research design, I document several stylized facts that illustrate how ESR binds in the data. Figure Ia plots the distribution of accounting profits relative to total assets for treated, sister, and control firms. The distributions for sister and control firms overlap closely, indicating similar profitability patterns prior to reform. By contrast, treated firms exhibit a distinctly higher concentration of observations with negative or near-zero profits, consistent with systematic under-reporting of taxable income ([Bilicka, 2019](#)). This bunching near zero visually identifies the core set of firms that the reform targets—those relying most heavily on intra-group debt to reduce reported profits.

Figure Ib presents this distribution for treated firms around the reform. Post-reform, the distribution shifts notably to the right, i.e., the share of treated firms reporting zero or negative profits declines, and the share reporting positive profits rises. This shift provides non-parametric descriptive evidence of reduced tax avoidance and higher taxable income among treated firms following ESR adoption. As shown in Ic and Id, the distributions for sister and control firms remain essentially unchanged over the same period, providing suggestive evidence that the shift is unique to treated entities.

In Online Appendix, I empirically quantify this shift through two complementary exercises. First, I estimate the normalized excess mass of firms bunching around zero profits-to-assets separately for treated and control firms in the pre- and post-reform periods, relative to a counterfactual density fitted outside the bunching window, and take the triple difference across groups and periods. Second, I estimate a difference-in-differences specification for the incidence of zero tax payments, comparing treated firms directly against controls. Both

corporate tax rate. Given the prevalence of near-zero reported profits among treated firms (see Section VI), I scale taxation by EBITDA to ensure a more stable denominator. Results are robust to an alternative ETR definition where taxation is normalized by reported profits.

exercises point the same way: the excess mass at zero falls sharply among treated firms relative to controls after the reform, with a triple difference of -1.95 , and the share of treated firms paying exactly zero tax declines by 4.6 percentage points relative to controls.

Figure [IIa](#) reinforces this interpretation by examining the distribution of treated firms around the normalized financial expense-to-EBITDA threshold. After ESR becomes binding, treated firms bunch immediately below the deductibility limit—typically 30 percent—showing excess mass above and reduced mass below the cap. This pattern is indicative of active financial adjustment to comply with the constraint on interest deductions. Importantly, an analogous placebo distribution, in Figure [IIb](#), for all firms shows no such bunching, confirming that the behaviour is specific to firms directly affected by the rule.

In complementary evidence, I plot effective tax rates by firm size separately for treated, sister, and control firms in Figure [IIc](#). Across the entire size distribution, treated firms consistently face lower effective tax rates, indicating that the firms most exposed to ESR systematically convert higher leverage into lower tax liabilities. A pre- versus post-reform comparison in Figure [IId](#) reveals a clear upward shift in effective tax rates for treated firms across the size distribution, consistent with a binding constraint that raises taxable income broadly rather than selectively. Performing the same exercise for sister and control firms in Figures [A.Ia](#) and [A.Ib](#) yields no discernible change in their effective tax rate distributions, reinforcing the interpretation that ESR directly curbs tax avoidance among treated affiliates rather than reflecting competing economy-wide shocks.

Figure [A.Ic](#) further examines the relationship between policy exposure and firm size. The fraction of treated firms increases monotonically across size deciles, suggesting that larger affiliates are more likely to engage in the targeted form of tax-motivated leverage. Appendix Figures [A.Id-A.If](#) complement this by plotting balance-sheet composition: among treated firms, debt-to-asset ratios exceed those of equity or retained earnings and rise with firm size, whereas the opposite holds for sister and control firms.

Taken together, the stylized facts show that ESR targeted precisely the affiliates most engaged in debt-based avoidance—large, highly leveraged, and persistently low-profit firms. The rightward shift in profit distributions, the sharp bunching around the deductibility threshold, and the broad-based increase in effective tax rates across the full size distribution jointly demonstrate substantial behavioural responses. These reduced-form, non-parametric patterns arise even before any empirical modelling and confirm that the reform induced meaningful changes in financial behaviour consistent with the mechanisms studied in the subsequent analysis.

VII Firm Level Results

I now present the causal estimates of the ESR policy and empirically test the predictions developed in Section [III](#).

VII.1 Effect on Treated Firms

To isolate the direct firm-level effects of the reform, I exclude sister firms from the analysis and compare the outcomes of treated firms only with that of control firms. This design ensures that the estimated effects capture the immediate behavioural response of directly exposed firms, rather than the intra-group reallocations that MNEs may undertake to optimize their post-reform tax positions.

Figures III and IV present the dynamic event-study estimates. The estimates reveal a sharp and persistent decline in the debt holdings of treated firms after the reform. This response is consistent with the reform’s intent: by capping the tax-deductibility of interest expenses, the policy raises the after-tax cost of debt-financed capital and reduces the tax advantage of debt relative to equity and retained earnings. Treated firms that were previously more leveraged thus experience an upward shift in their marginal cost of borrowing, leading to deleveraging.

Table II quantifies this effect: average debt holdings fall by USD 0.97 million, amounting to 9.3 percent of the pre-reform mean.¹¹ The persistence of this decline indicates that it reflects not just short-term balance sheet adjustments but a structural reconfiguration of financing strategies. In practice, intra-group loans account for a substantial portion of total debt among MNE firms (Bashir *et al.*, 2024). The sustained fall in debt therefore implies that the reform curtailed the use of such internal loans, consistent with the view that debt-based profit shifting is a major component of MNE tax planning. In this sense, the policy effectively reduced the debt bias in capital financing.

Table II reports the corresponding effect on interest expenses, which decline sharply by about 8 percent after the reform. This reduction is partly mechanical, following the fall in debt volumes, but also reflects broader financial adjustments. Treated firms may renegotiate intra-group loan contracts at lower interest rates, revise repayment schedules, or reduce hybrid and financial transactions that blur the line between debt and equity (OECD, 2013). Because these channels often facilitate the artificial transfer of profits to low-tax firms, the fall in interest expenses signifies a contraction in the overall space for intra-group financial engineering. The result, therefore, captures not just a reduction in financial costs but a collapse in tax-avoidance intensity along the debt channel.¹²

Mechanically, lower interest deductions increase reported profits, expanding the domestic tax base. Panel IIIc confirms this pattern: reported profits of treated firms rise visibly

¹¹ The number of *Observations* in the table exceeds the raw sample size because the estimator by de Chaisemartin & D’Haultfoeuille (2020) averages over effective sample sizes in each two-way comparison, which may overlap when treatment timing differs across units.

¹² Although these outcome variables measure total debt and interest rather than intra-group borrowing alone, lender-side evidence —presented in Online Appendix— confirms that the post-reform decline operates predominantly along the intra-group margin. Specifically, the affiliates that internally funded the treated firms—chiefly foreign net lenders—see their own interest income decline and their net lending move toward zero after the reform, tracing the direct unwinding of internal loans.

after the reform. Panel IIIId and Table II show that the rise in reported profits translates into a permanent increase in effective tax rates for treated firms. The effective tax rate increases by 1.3 percentage points, or 21.4 percent relative to the pre-reform mean. From a public finance perspective, this finding indicates that the policy strengthened tax enforcement along the debt channel and compressed the gap between statutory and effective taxation.¹³

However, the higher effective tax rate increases the user cost of capital, particularly for firms that relied heavily on intra-group debt. Consistent with this mechanism, Panel IVa shows a marked decline in investment rate at treated firms, averaging 10.6 percent relative to the pre-reform mean.¹⁴ Interestingly, the later stages of the event window reveal a gradual rebound in the rate. Table II shows that this recovery coincides with a corresponding rise in the equity share of financing, implying that firms dynamically rebalance their capital structures away from debt and toward equity. This substitution process involves frictions—such as equity issuance costs (Hovakimian *et al.*, 2001), internal capital budgeting constraints (Leary & Roberts, 2005), and the time required to reallocate retained earnings across firms (Faulkender *et al.*, 2012)—which explains the delayed adjustment.

Figures IVc and IVd show that capital and employment at treated firms also decline in parallel after the reform. Table II shows that employment falls on average by about three workers per affiliate, and capital shrinks by about 4%. These reductions mirror the investment response and suggest that the deleveraging process translated into a broader contraction in firm scale. The fall in employment, while modest, signals that the financing constraint induced by the reform constrained not only capital accumulation but also complementary labour demand (Curtis *et al.*, 2021). The decline in capital reflects both slower capital formation and potential disinvestment as firms restructure their balance sheets under tighter debt capacity.

Taken together, these findings confirm that the anti-avoidance reform had both nominal and real effects. Nominally, it reduced debt-based profit shifting by limiting the deductibility of interest payments and thereby increasing reported taxable profits. On the real margin, it raised the effective tax rate for debt-reliant firms, leading to lower investment, employment, and capital stock in the short to medium run. The combination of these effects underscores the classic policy trade-off in corporate taxation: while tighter anti-avoidance measures enhance fiscal efficiency and fairness by aligning taxation with real activity, they also risk curbing investment when financial frictions amplify their real effects (Hall & Jorgenson, 1967).

¹³ Because the effective tax rate is computed as taxation over EBITDA, its post-reform rise is in part mechanical. The avoidance interpretation instead rests on whether the response scales with the tax benefit of debt. In Online Appendix, I estimate a difference-in-differences design that interacts treatment with firm-level tax incentives and shows that both the post-reform debt reduction and the recovery of the taxable base are stronger where the firm's own statutory rate is higher and where its intra-group tax differential is wider.

¹⁴ Investment is defined relative to lagged fixed capital; hence the event window begins at lead -3.

VII.2 Effect on Sister Firms

A rise in the cost of capital at treated firms alters the internal allocation incentives within the multinational group (Giroud & Mueller, 2015; Giroud & Rauh, 2019; De Vito *et al.*, 2025). Once the ESR binds, the marginal product of debt-financed investment must exceed the now-reduced tax benefit from interest deductions, making debt a less attractive source of financing at constrained firms. This wedge increases the relative after-tax cost of capital at these affiliates compared with the rest of the group. Standard theory thus predicts that MNEs will reallocate investment toward affiliates on which the rule does not bind, until the internal after-tax returns are once again equalized across locations (Desai *et al.*, 2004). To examine this mechanism, I exclude the treated firms and compare the outcomes of their sister firms with those of control firms.

Table III reports the results from this exercise and shows that their average investment rises by approximately 9 percent relative to pre-reform mean. Note that in assigning the earliest treatment year within each group to all sister firms, I take a conservative approach: this uniform treatment timing likely attenuates the true extent of reallocation, since it ignores potential heterogeneity in exposure intensity and timing across firms. Nonetheless, the findings confirm that the reform induced a measurable shift of capital within MNE groups.

Table III also reports a parallel increase in employment at these firms, suggesting that the observed rise in investment corresponds to real expansion rather than balance-sheet reshuffling. Interestingly, effective tax rates at these firms remain largely unchanged, implying that the additional investments are internally financed and optimized within the group's global tax strategy rather than reflecting broader fiscal changes. In essence, the reform triggered a re-optimization of capital allocation within MNE networks, with investment and employment migrating from constrained to unconstrained firms.

The geography of these receiving firms clarifies the nature of this reallocation. Sister firms may be located in the same country as treated firms ("local sisters") or abroad ("foreign sisters"). Figures Va and Vb, together with Table III, show that cross-border reallocation drives the aggregate response. Investment rises by roughly 15 percent at foreign sister firms, while remaining largely unchanged among local sisters. Because jurisdictions adopted ESR asynchronously, such international reallocation can raise after-tax group profits only if capital flows toward countries that do not impose comparable restrictions. I therefore distinguish between foreign sisters located in ESR and non-ESR countries. Figures Vc and Vd demonstrate that the response concentrates entirely in non-ESR jurisdictions: investment increases sharply there, whereas foreign sisters in ESR countries show no meaningful change.

Balance-sheet and real outcomes at these non-ESR sister firms reinforce this interpretation. As depicted in Figure VI, debt rises sharply, capital and employment expand, and reported profits increase in parallel. The joint increase in debt and investment indicates that

these affiliates absorb the debt capacity shed by treated firms, partly restoring the group's overall leverage structure in more favourable tax locations. The expansion in capital and employment confirms that the response reflects real activity rather than accounting shifts.

VII.3 Why Foreign Rather Than Domestic Reallocation?

When the ESR binds at a treated affiliate, the group could in principle keep the activity at home by moving debt to a same-country sister that has not exhausted its own deduction capacity. However, the results above suggest that is not the case. In order to understand the underlying reasons, I compute for every treated group the interest expense disallowed at the reform, as well as for every same-country sister the capacity that remains unused under its own limit. Of the groups that had at least one treated firm, 78.4 percent cannot absorb the disallowed interest in full, and 71.5 percent could absorb less than a tenth of it. For the typical group, the domestic margin is therefore largely, making it an impractical substitute for cross-border reallocation.

Additionally, note that a same-country sister is subject to the identical fixed-ratio limit, i.e., relocating a loan to it changes which affiliate encounters the cap but leaves unchanged the rate at which the group's interest is deducted. Conversely, a cross-border move to a high-tax jurisdiction outside the ESR framework alters that rate, i.e., among non-ESR countries, high-tax jurisdictions are especially attractive for debt parking because the value of the interest deduction scales with the statutory rate (Liu, 2020). Results reported in Online Appendix support this interpretation. Both investment and debt increase disproportionately at *high-tax*, non-ESR affiliates rather than at low tax non-ESR ones. Because legal capacity is common to both the types, the divergence confirms that the value of the deduction—not mere capacity—is the operative consideration. In other words, a domestic sister, which offers capacity at an unchanged deduction rate, simply cannot compete on the margin that governs the reallocation decision.

Taken together, these results reveal an asymmetric response across the multinational network. Countries that implemented the ESR successfully curtailed domestic debt-based avoidance but did so at the cost of capital and employment outflows to unconstrained jurisdictions. In contrast, countries that refrained from adopting the policy benefited from inbound reallocation, effectively free-riding on others' enforcement efforts. This dynamic embodies the classic "winners and losers" problem in international tax coordination. From a policy perspective, this behaviour underscores the strategic substitutability of national anti-avoidance efforts: absent coordination, unilateral reforms can redirect rather than reduce global profit-shifting incentives, reinforcing the need for supranational coordination to prevent the emergence of new asymmetries in global capital allocation (Johannesen, 2022; Fejerskov Boas *et al.*, 2024).

VII.4 Effect of Coordination

The final piece of firm-level analysis turns to multinational groups that are fully exposed to the reform—that is, groups whose entire network of affiliates operates in jurisdictions that implemented ESR. For these entities, the tax-motivated cross-border reallocation margin collapses. Because every affiliate faces a uniform cap on interest deductibility, shifting leverage across borders no longer yields marginal tax advantages. Crucially, the elimination of this international arbitrage channel does not imply that the internal capital market ceases to function. Rather, under a globally binding policy environment, the MNE optimizes by managing internal liquidity constraints domestically, redirecting capital toward specific affiliates where internal funding is most valuable.

The empirical results in Figure VII and Table III strongly support this localized liquidity-management mechanism. Restricting the sample to sister firms within fully covered MNEs reveals that, post-reform, investment at domestic sister firms rises by approximately 11 percent, whereas investment in foreign sister affiliates remains statistically unchanged. This domestic investment response translates into a gradual, persistent accumulation of local capital stocks over time, with no evidence of countervailing capital flight. To isolate the precise channel driving this localized reallocation, I exploit cross-sectional heterogeneity across sister affiliates. As reported in the Online Appendix, the domestic investment spike is almost entirely concentrated among affiliates characterized by the highest pre-reform interest shields (measured by net financial expenses to EBITDA) and the lowest internal cash flows (measured by net profits plus depreciation scaled by assets).

This finding carries a profound policy implication. In essence, full coordination turns what would otherwise be a tax competition game into a tax cooperation equilibrium. Capital remains domestic, taxable income rises, and governments can strengthen their fiscal capacity without discouraging real investment. This mechanism captures the central contribution of this study: the effectiveness and real impact of anti-avoidance policies depend critically on the degree of international coordination. Partial reforms merely redistribute the tax base; coordinated enforcement aligns fiscal integrity with productive efficiency.

VII.5 Effect of Increasing Exposure

A more nuanced way to understand the role of policy coordination in shaping multinational investment behaviour is to examine how investment responses evolve as the intensity of policy exposure within a group increases. Rather than treating coordination as a binary distinction—some countries adopt ESR while others do not—this approach conceptualizes it as a gradual process of convergence. In practice, as more jurisdictions implement the rule, an MNE's ability to exploit cross-country differences in financing costs narrows progressively. Each new adoption expands the perimeter of enforcement and, in effect, erodes the firm's tax-motivated reallocation margin.

To capture this dynamic, I construct a treatment-intensity specification that tracks how investment patterns respond as the share of an MNE's firms covered by the ESR grows over time. In this framework, instead of assigning the earliest treatment year in a group to all sister firms, I record the sequential policy introductions across each jurisdiction and treat each new implementation as an event that raises the group's aggregate exposure. Group exposure therefore increases monotonically as more countries enforce the rule.

Empirically, I divide the sample into quintiles based on the degree of group-level exposure, measured as the fraction of firms covered by the reform in a given year. The first quintile represents groups with at least 20 percent of firms under ESR, the second with at least 40 percent, and so on. Running the main specification separately across these exposure quintiles for both local and foreign sister firms reveals a striking pattern. As shown in Figure IX, at low levels of exposure, investment rates are higher in foreign sister firms than in local ones—consistent with firms reallocating capital abroad to avoid newly binding restrictions at home. However, as exposure deepens, this relationship reverses. With each successive increase in coverage, investment abroad declines while investment at local firms rises. At full exposure—when all firms within a group operate under ESR—the investment rate at local sisters becomes strongly positive, whereas that at foreign sisters turns negative. This continuous rebalancing of capital flows underscores a fundamental equilibrium shift: as international coordination increases, the reallocation of investments occurs within countries rather than across borders.

Table IV reports three complementary perspectives on the effects of policy exposure. Columns (1) and (2) isolate the effect of one additional affiliate within a group coming under the reform. The estimates show that when one more affiliate becomes subject to ESR, investment at local sister firms rises by 0.066 percentage points (0.45 percent relative to the mean), while investment at foreign sisters falls by 0.22 percentage points (1.13 percent relative to the mean). In other words, as coverage expands within the MNE network, firms progressively redirect capital toward domestic affiliates that remain relatively unconstrained.

Columns (3) and (4) extend this reasoning to the country level. Each additional country adopting ESR increases investment at local firms by roughly 0.88 percentage points. To interpret the economic magnitude, consider an MNE operating in ten countries. If two more countries adopt ESR, local investment rises by 1.5 percentage points—equivalent to a 7.6 percent increase relative to pre-reform levels—while investment abroad declines by approximately 1.2 percentage points, or 6.2 percent relative to baseline.

Columns (5) and (6) provide a complementary perspective using a continuous exposure measure ranging from 0 (no exposure) to 1 (full exposure). The estimates quantify the magnitude of this adjustment across the full exposure spectrum. Moving from zero to full exposure raises the investment rate at local sister firms by 2.14 percentage points, while reducing investment abroad by 2.5 percentage points. Interpreted differently, a 10 percent

increase in exposure is associated with a 0.21 percentage point increase in local investment—a 1.44 percent rise relative to the baseline mean—and a 0.25 percentage point decrease abroad, corresponding to a 1.29 percent decline relative to baseline.

In sum, the treatment-intensity results reinforce the central insight of the paper: multilateral coordination transforms anti-avoidance rules from potentially distortionary instruments into efficiency-enhancing ones. Where unilateral measures may induce capital flight, coordinated enforcement neutralizes the cross-border arbitrage motive, retaining both capital and tax revenue within enforcing countries.

VII.6 Group Level Effects

I now turn to the group-level effects of the policy. A key limitation of this analysis is data coverage: while the sample includes roughly 73,000 unique multinational groups, consolidated financial statements are available for only about 8,500 of them. This limitation stems from cross-country differences in reporting requirements. Although most firms must publish unconsolidated statements in their host jurisdictions, group-level accounts are often unavailable when the parent company is headquartered in a country without mandatory disclosure rules. It is also important to note that simply summing unconsolidated statements to approximate consolidated group accounts is not appropriate, as this approach can lead to double counting of intra-group transactions and assets (Blouin & Robinson, 2025).

For the subset of groups with available data, I estimate the effects of the reform using the same empirical specification discussed in Section IV. The results presented in Figure VIII show that both investment and employment at the group level remained broadly stable after the policy's introduction. This finding reinforces the affiliate-level evidence: while the reform triggered a reallocation of capital and employment within groups—away from high-debt, high-tax firms toward their local or foreign sisters—the overall level of group-wide investment and employment was unaffected. The stability of group-level aggregates thus reflects an internal rebalancing rather than a contraction in total economic activity, consistent with evidence that internal capital markets allow firms to shift funds across affiliates at low cost (Rajan *et al.*, 2000).

In contrast, I observe a marked reversal in the profits trajectory of treated groups. Before the reform, these groups exhibit a consistent decline in their accounting profits—reflecting the intensification of tax avoidance behaviour through debt-based profit shifting. After the reform, this trend reverses, and group-level reported profits begin to rise gradually. This pattern aligns with the firm-level results: once interest deductions are capped at the affiliate level, the ability to shift profits through intra-group debt declines, leading to higher taxable income both at the affiliate and consolidated group levels. The results also suggest that groups were generally unable to offset the rise in taxation at treated affiliates by reducing taxes elsewhere, indicating limited scope for compensatory profit shifting across locations.

Note that it is plausible that some groups substitute debt-based shifting with other forms of tax planning, such as transfer pricing, trade misinvoicing, or cost reclassifying (Heiser *et al.*, 2025; Dharmapala, 2014). While the data do not permit testing these alternative channels, two factors help alleviate this concern. First, any such substitution incentives apply symmetrically to both treated and control groups, preventing bias in the estimated treatment effect. Second, other anti-avoidance measures introduced alongside ESR—such as hybrid mismatch and controlled foreign corporation (CFC) rules—targeted all firms, not specifically those engaging in debt-based avoidance. Therefore, any overlapping effects of these policies would plausibly be additive, implying that the estimated ESR effect is rather conservative.

VII.7 Robustness Checks

VII.7.1 Regression Discontinuity Design

To increase the confidence in the event study evidence, I implement a regression discontinuity (RD) design that estimates the effect of the ESR policy on firms located just above the policy's de minimis threshold. Conceptually, the RD framework isolates the marginal firm that is large enough to fall within the scope of the rule, allowing a comparison of firms immediately on either side of the eligibility cut-off.

For this exercise, I restrict the estimation sample to countries that combine a de minimis threshold with a fixed ratio rule and to three pre-reform periods and three post-reform periods to ensure a balanced panel. Because the de minimis thresholds vary across countries, I normalize each threshold to zero and restrict the bandwidth to USD 1.1 million. The running variable is the average pre-reform financial expense, and the outcome variable is the change between the post- and pre-reform average outcomes.

The identification assumption in this design is weaker than the parallel-trends assumption underlying the event-study specification. Here, identification requires that firms are unable to manipulate their average pre-reform financial expenses to fall just below the de minimis threshold. Such manipulation appears unlikely, as thresholds differ across countries, and the policy design in one jurisdiction provides limited guidance about the relevant cut-off in another. In addition, using the pre-reform average as the running variable helps reduce concerns about contemporary endogeneity.

As a first step, in Appendix Figure A.II I provide a graphical validation of the design by plotting binned scatter plots of the difference in raw means between pre- and post-reform outcomes around the de minimis threshold in bins of USD 0.1 million. The results reveal clear discontinuities at the threshold across multiple outcomes. Firms that were more likely to face the ratio threshold based on their pre-reform financial expenses reduce their debt, investment, capital stock, and employment after reform, while their reported profits and

effective tax rate increase. These discontinuities mirror the results obtained from the event-study analysis.

Importantly, the intensity of policy exposure varies with the distance from the *de minimis* threshold. Firms located farther to the right of the threshold—those with larger pre-reform average financial expenses—are subject to stricter policy constraints and therefore experience stronger behavioural responses. Conversely, firms close to the threshold face weaker effective incidence, implying smaller behavioural adjustments. This pattern suggests that the magnitude of discontinuities near the threshold may understate the overall impact of ESR, and that the slope of the fitted polynomial in the RD plots is more informative about the strength of behavioural responses. Consistent with this interpretation, the slope is sharply negative for debt, investment, capital, and employment, and positive for reported profits and effective tax rate.

To complement the visual analysis, I estimate a parametric RD specification focusing on firms with pre-reform average financial expenses above the cut-off:

$$(12) \quad \Delta Y_{is} = \alpha + \beta \times Fail_{is} + \gamma^b(z_{is} - \bar{z}_s) + \gamma^a Fail_{is} \times (z_{is} - \bar{z}_s) + \epsilon_{is}$$

where ΔY_{is} is the difference between the average post-reform and pre-reform outcomes for firm i in country s , z_{is} denotes the pre-reform average financial expense; and $\bar{z}_s$ is the country-specific *de minimis* threshold. The indicator $Fail_{is}$ equals one if $z_{is} > \bar{z}_s$. I fit a linear polynomial in the running variable on each side of the threshold.

In this setting, β captures the discontinuity at the cut-off, while γ reflects how the treatment effect varies with distance above the threshold. Given the structure of ESR, γ^a provides a more economically meaningful measure of the policy's intensity than β , as it quantifies the gradient of the response among firms increasingly exposed to the rule. The results of this exercise are reported in the Online Appendix.

Online Appendix further plots the distribution of pre-reform average financial expenses among treated firms, with country-specific *de minimis* thresholds indicated by dashed vertical lines. The figure shows that the distribution is widely dispersed rather than concentrated around the thresholds, which supports the choice of difference-in-differences as the main identification strategy rather than RD alone.

Note that the RD estimates are unlikely to be affected by the bunching evidence discussed in Section VI. The two analyses capture distinct aspects of firms' responses to ESR. The RD design focuses on the marginal firms located near the *de minimis threshold*, isolating the causal effect of the reform on firms whose eligibility is sensitive to the rule. In contrast, the bunching analysis describes a broader pattern of financial adjustments among all treated firms in response to the binding interest deduction limit near *ratio threshold*. Additionally, because the RD design relates on variation across *all firms* around the threshold and the bunching evidence examines the overall distribution of *treated firms*, the two approaches

measure different behaviours and do not mechanically influence each other.

VII.7.2 Alternative Difference-in-Difference Methodologies

I assess the robustness of the baseline results by re-estimating the policy’s causal effects using alternative staggered difference-in-differences estimators, as reported in Online Appendix. Specifically, I employ the canonical two-way fixed effects (TWFE) model and the approaches proposed by [Sun & Abraham \(2021\)](#), [Borusyak *et al.* \(2024\)](#), and [Cengiz *et al.* \(2019\)](#). All specifications include unit and year fixed effects, as well as time-varying country-level controls. The results, presented in Online Appendix, show that the dynamic estimates obtained from these estimators are highly consistent.

VII.7.3 Additional Checks

I further assess the robustness of the results through several complementary exercises. I trim and winsorize the data at alternative cut-offs, redefine ownership linkages using different ownership thresholds, and re-estimate the effects on a balanced sample. I also verify that the results are robust to scaling raw USD levels by lagged total assets and to log-transforming these variables. I further confirm that the results are not driven by any single country or industry. The baseline specification absorbs macroeconomic confounders indirectly, through time-varying jurisdiction-level controls; I verify that the estimates are robust when these confounders are instead absorbed directly, by replacing the controls with country $\times$ year fixed effects. Additionally, I conduct placebo tests by redefining treatment status in alternative pre-reform years to ensure that the estimates are not confounded by mean reversion or business cycle dynamics. The estimates are likewise robust when I exclude treated units in jurisdictions whose interest limitation applies only to related-party borrowing, when I exclude groups in which every affiliate is treated, and when I exclude firms with zero or negative pre-reform EBITDA.

VIII Conclusion

This paper provides new evidence on how multinational enterprises adjust their financing and real activities in response to coordinated anti-tax avoidance policies. The paper studies the effects of Earnings Stripping Rules (ESR), one of the most widely adopted anti-tax-avoidance policies of the last decade which limits tax avoidance through debt channels. Using affiliate-level and consolidated-group data across forty countries, and exploiting the staggered global rollout of ESR, I document that the policy meaningfully curbs debt-based profit shifting while generating important reallocation responses within the multinational groups. Treated affiliates reduce their leverage, interest expenses, and reported tax avoidance, but also scale back investment and employment as the cost of capital rises. Multinational groups offset these declines by shifting capital and labor toward unconstrained

affiliates, particularly in non-adopting high-tax jurisdictions where the tax advantage of internal debt persists.

The extent of this reallocation, however, depends critically on the degree of international coordination. A treatment-intensity design shows that as a larger share of a group's affiliates becomes subject to ESR, the cross-border escape margin contracts and eventually disappears. When exposure is partial, MNEs respond by reallocating activity abroad; when exposure is broad or complete, reallocation shifts toward domestic affiliates, and foreign activity remains unchanged. At the group level, the policy raises taxable income without depressing aggregate investment.

These findings highlight the central role of coordination in the design of effective and non-distortionary anti-avoidance rules. Unilateral adoption curbs profit shifting but induces capital flight; coordinated adoption contains avoidance while preserving domestic investment. In the limit, when all affiliates are covered, ESR operates as intended: it broadens the tax base, limits the scope for arbitrage, and does so without reducing the overall scale of real economic activity. The results therefore speak directly to ongoing international tax initiatives—most notably the OECD's Pillar 2 minimum tax—which similarly aim to reduce cross-country differentials that facilitate profit shifting.

Future work may extend these insights by examining substitution into other avoidance channels, the long-run effects on entry and organizational structure, and the interaction between ESR and emerging global minimum tax frameworks.

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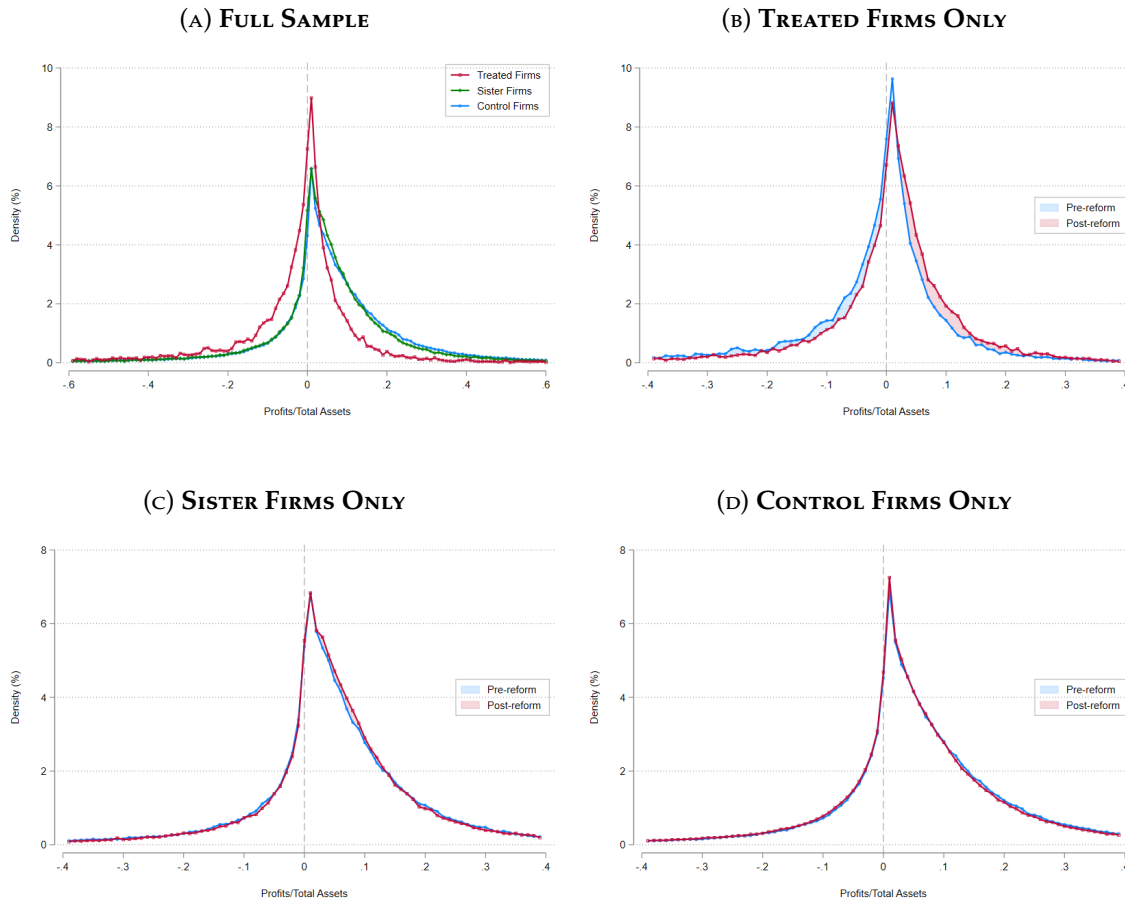
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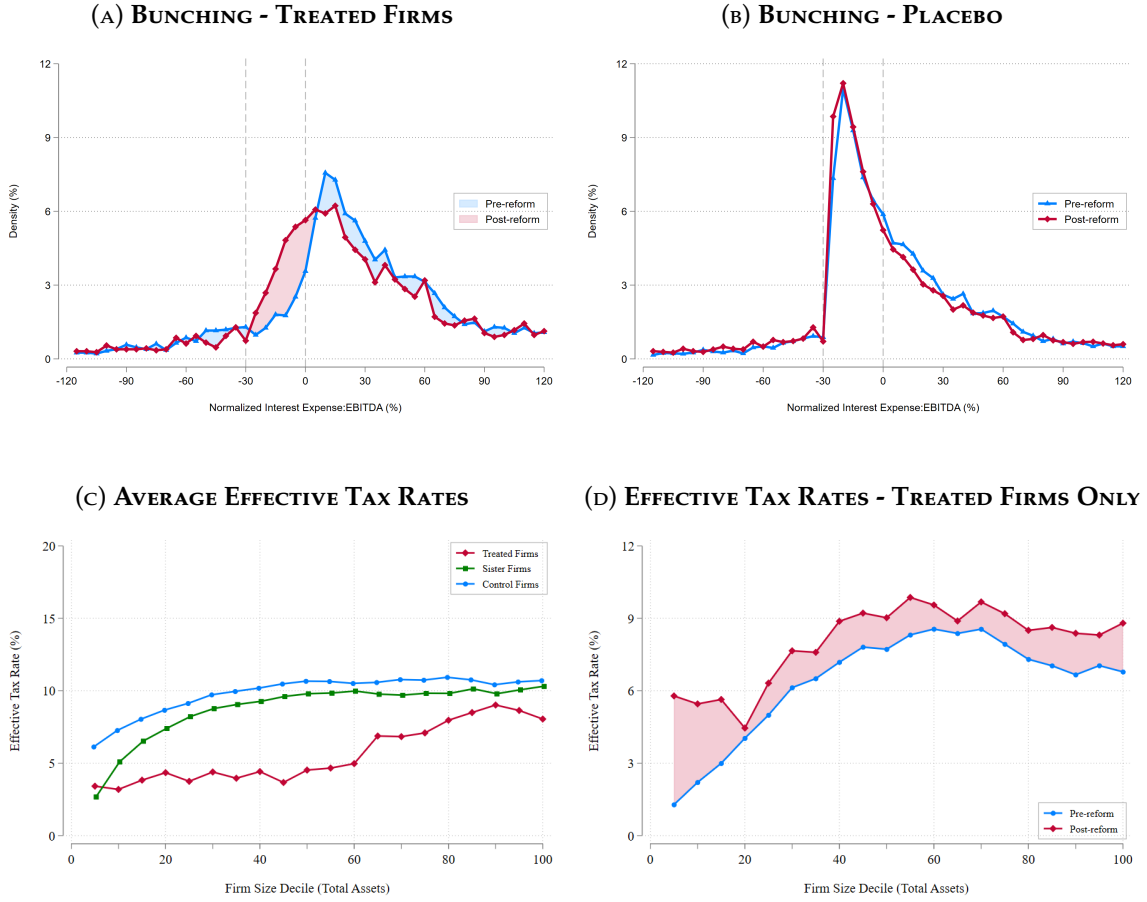
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FIGURE I: DISTRIBUTION OF ACCOUNTING PROFITS



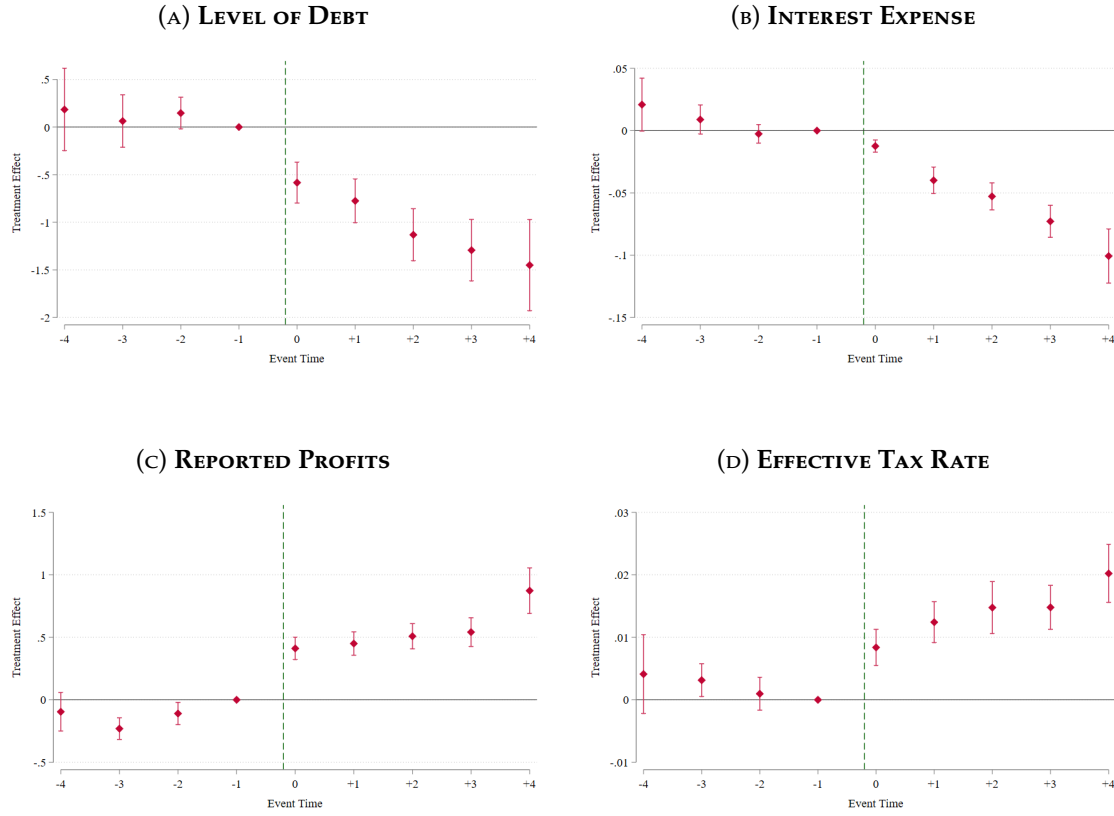
Notes: Panel A plots the density distribution of the ratio of accounting profits to total assets for each of the three types of firms, i.e., treated firms, sister firms, and control firms. Panels B-D plot the pre- and post-reform density distribution of the ratio of accounting profits to total assets for each of these firm types separately. The sample is restricted to only the firms with non-missing positive total assets across all the periods.

FIGURE II: ADDITIONAL STYLIZED FACTS



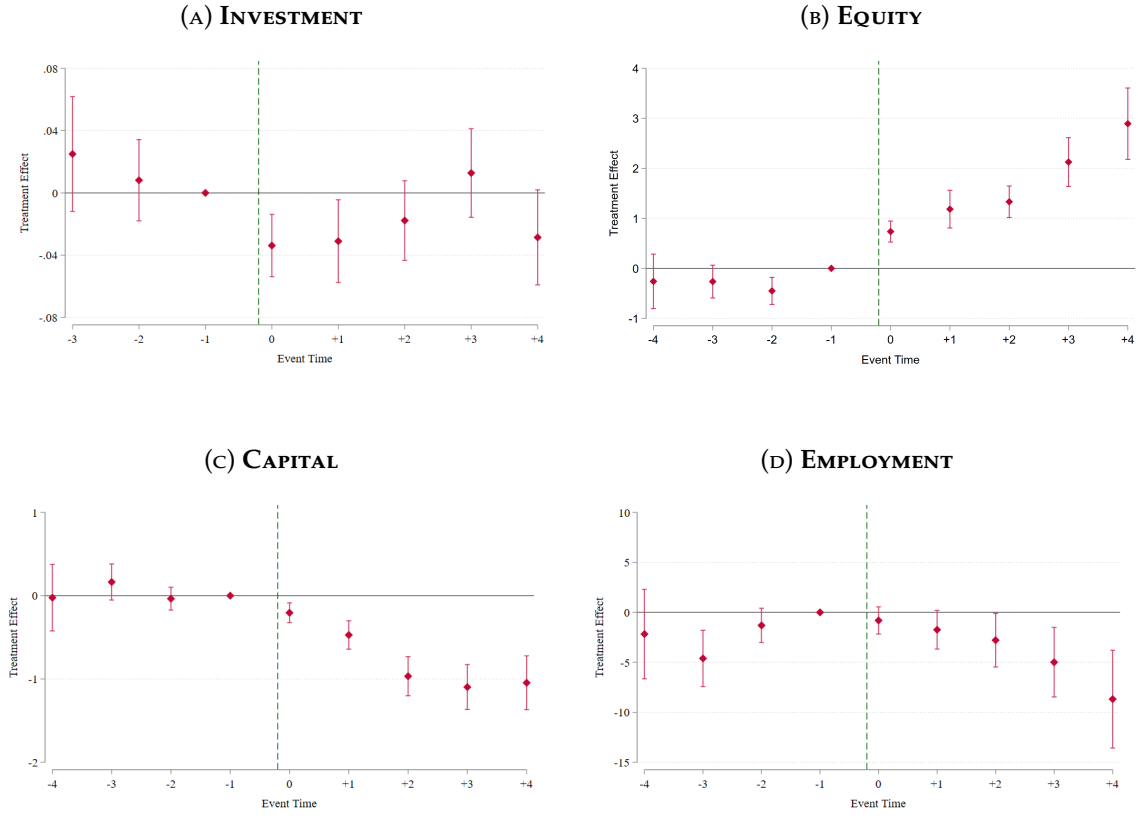
Notes: Panel A plots the pooled pre- and post-reform density of treated firms around the ESR threshold, normalized to zero across countries. For countries defining the rule using net (gross) interest to EBITDA, I use the net (gross) interest-to-EBITDA ratio. The sample is restricted to treated firms that, in the post-reform period, also failed the remaining policy tests (e.g., de minimis threshold, debt-to-equity test, etc.) and thus would have been subject to ESR if their interest-to-EBITDA ratio exceeded the threshold. Panel B plots the the pooled pre- and post-reform density for all firms in ESR-adopting countries that also failed the other policy criteria and would have been affected if their respective interest-to-EBITDA ratio had exceeded the national threshold. Panel C reports the binned average effective tax rates as a function of firm size for each of the three types of firms. Panel D plots the pre- and post-reform binned average effective tax rates as a function of firm size for treated firms only.

FIGURE III: EFFECTS OF ESR - TREATED FIRMS



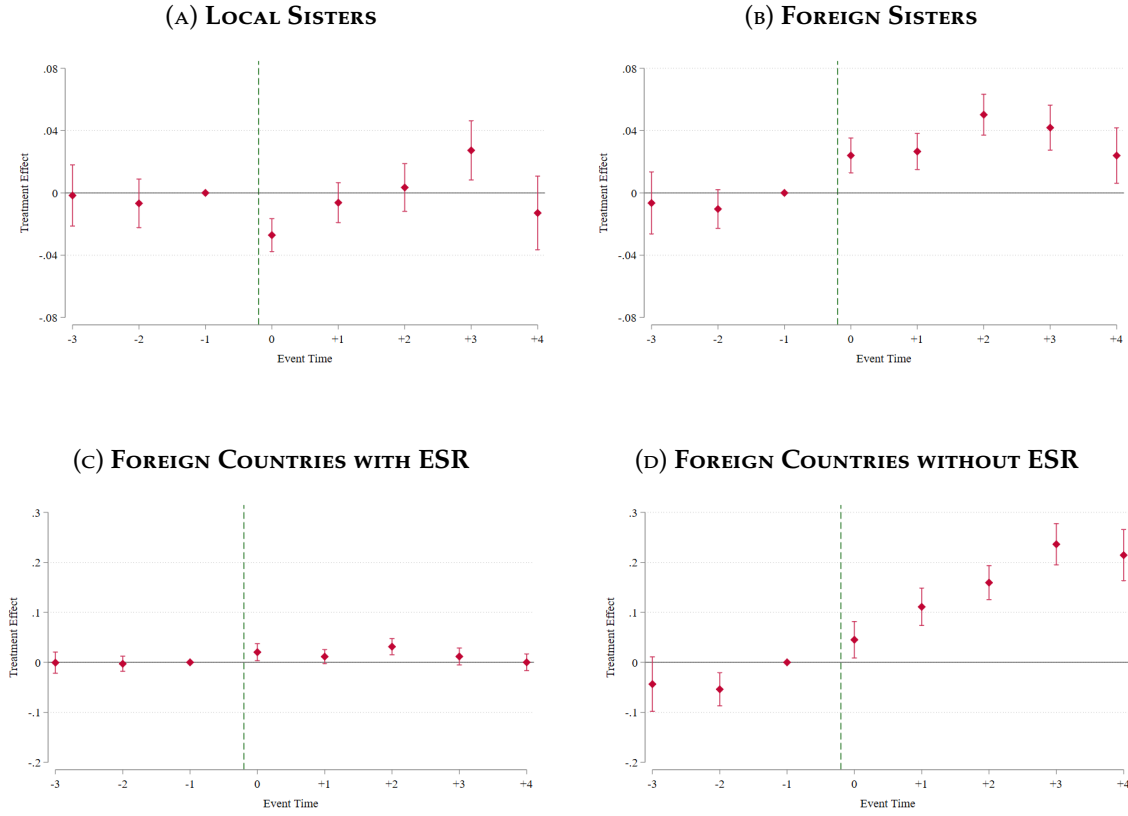
Notes: This figure reports dynamic firm-level treatment effects of ESR. Panels A, B, and C are in USD millions. Panel D reports rates in decimal form (e.g., 0.05 = 5%). The outcomes of treated firms are compared with that of control firms; sister firms are dropped from the analysis. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE IV: EFFECTS OF ESR - TREATED FIRMS



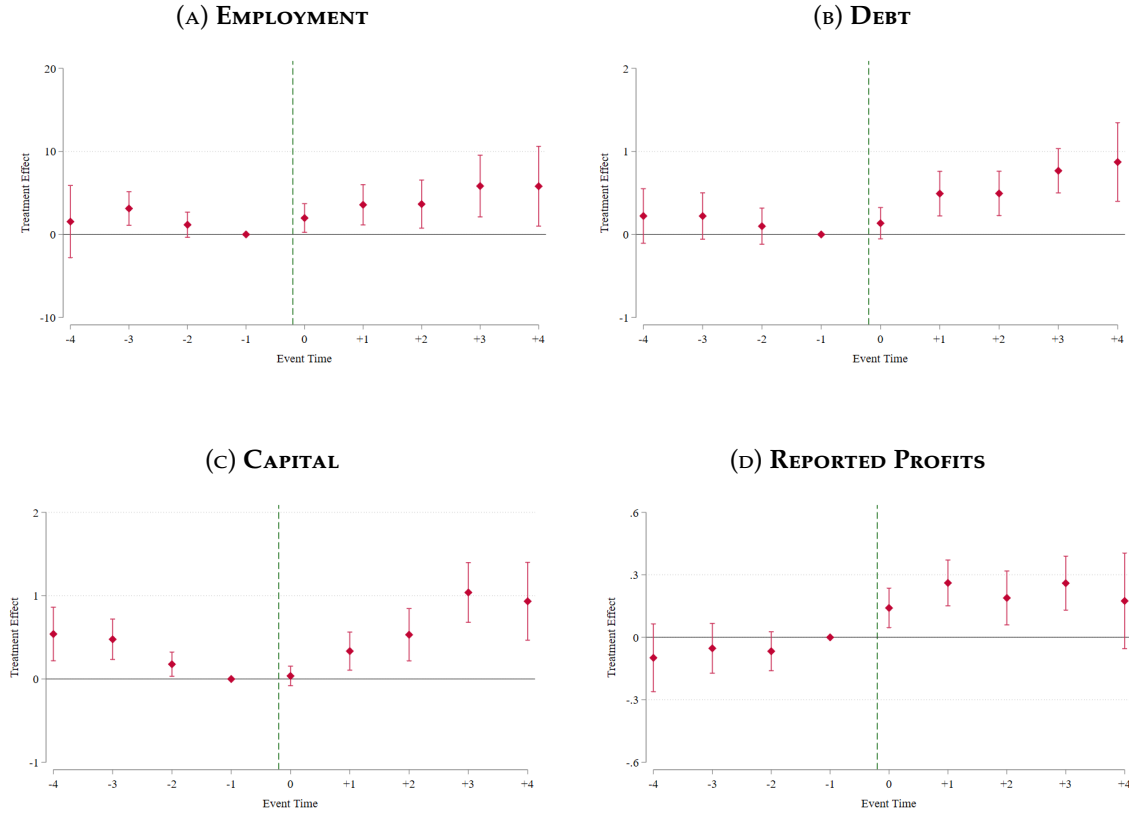
Notes: This figure reports dynamic firm-level treatment effects of ESR. Panel A reports rates in decimal form (e.g., 0.05 = 5%), Panels B and C are in USD millions, and Panel D reports total number of employees. Panel A (investment scaled by lagged capital) begins at $t^* - 3$ due to the lag structure. The outcomes of treated firms are compared with that of control firms; sister firms are dropped from the analysis. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE V: REALLOCATION OF INVESTMENTS



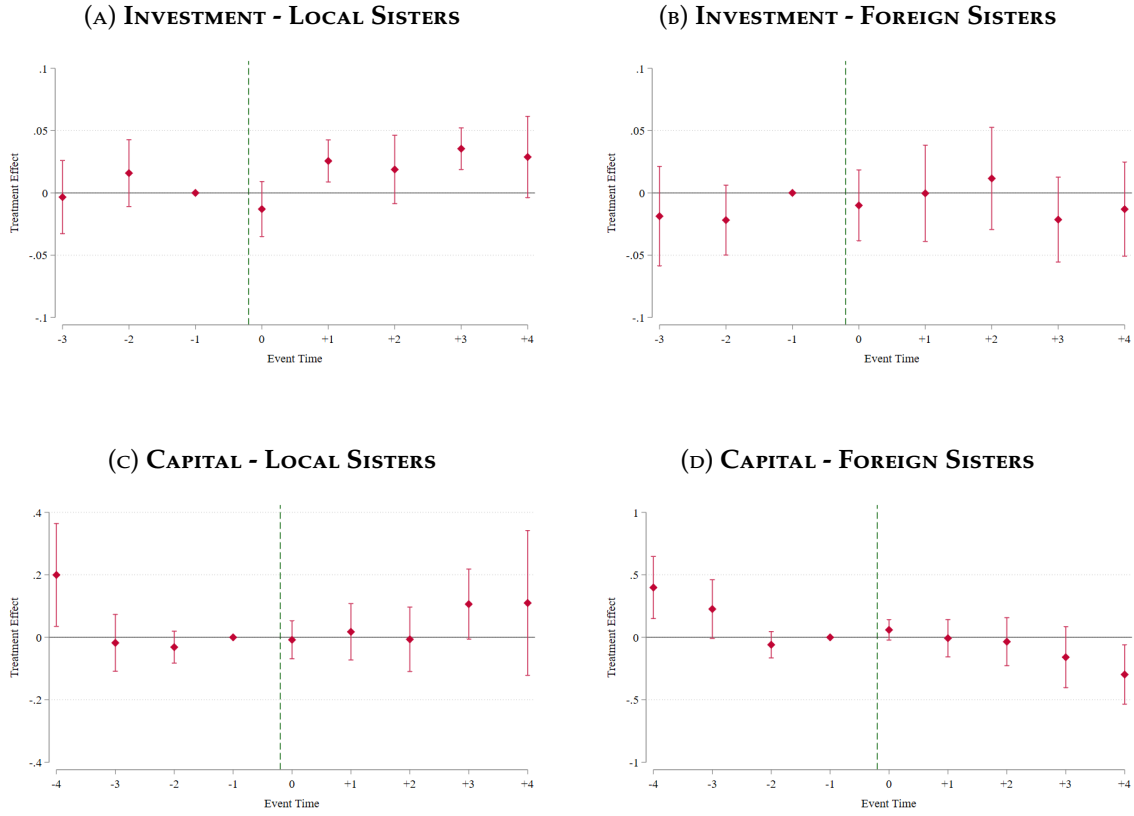
Notes: This figure reports dynamic firm-level effects of ESR. Investments are defined as gross investment scaled by lagged stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report effects on local and foreign sister affiliates, respectively. Panels C and D restrict the sample to foreign sisters whose home country did and did not introduce ESR, respectively. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE VI: REALLOCATION TO FOREIGN COUNTRIES WITHOUT ESR



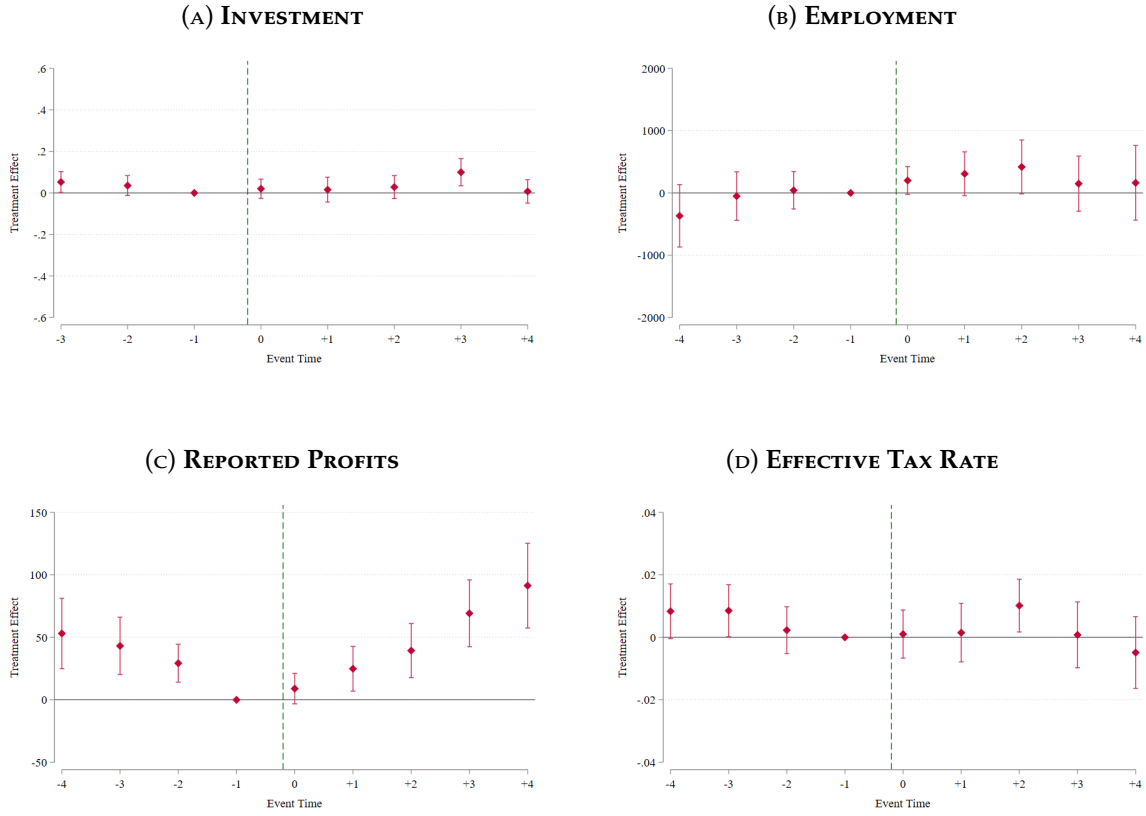
Notes: This figure reports dynamic firm-level effects of ESR. Panel A reports the number of employees. Panels B, C, and D are measured in USD millions. The outcomes of sister firms whose home country did not introduce ESR are compared with that of control group; treated firms and all other sister firms are dropped from the analysis. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE VII: EFFECT ON FULLY COVERED MNEs



Notes: This figure reports dynamic firm-level effects of ESR. The sample is restricted to sister firms of MNE groups in which all affiliates operated in jurisdictions that had implemented ESR (“Fully Covered” MNEs). The outcomes of such local and foreign sisters are compared against control firms. Investments are defined as gross investment scaled by lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Capital is reported in USD millions. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

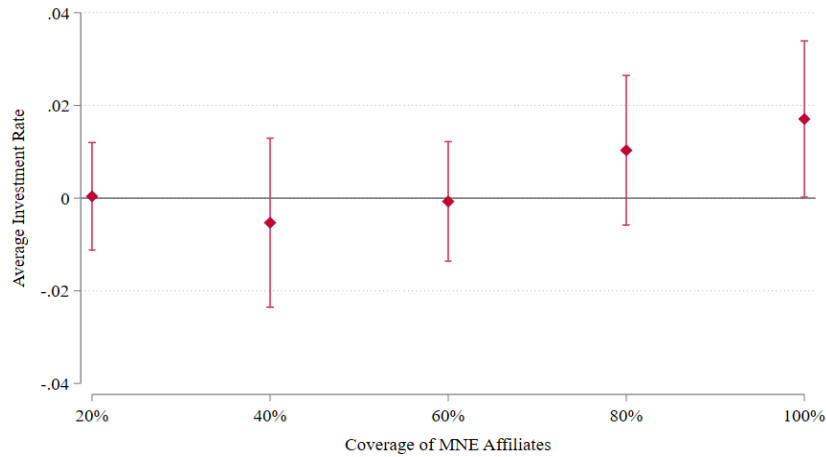
FIGURE VIII: EFFECT OF ESR - GROUP LEVEL



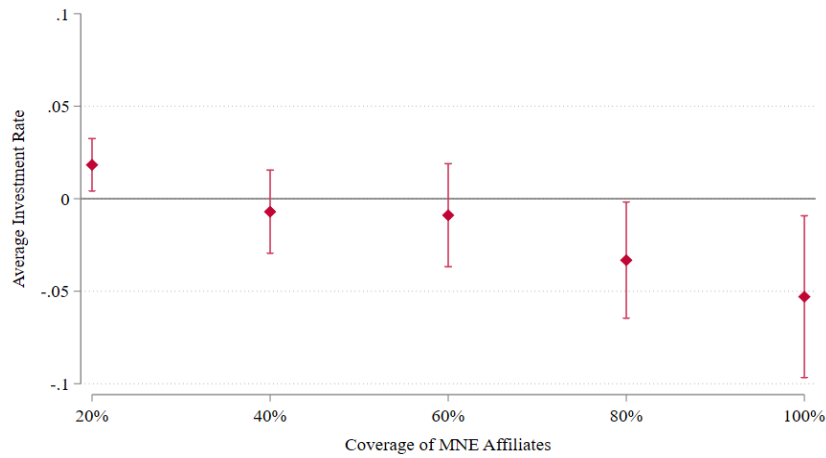
Notes: This figure reports dynamic group-level treatment effects of ESR. Panels A and D report rates in decimal form (e.g., 0.05 = 5%); Panel A (investment scaled by lagged capital) begins at $t^* - 3$ due to the lag structure. Panel B reports the total number of employees, and Panel C reports the estimates in USD million. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the group level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE IX: TREATMENT INTENSITY - MNE AFFILIATE'S COVERAGE

(A) AVERAGE INVESTMENTS AT LOCAL AFFILIATES



(B) AVERAGE INVESTMENTS AT FOREIGN AFFILIATES



Notes: This figure plots the dynamic effects of ESR on investments in local and foreign affiliates by treatment intensity. Treatment intensity is measured at the MNE-group level as the share of affiliates located in jurisdictions that have implemented the rule. For each group-year, affiliates are assigned based on the quintiles of this exposure measure, where each category reflects an “at least” interpretation—e.g., the second quintile corresponds to groups with at least 40% of affiliates covered by the rule, the third with at least 60%, and so on. For each intensity level, the sample is restricted to 3 pre-reform and 3 post-reform periods. All specifications include firm and year fixed effects and jurisdiction-level controls. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level.

TABLE I: SUMMARY STATISTICS

	Observations	Mean	P10	Median	P90	Distinct Units
<i>Firm Level Variables</i>						
Debt	1,186,799	25.65	0	.0013	10.25	172,549
Equity	1,186,585	79.12	-.1202	.9431	23.15	172,545
Capital	1,186,799	19.26	.0006	.3724	15.95	172,549
Employment	827,581	153.2	1	23	256	139,278
Investment	879,333	181.9	-.2474	.0613	.9946	151,601
Interest	739,505	1.363	.0001	.0109	.6258	129,263
Reported Profits	1,182,888	3.199	-.4011	.0829	3.536	171,635
Effective Tax Rate	1,133,245	.09611	0	.0784	.2212	170,695
<i>Firm Level Controls</i>						
ln(Population)	1,186,799	16.79	15.43	16.78	18.02	39
ln(GDP per Capita)	1,186,799	9.999	8.87	10.48	10.82	39
Corporate Tax Rate	1,186,709	.2397	.15	.214	.3443	39
<i>Group Level Variables</i>						
Debt	66,493	1269	0	40.19	2014	8,584
Equity	66,258	1756	5.446	120.4	3135	8,577
Capital	66,493	2091	2.937	88.48	3362	8,584
Employment	52,062	10644	94	1290	22939	7,582
Investment	32,639	81.6	-.1363	.1332	.7237	5,274
Interest	36,523	50.91	.0126	1.158	64.46	5,507
Reported Profits	64,749	256.4	-5.067	10.58	401.4	8,433
Effective Tax Rate	39,880	.1378	0	.1372	.2974	5,820
<i>Group Level Controls</i>						
ln(Population)	65,519	17.53	15.53	17.92	19.6	78
ln(GDP per Capita)	65,405	10.52	9.853	10.66	11	76
Corporate Tax Rate	66,461	.2643	.19	.27	.3443	78

Notes: This table provides summary statistics for the unbalanced panel data set consisting of four pre-reform and 4 post-reform years. *Distinct Units* is the number of distinct firms for "Firm Level Variables", number of distinct jurisdictions across which the firms are spread for "Firm Level Controls", number of distinct MNE groups for "Group Level Variables", and number of distinct jurisdictions across which the MNE groups are spread for "Group Level Controls". *Debt*, *Equity*, *Capital*, *Interest*, and *Reported Profits* are measured in USD millions. *Investment* and *Effective Tax Rate* are expressed as ratios in decimal form (e.g., 0.05 = 5%) and *Employment* is the number of employees.

TABLE II: TAX AVOIDANCE EFFECTS OF ESR - TREATED FIRMS

Outcomes (y_i):	Debt	Interest	Reported Profits	Effective Tax Rate	Capital	Employment	Investment	Equity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Failed_i \times Post_t$	-.973*** (.115)	-.048*** (.004)	.516*** (.045)	.013*** (.001)	-.689*** (.089)	-3.19*** (1.05)	-.02** (.01)	1.47*** (.161)
Pre-Reform Mean	10.4	.585	.818	.061	15	140	.193	13.4
Percentage Change	-9.34%	-8.20%	63.04%	21.41%	-4.60%	-2.28%	-10.61%	10.94%
Observations	1,996,371	1,020,738	1,990,010	1,824,780	1,996,371	1,285,673	1,522,340	1,995,594
Switchers	15,750	13,246	15,699	13,995	15,750	8,735	13,701	15,738

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of ESR at the firm level. The outcomes of treated firms are compared with the control group; sister firms are dropped from the analysis. Columns (1)-(3), (5), and (8) report estimates in USD million. Columns (4) and (6) are expressed as ratios in decimal form (e.g., $0.05 = 5\%$), and Column (7) is the number of employees. Pre-Reform Mean is the pre-reform sample mean of the treated firms. I control for firm and year fixed effects and time-varying jurisdiction-level controls. The standard errors are robust, bootstrapped and clustered at firm level. Observations is the number of observations used in the estimation, and Switchers is the number of firms for which treatment status changed from 0 to 1.

TABLE III: EFFECT OF ESR - SISTER FIRMS

	All Sisters Affiliates	Local Only	Foreign Only	Foreign ESR	Foreign No ESR	Local (Fully Covered)	Foreign (Fully Covered)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Investment							
<i>Failed_i × Post_t</i>	.019*** (.005)	-.003 (.005)	.034*** (.005)	.016** (.007)	.137*** (.013)	.017** (.008)	-.006 (.014)
Pre-Reform Mean	0.215	0.198	0.227	0.239	0.167	0.145	0.194
Percentage Change	8.98%	-1.58%	14.78%	6.77%	81.94%	11.49%	-3.01%
Observations	1,624,872	1,551,075	1,566,146	1,555,870	1,504,622	1,455,743	1,502,988
Switchers	90,098	35,588	54,510	46,798	7,712	12,939	7,680
Panel B: Employment							
<i>Failed_i × Post_t</i>	.707* (.388)	-.006 (.596)	1.07** (.461)	.595* (.344)	3.83*** (1.23)	-.09 (.74)	1.03 (.981)
Pre-Reform Mean	118.782	101.503	129.415	127.692	138.858	73.943	83.048
Percentage Change	0.60%	-0.01%	0.82%	0.47%	2.76%	-0.12%	1.23%
Observations	1,378,207	1,309,061	1,341,261	1,331,281	1,280,897	1,284,855	1,278,469
Switchers	66,119	22,760	43,359	37,296	6,063	8,191	5,165
Panel C: Effective Tax Rate							
<i>Failed_i × Post_t</i>	.002*** (0)	.003*** (.001)	.001 (.001)	0 (0)	.006*** (.002)	.003** (.001)	.002 (.001)
Pre-Reform Mean	0.093	0.080	0.101	0.097	0.122	0.092	0.081
Percentage Change	2.14%	4.36%	0.94%	0.20%	4.61%	3.44%	2.09%
Observations	1,989,817	1,877,026	1,896,224	1,881,056	1,800,665	1,825,008	1,798,894
Switchers	115,538	45,470	70,068	60,766	9,302	14,697	9,449

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of ESR at the firm level. Treated firms are excluded, and each category of sister firms is compared against the control group. *Investment* and *Effective Tax Rate* are expressed as ratios (e.g., $0.05 = 5\%$); *Employment* is the number of employees. Column (1) presents estimates for all sister firms. Columns (2) and (3) report results for local and sister firms, respectively. Columns (4) and (5) show estimates for foreign firms, distinguishing between those in jurisdictions that introduced ESR and those that did not. Columns (6) and (7) restrict the sample to MNE groups whose affiliates were fully covered by ESR; foreign firms in this category are located in jurisdictions that also introduced ESR rules. Pre-Reform Mean refers to the mean outcome for each firm category in the pre-reform period. All specifications include firm and year fixed effects and time-varying jurisdiction-level controls. Standard errors are robust, bootstrapped and clustered at the firm level.

TABLE IV: ESR POLICY: TREATMENT INTENSITY AND COORDINATION

Panel A: Treatment Intensity - MNE Affiliate's Coverage

	At least 20%		At least 40%		At least 60%		At least 80%		At least 100%	
	Local	Foreign	Local	Foreign	Local	Foreign	Local	Foreign	Local	Foreign
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Failed_i \times Post_t$	0.000 (0.007)	0.018*** (0.007)	-0.003 (0.008)	-0.008 (0.009)	0.002 (0.010)	-0.012 (0.019)	0.012 (0.010)	-0.035** (0.018)	0.018* (0.011)	-0.051** (0.023)
Pre-Reform Mean	0.259	0.280	0.285	0.324	0.279	0.356	0.197	0.393	0.268	0.562
Percentage Change	0.19%	6.54%	-1.18%	-2.38%	0.60%	-3.33%	5.94%	-8.97%	6.83%	-9.01%
Observations	1,187,797	1,218,165	1,164,957	1,164,247	1,162,713	1,150,459	1,160,753	1,143,417	1,159,950	1,142,310
Switchers	30,047	42,117	15,455	13,925	13,271	5,721	12,504	3,828	11,701	2,721

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Panel B: Effects of Coordinated ESR Policy

	Affiliate Exposure		Country Exposure		Full Group Exposure	
	Local	Foreign	Local	Foreign	Local	Foreign
	(1)	(2)	(3)	(4)	(5)	(6)
$Failed_i \times Post_t$	0.001** (0.000)	-0.002** (0.001)	0.008 (0.007)	-0.012** (0.006)	0.021** (0.010)	-0.025 (0.023)
Pre-Reform Mean	0.145	0.194	0.145	0.194	0.145	0.194
Percentage Change	0.45%	-1.15%	5.72%	-6.19%	14.44%	-13.10%
Observations	1,289,708	1,272,529	1,290,147	1,272,620	1,289,296	1,271,631
Switchers	13,723	8,190	14,040	8,235	13,429	7,461

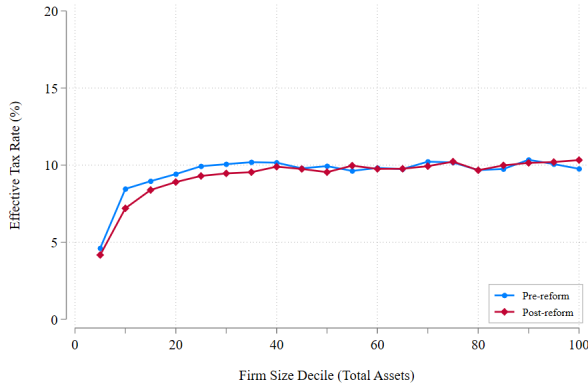
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

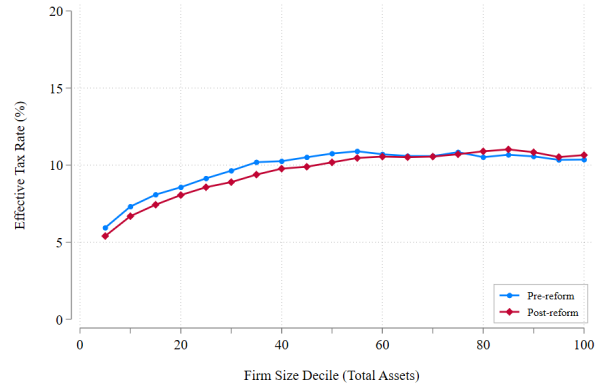
Notes: This table examines the impact of ESR adoption on investment (gross investment scaled by lagged capital). Panel A reports effects by quintiles of group-level ESR exposure, where each category reflects an "at least" interpretation—e.g., the second quintile corresponds to groups with at least 40% of affiliates covered by the rule and so on. Panel B reports the marginal effect of additional affiliates/countries falling under ESR and the effect of full exposure (0 to 1). All specifications include firm and year fixed effects and jurisdiction-level controls. Pre-Reform Mean is the pre-reform sample mean of the treated group. Standard errors are robust, bootstrapped and clustered at the firm level.

FIGURE A.I: ADDITIONAL STYLIZED FACTS

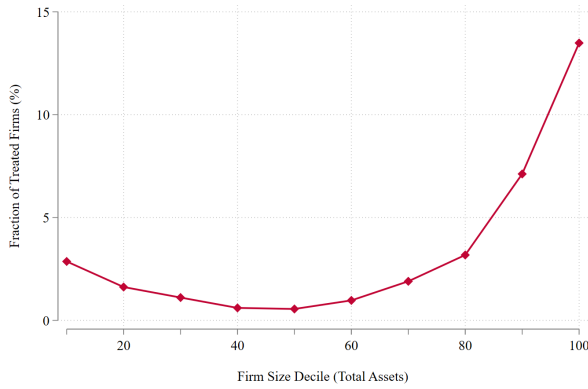
(A) EFFECTIVE TAX RATES - SISTER FIRMS ONLY



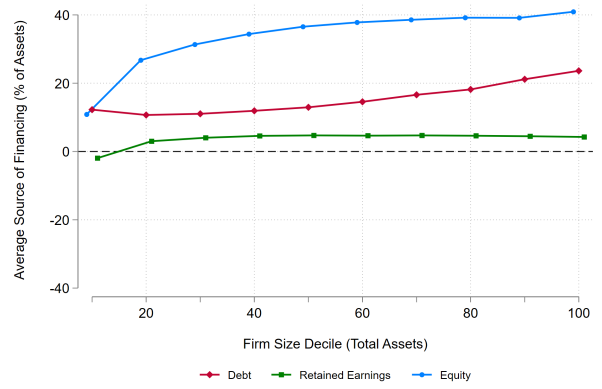
(B) EFFECTIVE TAX RATES - CONTROL FIRMS ONLY



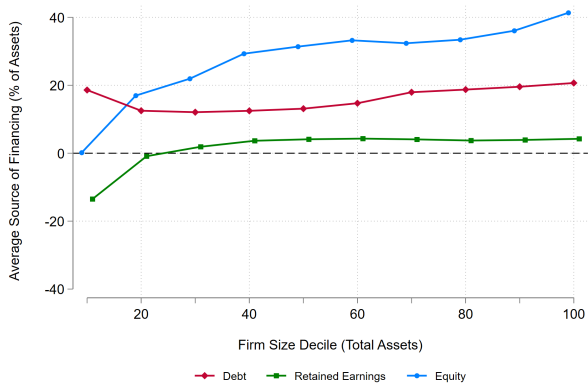
(C) SHARE OF TREATED FIRMS BY FIRM SIZE



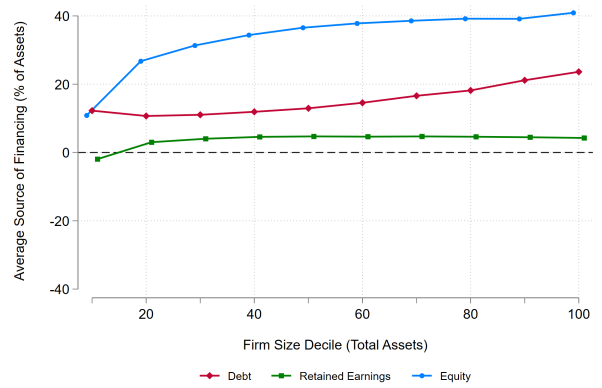
(D) CAPITAL STRUCTURE - TREATED FIRMS



(E) CAPITAL STRUCTURE - SISTER FIRMS

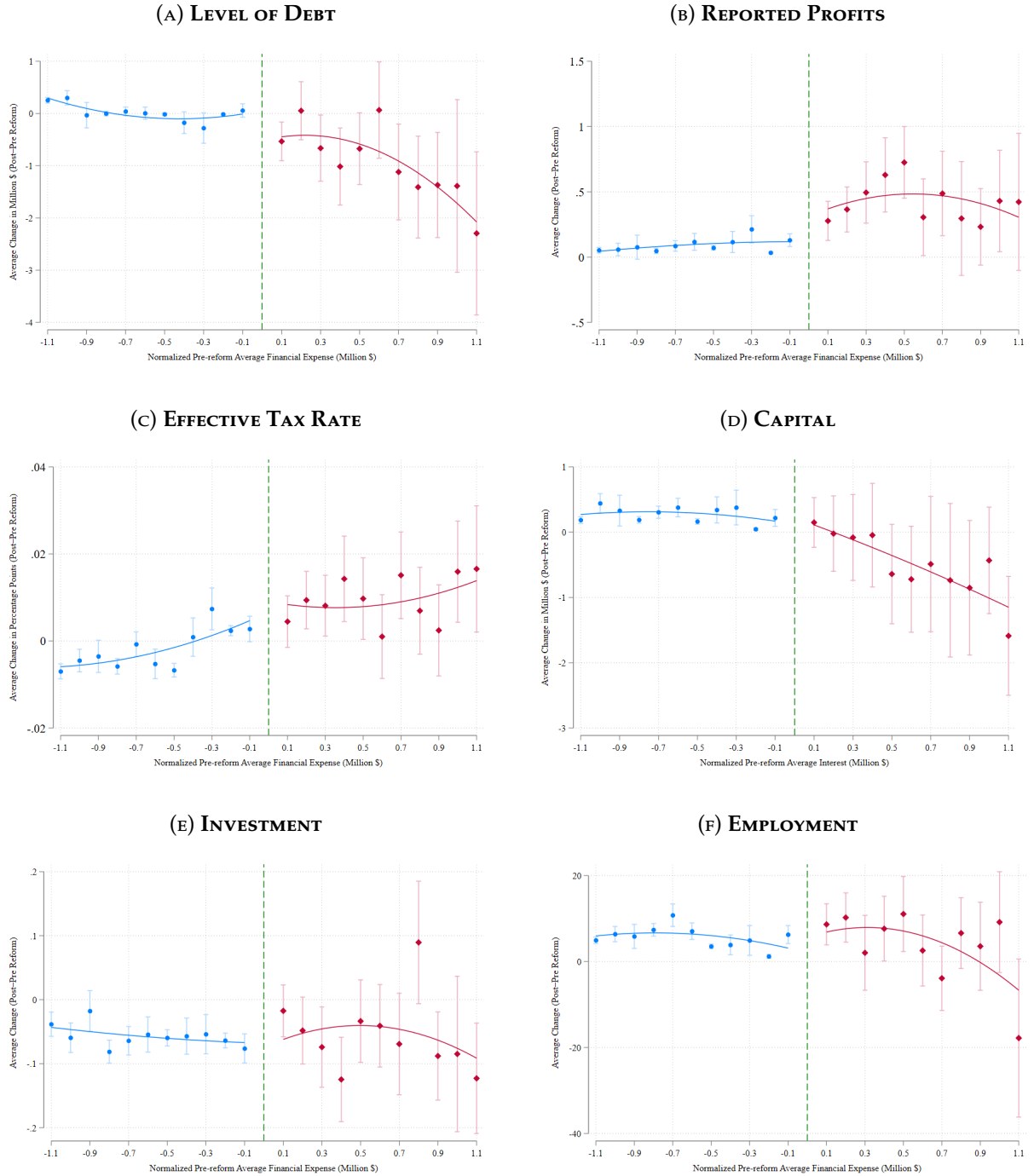


(F) CAPITAL STRUCTURE - CONTROL FIRMS



Notes: Panel A plots the share of treated firms across firm size deciles, where firm size deciles are constructed from the mean of total assets within each affiliate. The vertical axis reports the percentage of treated affiliates in each decile. The sample is restricted to three pre-reform years and to firms with non-missing positive total assets in all three periods. Panels B-D plot average debt-to-assets, retained earnings-to-assets, and equity-to-assets ratios by firm size decile, where firm size is measured by total assets. All ratios are winsorized at the 5% level.

FIGURE A.II: REGRESSION DISCONTINUITY ESTIMATES



Notes: This figure plots the average difference in the mean pre- and post-reform outcomes around the de-minimis cut-off. The running variable is the difference between pre-reform average financial expenses and the country-specific de-minimis threshold. For countries where ESR applies to net (gross) financial expenses, the figure reports net (gross) values. The average changes are reported in evenly spaced bins of 0.1 million USD. The solid circles depict the mean estimates and the bars depict confidence intervals at 95%. Panels A, B, and D are in USD millions. Panels C and E report rates in decimal form (e.g., 0.05 = 5%). Panel F reports total number of employees.

A Proofs for the Conceptual Framework

Proof of Lemma 1. With $A_j^0 = \infty$, (2) equals $(1 - \tau_c)y_j - u(b_j; \tau_c)K_j$; b_j enters only through u , giving (i). Summing over $j \in J_c$ and using $\sum_j y_j = F_c(K_c)$, the group problem is $\max \sum_c [(1 - \tau_c)F_c(K_c) - \omega(\tau_c)K_c]$ subject to $\sum_c K_c = \bar{K}$, giving (ii). Since only K_c enters, (iii) follows. $\square$

Proof of Proposition 1. (i) At given K_j , for $b \leq A_j^1/(iK_j)$ the cap is slack and the objective per unit of capital is $-u(b; \tau)$, strictly increasing in b below $b^*(\tau)$; for $b > A_j^1/(iK_j)$ the deduction equals the lump sum τA_j^1 and the objective is $-u(b; 0) + \tau A_j^1/K_j$, maximized at $b^*(0)$. Since the affiliate retains interest above its allowance at the date-1 optimum, $b^*(0) > A_j^1/(iK_j^1)$, the optimum is $b^*(0)$, which lies strictly below $b^*(\tau)$ because b^* is strictly increasing and $\tau > 0$. The leverage ratio and interest expense per unit of capital therefore fall strictly and the equity share $1 - b_j$ rises strictly; combined with the contraction of K_j in (ii), total debt and interest expense fall strictly.

(ii) The inframarginal deduction τA_j^1 is a lump sum, so the marginal unit of capital at j is financed at cost $\omega(0) = \omega(\tau) + \Delta(\tau)$. With (3) and (7), a higher marginal financing cost at j lowers its optimal capital relative to any affiliate whose ω is unchanged; Lemma 2 converts this first-order comparison into a statement about the date-1 optimum.

(iii) Post-reform interest satisfies $i b_j^1 K_j^1 > A_j^1$, so the deduction equals A_j^1 identically, and $A_j^1 < i b_j^0 K_j^0$ by (4): the tax base widens relative to operating income. The comparison is robust to the endogeneity of y_j : the deduction-to-operating-income ratio equals $i b_j / AP_c$ with $AP_c \equiv F_c(K_c)/K_c$, and post-reform $b_j^1 < b_j^0$ while AP_c weakly rises (adopting-country capital weakly falls by Lemma 2, and $F_c(0) = 0$ with concavity makes the average product decreasing in K_c), so the ratio falls. For the average effective tax rate, pre-reform taxes over pre-tax profit equal $\tau(y_j - i b_j^0 K_j^0)/(y_j - i b_j^0 K_j^0) = \tau$; post-reform the ratio is $\tau(y_j - A_j^1)/(y_j - i b_j^1 K_j^1)$, which exceeds τ if and only if $i b_j^1 K_j^1 > A_j^1$, which holds. $\square$

Proof of Lemma 2. In the coordinates $(d_j, K_j) = (b_j K_j, K_j)$, the leverage cost $K_j m(d_j/K_j)$ is the perspective function of the convex m and hence jointly convex, and $\tau \min\{i d_j, A_j^1\}$ is concave; the feasible set is convex and compact, so an optimum exists, and strict concavity of each F_c makes the aggregates $\{K_c\}$ unique. For the direction, note that given $\{K_c\}$ the problem separates across countries: let $H_c^t(K_c)$ denote the maximized date- t value of country c , so $H_c^0(K_c) = (1 - \tau_c)F_c(K_c) - \omega(\tau_c)K_c$. Each H_c^t is concave (partial minimization of a jointly convex financing cost), and the reform weakly lowers marginal values, $H_c^{1'} \leq H_c^{0'}$: the marginal unit of country capital costs $\omega(\tau_c)$ wherever it can still be financed with fully deductible interest—all of a no-policy country, and an adopting country up to its unexposed headroom—and strictly more beyond. The date-1 aggregates maximize $\sum_c H_c^1$ subject to $\sum_c K_c = \bar{K}$ with multiplier η^1 . If $\eta^1 > \eta$, then K_c would fall in every country (concavity and $H_c^{1'} \leq H_c^{0'}$), contradicting $\sum_c K_c = \bar{K}$; hence $\eta^1 \leq \eta$, so K_c weakly rises wherever H_c' is unchanged, and, the total being fixed, the countries whose marginal values fell lose

capital in aggregate. First-order comparisons at the pre-reform allocation therefore sign the equilibrium reallocation. $\square$

Proof of Proposition 2. Equation (10) follows from (9), noting for kind (a) that a within-country move leaves K_{c^*} and hence F_{c^*} unchanged exactly, while the transferred capital is financed at $\omega(\tau_{c^*})$ as long as ℓ 's interest remains below A_ℓ ; for kinds (a) and (c) the transferred unit is re-levered at $b^*(\tau_{c(h)})$, so a host absorbs at most $(A_j^1 - ib_h K_h)/[ib^*(\tau_{c(h)})]$ units before its own cap binds; no such bound applies to kind (b). In (i), strict dominance over kind (d) follows from $\Delta(\tau_{c(h)}) > 0$ for every adopter. For the absorption pattern, hosts of kind (b) absorb until their marginal values re-equalize: $dv_k = (1 - \tau_{c(k)})F'' dK_k$ common across hosts implies $dK_k \propto [(1 - \tau_{c(k)})|F''|]^{-1}$ under equal curvature, increasing in $\tau_{c(k)}$; the accompanying debt is $b^*(\tau_{c(k)}) dK_k$, increasing on both margins. In (ii), the dominance inequality and the bound D are immediate from (10); the cross-border gain $\Delta(\tau_{c^*}) - \Delta(\tau_{c(h)})$ is maximized at the foreign affiliate with the lowest statutory rate, giving the direction of any residual movement; and Lemma 2 converts each strict first-order comparison into a statement about the date-1 optimum. $\square$

Online Supplementary Appendix

A Jurisdiction Level Policy Details

This section provides an overview of the Earnings Stripping Rules (ESR) implemented by various countries between 2010 and 2021. A central feature of Action 4 under the OECD's Base Erosion and Profit Shifting (BEPS) initiative was the broadening of the definition of financial expenses to encompass items economically equivalent to interest, including financial lease charges, foreign exchange gains/losses, and service fees on financial products. In addition, Action 4 recommended that these rules apply not only to related-party loans but also to third-party debt.

Action 4 also proposed a set of escape clauses to accommodate legitimate financing needs and prevent overreach. These include the Grandfathering Clause, the Equity Escape, and the Group Ratio Rule, along with provisions allowing the carry-forward of disallowed interest expenses and unused deduction capacity to future periods.¹⁵

Because the dataset covers the period 2010-2021, policy details for Germany and Italy—both of which implemented ESR before 2010—are excluded. For all remaining rule-enforcing jurisdictions, the relevant policy details are briefly presented below.

- **Albania:** announced the policy in 2016 and introduced these rules on January 1st, 2018. It allows interest expenses to related parties of 30% of EBITDA. The excess interest expenses can be carried forward for up to 5 years.¹⁶
- **Argentina:** introduced these rules on January 1st, 2018. It allows gross interest expense of 30% of EBITDA and sets a *de minimis* threshold of ARS 1 million. The excess interest expenses can be carried forward for up to 5 years while the unused capacity can be carried forward for up to 3 years.¹⁷
- **Austria:** announced the policy in 2020 and introduced these rules on January 1st, 2021. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The excess interest expenses can be carried forward indefinitely. The rule includes a grandfathering clause and allows for group ratio.¹⁸
- **Belgium:** announced the policy in 2017 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold

¹⁵ Under the Grandfathering Clause, loans concluded before 17 June 2016 are exempt from the policy. The Equity Escape allows a firm to be exempt from ESR if its equity-to-total-assets ratio is no less than two percentage points below that of its consolidated group. The Group Ratio Rule permits a firm that fails the standard ESR test to deduct interest up to the ratio of net third-party interest to EBITDA at the consolidated group level, with the option to be taxed on a group rather than entity basis. Several jurisdictions also exempt loans used to finance long-term public infrastructure projects.

¹⁶ For details please see for example [here](#). The website was accessed on 13 Oct 2022.

¹⁷ For details please see for example [here](#). The website was accessed on 19 Jul 2025.

¹⁸ For details please see for example [here](#). The website was accessed on 19 Dec 2023.

of EUR 3 million. The excess interest expenses can be carried forward indefinitely and the law includes a grandfathering clause.¹⁹

- **Botswana:** announced the policy in 2018 and introduced these laws on July 1st, 2019. It allows net interest expense of 30% of EBITDA.²⁰
- **Bulgaria:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The excess interest expenses can be carried forward indefinitely. A parallel 3:1 debt-to-equity ratio rule on related-party borrowings is also present.²¹
- **Costa Rica:** announced the policy in 2018, and introduced these rules on January 1st, 2020. It allows gross interest expense of 30% of EBITDA and the allowed percentage is to gradually decrease down by 2% each year to 20%.²²
- **Croatia:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The excess interest expenses can be carried forward for up to three years.²³
- **Cyprus:** introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The policy includes a grandfathering clause and allows for group ratio. The excess interest expenses and unused capacity can be carried forward for up to 5 years.²⁴
- **Czech Republic:** announced the policy in 2018 and introduced these rules on April 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of CZK 80 million. The excess interest expenses can be carried forward indefinitely and a parallel 4:1 debt-to-equity ratio rule is also present on related party loans only.²⁵
- **Denmark:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of DKK 22313400. The excess interest expenses can be carried forward indefinitely. A parallel debt:equity of 4:1 and an assets escape clause is also present.²⁶

¹⁹ There are also some other escape provisions, ex., if the taxpayer is part of a Belgian group, a notional consolidation must take place, and interest (together with economically equivalent) costs and revenues paid to or received from other Belgian group members, is disregarded when determining excess borrowing costs. It also is necessary to neutralize the outcomes of other intra-group transactions that affect EBITDA. To ensure a consistent consolidation for the purposes of the interest limitation rule, the negative EBITDA of a Belgian group member is allocated to the other Belgian group members with a positive EBITDA, in proportion to each member's positive EBITDA. However, I define the rule at the most basic and baseline definition. For details please see for example [here](#). The website was accessed on 19 Dec 2023.

²⁰ For details please see for example [here](#). The website was accessed on 13 Oct 2022.

²¹ For details please see for example [here](#). The website was accessed on 19 Jul 2025.

²² For details please see for example [here](#). The website was accessed on 13 Oct 2022.

²³ For details please see for example [here](#). The website was accessed on 13 Oct 2022.

²⁴ For details please see for example [here](#). The document was accessed on 19 Jul 2025.

²⁵ For details please see for example [here](#). The document was accessed on 13 Oct 2022.

²⁶ I abstract from the asset escape clause due to data limitations. For details please see for example [here](#). The website was accessed on 18 Jul 2025.

- **Estonia:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The policy also allows for a group ratio.²⁷
- **Finland:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 25% of EBITDA and sets a *de minimis* threshold of EUR 0.5 million for related party loans and EUR 3 million for non-related parties. The excess interest expense can be carried forward indefinitely. The rule entails a grandfathering clause and also allows for group ratio.²⁸
- **France:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The excess interest expense can be carried forward indefinitely and the unused capacity can be carried forward for up to 5 years²⁹.
- **Gabon:** introduced these rules on January 1st, 2019. It allows the deductibility of related-party interest expenses to 25% of EBITDA provided that the debt:equity ratio is more than 1:1.5.³⁰
- **Greece:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The excess interest expense can be carried forward indefinitely.³¹
- **Hungary:** introduced these rules on January 1st, 2019.³² It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of HUF 939810000. The policy also entails an equity escape clause.³³
- **Iceland:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of ISK 100 million. The rule also allows for equity escape.³⁴

²⁷ For details please see for example [here](#). The website was accessed on 19 Jul 2025.

²⁸ For details please see for example [here](#). The website was accessed on 20 Dec 2023.

²⁹ For companies that have debt:equity greater than 1:5, there are additional rules that further limit the deductibility of interest expenses. While the external debt still could still be capped at 30% of EBITDA—provided that the portion of interest deemed to derive from external debt, calculated as total interest multiplied by the amounts put at the disposal of the company by unrelated parties increased by 1.5 x equity / total amounts put at the disposal of the company, the internal debt is capped at 10% of EBITDA. Additionally, 75% of the net financial expenses exceeding the threshold is tax deductible, provided that the equity:asset of the company is at least equal to, or is not lower by more than two percentage points, the equity of the consolidated group to which it belongs. I abstract from the case when debt:equity is greater than 1:5 and instead apply the 30% EBITDA rule and the *de-minimis* threshold. This is the most general definition of the law and is transparent of all the other complementarities. The other details mentioned further tighten the law only if the two mentioned laws are failed. Therefore, there is no over-identification of treated firms subject to such laws. For details please see for example [here](#). The website was accessed on 20 Dec 2023.

³⁰ For details please see for example [here](#). The website was accessed on 13 Oct 2022.

³¹ For details please see for example [here](#). The website was accessed on 19 Jul 2025.

³² I assume the policy announcement year to be 2018.

³³ For details please see for example [here](#) and [here](#). The websites were accessed on 13 Oct 2022 and 19 Dec 2023.

³⁴ For details please see for example [here](#). The website was accessed on 11 Jul 2024.

- **India:** announced the policy in February 2017 and introduced these rules on April 1st, 2018.³⁵ It allows gross interest expense of 30% of EBITDA and sets a *de minimis* threshold of INR 10 million. The excess interest expense can be carried forward for up to 8 years. The rule does not bind if the disallowed expense is lower than the interest paid/payable to the associated enterprises in the financial year.³⁶
- **Ireland:** introduced these rules on January 1st, 2021 and, hence, is out of the scope of this study.³⁷
- **Japan:** announced the policy in 2019 and introduced these rules on April 1st, 2020. It allows net interest expense of 20% of EBITDA and sets a *de minimis* threshold of JPY 20 million. The policy also allows for group ratio and has a parallel debt:equity test of 3:1. The excess interest expense can be carried forward for up to 7 years.³⁸
- **Korea:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA. The rule has a parallel debt:equity test of 2:1.³⁹
- **Latvia:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The policy also allows for equity escape.⁴⁰
- **Lithuania:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The policy also allows for group ratio and equity escape.⁴¹
- **Luxembourg:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The policy also entails a grandfathering clause and allows for equity escape. The excess interest expenses can be carried forward indefinitely and the unused interest capacity can be carried forward for up to 5 years.⁴²
- **Malaysia:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows gross interest expense of 20% of tax-EBITDA and sets a *de minimis* threshold of RM 500,000. The excess interest expenses can be carried forward indefinitely.⁴³

³⁵ While the accounting year runs from January to December, the fiscal year in India extends from April to March. For consistency with other countries in the dataset, the February 2017 announcement is treated as occurring in 2016.

³⁶ For details please see for example [here](#). The website was accessed on 19 Jul 2025.

³⁷ For details please see for example [here](#). The website was accessed on 13 Oct 2025.

³⁸ For details please see for example [here](#). The website was accessed on 23 Aug 2022.

³⁹ For details please see for example [here](#). The website was accessed on 18 Jul 2025.

⁴⁰ For details please see for example [here](#). The website was accessed on 18 Jul 2025.

⁴¹ For details please see for example [here](#). The document was accessed no 19 Jul 2025.

⁴² For details please see for example [here](#). The website was accessed on 20 Dec 2023.

⁴³ For details please see for example [here](#). The website was accessed on 13 Oct 2022.

- **Malta:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 3 million. The policy also allows for group ratio and entails a grandfathering clause. The excess interest expenses can be carried forward indefinitely while the unused capacity can be carried forward for up to 5 years.⁴⁴
- **Mexico:** announced the policy in 2019 and introduced these rules on January 1st, 2020. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of MxP 20 million. The excess interest expenses can be carried forward for up to 10 years and the rule is only applicable if debt:equity is greater than 3:1.⁴⁵
- **Mongolia:** introduced the policy on January 1st, 2020. It allows interest expense to related parties of 30% of EBITDA. The rule is only applicable if debt:equity is greater than 3:1.⁴⁶
- **Netherlands:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 1 million. The excess interest expense can be carried forward indefinitely.⁴⁷
- **Nigeria:** introduced these rules on January 1st, 2020. It allows interest expense of 30% of EBITDA. The excess interest expense can be carried forward for up to 5 years.⁴⁸
- **Norway:** announced the policy in 2013 and introduced these rules in 2014. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of NOK 5 million. The policy also allows for equity escape.⁴⁹
- **Poland:** announced the policy in 2017 and introduced the rules on January 1st, 2018. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of PLN 3 million. The excess interest expense can be carried forward for up to 5 years.⁵⁰
- **Portugal:** announced the policy in 2018 and introduced the rules on May 4th, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 1 million. The excess interest expenses and unused interest capacity can be carried forward for up to 5 years.⁵¹
- **Romania:** announced the policy in 2017 and introduced these rules on January 1st, 2018. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold

⁴⁴ For details please see for example [here](#). The website was accessed on 14 Oct 2022.

⁴⁵ For details please see for example [here](#). The website was accessed on 20 Dec 2023.

⁴⁶ For details please see for example [here](#). The website was accessed on 14 Oct 2022.

⁴⁷ For details please see for example [here](#) and [here](#). The documents were accessed on 14 Oct 2022 and 20 Dec 2023, respectively.

⁴⁸ For details please see for example [here](#) and [here](#). The websites were accessed on 19 Jul 2025 and 14 Oct 2022, respectively.

⁴⁹ For details please see for example [here](#).

⁵⁰ For details please see for example [here](#). The website was accessed on 18 Jul 2025.

⁵¹ For details please see for example [here](#) and [here](#). The website was accessed on 28 Dec 2023.

of EUR 1 million. For 2018, the deductions of excess borrowing costs were limited to 10% of tax-adjusted EBITDA for amounts exceeding EUR 200,000.⁵²

- **Slovakia:** announced the policy in 2014 and introduced these rules on January 1st, 2015. It allows gross interest expense of 25% of EBITDA.⁵³
- **South Africa:** introduced the policy on January 1st, 2015. It allows gross interest expense of 40% of EBITDA. The excess interest expense can be carried forward to the subsequent year.⁵⁴
- **Spain:** announced the policy in 2014 and was effective from 2015. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of EUR 1 million. The excess interest expenses can be carried forward indefinitely and the policy also allows for group ratio.⁵⁵
- **Sweden:** announced the policy in 2018 and introduced these rules on January 1st, 2019. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of SEK 5 million. The excess interest expenses can be carried forward for up to 6 years and the policy also allows for group ratio.⁵⁶
- **Uganda:** introduced the policy on July 1st, 2018. It allows gross interest expense of 30% of EBITDA. The excess interest expenses can be carried forward for up to 3 years.⁵⁷
- **UK:** announced the policy in 2016 and introduced these rules on April 1st, 2017. It allows net interest expense of 30% of EBITDA and sets a *de minimis* threshold of GBP 2 million. The excess interest expenses can be carried forward indefinitely and the policy also allows for group ratio.⁵⁸
- **US:** announced the policy in 2017 and introduced these rules on January 1st, 2018. It limits the deductibility of interest expense to the sum of (i) the taxpayer's business interest income for the year, (ii) 30% of the taxpayer's adjusted taxable income (ATI), and (iii) the taxpayer's floor plan financing interest expense of the year. The rule is only applicable if the average annual gross receipts are of \$25 million or less in the previous three years.⁵⁹
- **Vietnam:** introduced the policy on May 1st, 2017. It allows gross interest expense to related parties of 20% of EBITDA.⁶⁰

⁵² For details please see for example [here](#) and [here](#). The websites were accessed on 14 Oct 2022 and 18 Jul 2025, respectively.

⁵³ For details please see for example [here](#) and [here](#). The websites were accessed on 13 Oct 2022 and 18 Jul 2025, respectively.

⁵⁴ For details please see for example [here](#). The document was accessed on 14 Oct 2022.

⁵⁵ For details please see for example [here](#). The document was accessed on 30 Mar 2024.

⁵⁶ For details please see for example [here](#). The website was accessed on 18 Jul 2025.

⁵⁷ For details please see for example [here](#). The document was accessed on 19 Jul 2025.

⁵⁸ For details please see for example [here](#). The website was accessed on 18 Jul 2025.

⁵⁹ For details please see for example [here](#). The website was accessed on 19 Jul 2025.

⁶⁰ For details please see for example [here](#). The document was accessed on 14 Oct 2022.

- **Zambia:** introduced the policy on January 1st, 2019. It allows gross interest expense of 30% of EBITDA and the unused interest expense can be carried forward for up to 5 years.⁶¹

B Data Cleaning

The data for this project were obtained from the ORBIS Historical database via Moody’s Analytics online cluster in December 2022. Data extraction was conducted using PySpark, and the resulting datasets were downloaded from the SQL server onto a local machine. Financial variables were retrieved from the Detailed Financials Monthly table of ORBIS for the years 2010 onward. Information on country identifiers, ownership links, corporate structure, and headquarter-branch relationships was obtained from the Firmographics and Key Ownership tables. Industry classifications were later retrieved from the Wharton Research Data Services (WRDS) repository in 2024, following the expiration of the ORBIS Historical subscription. These datasets were merged with the financial data to construct a comprehensive firm-level panel.

The econometric analysis was restricted to multinational enterprises (MNEs), defined as corporate groups in which at least one affiliate operated outside the country of the global ultimate owner. Observations were limited to firms reporting non-missing values for *financial expenses*, *financial income*, *net financial expenses*, and *EBITDA*.

When multiple filings existed for a given year, only the latest report was retained. The sample was restricted to filings covering twelve months of operations, and among multiple filing types, only those classified as local filing were preserved. Observations reporting negative values for key balance-sheet and financial variables—including *debt*, *capital*, *fixed assets*, *intangible assets*, *tangible assets*, *total assets*, *number of employees*, *revenue*, *sales*, *depreciation*, and *interest paid*—were excluded. Countries with fewer than 100 observations were also dropped. Since Germany and Italy introduced the policy before 2010, all German and Italian firms were dropped to ensure non-contamination of control group. For rule-enforcing countries, when financial statements were reported in multiple currencies, only those denominated in the national currency—corresponding to the currency in which the policy’s de minimis threshold is defined—were retained.

All monetary variables were converted to U.S. dollars, and country-level macroeconomic controls were merged with the main dataset, restricting the final sample to firms with available annual exchange rate and control data. The analysis window includes four pre-treatment and four post-treatment periods. All continuous variables were winsorized at the 5% level to mitigate the influence of outliers.

⁶¹ For details please see for example [here](#). The website was accessed on 19 July 2025.

B.1 Definition of Variables

- **Debt.** Sum of long-term borrowings from credit institutions and bonds issued, and short-term financial debts
- **Equity.** Total equity (capital plus other shareholders' funds).
- **Capital.** Sum of all tangible assets—such as machinery, buildings, and land—and depreciation.
- **Total Assets.** Sum of all non-current and current assets.
- **Gross Investment spending.** Change in fixed capital assets plus depreciation, i.e., $K_t - K_{t-1} + \text{depreciation}$, where K_t denotes the book value of tangible and intangible fixed assets in year t .
- **Investments (I_t).** Ratio of current-year gross investment spending to beginning-of-year book value of tangible and intangible fixed assets, i.e., $[K_t - K_{t-1} + \text{depreciation}]K_{t-1}$.
- **Employment.** Total number of employees on the company's payroll.
- **EBITDA.** A measure of a firm's operating performance over a given year, calculated by excluding expenses related to interest, taxes, depreciation, and amortization from taxable earnings.
- **Interest Expense.** Total amount of interest paid on borrowings or liabilities.
- **Reported Profits.** Sum of operating profit and financial profit.
- **Taxation.** All taxes incurred by a firm in a given year (paid, accrued, or deferred).
- **Effective Tax Rate.** Ratio of Taxation and EBITDA

C Empirical Methodology - Additional Details

C.1 Treatment Definition - Additional Details

For all jurisdictions, the treatment event, t^* , is defined as the year in which the policy is announced. Treatment units are identified as firms that, on average, fail the jurisdiction-specific test in the three years preceding the policy—i.e., firms whose average interest deductions exceed the jurisdiction-specific limits in $t^* - 3$, $t^* - 2$, and $t^* - 1$. When computing the ratio of interest expenses to EBITDA, only observations with strictly positive EBITDA are included. For cases where EBITDA is zero or negative, the ESR fixed ratio test is failed if financial expenses (net or gross, depending on the jurisdiction) are strictly positive. Because this rule could in principle select firms on distress rather than tax planning, I verify that excluding treated units with zero or negative average pre-reform EBITDA leaves all results intact.

To identify treatment status at the affiliate level, the sample is restricted to (i) firms not domiciled in rule-enforcing jurisdictions, or (ii) firms domiciled in rule-enforcing jurisdictions with available financial data for all three pre-reform years. Firms with average deductions below the policy threshold but with annual deductions exceeding the threshold in any of the three pre-treatment years are excluded to ensure consistent treatment classification.

In jurisdictions combining debt-to-equity limits with ESR, the debt-to-equity ratio is calculated only when equity is strictly positive; if equity is zero or negative, the debt-to-equity test is automatically failed if debt is strictly positive. For jurisdictions with group escape clauses—requiring both the equity and earnings tests to be satisfied—the group-level equity ratio is computed and used as the benchmark for individual firms.

Due to data constraints, I abstract from grandfathered loans and infrastructure-related exemptions. Moreover, since the dataset does not capture the full universe of group affiliates within each jurisdiction, group-level ratios cannot be consistently computed. Consequently, I disregard the group ratio rule when identifying treatment units. In some jurisdictions, ESR applies only to related-party borrowings. Because the data do not permit a reliable separation of related-party and third-party interest expenses, I apply the rule to total financial expenses instead. This could in principle misclassify treatment in those jurisdictions; I therefore confirm that excluding treated units in related-party-only jurisdictions, and re-anchoring their sisters, leaves the estimates unchanged, and Section D provides counterparty evidence that the debt response reflects intra-group rather than third-party borrowing.

A few jurisdictions initially implemented ESR only partially. Poland introduced ESR in 2015 alongside existing Thin Capitalization Rules (TCR), allowing firms to choose which regime to apply, Portugal and Greece introduced ESR in 2013 and 2014, respectively, but their frameworks diverged from the OECD and ATAD standards, particularly regarding the definition of exceeding financial costs. However, Poland revoked the right of regime selection in 2017, while Portugal and Greece adopted the OECD-aligned broader definition of financial expenses in 2018. Accordingly, the policy year is defined as 2017 for Poland and 2018 for Portugal and Greece.

Some jurisdictions also revised their ESR parameters after implementation. For instance, Romania announced ESR in 2017, allowing net interest deductions up to 30% of EBITDA with a *de minimis* threshold of EUR 1 million. However, for 2017 only, the deductions of excess borrowing costs were capped at 10% of tax-adjusted EBITDA for amounts exceeding EUR 200,000. Consequently, treated firms in Romania are defined based on the 2017 rule, when the reform was fully implemented, even though the initial parameters differed from OECD recommendations. Similarly, Spain introduced these rules on January 1st, 2012. However, a broader anti-abuse rule which countered hybrid instruments was announced in 2014 and was effective from 2015; therefore, I define the policy year as 2014.

D Additional Analyses

D.1 Movements in the Mass of Zero-Profit Firms

The section on Stylized Facts in the main document reports that treated firms carry more mass at or near zero reported profits. A concentration of firms at zero is, however, common

to all firm types and need not by itself reflect avoidance, so the tax-motivated component of this pattern is identified not by its level but by whether the mass moves once ESR binds. I implement two complementary designs.

The first tests whether the excess mass of firms bunching around zero reported profits, scaled by total assets, falls after the reform specifically among treated firms. Following the bunching methodology of Kleven (2016), I estimate the normalized excess mass separately by treatment group $g \in \{T, C\}$ (treated, control) and period $\rho \in \{\text{pre}, \text{post}\}$ as

$$(13) \quad \hat{b}_{g,\rho} = \frac{\int_{z_L}^{z_U} (\hat{f}_{g,\rho}(z) - \hat{f}_{g,\rho}^0(z)) dz}{\hat{f}_{g,\rho}^0(0) (z_U - z_L)},$$

the observed excess density in the bunching window $[z_L, z_U]$ around the zero-profit threshold, relative to the counterfactual density $\hat{f}_{g,\rho}^0$ —obtained by fitting a flexible polynomial to the empirical distribution of profits-to-assets outside the window—evaluated at the threshold. I bin the profit-to-assets ratio at a width of 0.01 and fit a polynomial of degree five over the range $[-0.20, 0.20]$, excluding the bunching window $[z_L, z_U) = [-0.05, 0.02)$. Standard errors are obtained by a Poisson bootstrap over the binned counts with 300 replications. The parameter of interest is the triple difference

$$(14) \quad \widehat{\Delta^3 b} = (\hat{b}_{T,\text{post}} - \hat{b}_{T,\text{pre}}) - (\hat{b}_{C,\text{post}} - \hat{b}_{C,\text{pre}}),$$

which nets out any level difference in bunching between treated and control firms as well as any common time trend, isolating the differential movement of the mass at zero. The second design applies the same logic directly to the incidence of zero tax payments:

$$(15) \quad \text{zerotax}_{it} = \alpha + \gamma \text{Fail}_i + \lambda \text{Post}_t + \beta \text{Fail}_i \times \text{Post}_t + \varepsilon_{it},$$

where zerotax_{it} is an indicator equal to one if firm i pays no tax in year t , sister firms are excluded so that treated firms are compared directly with controls, and standard errors are clustered at the firm level.

Table G.VI reports the results. The excess mass of zero-profit treated firms falls sharply after the reform relative to controls, $\widehat{\Delta^3 b} = -1.95$, and the share of treated firms paying exactly zero tax declines by 4.6 percentage points relative to controls ($\hat{\beta} = -0.046$). The triple difference is negative for every polynomial degree between four and seven and for fitting half-windows of 0.15, 0.20, and 0.40, ranging from -1.45 to -3.63 across these specifications. Selection on distress and a mechanical accounting spike are both static features of the cross-section and cannot by themselves generate these post-reform movements, which instead trace the retreat from the income-suppressing use of intra-group debt.

D.2 Intra-Group versus Third-Party Debt: Lender-Side Evidence

The outcome variables in the direct-effect measure total debt and interest, which combine internal and external financing. Establishing that the post-reform decline operates predominantly along the intra-group margin requires evidence from the counterparty side of these loans. Each intra-group loan pairs a borrower's interest expense with a related lender's interest income, so a contraction in intra-group borrowing should lower the interest income of the group's own net-lender affiliates. To understand the estimation design, consider the following specification:

$$(16) \quad y_{ist} = \alpha_i + \mu_t + \beta \text{NetLender}_i \times \text{Post}_t + X'_{st} \delta + \varepsilon_{ist},$$

where y_{ist} is the interest income or net financial position of sister affiliate i , and NetLender_i identifies affiliates with a negative pre-reform net financial expense—net internal creditors—located either abroad or in the treated firm's own jurisdiction. Table G.VIII shows that the interest income of net-lender sisters falls after the reform, and by roughly twice as much for foreign lenders (-0.048) as for local ones (-0.027), consistent with foreign affiliates acting as the internal creditors. Panel B replaces $\text{NetLender}_i \times \text{Post}_t$ with $\text{Fail}_i \times \text{Post}_t$ interacted with an indicator for whether the group maintains an internal lending conduit. Treated affiliates in groups with a conduit enter the reform carrying nearly twice the debt and nearly twice the interest expense of those without one. Post-reform, the borrower-side decline is larger in groups that contain an internal lender, consistent with the intra-group channel accounting for a larger share of the overall debt reduction. External deleveraging alone would leave the income of these related affiliates largely unaffected.

D.3 A Tax-Incentive Gradient in the ESR Response

The effective tax rate is computed as taxation over EBITDA, so both its level and its post-reform rise are partly mechanical: interest deductions reduce the numerator alone. The avoidance interpretation therefore rests not on the level of the effective tax rate but on whether the response scales with the tax benefit of debt. I test this by augmenting the baseline specification with a triple interaction between treatment, the post-reform indicator, and a moderator Z_i capturing the value of the interest deduction to firm i , standardized to one standard deviation across treated firms and set to zero for control firms:

$$(17) \quad Y_{ist} = \alpha_i + \mu_t + \beta_1 (\text{Fail}_i \times \text{Post}_t) + \beta_2 (\text{Fail}_i \times \text{Post}_t \times Z_i) + X'_{st} \delta + \varepsilon_{ist},$$

estimated by two-way fixed-effects OLS with firm and year fixed effects, clustered at the firm level. Z_i is defined alternatively as the firm's own statutory rate or the intra-group tax differential—the gap between the firm's rate and the group's lowest pre-reform rate, which

governs the profit-shifting return to internal debt. Table G.VII shows that β_2 is systematically signed with the tax benefit of debt. Where the firm's own statutory rate is higher, and the interest deduction correspondingly more valuable, the post-reform debt reduction is larger (-0.333 per standard deviation) and the recovery of the taxable base stronger ($+0.160$). The same holds along the intra-group tax differential: the debt cut is larger (-0.316) and the tax-base recovery stronger ($+0.234$) where the differential is wider.

E Alternative DiD Specifications

In order to verify the robustness of my baseline estimated coefficients, I estimate the effects of ESR through alternative estimation methodologies and compare the results with the baseline methodology of de Chaisemartin & D'Haultfoeuille (2020). These include standard two-way fixed effects (TWFE) estimator, Sun & Abraham (2021), Borusyak *et al.* (2024), and Cengiz *et al.* (2019). Across all the specifications, I always control for unit fixed effects, year fixed effects, and jurisdiction-level time varying controls, i.e., $\log(\text{population})$, $\log(\text{GDP})$, and tax rates , to account for $\text{jurisdiction} \times \text{years}$ fixed effects.

The baseline estimates following de Chaisemartin & D'Haultfoeuille (2020) use all units whose treatment status does not change at a given date as controls, including units treated earlier or later. In contrast, the estimators of Sun & Abraham (2021) and Cengiz *et al.* (2019) allow the control group to be restricted to never-treated units. I therefore re-estimate the model using only never-treated units as controls. Comparing these coefficients with the baseline results tests whether the findings depend on the choice of control group. Additionally, the estimates by Borusyak *et al.* (2024) estimate the placebo and treatment coefficients in an asymmetric way (Roth, 2026). Therefore, I scale the placebo coefficients to 0 in period $t^* - 1$.

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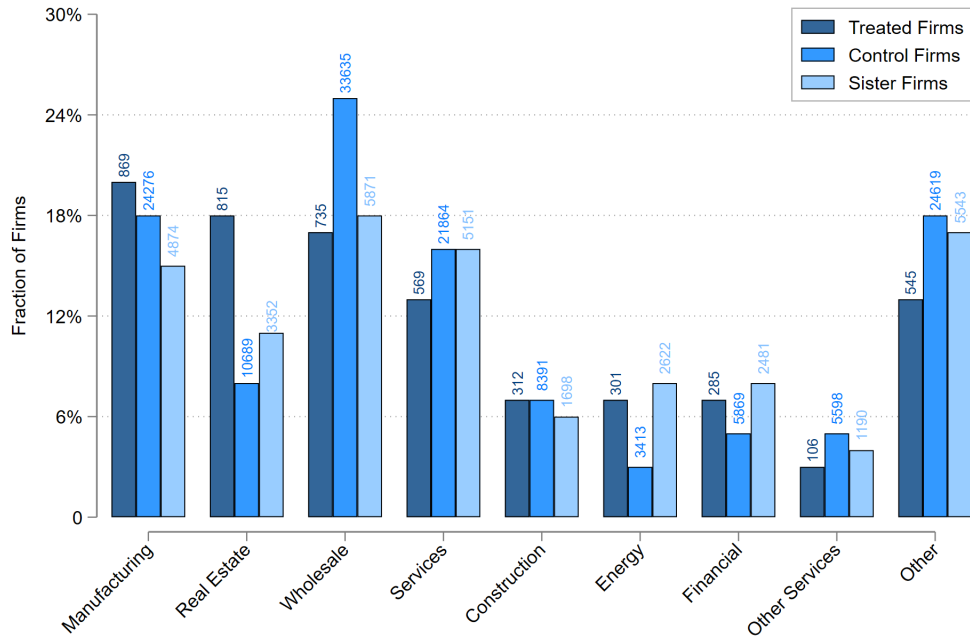
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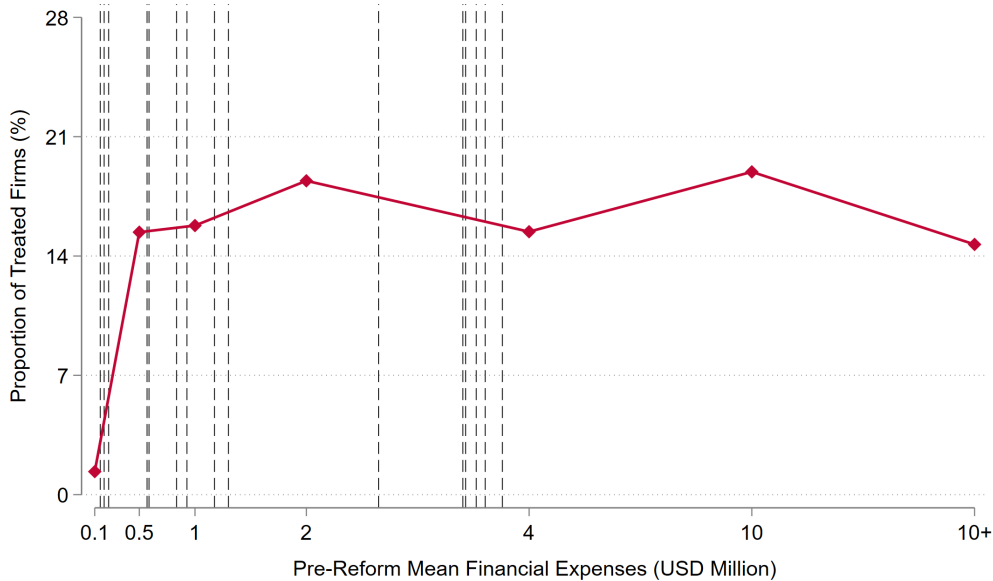
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FIGURE F.I: INDUSTRY CLASSIFICATION AND FINANCIAL EXPENSE DISTRIBUTION

(A) INDUSTRY CLASSIFICATION

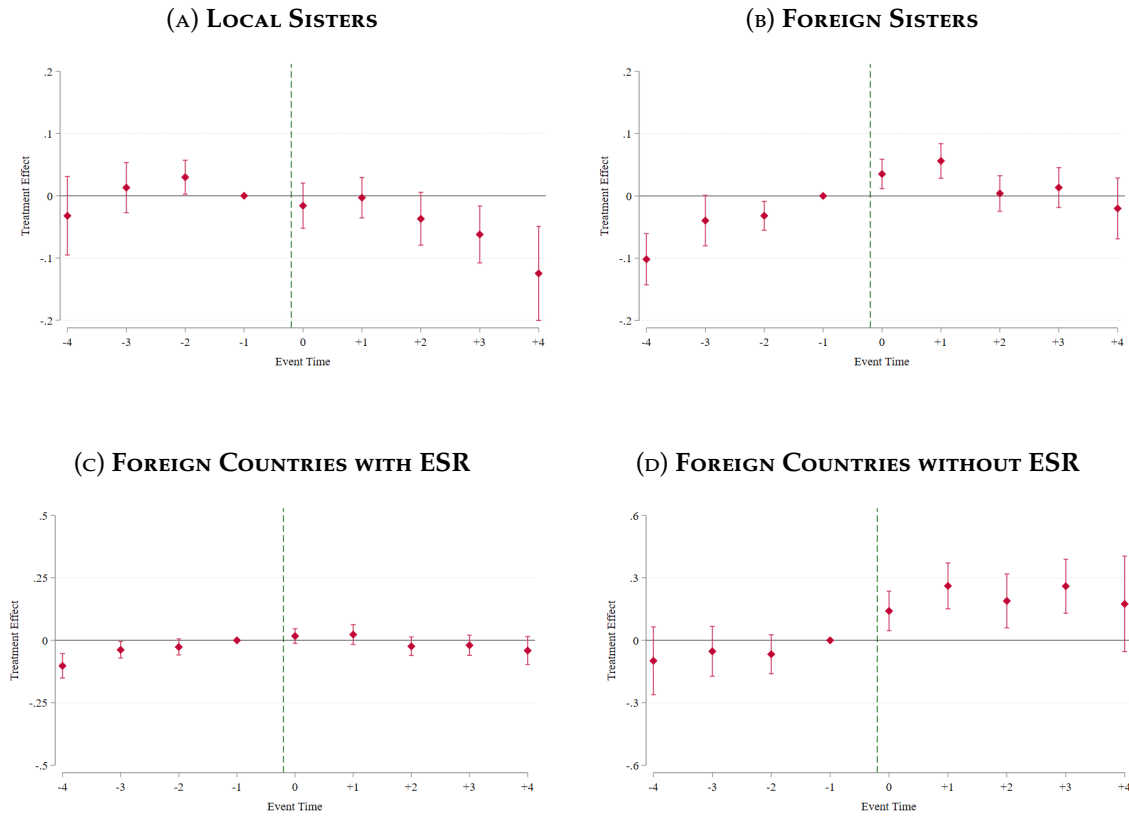


(B) DISTRIBUTION OF PRE-REFORM AVERAGE FINANCIAL EXPENSE



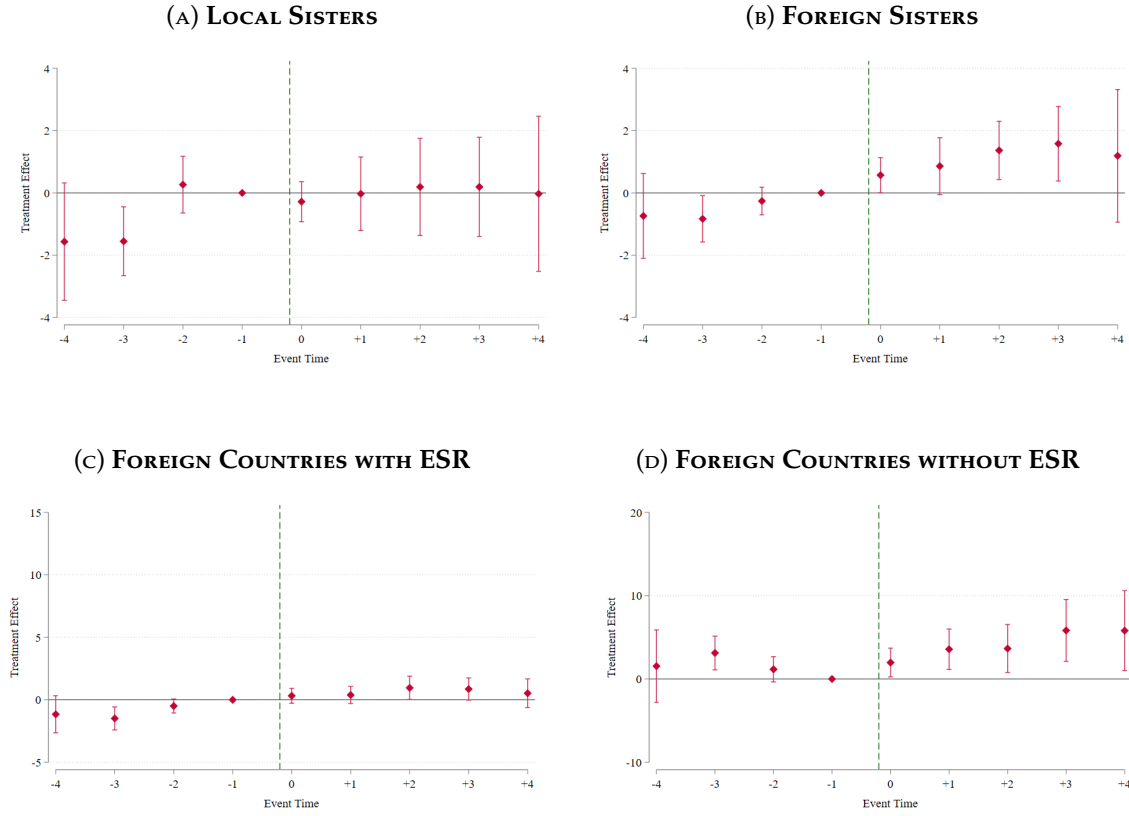
Notes: Panel A shows the industry distribution of the estimation sample at firm level. The vertical bars display the share of each firm type within each industry, and the numbers above the bars indicate the corresponding firm counts. “Wholesale” refers to wholesale and retail trade. “Energy” combines electricity, gas, steam, and air-conditioning supply with mining and quarrying. Panel B shows the distribution of three year pre-reform average financial expenses for treated firms. For countries where ESR applies to net (gross) financial expenses, the figure reports net (gross) values. All amounts are expressed in USD millions. The black dashed lines indicate country-specific de minimis thresholds. Slovak firms are excluded, as Slovakia’s ESR did not include a de minimis threshold.

FIGURE F.II: EFFECT ON REPORTED PROFITS



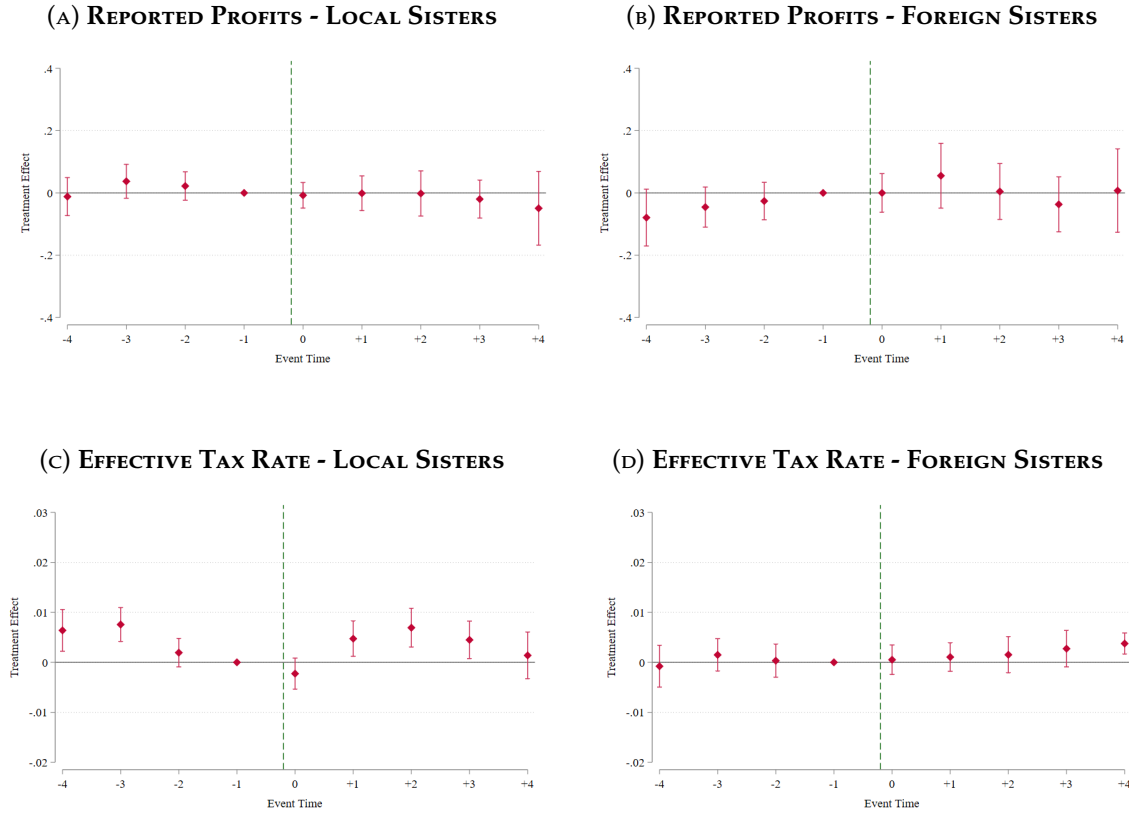
Notes: This figure reports dynamic firm-level effects of ESR (in USD million). The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report effects on local and foreign sister affiliates, respectively. Panels C and D restrict the sample to foreign sisters whose home country did and did not introduce ESR, respectively. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE F.III: EFFECT ON EMPLOYMENT



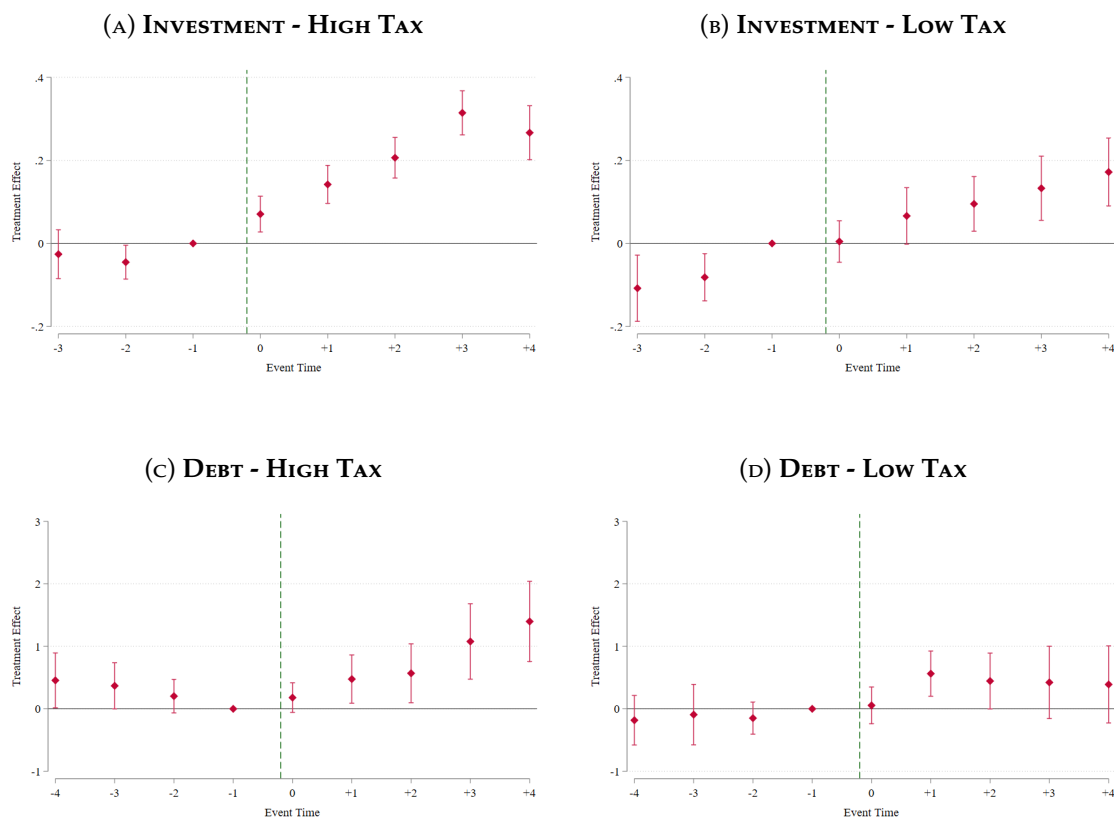
Notes: This figure reports dynamic firm-level effects of ESR. The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report effects on local and foreign sister affiliates, respectively. Panels C and D restrict the sample to foreign sisters whose home country did and did not introduce ESR, respectively. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE F.IV: EFFECT ON FULLY COVERED MNEs



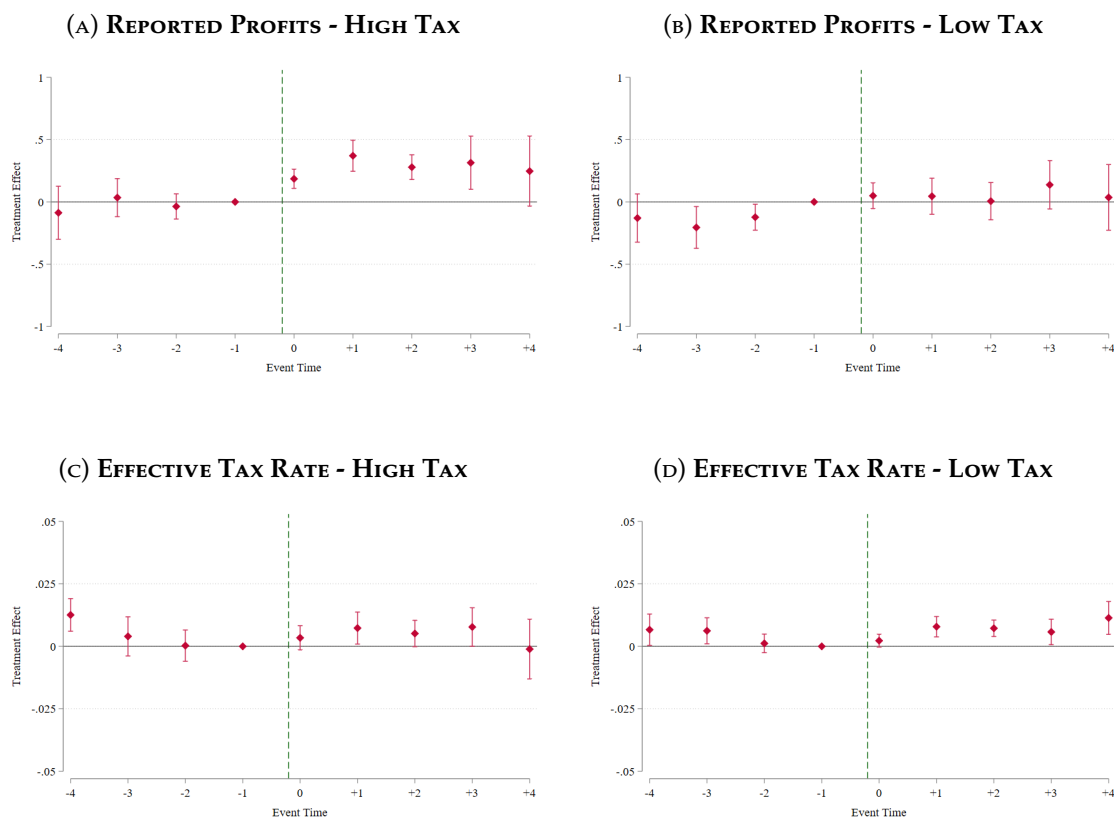
Notes: This figure reports dynamic firm-level effects of ESR. The sample is restricted to sister firms of MNE groups whose all affiliates operated in jurisdictions that had implemented ESR (“Fully Covered” MNEs). The outcomes of such local and foreign sisters are compared against control firms. Reported Profits are measured in USD million and Effective Tax Rates are reported in decimal form (e.g., 0.05 = 5%). The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE F.V: FOREIGN COUNTRIES WITHOUT ESR



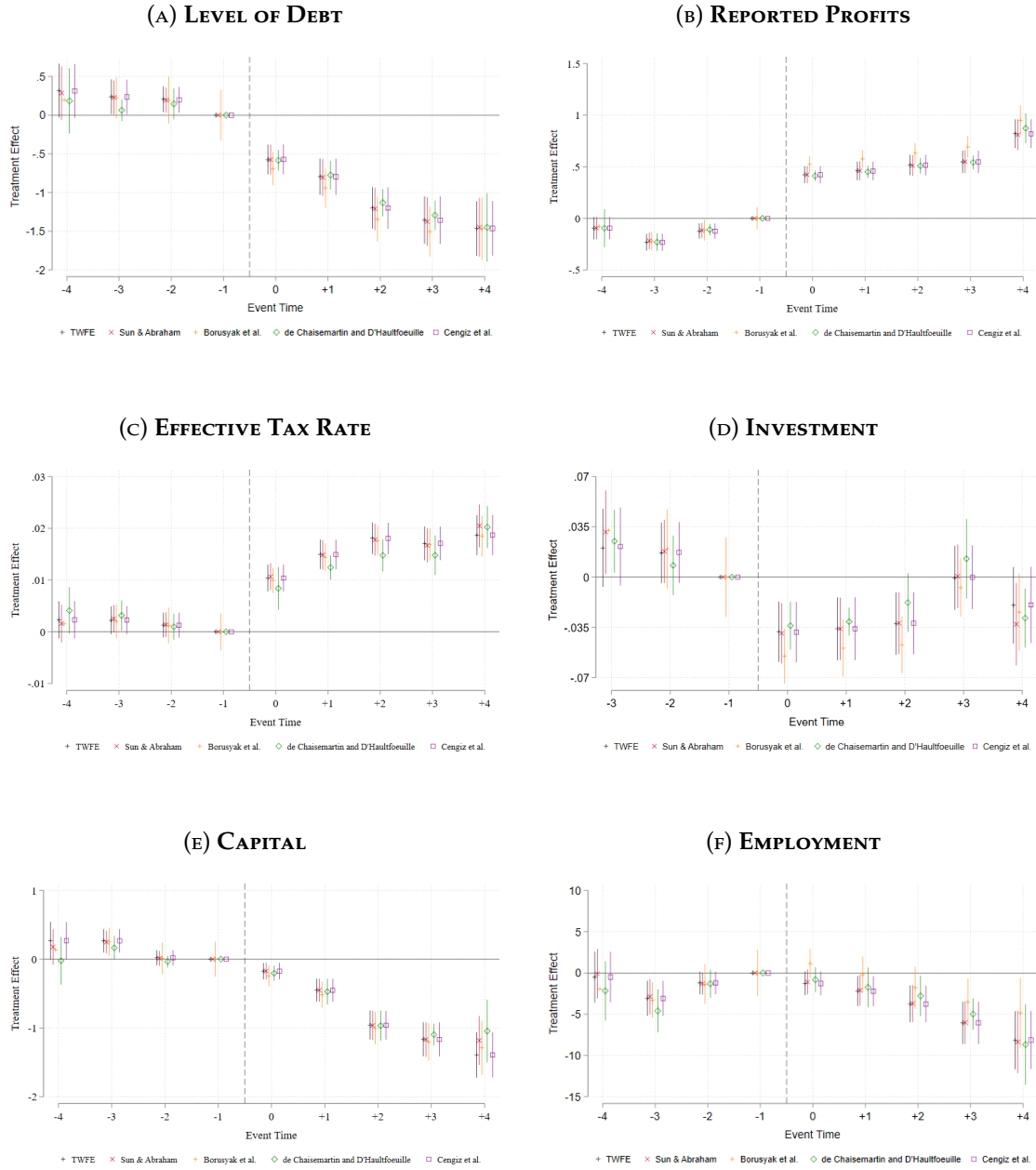
Notes: This figure presents dynamic firm-level effects of ESR. The sample includes control firms and sister affiliates of treated firms located in jurisdictions that never adopted the policy. Sister affiliates are further classified as high-tax or low-tax based on whether the average corporate tax rate in their jurisdiction is above or below the global minimum rate of 15%. Investment is defined as gross investment scaled by lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Debt is reported in USD million. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE F.VI: FOREIGN COUNTRIES WITHOUT ESR



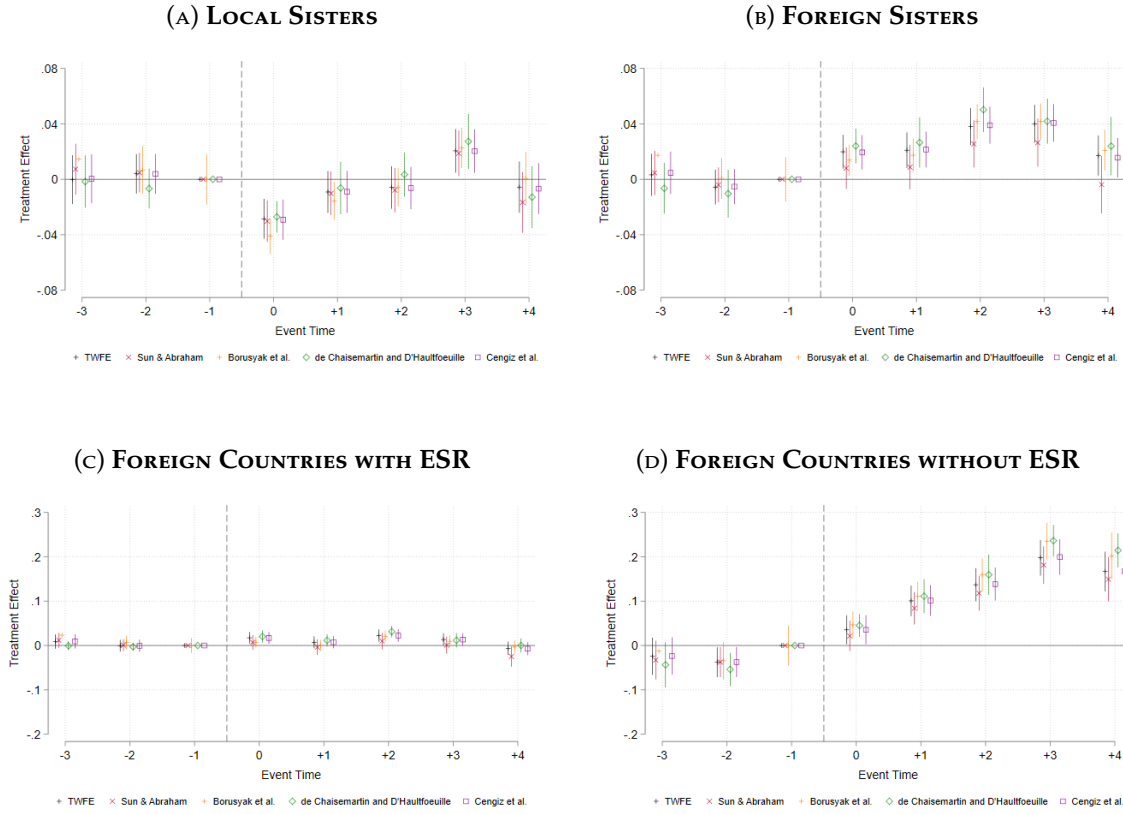
Notes: This figure presents dynamic firm-level effects of ESR. The sample includes control firms and sister affiliates of treated firms located in jurisdictions that never adopted the policy. Sister affiliates are further classified as high-tax or low-tax based on whether the average corporate tax rate in their jurisdiction is above or below the global minimum rate of 15%. Reported Profits are measured in USD million and Effective Tax Rates are reported in decimal form (e.g., 0.05 = 5%). The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped and clustered at the firm level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE F.VII: EFFECTS OF ESR - TREATED FIRMS



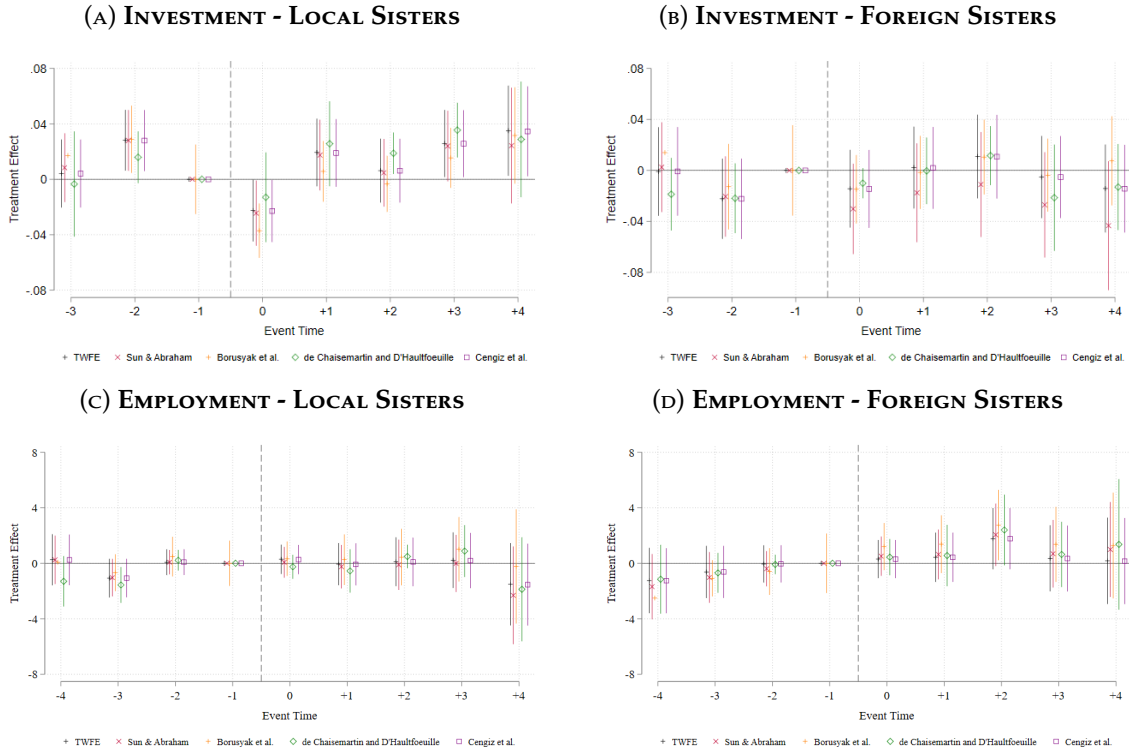
Notes: This figure reports dynamic firm-level treatment effects of ESR. Panels A, B, and E are in USD millions. Panels C and D report rates in decimal form (e.g., 0.05 = 5%); Panel D (investment scaled by lagged capital) begins at $t^* - 3$ due to the lag structure. Panel F reports total number of employees. The outcomes of treated firms are compared with that of control firms; sister firms are dropped from the analysis. The symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are clustered at the firm level. The event time at the x-axis is measured in years and the black dashed line depicts the year of policy introduction. Additional details on the estimation designs are provided in Appendix E.

FIGURE F.VIII: REALLOCATION OF INVESTMENTS



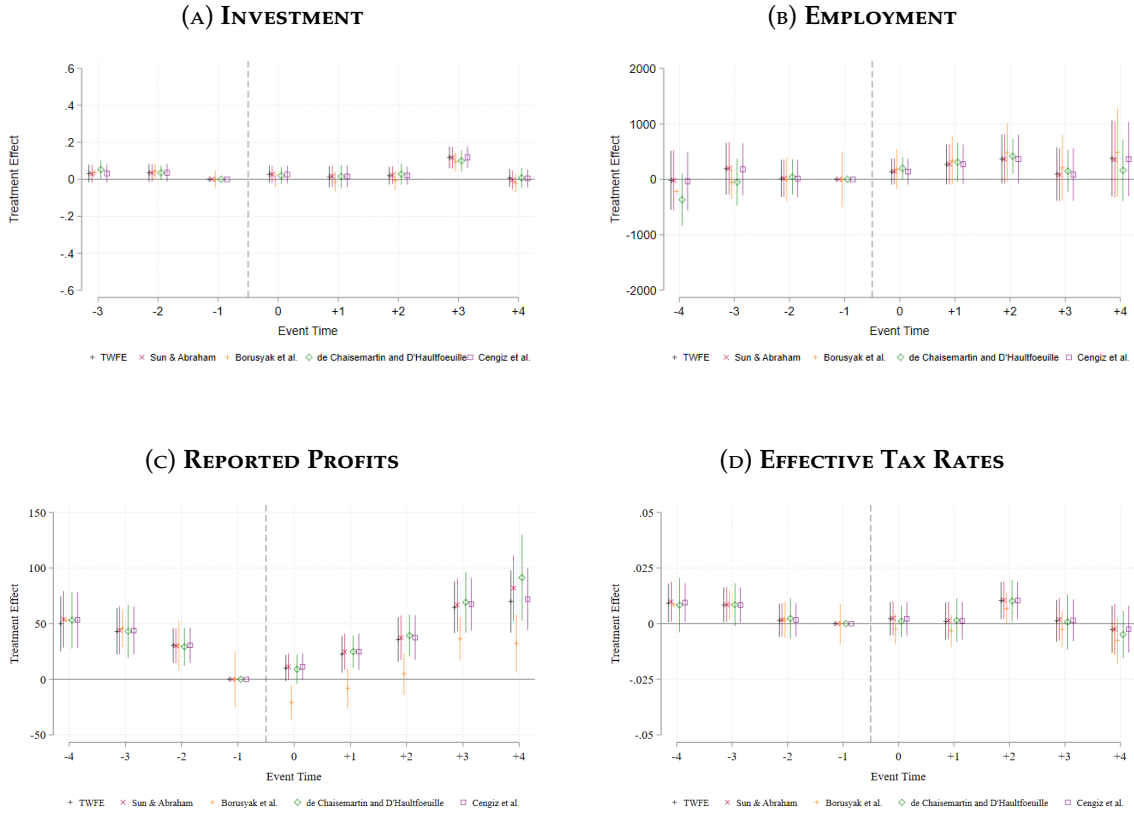
Notes: This figure reports dynamic firm-level effects of ESR. Investments are defined as gross investment scaled by lagged stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report effects on local and foreign sister affiliates, respectively. Panels C and D restrict the sample to foreign sisters whose home country did and did not introduce ESR, respectively. The symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are clustered at the firm level. The event time at the x-axis is measured in years and the black dashed line depicts the year of policy introduction. Additional details on the estimation designs are provided in Appendix E.

FIGURE F.IX: EFFECT ON FULLY COVERED MNEs



Notes: This figure reports dynamic firm-level effects of ESR. The sample is restricted to sister firms of MNE groups in which all affiliates operated in jurisdictions that had implemented ESR ("Fully Covered" MNEs). The outcomes of such local and foreign sisters are compared against control firms. Investments are defined as gross investment scaled by lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Capital is reported in USD millions. The symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are clustered at the firm level. The event time at the x-axis is measured in years and the black dashed line depicts the year of policy introduction. Additional details on the estimation designs are provided in Appendix E.

FIGURE F.X: EFFECT OF ESR - GROUP LEVEL



Notes: This figure reports dynamic group-level treatment effects of ESR. Panels A and D report rates in decimal form (e.g., 0.05 = 5%); Panel A (investment scaled by lagged capital) begins at $t^* - 3$ due to the lag structure. Panel B reports the total number of employees, and Panel C reports the estimates in USD million. The symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are clustered at the group level. The event time at the x-axis is measured in years and the black dashed line depicts the year of policy introduction. Additional details on the estimation designs are provided in Appendix E.

TABLE G.I: COUNTRY STATISTICS

Country	Firms			Groups		
	Treat	Sister	Control	Treat	Control	Control
<i>Afghanistan</i>	0.000	0.000	0.000	0.000	0.000	0.014
<i>Albania</i>	0.000	0.000	0.000	0.000	0.000	0.067
<i>Algeria</i>	0.000	0.128	0.103	0.033	0.973	0.973
<i>Andorra</i>	0.000	0.000	0.000	0.033	0.023	0.023
<i>Angola</i>	0.000	0.000	0.000	0.000	0.056	0.056
<i>Anguilla</i>	0.000	0.000	0.000	0.000	0.253	0.253
<i>Antigua and Barbuda</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Argentina</i>	0.000	0.000	0.000	0.000	0.040	0.040
<i>Armenia</i>	0.000	0.000	0.000	0.000	0.084	0.084
<i>Aruba</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Australia</i>	0.000	4.108	3.295	0.426	1.235	1.235
<i>Austria</i>	1.159	3.332	1.629	3.143	3.428	3.428
<i>Azerbaijan</i>	0.000	0.000	0.000	0.000	0.120	0.120
<i>Bahamas</i>	0.000	0.000	0.000	0.098	0.061	0.061
<i>Bahrain</i>	0.000	0.000	0.000	0.033	0.016	0.016
<i>Bangladesh</i>	0.000	0.000	0.000	0.033	0.031	0.031
<i>Barbados</i>	0.000	0.000	0.000	0.033	0.010	0.010
<i>Belarus</i>	0.000	0.000	0.000	0.000	0.241	0.241
<i>Belgium</i>	1.772	6.015	5.897	1.637	3.302	3.302
<i>Belize</i>	0.000	0.000	0.000	0.098	0.123	0.123
<i>Benin</i>	0.000	0.000	0.000	0.000	0.020	0.020
<i>Bermuda</i>	0.000	0.000	0.000	0.327	0.207	0.207
<i>Bolivia</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Bosnia and Herzegovina</i>	0.000	0.368	0.951	0.000	0.702	0.702
<i>Botswana</i>	0.000	0.000	0.000	0.000	0.004	0.004
<i>Brazil</i>	0.000	0.866	0.319	0.229	0.164	0.164
<i>British Virgin Islands</i>	0.000	0.000	0.000	0.589	0.919	0.919
<i>Brunei</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Bulgaria</i>	0.153	1.477	2.963	1.212	0.957	0.957
<i>Burkina Faso</i>	0.000	0.000	0.000	0.000	0.004	0.004
<i>Burundi</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Total Entities</i>	4572	32086	135891	3054	69984	69984
Country	Firms			Groups		
	Treat	Sister	Control	Treat	Control	Control
<i>Cabo Verde</i>	0.000	0.000	0.000	0.000	0.013	0.013
<i>Cambodia</i>	0.000	0.000	0.000	0.000	0.061	0.061
<i>Cameroon</i>	0.000	0.000	0.000	0.000	0.073	0.073
<i>Canada</i>	0.000	0.000	0.000	0.720	0.837	0.837
<i>Cayman Islands</i>	0.000	0.000	0.000	1.048	0.472	0.472
<i>Central African Republic</i>	0.000	0.000	0.000	0.000	0.011	0.011
<i>Chad</i>	0.000	0.000	0.000	0.000	0.004	0.004
<i>Chile</i>	0.000	0.000	0.000	0.033	0.040	0.040
<i>China</i>	0.000	0.000	0.000	0.819	2.175	2.175
<i>Colombia</i>	0.000	0.000	0.000	0.000	0.023	0.023
<i>Comoros</i>	0.000	0.000	0.000	0.000	0.006	0.006
<i>Costa Rica</i>	0.000	0.000	0.000	0.000	0.017	0.017
<i>Croatia</i>	0.262	0.988	1.897	0.327	1.240	1.240
<i>Cuba</i>	0.000	0.000	0.000	0.000	0.016	0.016
<i>Curacao</i>	0.000	0.000	0.000	0.131	0.071	0.071
<i>Cyprus</i>	0.000	0.000	0.000	3.143	2.964	2.964
<i>Czechia</i>	1.247	4.691	6.361	6.745	1.993	1.993
<i>Côte d'Ivoire</i>	0.000	0.000	0.000	0.000	0.033	0.033
<i>Dem Rep of the Congo</i>	0.000	0.000	0.000	0.000	0.011	0.011
<i>Denmark</i>	0.000	0.000	0.000	1.441	1.260	1.260
<i>Djibouti</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Dominica</i>	0.000	0.000	0.000	0.033	0.013	0.013
<i>Dominican Republic</i>	0.000	0.000	0.000	0.033	0.006	0.006
<i>Ecuador</i>	0.000	0.000	0.000	0.000	0.003	0.003
<i>Egypt</i>	0.000	0.000	0.000	0.033	0.131	0.131
<i>Eritrea</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Estonia</i>	0.000	0.000	0.000	0.000	0.687	0.687
<i>Fiji</i>	0.000	0.000	0.000	0.000	0.004	0.004
<i>Finland</i>	1.444	1.686	1.403	0.982	0.636	0.636
<i>France</i>	7.349	32.101	19.464	4.224	4.300	4.300
<i>Gabon</i>	0.000	0.000	0.000	0.000	0.016	0.016
<i>Total Entities</i>	4572	32086	135891	3054	69984	69984
Country	Firms			Groups		
	Treat	Sister	Control	Treat	Control	Control
<i>Georgia</i>	0.000	0.034	0.057	0.000	0.077	0.077
<i>Germany</i>	0.000	0.000	0.000	5.763	9.046	9.046
<i>Ghana</i>	0.000	0.000	0.000	0.000	0.019	0.019
<i>Gibraltar</i>	0.000	0.000	0.000	0.033	0.127	0.127
<i>Greece</i>	0.437	1.025	0.347	0.917	1.427	1.427
<i>Grenada</i>	0.000	0.000	0.000	0.000	0.003	0.003
<i>Guatemala</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Guinea</i>	0.000	0.000	0.000	0.000	0.009	0.009
<i>Guinea-Bissau</i>	0.000	0.000	0.000	0.000	0.007	0.007
<i>Guyana</i>	0.000	0.000	0.000	0.000	0.004	0.004
<i>Haiti</i>	0.000	0.000	0.000	0.000	0.014	0.014
<i>Honduras</i>	0.000	0.000	0.000	0.000	0.004	0.004
<i>Hong Kong</i>	0.000	0.000	0.000	0.327	0.283	0.283
<i>Hungary</i>	0.547	2.584	2.398	8.382	1.873	1.873
<i>Iceland</i>	0.109	0.069	0.076	0.131	0.077	0.077
<i>India</i>	4.484	0.231	0.131	1.539	0.447	0.447
<i>Indonesia</i>	0.000	0.000	0.000	0.033	0.033	0.033
<i>Iran</i>	0.000	0.000	0.000	0.000	0.160	0.160
<i>Iraq</i>	0.000	0.000	0.000	0.000	0.041	0.041
<i>Ireland</i>	0.000	2.895	3.069	0.622	0.659	0.659
<i>Israel</i>	0.000	0.000	0.000	0.688	0.832	0.832
<i>Italy</i>	0.000	0.000	0.000	3.569	7.680	7.680
<i>Jamaica</i>	0.000	0.000	0.000	0.000	0.007	0.007
<i>Japan</i>	0.919	0.159	0.168	3.078	1.419	1.419
<i>Jordan</i>	0.000	0.000	0.000	0.033	0.080	0.080
<i>Kazakhstan</i>	0.000	0.000	0.000	0.065	0.106	0.106
<i>Kenya</i>	0.000	0.000	0.000	0.000	0.001	0.001
<i>Kosovo</i>	0.000	0.000	0.000	0.000	0.014	0.014
<i>Kuwait</i>	0.000	0.000	0.000	0.033	0.054	0.054
<i>Kyrgyzstan</i>	0.000	0.000	0.000	0.000	0.006	0.006
<i>Laos</i>	0.000	0.000	0.000	0.000	0.024	0.024
<i>Total Entities</i>	4572	32086	135891	3054	69984	69984

Notes: This table reports country coverage for the estimation sample. Column 1 lists jurisdictions. Columns 2, 3 and 4 report the share of treated, sister and control firms located in each jurisdiction, respectively. Columns 5 and 6 show the share of treated and control MNE groups headquartered in each jurisdiction. The total number of firms and groups in each classification are reported at the end of the table.

TABLE G.II: CONTINUED

Country	Firms			Groups		
	Treat	Sister	Control	Treat	Control	Control
Latvia	0.000	0.580	2.712	0.131	0.164	
Lebanon	0.000	0.000	0.000	0.196	0.337	
Liberia	0.000	0.000	0.000	0.065	0.034	
Libya	0.000	0.000	0.000	0.000	0.016	
Liechtenstein	0.000	0.000	0.000	0.393	0.313	
Lithuania	0.000	0.000	0.000	0.098	0.757	
Luxembourg	0.394	0.240	0.235	3.897	1.549	
Macau	0.000	0.000	0.000	0.065	0.027	
Madagascar	0.000	0.000	0.000	0.000	0.050	
Malaysia	0.000	0.000	0.000	0.164	0.171	
Mali	0.000	0.000	0.000	0.000	0.017	
Malta	0.000	0.000	0.000	0.164	0.221	
Marshall Islands	0.000	0.000	0.000	0.098	0.060	
Mauritania	0.000	0.000	0.000	0.000	0.007	
Mauritius	0.000	0.000	0.000	0.491	0.076	
Mexico	0.000	0.000	0.000	0.229	0.050	
Moldova	0.000	0.000	0.000	0.098	0.363	
Monaco	0.000	0.000	0.000	0.033	0.097	
Mongolia	0.000	0.000	0.000	0.000	0.003	
Montenegro	0.000	0.103	0.389	0.000	0.327	
Morocco	0.000	0.991	0.748	0.000	0.812	
Mozambique	0.000	0.000	0.000	0.000	0.007	
Namibia	0.000	0.000	0.000	0.000	0.001	
Nepal	0.000	0.000	0.000	0.000	0.009	
Netherlands	0.131	0.228	0.351	3.733	3.579	
New Zealand	0.000	0.686	0.873	0.000	0.159	
Nicaragua	0.000	0.000	0.000	0.000	0.011	
Niger	0.000	0.000	0.000	0.000	0.003	
Nigeria	0.000	0.000	0.000	0.000	0.031	
North Korea	0.000	0.000	0.000	0.000	0.013	
North Macedonia	0.000	0.115	0.801	0.000	0.450	
Total Entities	4572	32086	135891	3054	69984	
South Africa	0.000	0.000	0.000	0.131	0.124	
South Korea	5.884	1.521	0.749	3.209	0.366	
Spain	2.056	0.792	1.124	1.899	2.763	
Sri Lanka	0.000	0.012	0.009	0.033	0.039	
Sudan	0.000	0.000	0.000	0.000	0.004	
Suriname	0.000	0.000	0.000	0.000	0.199	
Sweden	10.433	7.246	4.406	3.602	2.096	
Switzerland	0.000	0.000	0.000	2.390	2.585	
Syria	0.000	0.000	0.000	0.065	0.229	
Taiwan	0.000	0.000	0.000	0.065	0.151	
Tajikistan	0.000	0.000	0.000	0.000	0.009	
Tanzania	0.000	0.000	0.000	0.000	0.009	
Thailand	0.000	0.000	0.000	0.065	0.084	
Togo	0.000	0.000	0.000	0.000	0.016	
Tunisia	0.000	0.000	0.000	0.000	0.404	
Turkey	0.000	0.000	0.000	0.982	2.213	
Turkmenistan	0.000	0.000	0.000	0.000	0.011	
Uganda	0.000	0.000	0.000	0.000	0.006	
Ukraine	0.000	1.166	5.917	0.458	0.824	
United Arab Emirates	0.000	0.000	0.000	0.327	0.279	
United Kingdom	6.037	7.277	4.575	6.320	4.858	
United States	0.000	0.000	0.000	7.400	5.648	
Unknown	0.000	0.000	0.000	0.000	0.001	
Uruguay	0.000	0.000	0.000	0.000	0.020	
Uzbekistan	0.000	0.000	0.000	0.000	0.067	
Vanuatu	0.000	0.000	0.000	0.000	0.001	
Venezuela	0.000	0.000	0.000	0.033	0.019	
Vietnam	0.000	0.000	0.000	0.000	0.141	
Yemen	0.000	0.000	0.000	0.000	0.003	
Zambia	0.000	0.000	0.000	0.000	0.001	
Zimbabwe	0.000	0.000	0.000	0.000	0.004	
Total Entities	4572	32086	135891	3054	69984	

Country	Firms			Groups		
	Treat	Sister	Control	Treat	Control	Control
Norway	0.066	0.009	0.007	1.146	1.035	
Oman	0.000	0.000	0.000	0.033	0.007	
Pakistan	0.000	0.000	0.000	0.000	0.084	
Palestine	0.000	0.000	0.000	0.000	0.004	
Panama	0.000	0.000	0.000	0.164	0.266	
Papua New Guinea	0.000	0.000	0.000	0.000	0.003	
Paraguay	0.000	0.000	0.000	0.000	0.001	
Peru	0.000	0.000	0.000	0.000	0.026	
Philippines	0.000	0.935	0.863	0.000	0.046	
Poland	11.242	5.002	4.893	1.637	1.097	
Portugal	3.281	5.439	4.080	0.884	1.966	
Qatar	0.000	0.000	0.000	0.098	0.024	
Republic of the Congo	0.000	0.000	0.000	0.000	0.033	
Romania	20.626	2.699	8.870	3.143	0.602	
Russia	0.000	0.000	0.000	0.753	2.912	
Rwanda	0.000	0.000	0.000	0.000	0.009	
Saint Kitts and Nevis	0.000	0.000	0.000	0.033	0.014	
Saint Lucia	0.000	0.000	0.000	0.000	0.001	
Saint Vincent	0.000	0.000	0.000	0.033	0.031	
Samoa	0.000	0.000	0.000	0.000	0.011	
San Marino	0.000	0.000	0.000	0.000	0.009	
Sao Tome	0.000	0.000	0.000	0.000	0.009	
Saudi Arabia	0.000	0.000	0.000	0.098	0.066	
Senegal	0.000	0.000	0.000	0.000	0.036	
Serbia	0.000	1.275	6.020	0.098	0.866	
Seychelles	0.000	0.000	0.000	0.295	0.259	
Sierra Leone	0.000	0.000	0.000	0.000	0.006	
Singapore	0.000	0.000	0.000	0.622	0.260	
Slovakia	19.969	0.171	0.188	0.950	1.197	
Slovenia	0.000	0.757	1.662	0.327	1.505	
Solomon Islands	0.000	0.000	0.000	0.000	0.003	
Total Entities	4572	32086	135891	3054	69984	

Notes: This table reports country coverage for the estimation sample. Column 1 lists jurisdictions. Columns 2, 3 and 4 report the share of treated, sister and control firms located in each jurisdiction, respectively. Columns 5 and 6 show the share of treated and control MNE groups headquartered in each jurisdiction. The total number of firms and groups in each classification are reported at the end of the table.

TABLE G.III: EFFECT OF ESR - GROUP LEVEL

Outcomes (y_i):	Investment	Employment	Reported Profits	Effective Tax Rate
	(1)	(2)	(3)	(4)
$Failed_i \times Post_t$	.0356* (.0199)	254 (163)	42.9*** (8.34)	.00231 (.00373)
Pre-Reform Mean	.132	18992	273	.153
Percentage Change	26.87%	1.34%	15.71%	1.51%
Observations	61,454	98,303	127,053	74,865
Switchers	1,785	2,560	3,450	2,085

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of ESR at the group level. Columns (1) and (4) are expressed as ratios in decimal form (e.g., 0.05 = 5%), Column (2) is the number of employees, and Column (3) is reported in USD million. Pre-Reform Mean is the sample mean of the treated groups in pre-reform periods. All specifications include group and year fixed effects and time-varying jurisdiction-level controls. Standard errors are robust, bootstrapped and clustered at the group level. Observations is the number of observations used in the estimation, and Switchers is number of groups whose treatment status changed from 0 to 1.

TABLE G.IV: SISTER AFFILIATES IN FOREIGN COUNTRIES WITHOUT ESR

Outcomes (y_i):	Investment		Debt		Reported Profits		Effective Tax Rate	
	Low-Tax	High-Tax	Low-Tax	High-Tax	Low-Tax	High-Tax	Low-Tax	High-Tax
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Failed_i \times Post_t$	.0834*** (.0208)	.177*** (.014)	.366** (.176)	.599*** (.171)	.0542 (.0599)	.279*** (.0512)	.00643*** (.00163)	.00507*** (.00244)
Pre-Reform Mean	.192	.156	8.26	8.44	1.34	2.03	.0622	.148
Percentage Change	43.56%	113.77%	4.43%	7.10%	4.04%	13.73%	10.32%	3.43%
Observations	1,385,522	1,394,639	1,803,646	1,814,465	1,797,428	1,812,955	1,648,702	1,657,780
Switchers	2,760	4,952	3,587	6,136	3,533	6,135	3,363	5,939

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effects of ESR on Low-Tax and High-Tax sister affiliates domiciled in jurisdictions that never introduced ESR. Low-Tax (High-Tax) affiliates are the ones where the average corporate tax rate across the estimation sample remained below (above) the global minimum corporate income tax rate, i.e., 15%. Treated affiliates are excluded, and each category of sister affiliates is compared against the control group. Columns (1) and (4) are expressed as ratios in decimal form (e.g., $0.05 = 5\%$), and Columns (2) and (3) are reported in USD million. Pre-Reform Mean refers to the mean outcome for each affiliate category in the pre-reform period. All specifications include firm and year fixed effects and time-varying jurisdiction-level controls. Standard errors are robust, bootstrapped and clustered at the firm level.

TABLE G.V: REGRESSION DISCONTINUITY - ADDITIONAL RESULTS

Outcomes (y_i):	Debt	Reported Profits	Effective Tax Rate	Capital	Investment	Employment
	(1)	(2)	(3)	(4)	(5)	(6)
β	-0.054 (0.092)	0.315*** (0.034)	0.002 (0.003)	0.118 (0.089)	0.030 (0.023)	7.826*** (1.320)
γ^a	-1.050*** (0.182)	0.035 (0.069)	-0.006 (0.005)	-0.991*** (0.178)	-0.004 (0.045)	-5.758** (2.723)
Observations	33,867	40,625	35,524	40,321	36,920	30,000

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports RD estimates of firm-level outcomes around the policy threshold (bandwidth = USD 1.1 million). The running variable is the normalized pre-reform average interest expenses, and the dependent variable is the change in the outcome between the post- and pre-reform periods. Columns (1)-(2), and (4) report estimates in USD million. Columns (3) and (5) are expressed as ratios in decimal form (e.g., 0.05 = 5%), and Column (6) is the number of employees. β is the discontinuity estimate, γ the slope of the fitted curve above the cutoff, and "Observations" is the number of firms included in the estimation.

TABLE G.VI: THE MASS AT ZERO FALLS AFTER ESR

	Treated	Control
	(1)	(2)
Panel A: Excess Mass of Zero-Profit Firms		
Pre-reform	4.891*** (0.262)	1.781*** (0.044)
Post-reform	2.992*** (0.196)	1.831*** (0.038)
Δ (Post – Pre)	-1.899	0.051
Triple difference (Treated – Control)	-1.950*** (0.332)	
Panel B: Share Paying Exactly Zero Tax		
Pre-reform	0.246	0.189
Post-reform	0.208	0.197
Δ (Post – Pre)	-0.038	0.008
Difference-in-differences	-0.046*** (0.005)	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports two tests of whether the mass of firms at or near zero reported profits moves after ESR; sister firms are excluded from the analysis. Panel A reports the normalized excess mass of firms bunching just below zero reported profits, scaled by total assets, estimated separately by group and period with a bootstrapped bunching estimator; Δ (Post – Pre) is the within-group change, and the triple difference nets out the level difference in bunching between treated and control firms as well as any common time trend. Panel B reports the corresponding statistics for the share of firms paying exactly zero tax; the Pre- and Post-reform rows are raw group means, and the difference-in-differences is the coefficient on treatment interacted with the post-reform indicator. Standard errors are in parentheses and are bootstrapped for Panel A and clustered at the firm level for the Panel B difference-in-differences.

TABLE G.VII: THE ESR RESPONSE SCALES WITH THE TAX BENEFIT OF DEBT

Outcome:	$Fail_i \times Post_t \times Z_i$	
	Own statutory rate (1)	Intra-group tax differential (2)
Debt	-0.333** (0.129)	-0.316** (0.134)
Taxable profit	0.160*** (0.036)	0.234*** (0.038)

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficient β_2 on the triple interaction $Fail_i \times Post_t \times Z_i$, where Z_i is a moderator standardized per one standard deviation across treated firms and set to zero for control firms. Column (1) uses the firm's own statutory rate as Z_i ; Column (2) uses the gap between the firm's rate and the group's lowest pre-reform rate. Debt and Taxable Profit are reported in USD million. All specifications include firm and year fixed effects and time-varying jurisdiction-level controls. Standard errors are clustered at the firm level.

TABLE G.VIII: LENDER-SIDE EVIDENCE ON INTRA-GROUP DEBT

Panel A: Net-Lender Sisters				
Outcomes (y_i):	Interest Income		Net Financial Position	
	Foreign Sisters	Local Sisters	Foreign Sisters	Local Sisters
	(1)	(2)	(3)	(4)
$NetLender_i \times Post_t$	-0.048*** (0.005)	-0.027*** (0.007)	0.063*** (0.003)	0.059*** (0.004)
Pre-Reform Mean	0.494	0.446	-0.145	-0.135
Percentage Change	-9.76%	-5.97%	-43.89%	-43.36%
Observations	479,915	470,445	526,238	516,260
Switchers	23,032	14,227	24,872	15,137
Panel B: Treated Borrowers				
Outcomes (y_i):	Debt		Interest Expense	
	With Internal Lender	Without Internal Lender	With Internal Lender	Without Internal Lender
	(1)	(2)	(3)	(4)
$Failed_i \times Post_t$	-1.098*** (0.182)	-0.795*** (0.158)	-0.054*** (0.006)	-0.039*** (0.007)
Pre-Reform Mean	12.781	7.065	0.709	0.354
Percentage Change	-8.59%	-11.25%	-7.65%	-10.91%
Observations	1,985,895	1,957,987	1,012,496	996,627
Switchers	9,241	6,509	8,179	5,067

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the counterparty leg of intra-group lending. Panel A reports the effects of ESR on the interest income and net financial position of net-lender sister affiliates—affiliates with a negative pre-reform net financial expense, i.e., net internal creditors—split by whether they are located abroad (Columns 1 and 3) or in the treated firm's own jurisdiction (Columns 2 and 4). Panel B reports the effects of ESR on the debt and interest expense of treated borrowers, split by whether the group maintains an internal lending conduit, i.e., whether it contains at least one net-lender sister affiliate. All outcomes are reported in USD million. Pre-Reform Mean is the mean of the outcome for the relevant subsample in the last pre-reform period, and Percentage Change expresses the coefficient relative to that mean. All specifications include firm and year fixed effects and time-varying jurisdiction-level controls. Standard errors are robust, bootstrapped and clustered at the firm level. Switchers is the number of firms whose treatment status changed from 0 to 1.

TABLE G.IX: DOMESTIC INVESTMENT RESPONSE UNDER FULL COORDINATION

Outcomes (y_i): Sample Split (Z_i):	Investment			
	Interest Shield		Cash Flow	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
$Sister_i \times Post_t$	0.034*** (0.011)	-0.001 (0.015)	-0.001 (0.012)	0.035*** (0.008)
Pre-Reform Mean	0.146	0.248	0.240	0.148
Percentage Change	23.27%	-0.38%	-0.39%	23.94%
Observations	1,437,728	1,279,709	1,445,905	1,279,810
Switchers	6,565	6,373	6,584	6,321

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports average post-reform investment responses of the local sister affiliates of fully covered multinational groups; $Sister_i$ equals one for these affiliates, which are compared against the control group. The sample is split at the median of the moderator Z_i among the local sisters of fully covered groups, and the coefficient is estimated separately within each half of the split. Z_i is the pre-reform interest shield in Columns (1) and (2), defined as net financial expense over EBITDA, and pre-reform cash flow in Columns (3) and (4), defined as post-tax profit plus depreciation over total assets; both are averaged over the three years preceding the reform. All specifications control for firm and year fixed effects and time-varying jurisdiction-level controls. Pre-Reform Mean is the mean of the outcome for the relevant subsample in the last pre-reform period, and Percentage Change expresses the coefficient relative to that mean. Standard errors are robust, bootstrapped and clustered at the firm level. Switchers is the number of firms whose treatment status changed from 0 to 1.