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

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Abstract

Anti-tax avoidance policies aim to curb profit shifting by MNEs, yet their effects on capital costs and economic growth remain a critical question. This study examines the causal impacts of Earnings Stripping Rule (ESR)—a core anti–tax-avoidance measure adopted by more than ninety countries in the last decade—that limits profit shifting through debt channels but increases the cost of debt-financed capital. Using a large panel dataset on global MNE operations and a staggered difference-in-difference design, I compare the real activities of MNEs affected by ESR with those of unaffected groups. I find that ESR effectively reduces profit shifting and tax avoidance but also lowers investments in affected subsidiaries. MNEs offset these declines by reallocating capital and employ-ment toward unconstrained affiliates, primarily abroad. However, as the policy coverage expands across a group's global footprint, this reallocation shifts from foreign to domes-tic units, closing the international escape margin and raising group-level tax liabilities. These findings suggest that international coordination is crucial for designing effective, non-distortionary anti-avoidance policies.

Keywords: Tax Avoidance, Multinational Investment, Profit Shifting
JEL Classi ication: F23, H25, H26, H32

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I Introduction

Tax avoidance by multinational enterprises (MNEs) undermines economic fairness and reduces global corporate income tax revenue by USD 100–240 billion per year — equivalent to 4–10% of worldwide corporate tax receipts (OECD, 2024).[1] To curb such practices, governments have implemented a wide range of anti-avoidance rules. [2] 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-tax avoidance policies effectively limit tax avoidance without stifling investment and economic growth?

This paper sheds light on this question by analyzing the causal impacts of Earnings Stripping Rule (ESR), a key anti-avoidance measure recommended by the OECD in 2015 and adopted by more than 90 jurisdictions within the last decade (OECD, 2025). ESR targets one of the prominent profit-shifting channels: internal debt shifting. Under standard tax systems, interest income is taxable whereas interest expenses are deductible, creating opportunities for MNEs to load debt into high-tax affiliates and shift profits to low-tax jurisdictions through interest payments (Graham, 1996; Mooij and Hebous, 2017).[3]

The ESR policy caps a firm's interest 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). The policy thus 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. The tension between revenue objectives and real economic activity makes ESR an ideal setting to study whether anti-avoidance rules can be both effective and non-distortionary.

I address this question using affiliate-level and consolidated-group data from ORBIS between 2010 and 2021, covering more than 170,000 MNE affiliates across forty countries.[4] ORBIS offers two critical advantages for this analysis: detailed ownership structures that allow identification of group networks, and both consolidated and unconsolidated financial statements that permit tracing capital, employment, and taxation within and across multinational groups. These features enable separation of real effects from accounting adjustments.

The empirical strategy exploits the staggered adoption of ESR across countries and relies

[1] Please see Piketty et al. [2018], Bilicka [2019], Clausing et al. [2021], Akcigit and Ates [2023], Torslov et al. [2023] and Bachas et al. [2023] for a detailed discussion on the effects of these practices.

[2] Concurrently, more than 140 jurisdictions globally are introducing about 15 different action plans to limit tax avoidance by MNEs (OECD, 2024).

[3] 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).

[4] Please see Dharmapala [2014] for an overview of the studies that use this database.

on difference-in-differences event-study designs. I conduct the analysis at both the affiliate and group levels to capture direct and broader intra-group effects of ESR. At the affiliate level, treated firms are those whose three-year pre-reform average interest expenses exceeded the ESR limit. Since the reform may prompt reallocation of debt and capital within groups from treated firms to non-treated firms (Wilson, 2009; Giroud and Rauh, 2019), I identify "sister firms" — those in the same group as treated firms but not exceeding the ESR threshold — as a separate category for comparison. Firms unrelated to treated firms, hence belonging to different MNE groups, serve as "control firms". I then conduct two analyses: one comparing treated firms to control firms to measure the direct effects of ESR, and another comparing sister firms to control firms to assess potential reallocation effects within MNE groups. At the group level, I classify MNEs with at least one treated affiliate as treated groups.

Before turning to the causal analysis, I document two stylized facts that reveal how ESR binds. First, treated firms exhibit a distinct pre-reform distribution of profits to assets, with a large mass at zero and negative profits — consistent with aggressive tax avoidance behavior. After reform, this distribution shifts markedly to the right, whereas the distributions for sister and control firms remain stable. In other words, post-reform the proportion of treated firms reporting negative or zero profits decreases and those reporting positive profits increases. This non-parametric shift indicates reform-induced compression of tax avoidance. Second, plotting the distribution of interest-to-EBITDA ratios around the ESR cap reveals sharp post-reform bunching just below the threshold, consistent with active compliance.

The event-study results deepen these patterns. When comparing treated firms to control firms, the empirical analysis reveals that ESR reduces tax avoidance, albeit at the expense of lower investments. With interest expenses being taxed, debt becomes expensive and debt-financed capital cost increases (Hall and Jorgenson, 1967). Consistent with this behavioral anticipation, the event-study shows a sharp and persistent fall in debt and interest expenses at treated firms. This decline translates into higher reported profits and tax liabilities, indicating a near one-for-one pass-through from reduced avoidance to higher taxation. However, the higher user cost of capital leads to lower investment—approximately 11%—and comparable reductions in employment and capital.

These reductions, however, do not translate into aggregate declines in group activity. MNEs reallocate resources toward unconstrained affiliates. Investment at sister firms increases by roughly 9%, with a similar expansion in employment. This reallocation is strongly asymmetric: foreign sister firms account for nearly all the rise, particularly in high-tax non-adopting countries where interest expenses remain 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 and Mueller, 2015). Thus, when adoption is partial, ESR reduces domestic avoidance but induces capital flight to non-adopting jurisdictions.

A different patterm emerges when all affiliates within an MNE fall under ESR—either because the group operates only in rule enforcing countries or because coordination expands to cover the group's entire footprint. For these MNEs, the escape margin collapses, i.e., with the cost of debt rising uniformly, reallocating capital abroad yields no tax advantage. For these MNEs, investment at domestic sister firms rises by about 11%, foreign investment remains unchanged, and employment shows no reallocation. In other words, firms re-optimize entirely within domestic boundaries, channeling resources toward unconstrained domestic units. In this coordinated setting, ESR raises taxable income without displacing real activity abroad, and instead improves the efficiency of internal capital allocation.

Consolidated group-level evidence reinforces these findings. At the group level, total investment and employment remain stable after the reform, indicating that the policy reallocated capital and jobs within groups rather than reducing overall activity. Group-level tax payments, however, rise once interest deductions are capped — confirming that the reform limited tax avoidance even in aggregate accounts. Although substitution into other avoidance channels cannot be ruled out (Dharmapala, 2014), any such effects would bias the estimates downward. Taken together, the group-level estimates show that coordinated ESR broadens the tax base without reducing total economic activity, once capital reallocation is accounted for.

To further illustrate the mechanisms, I replace the binary treatment variable with a continuous measure of exposure and study ESR as a process of expanding coordination. As more of a group's affiliates become subject to ESR, the scope for exploiting cross-country tax differences shrinks, progressively narrowing the reallocation margin. The treatment-intensity design confirms this dynamic: at low exposure, MNEs reallocate to foreign affiliates; as exposure increases, this pattern weakens, reverses, and ultimately produces a strictly domestic reallocation. The magnitude is substantial. When two more countries adopt ESR within a ten-country MNE, domestic investment rises by about 8% and foreign investment falls by 6%. Moving from zero to full exposure increases domestic sister-firm investment by 14% and reduces foreign investment by 13%. Each additional adopter therefore compounds containment of capital flight and progressively eliminates leakage.

I complement these results with a regression discontinuity (RD) design around the de minimis threshold. The RD estimates the difference between pre- and post-reform outcomes as a function of pre-reform average interest expense. Unlike the parallel-trends assumption in difference-in-differences, the RD relies on the testable assumption that firms cannot manipulate their pre-reform average interest to lie just below the threshold (Lee and Lemieux, 2010). Graphical analysis shows no evidence of manipulation, and the RD reveals sharp discontinuities in debt, interest expenses, investment, and taxation. Firms farther above the threshold show stronger adjustments, consistent with the policy biting more for high-interest firms. Extensive robustness checks—including alternative staggered DiD estimators, bal-

anced samples, leave-one-out designs with different 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 flight. As coordination expands, the escape margin collapses, reallocation shifts domestically, and tax revenue rises without harming real activity. In the limit, multilateral adoption transforms ESR from a policy that risks erosion of domestic investment into one that protects the tax base and improves internal capital allocation.

This paper contributes to several strands of literature. First, it adds to the growing literature evaluating the real effects of anti-avoidance rules by documenting 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). The results 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 and Dharmapala, 2011; Giroud and 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. While previous studies have examined the effects of limiting interest deductions in specific contexts — such as Norway (Andresen and Thorvaldsen, 2022), the UK (Bilicka et al., 2022), Finland (Harju et al., 2023), Uganda (Bashir et al., 2024) and the US (Goodman et al., 2025; Sanati and Beyhaghi, 2025) — they tend to assess each jurisdiction in isolation. In contrast, this research provides a cross-country analysis, allowing for tracking of capital and investment spending across sister firms within MNE groups and credibly ascertaining the group-level effects of the reform. 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 and Laeven, 2008; Mintz and 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 deductions reduce taxable profits, while the corresponding income is booked in low-tax affiliates, where it faces a lighter tax burden (Hall and Jorgenson, 1967; Graham, 1996). This practice, known as tax-motivated debt shifting, erodes the tax base of high-tax countries without necessarily altering the group's real investment decisions (Huizinga et al., 2008, De Mooij, 2012).[5]

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 (typically EUR 3 million) and a fixed ratio (typically 30% of earnings before interest, tax, depreciation, and amortization (EBITDA)). Interest expenses that exceed both of these thresholds are inadmissible for tax deduction purposes.[6] These excess interest 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 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 and Miller, 1963; Auerbach and Hassett, 1992).

Following the OECD recommendation, the European Union's Anti-Tax Avoidance Di-

[5]Early attempts to contain such behavior 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).

[6]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 or certain bank charges). For ease of exposition, I refer to these collectively as interest expenses.

rective (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 Appendix Section A.

This coordinated global rollout is central to the empirical design of this paper. The country-level ESR announcement serves as a plausibly exogenous shock, as timing was largely determined by jurisdictions themselves rather than firm-level conditions. While implementation details slightly differ across jurisdictions—such as the de minimis threshold, ratio threshold, and escape clauses allowing group-level exemptions—the core mechanism remains consistent: ESR caps the share of earnings that can be shielded through interest 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 simple two-period model of multinational firms that clarifies the mechanisms through which ESR alters internal financing, cost of capital, and the cross- and within-country reallocation of investment. The framework builds directly on the intuition in Mintz and Smart [2004], Huizinga et al. [2008] and Moen et al. [2019] but extends these contributions along two dimensions that matter for my empirical setting. First, time is explicit: firms make choices in a baseline period with full interest deductibility and in a policy period where a subset of countries introduce ESR that reduces the fraction of deductible interest. Second, space is explicit: when an affiliate reduces capital or leverage, the firm can reallocate marginal capital either to foreign affiliates (cross-border reallocation) or to other affiliates in the same country (domestic reallocation). The model is intentionally parsimonious: it isolates the tax-shield channel of internal debt while keeping equity costs, internal interest rates, and reallocation frictions exogenous and transparent so that comparative statics are straightforward and testable.

Consider an MNE with N affiliates indexed by $j \in \{1, \ldots, N\}$. Affiliate j is located in country $c(j) \in \{1, \ldots, M\}$ which levies a statutory corporate tax rate $\tau_{c(j)} \in [0, 1)$. Without loss of generality order countries so that $\tau_1 \leq \tau_2 \leq \cdots \leq \tau_M$. Time is discrete with two dates $t \in \{0, 1\}$. At $t = 0$ the tax environment features full interest deductibility everywhere; at $t = 1$ a subset of countries, $C^{\text{ESR}} \subseteq \{1, \ldots, M\}$, adopt ESR that reduces the fraction of interest

expenses that are tax deductible.

Affiliate j chooses a capital stock $K_j \geq 0$ and an internal debt ratio $\lambda_j \in [-1, 1]$. $\lambda_j K_j$ is interpreted as affiliate j's net internal borrowing: $\lambda_j > 0$ denotes a net borrower and $\lambda_j < 0$ a net lender. The remaining share $(1 - \lambda_j)K_j$ is financed with equity. The cost of equity is $r > 0$ and the internal interest rate on intra-group loans is $i > 0$; both are taken as exogenous. Let $\rho^t_{c(j)} \in [0, 1]$ denote the fraction of interest expense that is tax deductible for affiliates in country $c(j)$ at date t. By construction $\rho^0_{c(j)} = 1$ for all j, while $\rho^1_{c(j)} = \rho_{c(j)} \in [0, 1)$ for countries in C^{ESR} and remains 1 elsewhere. Internal lending must clear across affiliates:

$$\sum_{j=1}^{N} \lambda_j K_j = 0. \tag{1}$$

Each affiliate produces output according to a strictly concave production function $f_j(K_j)$ with $f_j(0) = 0$, $f_j' > 0$, $f_j'' < 0$. Affiliate j's after-tax profit in period t can be written compactly as

$$\pi^t_j = (1 - \tau_{c(j)}) f_j(K_j) - r(1 - \lambda_j)K_j - i\lambda_j K_j (1 - \rho^t_{c(j)} \tau_{c(j)}). \tag{2}$$

The first term is operating profit net of the statutory tax, the second term is the equity financing cost on the equity component $(1 - \lambda_j)K_j$, and the third term is the net cost of internal interest payments after accounting for the deductible fraction $\rho^t_{c(j)}$. Under full deductibility ($\rho = 1$) a borrower effectively benefits from the statutory tax rate; when ρ falls the tax shield is attenuated and the after-tax cost of internal debt rises. The parent firm maximizes consolidated after-tax profits $\Pi^t = \sum_{j=1}^{N} \pi^t_j$ subject to the internal capital clearing condition (1).

At $t = 0$ all $\rho^0_{c(j)} = 1$. The Lagrangian for the consolidated problem is

$$\mathcal{L}_0 = \sum_{j=1}^{N} [(1 - \tau_{c(j)}) f_j(K_j) - r(1 - \lambda_j)K_j - i\lambda_j K_j (1 - \tau_{c(j)})] + \mu \sum_{j=1}^{N} \lambda_j K_j,$$

where μ is the multiplier on the clearing constraint. Differentiating with respect to λ_j (interior case) yields

$$\frac{1}{K_j} \frac{\partial \mathcal{L}_0}{\partial \lambda_j} = r - i(1 - \tau_{c(j)}) + \mu = 0. \tag{3}$$

Because μ is common across affiliates, equality (3) cannot hold for affiliates with different $\tau_{c(j)}$ unless the solution is a corner on some λ_j. This observation recapitulates the well-known result (Mintz and Smart, 2004): under linearity of leverage decisions at fixed K_j, internal lending concentrates in affiliates with the lowest tax rate(s). In other words, since shifting one dollar of internal lending from lender m to borrower b produces a tax saving proportional

to $i(\tau_{c(b)} - \tau_{c(m)})$, the firm prefers lending from the lowest-tax affiliate (the "tax haven").

Differentiating with respect to capital produces the familiar marginal condition:

$$(1 - \tau_{c(j)}) f_j'(K_j) = r(1 - \lambda_j) + i\lambda_j(1 - \tau_{c(j)}). \tag{4}$$

The left-hand side is the marginal after-tax product of capital; the right-hand side aggregates the marginal financing cost from equity and the after-tax cost of debt. Note that leverage λ_j enters the user cost non-linearly and changes the wedge between pre- and post-tax marginal returns.

When some countries adopt ESR at $t = 1$, the deductible fraction becomes $\rho^1_{c(j)} = \rho_{c(j)} < 1$ for affected countries. The new Lagrangian is

$$\mathcal{L}_1 = \sum_{j=1}^{N} [(1 - \tau_{c(j)}) f_j(K_j) - r(1 - \lambda_j)K_j - i\lambda_j K_j(1 - \rho_{c(j)}\tau_{c(j)})] + \mu_1 \sum_{j=1}^{N} \lambda_j K_j.$$

Manipulating the respective FOCs of this problem, I can formulate testable predictions about the effects of ESR as follows.

III.1 Effect on Debt Holdings

The first-order condition with respect to leverage becomes

$$\frac{1}{K_j} \frac{\partial \mathcal{L}_1}{\partial \lambda_j} = r - i(1 - \rho_{c(j)}\tau_{c(j)}) + \mu_1 = 0. \tag{5}$$

Comparing (5) with (3) shows directly that a reduction in $\rho_{c(j)}$ raises the effective marginal cost of borrowing for affiliates in treated countries. For affiliates that are ex ante borrowers (i.e., those for which $\lambda_j > 0$ in the baseline) this incentive effect reduces optimal leverage. Intuitively, ESR weakens the tax advantage of internal debt and therefore softens the incentive to shift debt into high-tax locations.

Prediction 1: *The introduction of ESR in a jurisdiction will result in a decrease in the debt levels of MNE affiliates operating within that jurisdiction.*

III.2 Effect on Investments

The capital Euler equation under ESR reads

$$(1 - \tau_{c(j)}) f_j'(K_j) = r(1 - \lambda_j) + i\lambda_j(1 - \rho_{c(j)}\tau_{c(j)}). \tag{6}$$

Because $\rho_{c(j)}$ appears with a negative sign on the right-hand side, a fall in $\rho_{c(j)}$ raises the marginal financing cost and therefore lowers the optimal K_j (holding other margins fixed).

The economic mechanism is transparent: when the tax shield is reduced, the user cost of capital increases and projects that were previously profitable under the pre-ESR user cost may no longer clear.

Prediction 2: *Following the enforcement of ESR, investments within the affected affiliates will decrease.*

III.3 Effect on Reallocation Margins:

In practice ESR operates via deductible caps or thresholds (for instance, an amount tied to EBITDA). Let T_c denote the country-specific deductible threshold (expressed in the units of annual interest expense or an appropriate transform). Define the binding indicator

$$B_j \equiv \mathbf{1}\{i\lambda_j K_j > T_{c(j)}\}. \tag{7}$$

Only affiliates that are both located in ESR countries and satisfy $B_j = 1$ experience a reduction in deductible interest. This distinction matters empirically because many affiliates lie below de-minimis thresholds and are therefore unaffected by ESR despite being in adopting countries.

A reduction in K_j or λ_j at a treated affiliate opens a reallocation margin: the MNE may redeploy marginal capital either to a foreign affiliate k or to another local affiliate ℓ. To compare these margins consider moving an infinitesimal unit dK from treated affiliate j to candidate target k. The marginal net benefit of the move can be written (to first order) as

$$dNB_{j\to k} = \Big[(1-\tau_{c(k)})f_k'(K_k) - (1-\tau_{c(j)})f_j'(K_j)\Big]dK + i\Big[\rho_{c(k)}\tau_{c(k)}\lambda_k - \rho_{c(j)}\tau_{c(j)}\lambda_j\Big]dK - \Phi'_{j\to k}(0)dK, \tag{8}$$

where $\Phi_{j\to k}(\cdot)$ denotes the (convex) friction cost of reallocating capital from j to k and $\Phi'_{j\to k}(0)$ is its marginal cost at zero. The first bracket captures productivity (after-tax) differences; the second term captures differences in marginal tax shields associated with moving debt; the last term captures frictional costs (cross-border frictions are assumed larger than domestic frictions).

If the candidate target k is an untreated foreign affiliate ($\rho_{c(k)} = 1$) and the origin j is treated ($\rho_{c(j)} < 1$ and $B_j = 1$), then the tax-shield term in (8) is typically positive and favors foreign reallocation. By contrast, when ESR adoption widens and many potential foreign targets also have $\rho < 1$, the tax advantage shrinks (or vanishes) and the reallocation decision is driven by the productivity differential net of frictions; in such cases domestic reallocation to unconstrained local affiliates (i.e., $B_m = 0$) with low domestic frictions may dominate. Also, note that because only affiliates with pre-reform interest above the de-minimis thresholds are binding, the empirical effects should be concentrated among these binding firms; non-binding affiliates will show little or no direct treatment effect.

Prediction 3: *With partial (full) introduction of ESR across countries, investments reallocate towards unconstrained foreign (domestic) affiliates.*

Additional details on the mechanisms of reallocation are provided in Appendix Section B. Using the data mentioned in detail below, I empirically test the predictions formulated in this section.

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.[7]

Note that the policy may also trigger indirect effects through intra-group reallocation of capital and debt (Giroud and Rauh, 2019; Liu, 2020). 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.[8] Such behavioral responses may contaminate the measured treatment effects if not explicitly modeled. 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.[9]
- *Sister Firms*: Other affiliates within the same multinational group that do not fail the ESR at the baseline.[10]

[7]ESR regimes in several jurisdictions incorporate additional escape provisions. A common feature is the debt–equity test, under which firms with a debt–equity ratio below a specified threshold may deduct all interest expenses even if they fail both the fixed-ratio and the de minimis tests. Some countries also allow an equity escape, permitting full deductibility when a firm's debt–assets (or debt–equity) ratio does not exceed that of its consolidated group. In addition, a number of jurisdictions operate group taxation, applying ESR to the consolidated domestic group's interest position rather than to individual affiliates. Because these provisions can induce behavioral responses (Andresen and Thorvaldsen, 2023), I classify an affiliate as treated only if it fails all relevant elements of the country-specific ESR design. Further details on country-specific ESR provisions are provided in Appendix A.

[8]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.

[9]For example, in the United Kingdom—which announced ESR in 2016—treated firms are those whose average net financial expenses during 2013-2015 exceeded both the de minimis allowance (2 million GBP) and 30% of EBITDA.

[10]In the UK example, sister firms include (i) UK affiliates within the same group that do not fail the UK ESR; (ii) foreign affiliates within the same group located in ESR-adopting countries that do not fail their own jurisdiction's ESR test; and (iii) all foreign affiliates in countries that never introduced ESR.

- *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 control group, excluding treated firms. Using a common control group across specifications ensures comparability and enables a clean 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 (Liu, 2020). To estimate the dynamic policy effects, I employ the event-study estimator of Chaisemartin et al. [2024], which accommodates unbalanced panels and staggered treatment timing:

$$y_{ist} = \alpha_i + \mu_t + \sum_{\substack{p=-4 \\ p\neq -1}}^{4} \beta_p \, treat_i \times \mathbb{1}(t - t_s^* = p) + X_{st}^{'} \, \delta + \varepsilon_{ist} \tag{9}$$

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 macro controls such as the statutory corporate tax rate, log of population, and log of GDP per capita.

For average treatment effects, I collapse the event-time interactions into a single post-policy indicator. Because variables like taxation and equity can take negative values, I report estimates in levels, i.e., USD millions, and normalize them by the mean of the treatment group in the pre-reform years for interpretability.[11] 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.[12] I assess this assumption

[11] Please see Chen and Roth [2024] for a discussion of why log-transformations are problematic when variables include zeros or negative values.

[12] This assumption requires that treated firms were not already on differential trajectories relative to control firms prior to ESR adoption, and that the timing of the policy is plausibly exogenous to firm-level financing or investment decisions.

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 parallel trends. Appendix E further shows that alternative staggered difference-in-differences estimators 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 few potential threats to identification merit discussion. First, 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, ensuring that identification comes from within-country, within-firm variation. Second, because treatment status depends mechanically on pre-reform interest expenses, firms could attempt to manipulate these expenses in anticipation of the rule. This 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.6.1 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 (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. 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], thereby mitigating biases similar to those discussed in Clausing [2016]. Nonetheless, as highlighted by Torslov et al. [2023], coverage limitations in ORBIS may still lead to underestimation of policy effects.

The main accounting variables are debt, capital, employment, interest expenses, and taxation. 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. Additional details on data cleaning steps and variable definitions are provided in the Appendix Section C and C.1, respectively.

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. Appendix Figure F.I further shows that treated firms are not disproportionately concentrated in any particular industry, and that control firms outnumber treated and sister affiliates in every industry category. These patterns indicate that identification is unlikely to be driven by industry-specific shocks or compositional imbalances.

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 underreporting 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.

Appendix Figure F.II presents these distributions separately for pre- and post-reform years. While the distributions for sister and control firms remain essentially unchanged across periods, the post-reform distribution for treated firms shifts sharply to the right. 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.

Appendix Figure F.III reinforces this interpretation by examining the distribution of treated firms around the normalized interest-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 for all firms shows no such bunching, confirming that the behavior is specific to firms directly affected by the rule.

Figure Ib 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 Figure F.IV complements 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. These patterns indicate that larger treated firms systematically rely more on internal debt as a profit-shifting instrument.

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 and the sharp bunching around the deductibility threshold both demonstrate substantial behavioral responses. These reduced-form, non-parametric patterns arise even before any empirical modeling and confirm that the reform induced meaningful changes in financial behavior 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 behavioral response of directly exposed firms, rather than the intra-group reallocations that MNEs may undertake to optimize their post-reform tax positions.

Figure II presents 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 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.[13] 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

[13]The number of *Observations* in the table exceeds the raw sample size because the estimator by Chaisemartin et al. [2024] averages over effective sample sizes in each two-way comparison, which may overlap when treatment timing differs across units.

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.

Panel IIb plots the corresponding trajectory of interest expenses, which decline sharply and permanently 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.

Mechanically, lower interest deductions increase reported profits, expanding the domestic tax base. Panel IIc confirms this pattern: tax liabilities of treated firms rise visibly after the reform, by roughly the same magnitude as the decline in interest expenses. This near one-to-one correspondence implies a direct pass-through from reduced avoidance to higher taxation. The taxation variable includes all taxes paid, accrued, or deferred, suggesting that the observed increase reflects both current-period tax payments and the recognition of deferred tax assets or liabilities. From a public finance perspective, this finding indicates that the policy achieved its core objective — broadening the corporate tax base and realigning reported profits with real economic activity.

However, reducing the tax advantage of debt inevitably raises the user cost of capital, particularly for firms that rely heavily on intra-group debt financing. As expected, this higher cost translates into lower real investment. Panel IIIa shows a marked decline in investment 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 investment. Panel IIIb shows that this recovery coincides with a corresponding rise in the equity share of financing, implying that firms eventually rebalanced their capital structures away from debt and toward equity. This substitution process involves frictions—such as equity issuance costs, internal capital budgeting constraints, and the time required to reallocate retained earnings across firms—which explains the delayed adjustment. However, the equity does not rise sufficiently to offset the net reduction in investments (Sanati and Beyhaghi, 2025).

Figures IIIc and IIId show that employment and capital at treated firms also decline in parallel after the reform. Table III 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 contrac-

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

tion 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 labor 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. Realistically, it raised the cost of capital 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 and output when financial frictions amplify their real effects (Hall and Jorgenson, 1967).

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 and Mueller, 2015; Giroud and Rauh, 2019; De Vito et al., 2025). When the ESR binds, debt-financed investment becomes more expensive at affected firms, making the relative cost of capital lower at their unconstrained sister affiliates. 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.

Appendix Figure F.V plots the dynamic treatment effects from this exercise. The results indicate a clear post-reform increase in investment at the sister firms. Table IV shows that their average investment rises by approximately 9 percent relative to pre-reform mean. 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.

Panel F.Vb reveals 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, tax liabilities 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 location of these receiving firms provides further insight into the nature of this reallocation. Sister firms may be located in the same country as treated firms ("local sisters") or abroad ("foreign sisters"). These two channels embody different adjustment mechanisms. Reallocation to local sisters likely reflects low transaction and compliance costs, and the exploitation of unused debt capacity within the same regulatory and fiscal environment. By contrast, reallocation to foreign sisters indicates cross-border arbitrage—capital flight toward jurisdictions where debt remains a more tax-efficient financing source.

Figures IVa and IVb, along with Table IV, show that the latter margin dominates. Investment increases substantially—by about 15 percent—at foreign sister firms, while remaining largely unchanged among local sisters. Employment patterns mirror this result: as Figures Va and Vb illustrate, foreign sister firms experience a clear rise in employment, whereas local firms do not. These patterns are consistent with an international reallocation of both financial and real capital to firms operating under less stringent tax environments (Graham, 1996; Giroud and Rauh, 2019).

Because the ESR was implemented asynchronously across jurisdictions, such international reallocation can only maximize after-tax firm profits if it flows to countries that did not adopt similar restrictions. To test this mechanism, I distinguish between foreign sister firms located in ESR and non-ESR countries. Figures IVc, IVd, Vc and Vd show that the reallocation is concentrated entirely in the latter: investment increases sharply in non-ESR countries, while employment exhibits a more muted but positive trend. This asymmetry reveals a policy-avoidance equilibrium—firms shift both debt and investment to jurisdictions that remain outside the coordinated anti-avoidance framework, exploiting cross-country policy heterogeneity to restore their pre-reform tax advantages. From a policy perspective, this behavior underscores the strategic substitutability of national anti-avoidance efforts: absent coordination, unilateral reforms can redirect rather than reduce global profit-shifting incentives.

The composition of these recipient countries further clarifies the nature of this reallocation. 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). Appendix Figures F.IX and F.X and Appendix Table IX support this interpretation. Both investment and debt increase disproportionately at high-tax, non-ESR affiliates, consistent with tax-motivated internal lending. While these inflows raise nominal tax payments, the ratio of tax to capital declines significantly following ESR adoption. This suggests that while overall tax collections rise, the fiscal return per unit of capital falls in these affiliates.

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 ju-

risdictions. 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. The evidence highlights that without cross-country policy alignment, national anti-avoidance measures may redistribute rather than reduce profit-shifting activity, reinforcing the need for supranational coordination to prevent the emergence of new asymmetries in global capital allocation (Johannesen, 2022).

VII.3 Effect of Coordination

The final piece of analysis turns to multinational groups that are fully exposed to the reform—that is, all their affiliates operate in jurisdictions that implemented ESR. For these groups, the reform removes the possibility of offsetting the higher cost of debt at one affiliate by reallocating investments or debt to another affiliate located in a non-enforcing country. In other words, their entire internal capital network becomes uniformly constrained. Consequently, the relative tax advantage of offshoring investments disappears, and the cost of reallocating capital abroad exceeds any potential fiscal gain.

The empirical results in Figure VI and Table IV vividly confirm this theoretical prediction. Restricting the sample to the sister firms of fully covered MNEs and comparing them to the control group reveals that, post-reform, investments at local sister firms rise by approximately 11 percent, while investment in foreign sister firms remains statistically unchanged. Employment levels at both sets of firms remain stable, implying that the adjustment operates primarily through the reallocation of capital, not labor. This pattern of within-country reallocation underscores that, when firms cannot exploit cross-border asymmetries, they optimize internally — channeling funds toward firms with higher marginal returns to capital within the same jurisdiction.

This finding carries a profound policy implication. It shows that international coordination in anti-avoidance policy fundamentally changes the equilibrium response of multinational firms. When only a subset of countries enforces ESR, firms reallocate debt and investments toward jurisdictions without such rules, eroding the enforcing countries' tax bases and creating fiscal externalities across borders. By contrast, when all major jurisdictions adopt the rule, the escape margin collapses. Firms can no longer shift profits through financial reallocation, and instead, the policy triggers an internal efficiency gain by redirecting resources to more unconstrained domestic firms.

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.4 Effect of Increasing Exposure

A more nuanced way to understand the role of policy coordination in shaping multinational investment behavior 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 Table V and Figure VIII, 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 VI 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, four of which already enforce the rule. 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. These results suggest that as coordination deepens, the incentive to move capital internationally diminishes sharply.

Columns (5) and (6) provides 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.

Taken together, these findings highlight a powerful mechanism: the marginal effect of each new country joining the coordinated framework compounds the containment of capital flight. Early unilateral adopters face leakages as firms reallocate to non-enforcing jurisdictions, but as adoption spreads, the reallocation margin narrows. Ultimately, when exposure becomes near-universal, investment responses become localized, and capital is retained within national borders. This dynamic illustrates that policy coordination is not merely about harmonizing legal standards—it reshapes the geography of investment and restores the link between domestic tax policy and the domestic tax base.

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.5 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.

For the subset of groups with available data, I estimate the effects of the reform using the same empirical specification as before. The results, presented in Figure VII and Appendix Table 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 (Liu, 2020).

In contrast, I observe a marked reversal in the taxation trajectory of treated groups. Before the reform, these groups exhibit a consistent decline in their tax liabilities—reflecting the intensification of tax avoidance behavior through debt-based profit shifting. After the reform, this trend reverses, and group-level tax payments 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 fact that this reversal is visible even in aggregate group accounts provides compelling evidence that the policy curtailed debt-based tax avoidance effectively.

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 mitigate 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 be additive rather than confounding, implying that the estimated ESR effect is conservative rather than overstated.

Overall, the group-level analysis underscores a central conclusion of this paper: coordinated anti-avoidance reforms can raise multinational tax liabilities without inducing disinvestment or capital flight, thereby reconciling fiscal fairness with economic efficiency.

VII.6 Robustness Checks

VII.6.1 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 cutoff.

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 ($t^* - 1$, $t^* - 2$, and $t^* - 3$) and three post-reform periods (t^*, $t^* + 1$, and $t^* + 2$). This structure ensures a balanced panel across the pre- and post-reform years. 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 interest 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 interest expenses to fall just below the de minimis threshold. Manipulation is unlikely because thresholds differ substantially across countries; the policy design in one jurisdiction provides no information about the relevant cutoff in another. Moreover, using the pre-reform average as the running variable mitigates contemporaneous endogeneity concerns.

As a first step, in Appendix Figures F.XI and F.XII 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 all figures, the bandwidth is restricted to USD 1.1 million around the cutoff, and each bin represents 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 interest expenses reduce their debt, interest expenses, investment, capital stock, and employment after reform, while their tax payments 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 interest expenses—are subject to stricter policy constraints and therefore experience stronger behavioral responses. Conversely, firms close to the threshold face weaker effective incidence, implying smaller behavioral 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

behavioral responses. Consistent with this interpretation, the slope is sharply negative for debt, interest expenses, investment, capital, and employment, and positive for taxation.

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

$$\Delta Y_{is} = \alpha + \beta \times Fail_{is} + \gamma^b (z_{is} - \bar{z}_s) + \gamma^a Fail_{is} \times (z_{is} - \bar{z}_s) + \epsilon_{is} \tag{10}$$

where $\Delta Y_i s$ 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 interest expense; and $\bar{z}_s$ is the country-specific de minimis threshold. The indicator $Fail_{is}$ equals one if $z_{is} > \bar{z}_s$. The baseline specification fits a linear polynomial in the running variable on each side of the threshold, and the results are robust to including quadratic terms.

In this setting, β captures the discontinuity at the cutoff, 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. Appendix Table X reports these estimates. Although the γ^a coefficients for interest expenses and investment are imprecisely estimated, their negative signs indicate stronger reductions for firms located farther from the cutoff—consistent with a larger effective constraint and higher policy exposure.

Appendix Figure F.XIII further plots the distribution of pre-reform average interest 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.

Finally, the RD estimates are unaffected by the bunching evidence discussed in Section VI. The two analyses rely on different running variables and serve distinct purposes. The bunching analysis examines the distribution of the interest-to-EBITDA ratio of treated firms around the policy threshold, while the RD framework relies on pre-reform average interest expenses, which are not subject to manipulation. The non-parametric decline in the reported interest-to-EBITDA ratio and the RD-based decline in interest expenses jointly indicate that firms farther from the threshold reduce their interest costs as a behavioral response to the ESR reform.

VII.6.2 Alternative Difference-in-Difference Methodoogies

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 Appendix E.

Specifically, I employ the canonical two-way fixed effects (TWFE) model and the approaches proposed by Sun and Abraham [2021], Borusyak et al. [2024], and Cengiz et al. [2019]. All specifications include unit and year fixed effects, as well as country-level controls. The results, presented in Appendix Figures F.XIV to F.XX, show that the dynamic estimates obtained from these estimators are highly consistent. The close alignment between the TWFE estimates and those from the alternative methods further indicates that the dynamic effects are not biased by negative weighting, as highlighted by Goodman-Bacon [2018] and Roth et al. [2023]. Further details are provided in Appendix E.

VII.6.3 Additional Checks

I further assess the robustness of the results through several complementary exercises. First, I trim and winsorize the data at alternative cutoffs, redefine ownership linkages using different ownership thresholds, and re-estimate the effects on a balanced sample. Second, I confirm that the results are not driven by any single country or industry. Finally, 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.

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 full exposure, the policy raises taxable income without depressing aggregate investment, instead improving the internal allocation of capital within the group.

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. Nonetheless, the evidence in this paper demonstrates that well-coordinated anti-avoidance policies can simultaneously protect the corporate tax base and support efficient capital allocation within multinational groups.

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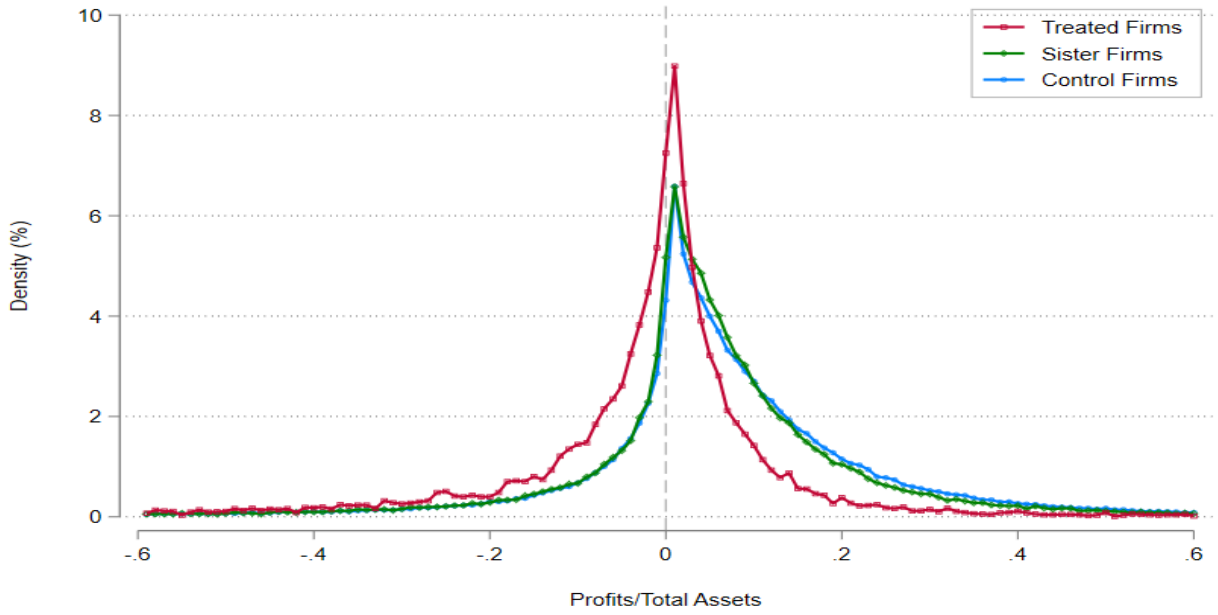
IX Figures

FIGURE I: **STYLIZED FACTS**

(A) **DISTRIBUTION OF ACCOUNTING PROFITS**

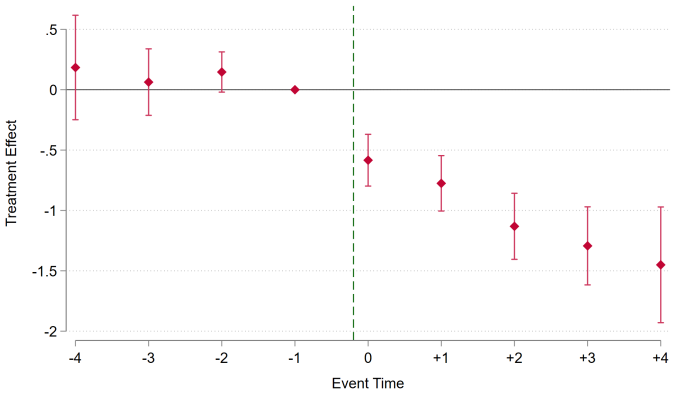


(B) **SHARE OF TREATED FIRMS BY FIRM SIZE**

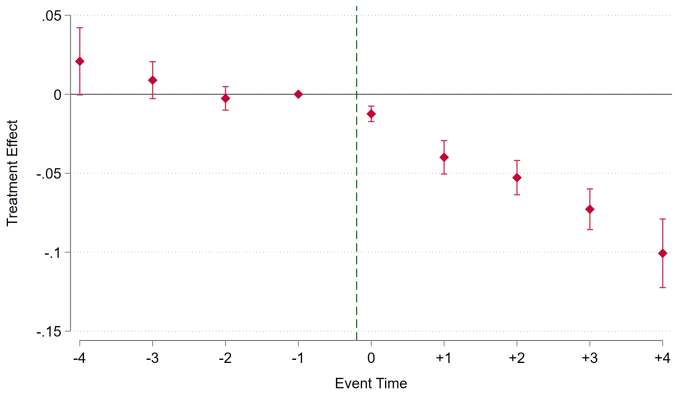
Notes: Panel (A) plots the density distribution of the ratio of accounting profits to total assets for each of the three types of firms, i.e., treated firms, sister firms, and control firms. The ratio is restricted between -0.6 and 0.6 and divided into 360 bins. Panel B plots the share of treated firms across firm size deciles, where size is defined by the distribution of total assets. Firm size deciles are constructed from the mean total assets within each affiliate, and the vertical axis reports the percentage of affiliates in each decile classified as treated. In both the figures, the sample is restricted to three pre-reform years only and to firms with non-missing positive total assets across these three periods.

FIGURE II: **EFFECTS OF ESR - TREATED FIRMS**

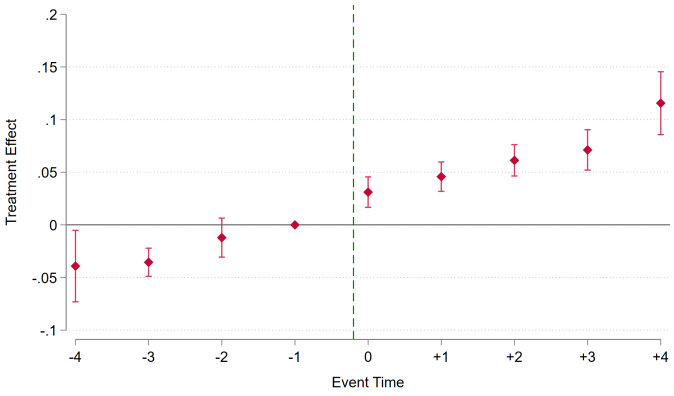
(A) **LEVEL OF DEBT**



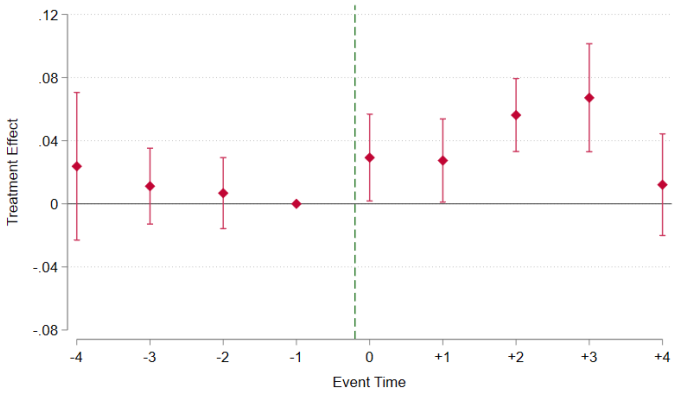
(B) **INTEREST EXPENSE**



(C) **TAXATION**

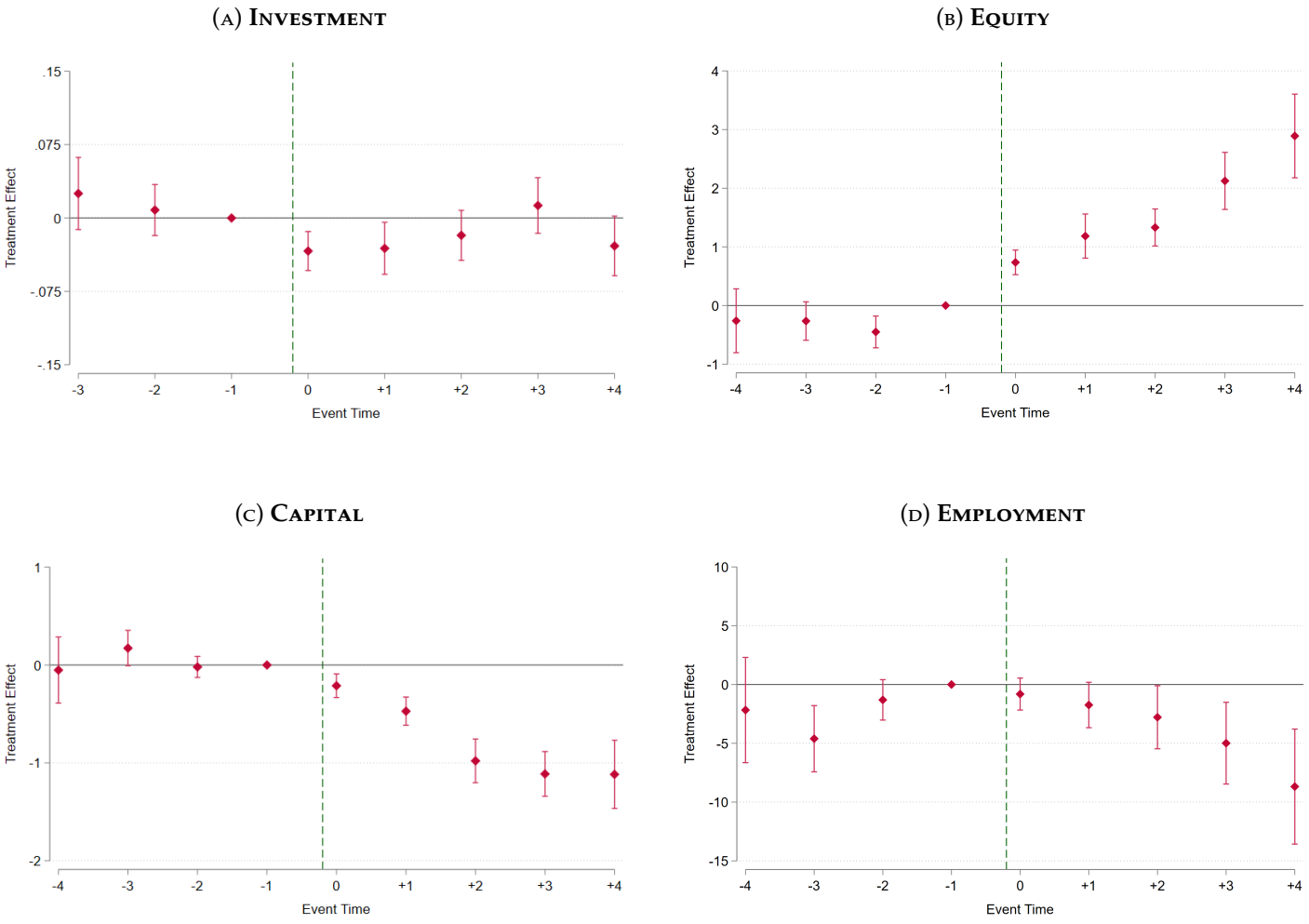


(D) **TAX:CAPITAL**



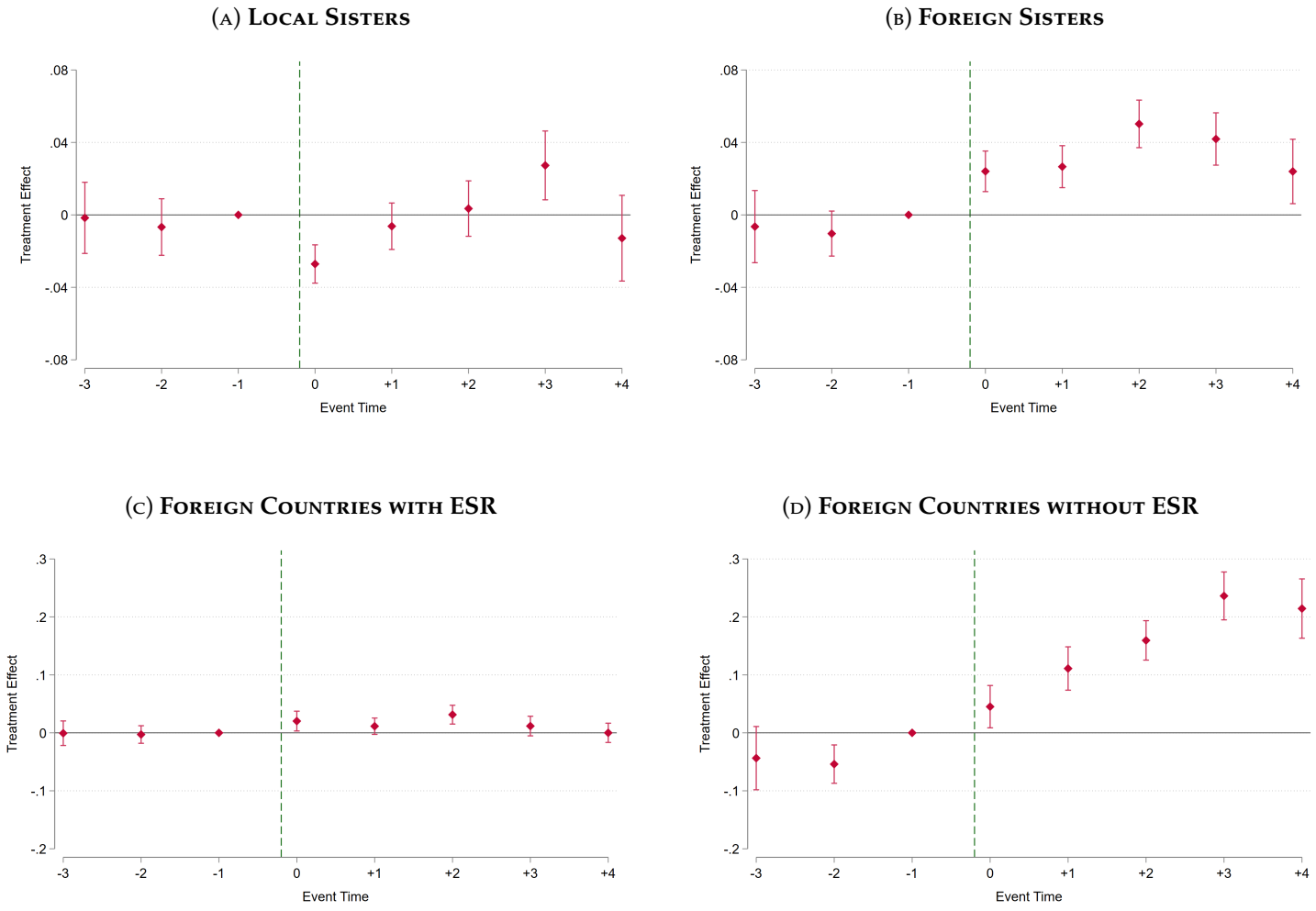
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). Panels A, B, and C present outcomes in USD millions. The outcomes of treated firms are compared with that of control firms; sister firms are dropped from the analysis. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped with 20 repetitions 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 III: **EFFECTS OF ESR - TREATED FIRMS**



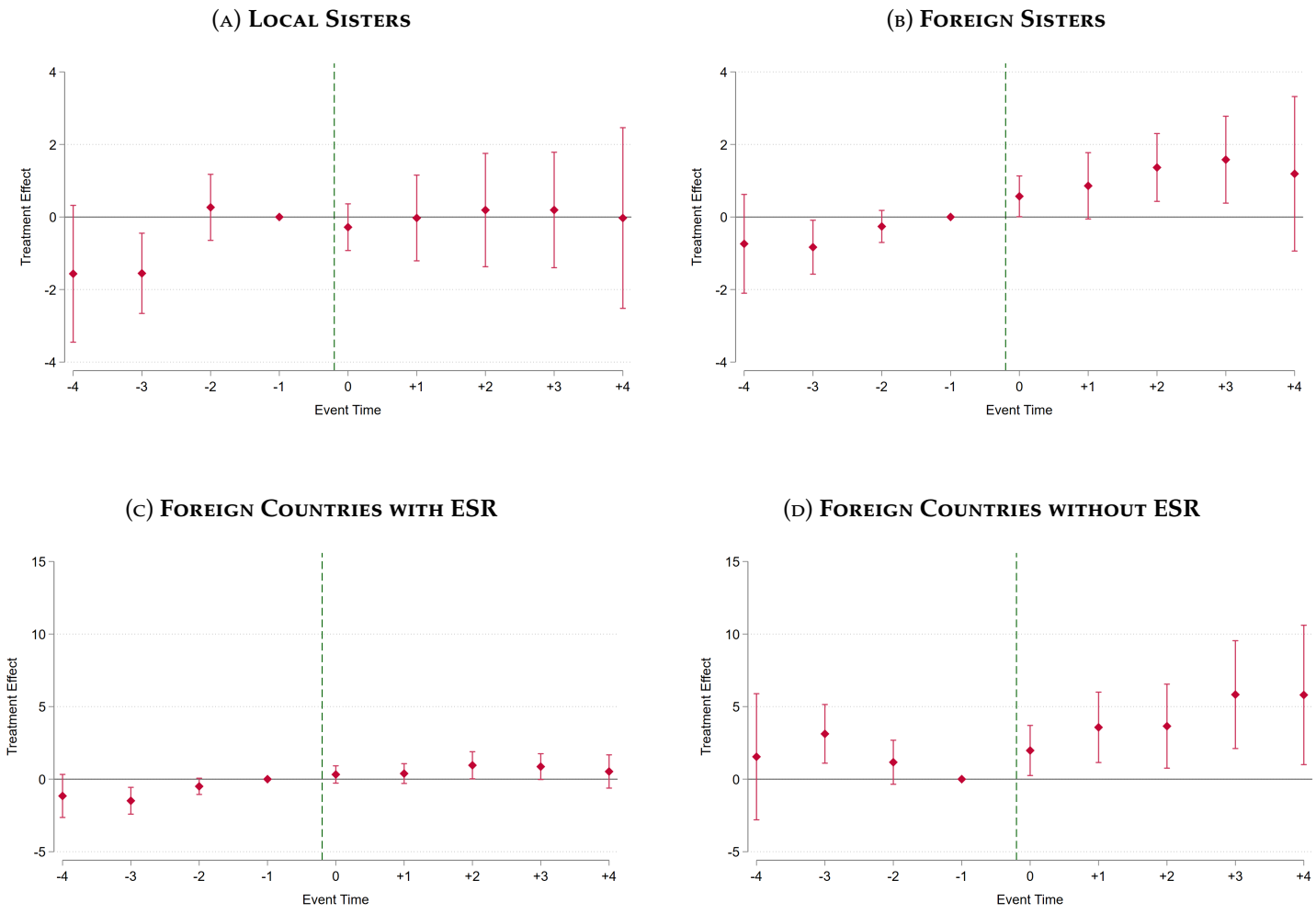
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). Panel A shows the investment rate, defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Panels B and C present outcomes in USD millions. Panel D reports effects on employment, measured as the number of employees. 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 with 20 repetitions 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: **REALLOCATION OF INVESTMENTS**



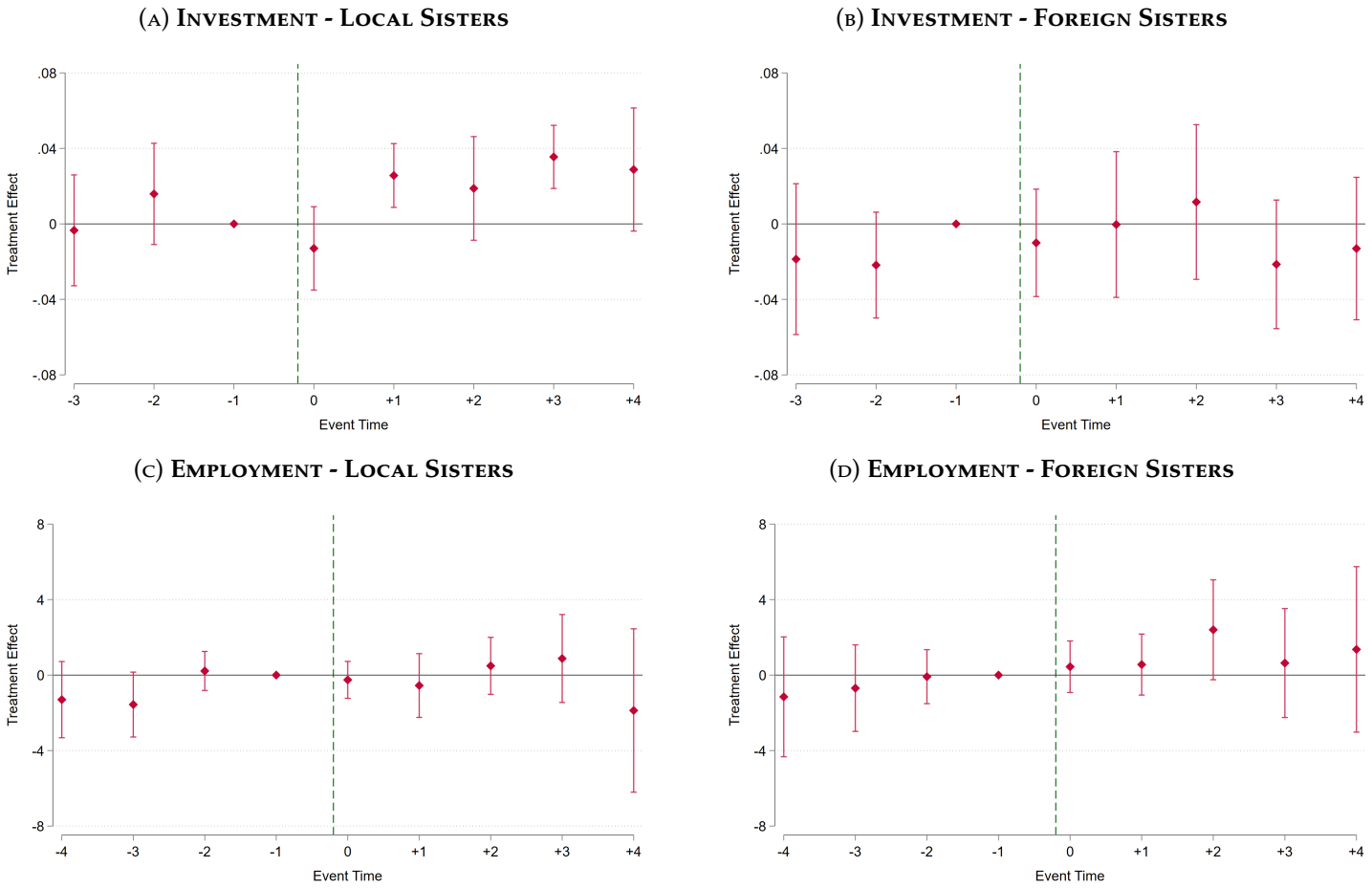
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). Investments are defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. The outcomes of sister firms are compared with that of 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 reference year is $t^* - 1$ and we include firm fixed effects, year fixed effects, and time-varying 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 with 20 repetitions 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: Effect on Employment



Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). Employment is defined as the number of employees. 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 with 20 repetitions 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.

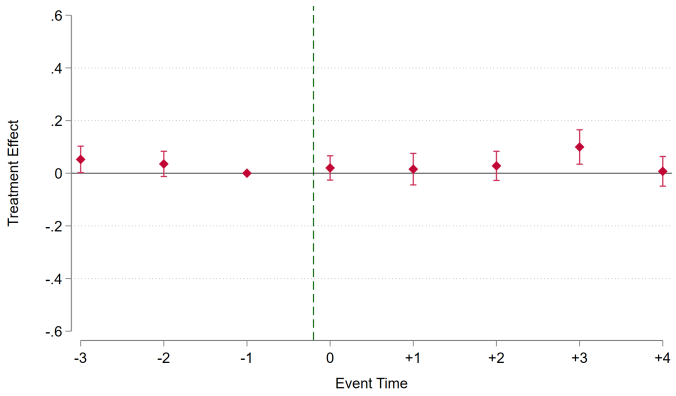
Figure VI: Effect on Fully Covered MNEs



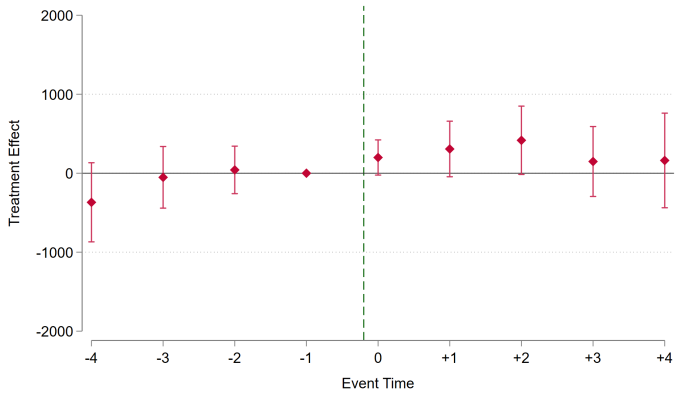
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). The sample is restricted to sister firms of MNE groups in which all affiliates operated in jurisdictions that had implemented ESR ("Fully Covered" MNEs). The outcomes of such local and foreign sisters are compared against control firms. Investments are defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Employment is defined as the number of employees. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust, bootstrapped with 20 repetitions 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 OF ESR - GROUP LEVEL**

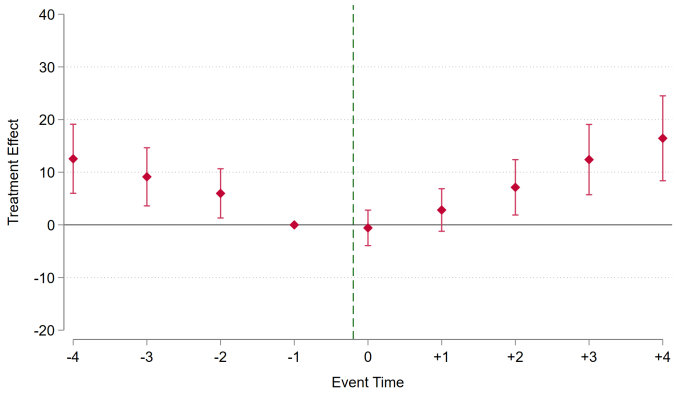
(A) **INVESTMENT**



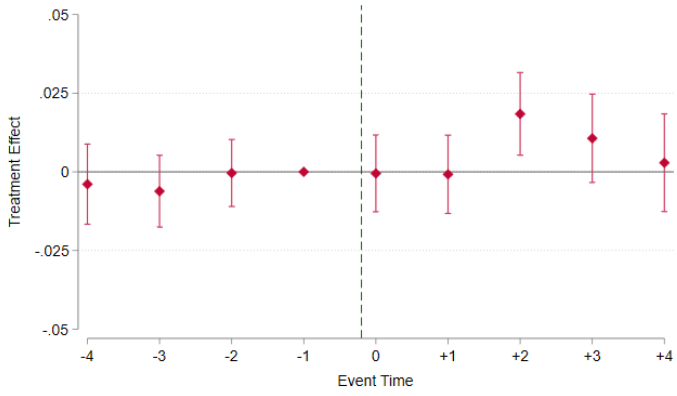
(B) **EMPLOYMENT**



(C) **TAXATION**



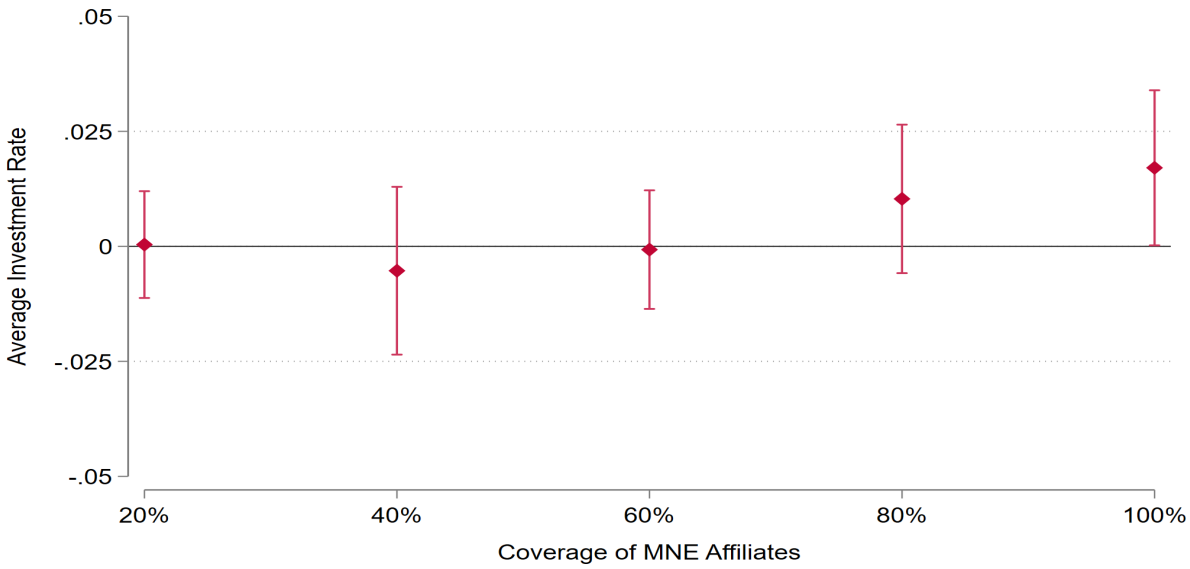
(D) **TAX:CAPITAL**



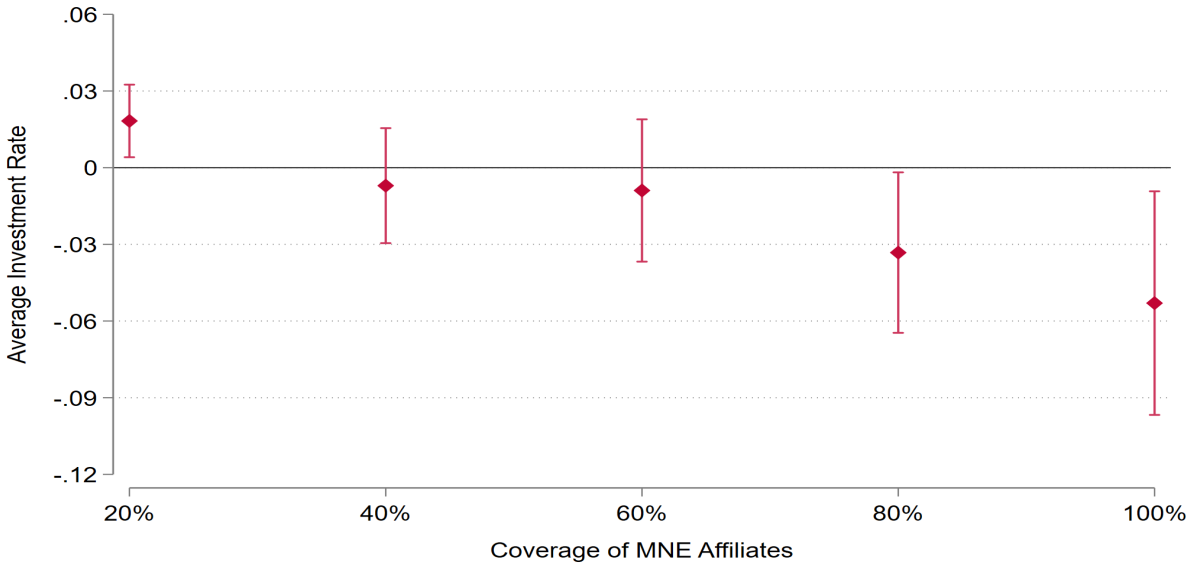
Notes: This figure reports dynamic group-level treatment effects of ESR estimated using equation (9). Panel A reports the effect on investment defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from t^*-3 onward. Panel B reports the effect on employment, defined as the number of employees, and Panel C reports the effect on taxation measured 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 with 50 repetitions 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 VIII: **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 fixed effects, 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 with 20 repetitions and clustered at the firm level.

X Tables

TABLE I: **SUMMARY STATISTICS**

	Observations	Mean	P10	Median	P90	Distinct Units
Firm Level Variables						
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	20.09	.0013	.3582	14.92	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
Taxation	1,085,147	.6415	0	.01	.7026	167,436
Tax:Capital	1,021,317	72.35	0	.0292	1.866	161,116
Firm Level Controls						
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
Group Level Variables						
Debt	66,493	1269	0	40.19	2014	8,584
Equity	66,258	1756	5.446	120.4	3135	8,577
Capital	66,493	1321	1.345	57.64	1878	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
Taxation	65,552	68.26	-.0611	3.006	100.7	8,556
Tax:Capital	65,076	.2361	-.0033	.0563	.4265	8,546
Group Level Controls						
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 our unbalanced panel data set consisting of four pre-refom and 4 post-reform years. *Distinct Units* is the number of distinct firms for "Firm Level Variables", number of distinct jurisdictions across which the affiliates 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*, *Capital*, *Interest*, and *Taxation* are reported in USD millions. *Investment* is the gross investment scaled by lagged fixed assets and *Employment* is the number of employees.

TABLE II: **TAX AVOIDANCE EFFECTS OF ESR - TREATED FIRMS**

Outcomes (y_i):	Debt	Interest	Taxation	Tax:Capital
	(1)	(2)	(3)	(4)
$Failed_i \times Post_t$	-0.97***	-0.05***	0.06***	0.05***
	(8.44)	(10.76)	(10.92)	(3.11)
Pre-Reform Mean	10.42	0.59	0.29	0.26
Percentage Change	-9.34%	-8.20%	20.06%	19.78%
Observations	1,996,371	1,020,738	1,733,688	1,633,753
Switchers	15,750	13,246	12,843	12,139

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR at the affiliate level. *Debt*, *Interest*, and *Taxation* are measured in USD million. The outcomes of treated affiliates are compared with the control group; sister affiliates are dropped from the analysis. Pre-Reform Mean is the sample mean of the treated affiliates in pre-reform periods. I control for firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. The standard errors are robust, bootstrapped with 20 repetitions, 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: **REAL EFFECTS OF ESR - TREATED FIRMS**

Outcomes (y_i):	Capital	Employment	Investment	Equity
	(1)	(2)	(3)	(4)
$Failed_i \times Post_t$	-0.70***	-3.19***	-0.02**	1.46***
	(9.03)	(3.03)	(2.14)	(9.11)
Pre-Reform Mean	16.24	140.18	0.19	13.39
Percentage Change	-4.34%	-2.28%	-10.61%	10.94%
Observations	1,996,371	1,285,673	1,522,340	1,995,594
Switchers	15,750	8,735	13,701	15,738

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR at the affiliate level. *Capital* and *Equity* are measured in USD million, *Investment* as the ratio of gross investment spending scaled by lagged fixed assets, and *Employment* as the number of employees. The outcomes of treated affiliates are compared with the control group; sister affiliates are dropped from the analysis. Pre-Reform Mean is the sample mean of the treated affiliates in pre-reform periods. I control for firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. The standard errors are robust, bootstrapped with 20 repetitions, 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 IV: **EFFECT OF ESR - SISTER AFFILIATES**

	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							
$Failed_i \times Post_t$	0.02***	-0.00	0.03***	0.02**	0.14***	0.02**	-0.01
	(3.94)	(0.61)	(6.79)	(2.39)	(10.92)	(2.03)	(0.42)
Pre-Reform Mean	0.22	0.20	0.23	0.24	0.17	0.15	0.19
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							
$Failed_i \times Post_t$	0.71*	-0.01	1.07**	0.59*	3.83***	-0.09	1.03
	(1.82)	(0.01)	(2.32)	(1.73)	(3.12)	(0.12)	(1.05)
Pre-Reform Mean	118.78	101.50	129.41	127.69	138.86	73.94	83.05
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: Taxation							
$Failed_i \times Post_t$	0.00**	-0.00	0.01***	0.00	0.05***	-0.00	-0.00
	(2.24)	(0.03)	(2.88)	(0.27)	(5.28)	(0.99)	(0.00)
Pre-Reform Mean	0.30	0.22	0.36	0.34	0.43	0.15	0.22
Percentage Change	1.51%	-0.03%	2.06%	0.29%	10.64%	-2.76%	-0.01%
Observations	1,895,033	1,785,857	1,805,166	1,790,259	1,712,908	1,734,698	1,710,689
Switchers	112,458	44,396	68,062	58,903	9,159	14,088	9,035

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR at the affiliate level. *Investment* is measured as gross investment scaled by lagged fixed assets; *Employment* is the number of employees; and *Taxation* is expressed in USD millions. Treated affiliates are excluded, and each category of sister affiliates is compared against the control group. Column (1) presents estimates for all sister affiliates. Columns (2) and (3) report results for local and sister affiliates, respectively. Columns (4) and (5) show estimates for foreign affiliates, 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 affiliates in this category are located in jurisdictions that also introduced ESR rules. 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 bootstrapped with 20 repetitions and clustered at the firm level.

TABLE V: **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	0.018***	-0.003	-0.008	0.002	-0.012	0.012	-0.035**	0.018*	-0.051**
	(0.07)	(2.64)	(0.41)	(0.87)	(0.17)	(0.64)	(1.19)	(2.00)	(1.71)	(2.18)
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

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR on investments (measured as gross investment scaled by lagged fixed assets) 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 specification include firms fixed effects, year fixed effects and jurisdiction-level controls. Pre-Reform Mean refers to the mean outcome for each affiliate category in the pre-reform period. Standard errors are bootstrapped with 20 repetitions, clustered at the firm level.

TABLE VI: **EFFECTS OF COORDINATED ESR POLICY**

	Affiliate Exposure		Country Exposure		Group Exposure	
	Local	Foreign	Local	Foreign	Local	Foreign
	(1)	(2)	(3)	(4)	(5)	(6)
$Failed_i \times Post_t$	0.001**	-0.002**	0.008	-0.012**	0.021**	-0.025
	(2.42)	(2.00)	(1.22)	(2.04)	(2.10)	(1.09)
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

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the effect of rising group-level exposure to ESR on investment at sister affiliates. *Group Exposure* is measured as the share of an MNE's affiliates operating under ESR in a given year and ranges from 0 (no exposure) to 1 (full exposure). Columns (1) and (2) isolate the marginal effect of one additional affiliate within the group becoming subject to ESR. Columns (3) and (4) capture the effect of one additional country adopting ESR. Columns (5) and (6) estimate the effect of moving from zero to full exposure on investment at local and foreign sister affiliates. Investment is defined as the ratio of gross investment spending scaled by lagged fixed assets. Pre-Reform Mean refers to the average investment rate of the respective affiliate category in pre-reform periods. All specifications include firm fixed effects, year fixed effects, and jurisdiction-level time-varying controls. Standard errors are clustered at the firm level and are bootstrapped with 20 repetitions.

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 and interest, including financial lease charges, foreign exchange gains and 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][18].

- **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.[19]

[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 October 2022.

[17]There are some exceptions for the application of the rule. For instance, the limitation does not apply if the taxpayer can prove that the ratio between the interest and the net income of the Argentine taxpayer is lower than or equal to the same ratio applicable for its economic group in relation to debts with unrelated creditors, or if it is evidenced - through reliable means - that the beneficiary of the interest has actually paid tax on such income in accordance with the Argentine law.

[18]For details please see for example here. The website was accessed on 19 July 2025.

[19]For details please see for example here. The website was accessed on 19 December 2023.

- **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 of EUR 3 million. The excess interest expenses can be carried forward indefinitely and the law includes a grandfathering clause.[20]

- **Botswana:** announced the policy in 2018 and introduced these laws on July 1st, 2019. It allows net interest expense of 30% of EBITDA.[21]

- **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.[22]

- **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%.[23]

- **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.[24]

- **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 and allows for group ratio. The excess interest expenses and unused capacity can be carried forward for up to 5 years.[25]

- **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.[26]

[20]There are 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 intragroup 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, we define the rule at the most basic and baseline definition. For details please see for example here. The website was accessed on 19 December 2023.

[21]For details please see for example here. The website was accessed on 13 October 2022.

[22]For details please see for example here. The website was accessed on 19 July 2025.

[23]For details please see for example here. The website was accessed on 13 October 2022.

[24]For details please see for example here. The website was accessed on 13 October 2022.

[25]For details please see for example here. The document was accessed on 19 July 2025.

[26]For details please see for example here. The document was accessed on 13 October 2022.

- **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 and an assets escape clause is also present.[27]

- **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.[28]

- **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.[29]

- **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 years[30].

- **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.[31]

- **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.[32]

[27]For details please see for example here. The website was accessed on 18 July 2025.

[28]For details please see for example here. The website was accessed on 19 July 2025.

[29]For details please see for example here. The website was accessed on 20 December 2023.

[30]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. We 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 December 2023.

[31]For details please see for example here. The website was accessed on 13 October 2022.

[32]For details please see for example here. The website was accessed on 19 July 2025.

- **Hungary:** introduced these rules on January 1st, 2019.[33] 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.[34]

- **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.[35]

- **India:** announced the policy in February 2017 and introduced these rules on April 1st, 2018.[36] 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.[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

[33] We assume the policy announcement year to be 2018.

[34] For details please see for example here and here. The websites were accessed on 13 October 2022 and 19 December 2023.

[35] For details please see for example here. The website was accessed on 11 July 2024.

[36] 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.

[37] For details please see for example here. The website was accessed on 19 July 2025.

[38] For details please see for example here. The website was accessed on 23 August 2022.

[39] For details please see for example here. The website was accessed on 18 July 2025.

[40] For details please see for example here. The website was accessed on 18 July 2025.

[41] For details please see for example here. The document was accessed no 19 July 2025.

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]

- **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]

[42]For details please see for example here. The website was accessed on 20 December 2023.
[43]For details please see for example here. The website was accessed on 13 October 2022.
[44]For details please see for example here. The website was accessed on 14 October 2022.
[45]For details please see for example here. The website was accessed on 20 December 2023.
[46]For details please see for example here. The website was accessed on 14 October 2022.
[47]For details please see for example here and here. The documents were accessed on 14 October 2022 and 20 December 2023, respectively.
[48]For details please see for example here and here. The websites were accessed on 19 July 2025 and 14 October 2022, respectively.
[49]For details please see for example here.

- **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.[51] 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.[52]

- **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 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.[53]

- **Slovakia:** announced the policy in 2014 and introduced these rules on January 1st, 2015. It allows gross interest expense of 25% of EBITDA.[54]

- **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.[55]

- **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.[56]

- **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.[57]

- **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.[58]

[50] For details please see for example here. The website was accessed on 18 July 2025.
[51] We assign the treatment year as 2018 for Portuguese firms.
[52] For details please see for example here and here. The website was accessed on 28 December 2023.
[53] For details please see for example here and here. The websites were accessed on 14 October 2022 and 18 July 2025, respectively.
[54] For details please see for example here and here. The websites were accessed on 13 October 2022 and 18 July 2025, respectively.
[55] For details please see for example here. The document was accessed on 14 October 2022.
[56] For details please see for example here. The document was accessed on 30 March 2024.
[57] For details please see for example here. The website was accessed on 18 July 2025.
[58] For details please see for example here. The document was accessed on 19 July 2025.

- **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.[59]

- **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.[60]

- **Vietnam:** introduced the policy on May 1st, 2017. It allows gross interest expense to related parties of 20% of EBITDA.[61]

- **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.[62]

[59]For details please see for example here. The website was accessed on 18 July 2025.
[60]For details please see for example here. The website was accessed on 19 July 2025.
[61]For details please see for example here. The document was accessed on 14 October 2022.
[62]For details please see for example here. The website was accessed on 19 July 2025.

B Appendix: Reallocation across affiliates (expanded)

A reduction in capital at a treated affiliate j generates a reallocation margin. To model the choice between foreign and domestic reallocation more precisely I introduce (i) country-level ESR adoption indicators, (ii) affiliate-level binding indicators (whether an affiliate is above the deductibility threshold), and (iii) marginal reallocation conditions that account for tax-shield changes and frictions.

Threshold and binding indicator. For each country c define a deductible-interest threshold T_c (e.g. expressed in absolute interest expense or as a function of EBITDA). An affiliate m is *binding* with respect to the ESR if its pre-reform interest deduction would exceed the threshold after the reform; formally define the indicator

$$B_m \;=\; \mathbf{1}\{i\lambda_m K_m > T_{c(m)}\}.$$

If $B_m = 1$ the affiliate suffers a reduction in tax deductibility when its country adopts an ESR; if $B_m = 0$, the affiliate remains effectively unconstrained (for example because its interest expense lies below the threshold).

Let $s_c \in \{0, 1\}$ be the country-level ESR adoption indicator: $s_c = 1$ if country c adopts an ESR (so that its deductible fraction becomes $\rho_c < 1$), and $s_c = 0$ otherwise (full deductibility, $\rho_c = 1$).

Marginal tax-shield effect when moving ΔK. Consider moving an infinitesimal unit of capital $\mathrm{d}K$ from treated affiliate j to candidate target k. At first order the change in the group's tax-shield component equals the difference between the marginal deductibility at the two locations. Using the ρ-notation the marginal change in (annual) tax shield is

$$\mathrm{d}\mathcal{T}_{j\to k} \;=\; i\,\mathrm{d}K\,\Big[\rho_{c(k)}\tau_{c(k)}\lambda_k - \rho_{c(j)}\tau_{c(j)}\lambda_j\Big],$$

where $\rho_{c(\cdot)} = 1$ if $s_{c(\cdot)} = 0$ and $\rho_{c(\cdot)} = \rho_{c(\cdot)} < 1$ if $s_{c(\cdot)} = 1$. For small reallocations I evaluate λ's at pre-move levels or allow them to adjust at higher order. Crucially, if the target country $c(k)$ has $s_{c(k)} = 0$ and the origin country $c(j)$ has $s_{c(j)} = 1$ (and both affiliates are otherwise comparable), then $\rho_{c(k)}\tau_{c(k)}\lambda_k - \rho_{c(j)}\tau_{c(j)}\lambda_j$ is typically positive and the tax-shield term favors foreign reallocation.

Marginal net benefit (general). Include productivity and friction terms to obtain the marginal net benefit of relocating $\mathrm{d}K$ from j to k:

(11)

$$\mathrm{d}NB_{j\to k} \;=\; \Big[(1-\tau_{c(k)})f_k'(K_k) - (1-\tau_{c(j)})f_j'(K_j)\Big]\mathrm{d}K + i\Big[\rho_{c(k)}\tau_{c(k)}\lambda_k - \rho_{c(j)}\tau_{c(j)}\lambda_j\Big]\mathrm{d}K - \Phi_{j\to k}'(0)\,\mathrm{d}K,$$

where $\Phi_{j\to k}(\cdot)$ is the (convex) friction cost of reallocating capital from j to k, with $\Phi'_{j\to k}(0)$ its marginal cost at zero. The first bracket is the productivity (after-tax) differential; the second term is the marginal tax-shield gain; the last term is the marginal friction cost.

Condition for profitable foreign reallocation. If k is an *untreated foreign affiliate* (so $s_{c(k)} = 0$) and j is a treated one (so $s_{c(j)} = 1$ and $B_j = 1$), then $\rho_{c(k)} = 1$ and $\rho_{c(j)} = \rho_{c(j)} < 1$. For a marginal move to be profitable we require

$$(1-\tau_{c(k)})f'_k(K_k) - (1-\tau_{c(j)})f'_j(K_j) + i[\tau_{c(k)}\lambda_k - \rho_{c(j)}\tau_{c(j)}\lambda_j] > \Phi^{F'}_{j\to k}(0), \tag{12}$$

where $\Phi^{F'}_{j\to k}(0)$ denotes the marginal cross-border reallocation cost.

When the foreign country also adopts ESR. If $c(k)$ subsequently adopts an ESR (so $s_{c(k)}$ changes from 0 to 1 and $\rho_{c(k)} < 1$), the tax term in (12) becomes

$$i[\rho_{c(k)}\tau_{c(k)}\lambda_k - \rho_{c(j)}\tau_{c(j)}\lambda_j],$$

which may be small or negative. In particular, if $\rho_{c(k)}\tau_{c(k)}\lambda_k \approx \rho_{c(j)}\tau_{c(j)}\lambda_j$ the entire tax advantage vanishes, and the foreign reallocation condition reduces to comparing pure after-tax productivity differentials to the (typically larger) cross-border friction:

$$(1-\tau_{c(k)})f'_k(K_k) - (1-\tau_{c(j)})f'_j(K_j) > \Phi^{F'}_{j\to k}(0).$$

Because $\Phi^{F'}_{j\to k}(0)$ is typically large relative to domestic frictions, profitable foreign reallocation becomes unlikely.

Domestic reallocation and selection of unconstrained local targets. Consider reallocating to another affiliate ℓ in the same country $c(j)$. Two cases arise:

- **Unconstrained local target** ($B_\ell = 0$): the local target's interest expense lies below the threshold and hence $\rho_{c(j)}$ does not bind for ℓ. For such ℓ the tax-shield term when moving $\mathrm{d}K$ from j to ℓ equals

  $$i[\rho_{c(j)}\tau_{c(j)}\lambda_\ell - \rho_{c(j)}\tau_{c(j)}\lambda_j] = i\rho_{c(j)}\tau_{c(j)}(\lambda_\ell - \lambda_j).$$

  If $\lambda_\ell < \lambda_j$ (the unconstrained target has lower existing leverage) this term can be positive: moving capital to ℓ can lower aggregated taxed interest because the net borrower position concentrates where it yields greater operational return without breaching the deductible cap.

- **Constrained local target** ($B_\ell = 1$): both affiliates are constrained; the marginal tax advantage of moving capital between them is attenuated or zero.

The marginal net benefit of domestic reallocation to an unconstrained local target simplifies to

$$(1-\tau_{c(j)})f'_\ell(K_\ell) - (1-\tau_{c(j)})f'_j(K_j) + i\rho_{c(j)}\tau_{c(j)}(\lambda_\ell - \lambda_j) > \Phi^{D'}_{j\to\ell}(0), \tag{13}$$

where $\Phi^{D'}$ is the marginal domestic reallocation cost. Notice that (i) the first bracket uses the same country tax rate, and (ii) domestic frictions $\Phi^{D'}_{j\to\ell}(0)$ are typically smaller than $\Phi^{F'}_{j\to k}(0)$.

Comparison: when domestic reallocation dominates foreign reallocation. Suppose initially $c(k)$ is untreated and $c(j)$ treated. The MNE chooses the margin with the highest marginal net benefit. Domestic reallocation strictly dominates foreign reallocation for small moves (marginal comparison) if

$$\begin{aligned}(1-\tau_{c(j)})f'_\ell(K_\ell) - (1-\tau_{c(j)})f'_j(K_j) + i\rho_{c(j)}\tau_{c(j)}(\lambda_\ell - \lambda_j) - \Phi^{D'}_{j\to\ell}(0) \\ > (1-\tau_{c(k)})f'_k(K_k) - (1-\tau_{c(j)})f'_j(K_j) + i[\tau_{c(k)}\lambda_k - \rho_{c(j)}\tau_{c(j)}\lambda_j] - \Phi^{F'}_{j\to k}(0).\end{aligned} \tag{14}$$

In other words, smaller foreign friction $\Phi^{F'}$ favors foreign moves; a larger tax-shield advantage (when $s_{c(k)} = 0$) favors foreign moves; but a favorable productivity or leverage profile at local unconstrained targets and much smaller $\Phi^{D'}$ favor domestic reallocation.

Effect of universal ESR adoption. If all countries in the MNE's portfolio adopt ESRs (so $s_c = 1$ for all relevant c), then the tax-shield differential between any pair of affiliates largely disappears (both ρ-terms are $\rho < 1$), so the inequalities (12) and (13) reduce to productivity vs friction comparisons. Because $\Phi^{D'} \ll \Phi^{F'}$ empirically, domestic reallocation typically dominates.

In sum, for an infinitesimal reallocation $\mathrm{d}K$ from treated affiliate j,

1. If there exists an untreated foreign affiliate k with τ- and λ-profile such that (12) holds, then the MNE reallocates marginal capital to that foreign affiliate.

2. If all foreign affiliates in the MNE's network satisfy $s_c = 1$ (ESR adopted everywhere) but there exists an unconstrained local affiliate ℓ with (13) holding, the MNE reallocates domestically to ℓ.

3. If neither inequality holds for any candidate target, no marginal reallocation occurs.

C 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.[63] 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.

C.1 Definition of Variables

- **Debt.** Sum of long-term borrowings from credit institutions and bonds issued, and short-term financial debts (e.g., borrowings from credit institutions, current portion of

[63] Detailed extraction scripts are available in the supplementary code files.

long-term debt, bonds, etc.).

- **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., $K_t - K_{t-1} +$ 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., $\frac{K_t - K_{t-1} + \text{depreciation}}{K_{t-1}}$
- **Employment.** Total number of employees on the company's payroll.
- **EBITDA.**[64] 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.
- **Taxation.** All taxes incurred by a firm in a given year (paid, accrued, or deferred).

[64] Earnings Before Interest, Taxes, Depreciation, and Amortization.

D Empirical Methodology - Additional Details

D.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 test is considered failed if financial expenses (net or gross, depending on the jurisdiction) are strictly positive.

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 test is automatically failed. 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[65].

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 interest expenses instead. I also abstract from asset escape clause when identifying treatment units in Denmark.

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.

[65] Equity ratio is defined as equity divided by total assets.

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.

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 Chaisemartin et al. [2024]. Additionally, for all the parameters of interest for all three sets of analysis, i.e., ESR effects on treated affiliates, sister affiliates, and the treated groups, I always control for unit fixed effects, year fixed effects, and jurisdiction-level time varying controls, i.e., $log(population)$, $log(GDP)$, and $taxrates$, to account for $jurisdiction \times years$ fixed effects.

The first alternative estimation is the standard two-way fixed effects (TWFE) estimators. Next I run the Sun [2022] STATA estimation as proposed by Sun and Abraham [2021]. Thereafter, I estimate Borusyak [2023] coefficients in STATA based on Borusyak et al. [2024] with the conditions of *autosample* set *TRUE* and with a tolerance of 0.005.[66] Successively, I estimate the Bleiberg [2021] STATA coefficients put forth by Cengiz et al. [2019].

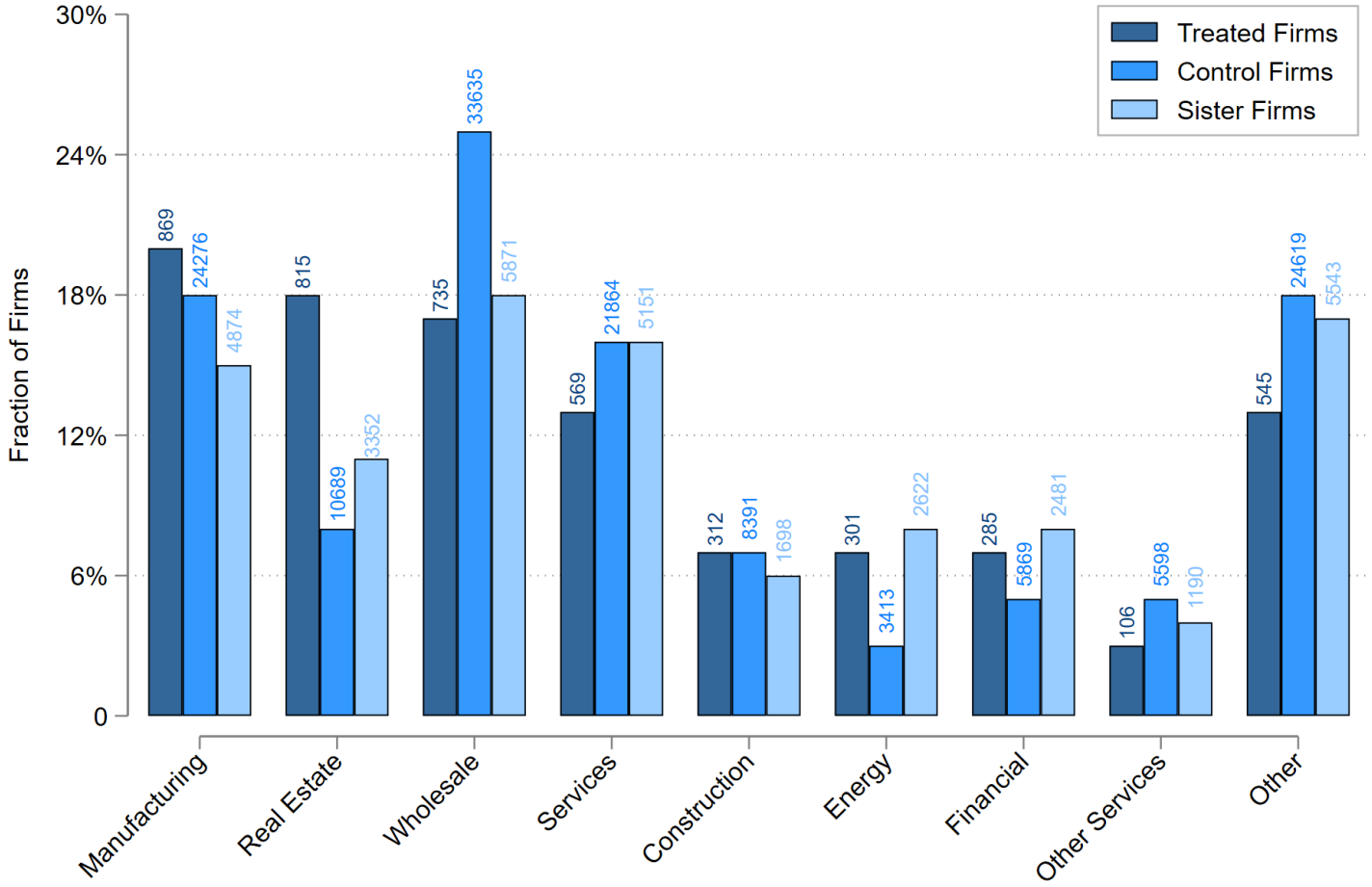
Note that in the setting of this project, the treated units get treated only once in the baseline years and their treatment status never changes. Additionally, while the baseline results estimated by the STATA command Chaisemartin et al. [2024] proposed by de Chaisemartin and D'Haultfoeuille [2023] doesn't allow to restrict the control sample to never treated units, i.e., while estimating the dynamic event study coefficients, it compares the outcomes of units whose treatment status changed (i.e., treated units) against those that didn't (i.e., control units and treated units who received treatment at earlier/later dates), a few other estimators do allow to restrict the analysis to *never treated units*. Therefore, I restrict the control cohort to never treated units when estimating the coefficients based on Sun and Abraham [2021] and Cengiz et al. [2019]. Thus, the coefficients retrieved in these settings also serve as a test of non-confoundedness of my baseline results due to different control group. In other words, a visual comparison of the coefficients from these two settings with that of the baseline coefficients based on de Chaisemartin and D'Haultfoeuille [2023] serves as a test for bias in my baseline setting.

Importantly, unlike the other estimators, the estimates by Borusyak et al. [2024] estimate the placebo and treatment coefficients in an asymmetric way (Roth, 2024). Therefore, I scale the placebo coefficients of these two estimators to equal to 0 in the $t^* - 1$.

[66] For the graphs on fully covered MNE groups the tolerance is set to 0.01 due to limited observations.

F Additional Figures

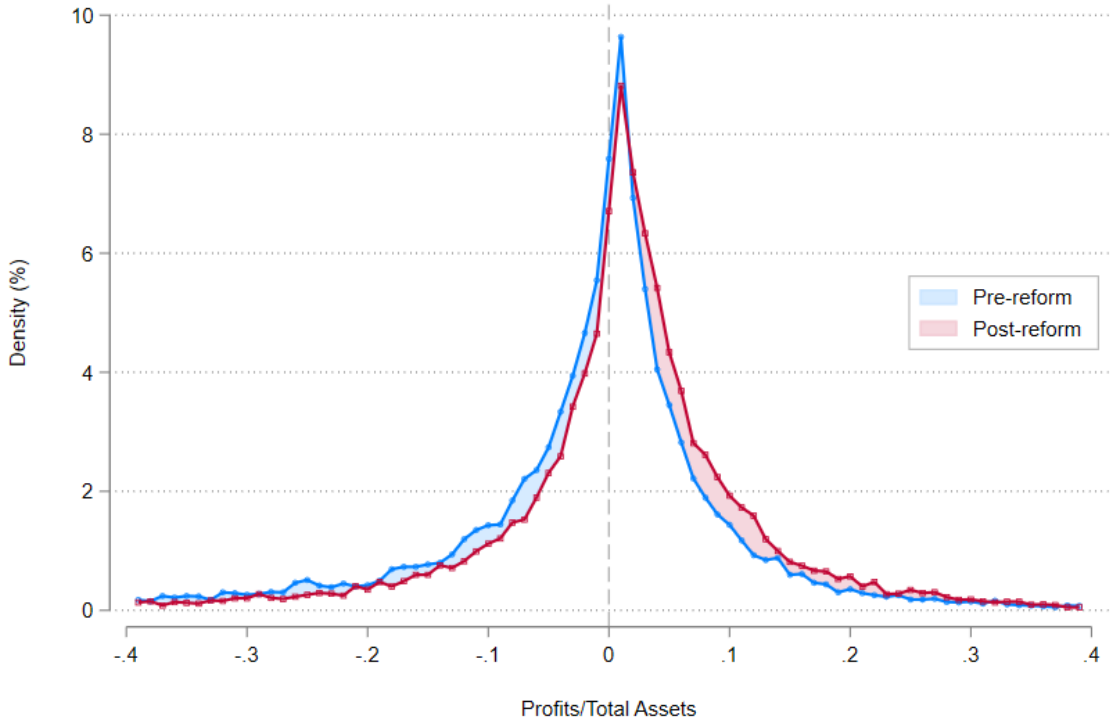
FIGURE F.I: **INDUSTRY CLASSIFICATION**



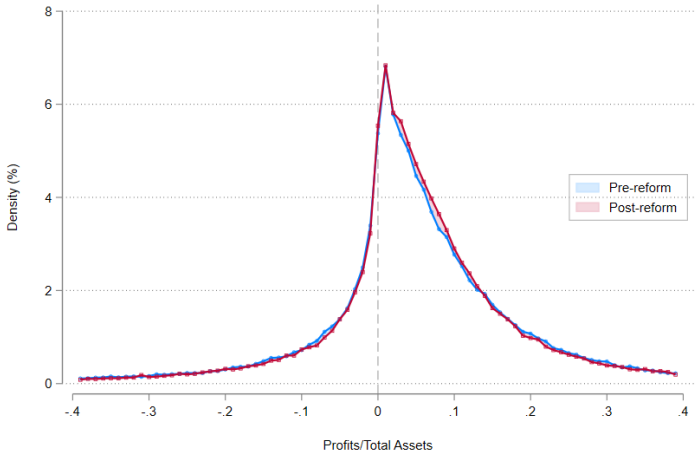
Notes: This figure shows the industry distribution of the estimation sample for treated, sister, and control firms. The vertical bars display the share of each firm type within each industry, and the numbers above the bars indicate the corresponding firm counts. "Wholesale" refers to wholesale and retail trade. "Services" include professional and administrative services. "Energy" combines electricity, gas, steam, and air-conditioning supply with mining and quarrying.

FIGURE F.II: **DISTRIBUTION OF ACCOUNTING PROFITS**

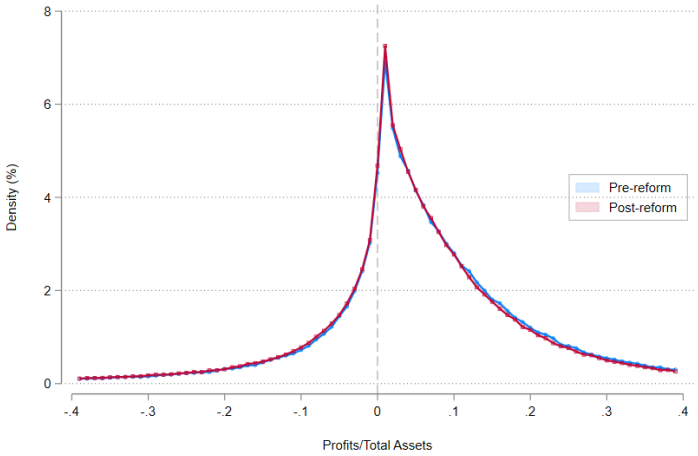
(A) **TREATED FIRMS**



(B) **SISTER FIRMS**



(C) **CONTROL FIRMS**

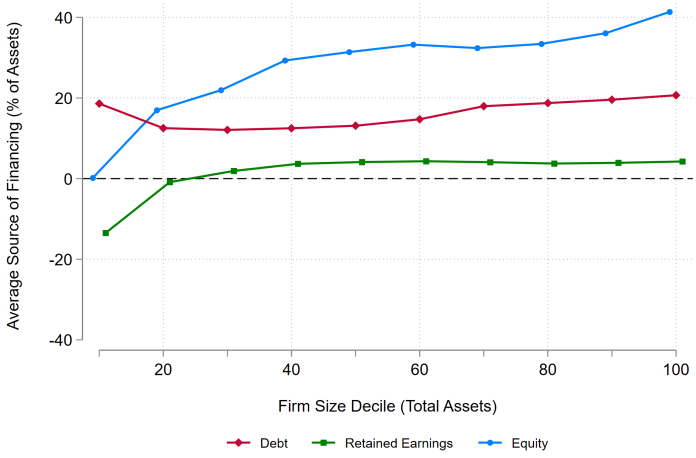


Notes: This figure plots the pre- and post-reform density distribution of the ratio of accounting profits to total assets for each of the three types of firms, i.e., treated firms, sister firms, and control firms. The ratio is restricted between -0.4 and 0.4 and divided into 79 bins. In all the figures, the sample is restricted to only the firms with non-missing positive total assets across all the periods.

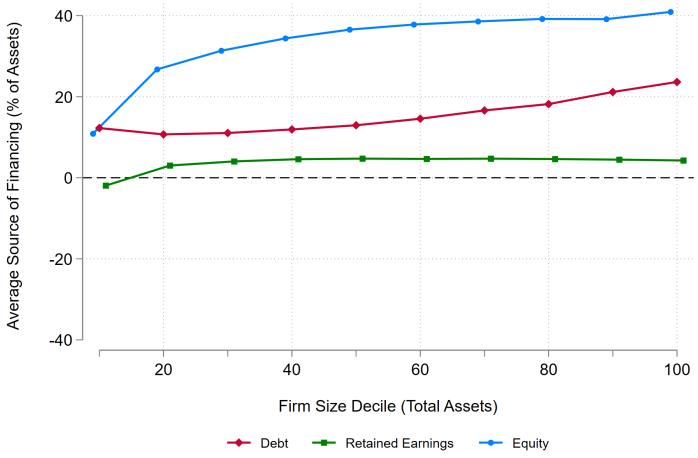
FIGURE F.IV: **CAPITAL STRUCTURE BY FIRM SIZE**

(A) **TREATED FIRMS**

(B) **SISTER FIRMS**



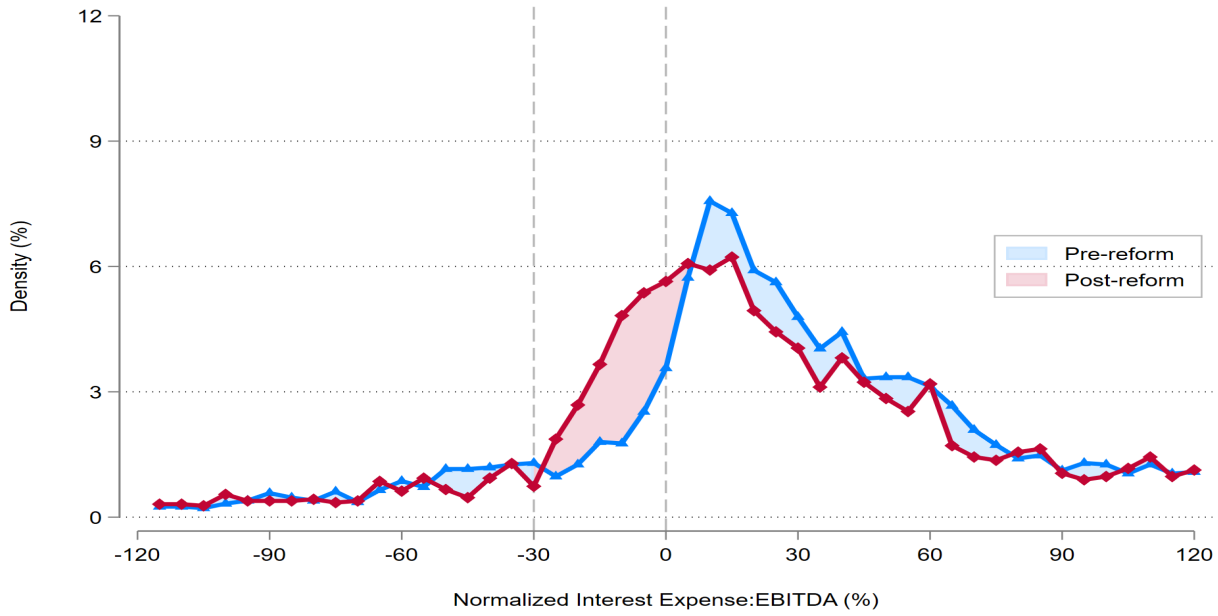
(C) **CONTROL FIRMS**



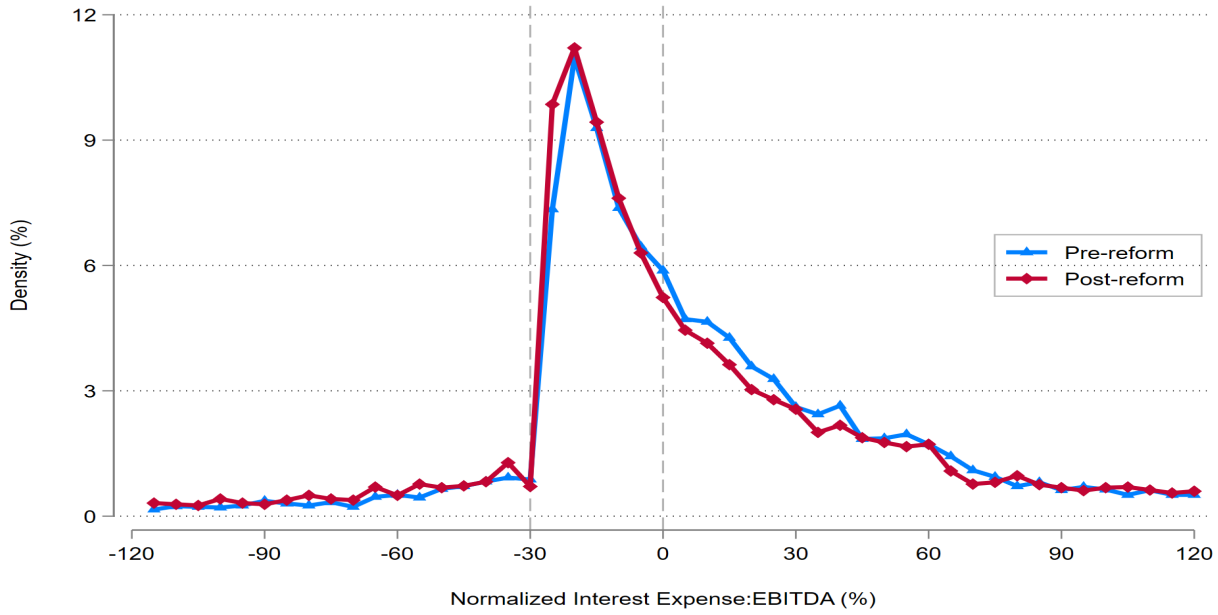
Notes: This figure plots average debt-to-assets, retained earnings-to-assets, and equity-to-assets ratios by firm size decile, where firm size is measured by total assets. All ratios are winsorized at the 5% level. Panel A reports the distributions for treated firms, Panel B for sister firms, and Panel C for control firms.

FIGURE F.III: **FIRM DENSITY AROUND ESR THRESHOLD**

(A) **TREATED FIRMS ONLY**



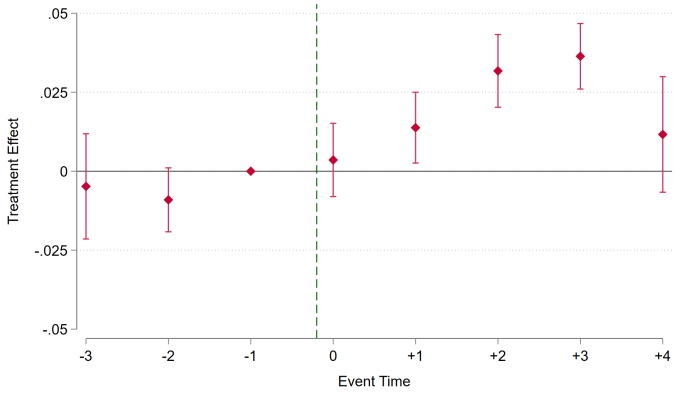
(B) **FULL SAMPLE**



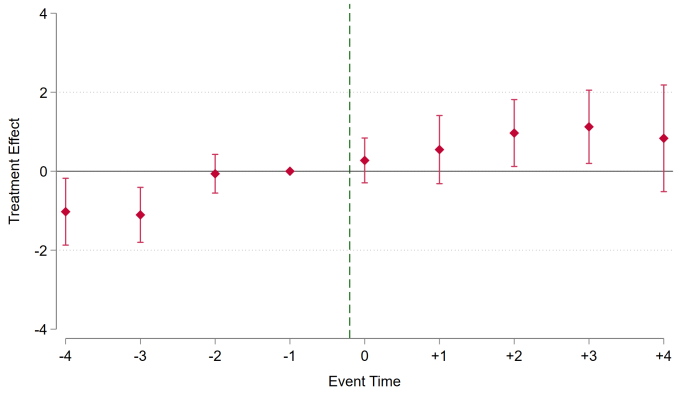
Notes: Panel A plots the pooled pre- and post-reform density of treated firms around the ESR threshold, normalized to zero across countries. For countries defining the rule using net interest to EBITDA, I use the net interest–to–EBITDA ratio; for those using gross interest to EBITDA, I use the gross 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) and thus would have been subject to ESR if their interest-to-EBITDA ratio exceeded the threshold. Panel B presents a placebo exercise and plots the same distributions 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. Since Slovakia did not introduce a de minimis threshold or escape clause, Slovak firms are excluded from both panels. The ratio is restricted between -120% and 120% and divided into 48 bins.

FIGURE F.V: **EFFECT OF ESR - SISTER AFFILIATES**

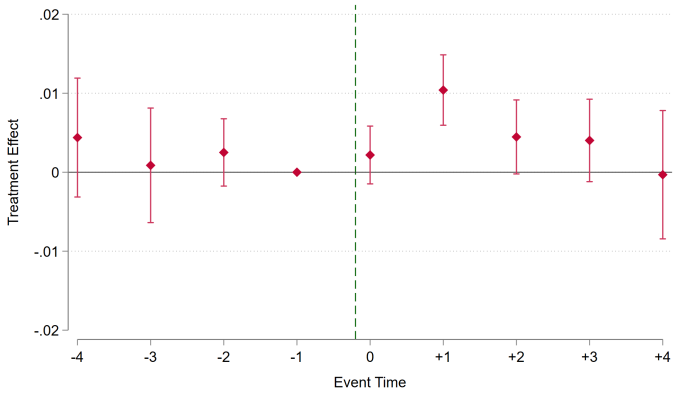
(A) **INVESTMENT**



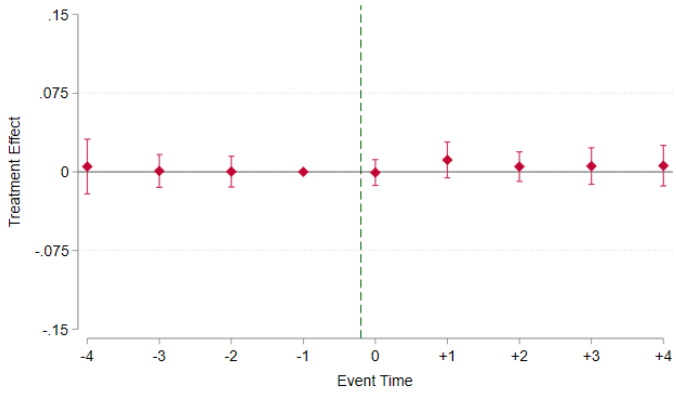
(B) **EMPLOYMENT**



(C) **TAXATION**

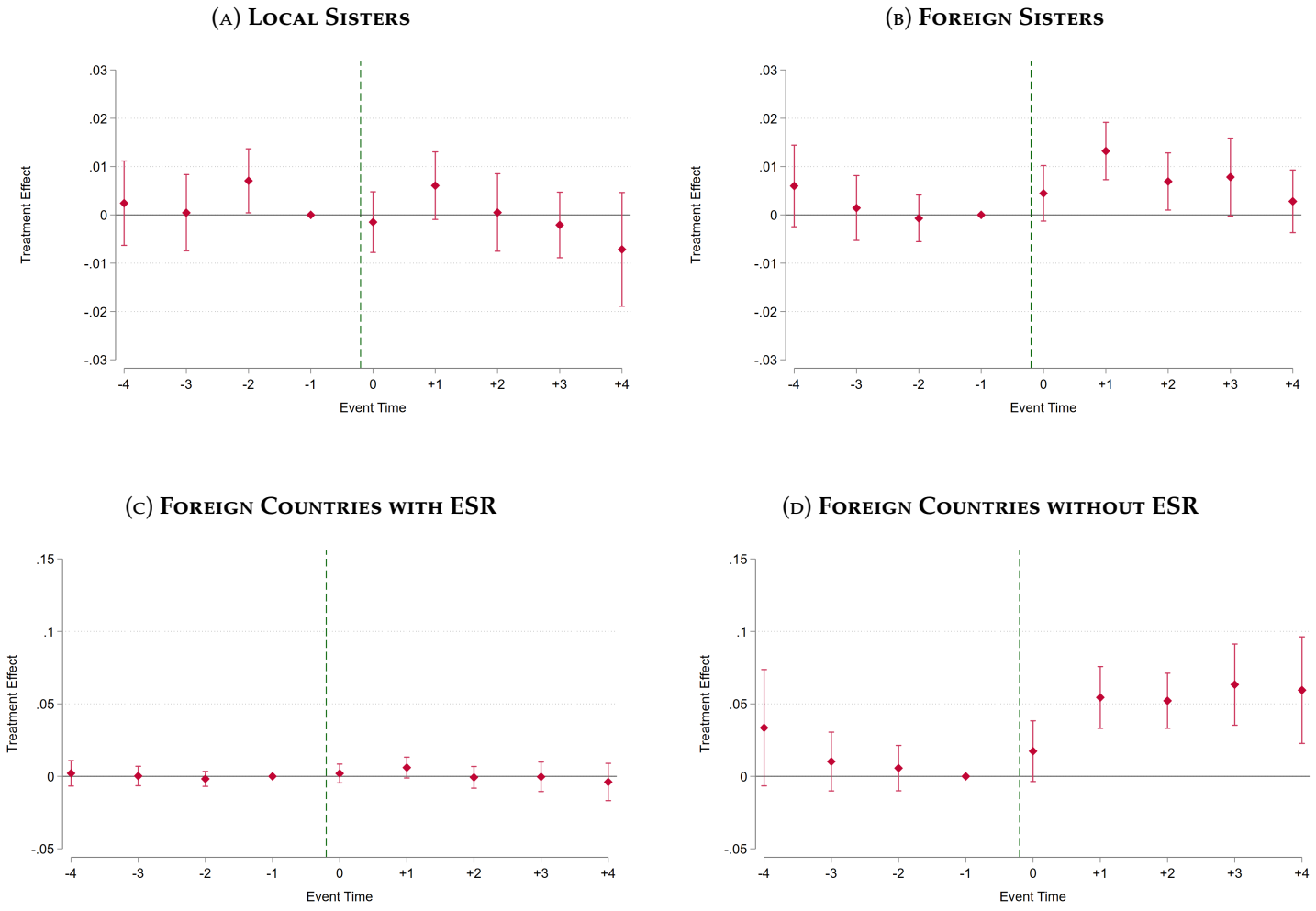


(D) **TAX:CAPITAL**



Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). Panel A reports the effect on investment defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Panel B reports the effect on employment, defined as the number of employees, and Panel C reports the effect on taxation measured in USD million. The outcomes of sister firms are compared with that of control firms; treated 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 with 20 repetitions 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.

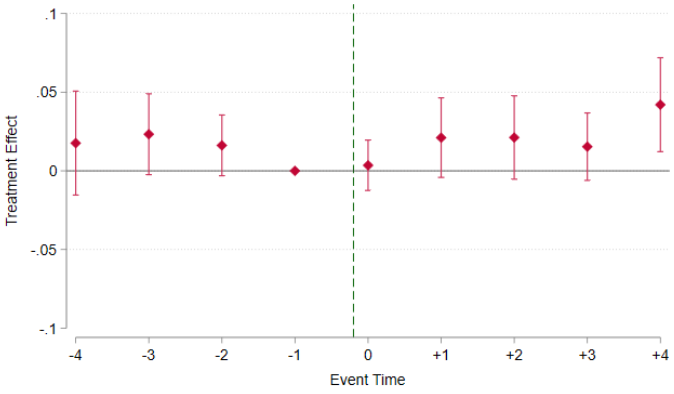
FIGURE F.VI: EFFECT ON TAXATION



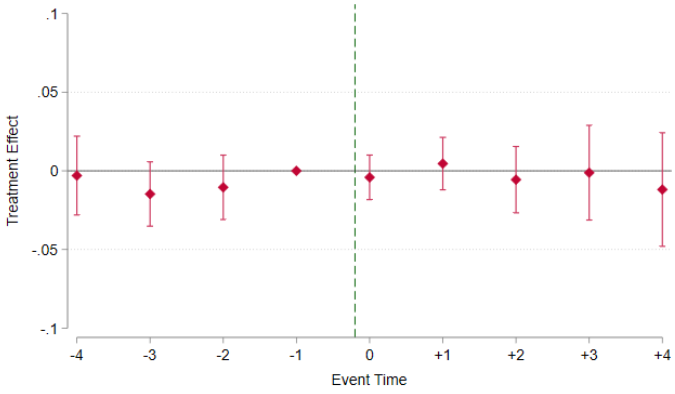
Notes: This figure reports dynamic firm-level treatment effects of ESR (in USD million) estimated using equation (9). The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report effects on local and foreign sister affiliates, 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 with 20 repetitions 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: **EFFECT ON TAX:CAPITAL**

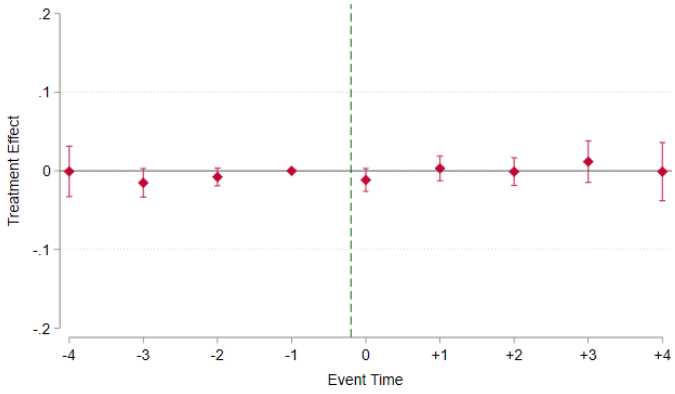
(A) **LOCAL SISTERS**



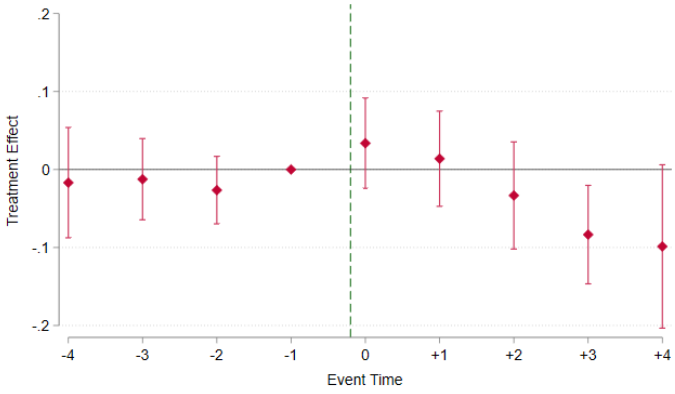
(B) **FOREIGN SISTERS**



(C) **FOREIGN COUNTRIES WITH ESR**



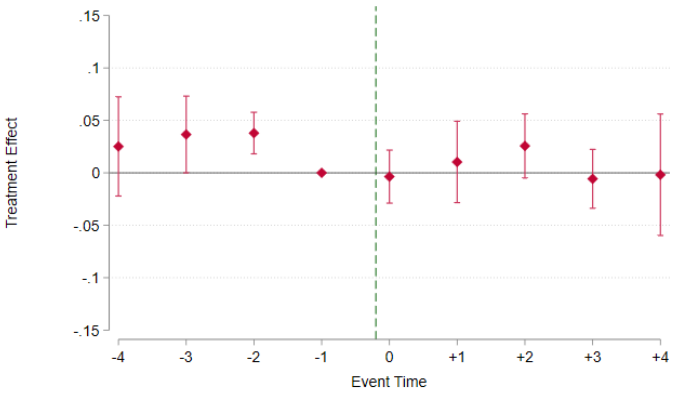
(D) **FOREIGN COUNTRIES WITHOUT ESR**



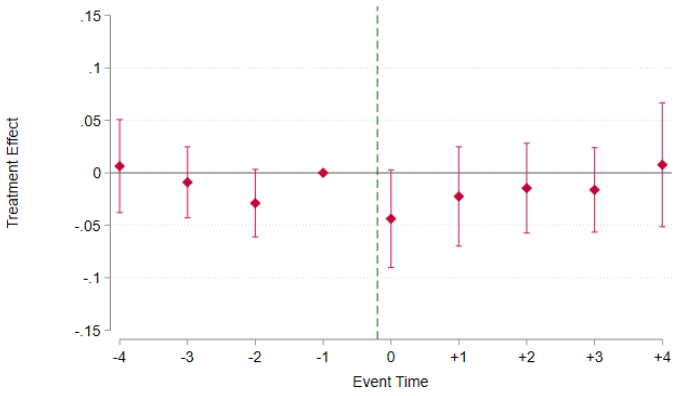
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report effects on local and foreign sister affiliates, respectively. Panels 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 with 20 repetitions 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.VIII: EFFECT ON FULLY COVERED MNES

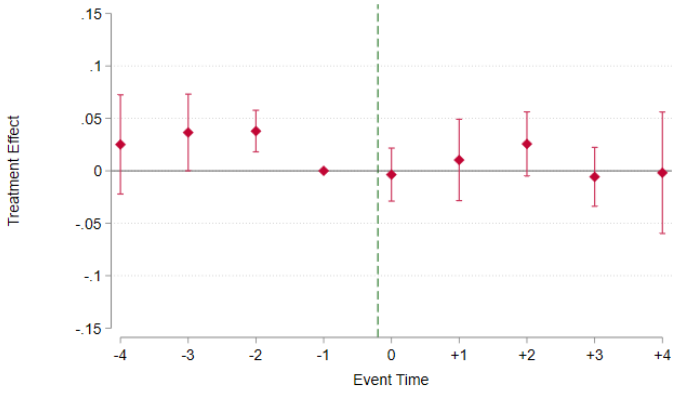
(A) TAXATION - LOCAL SISTERS



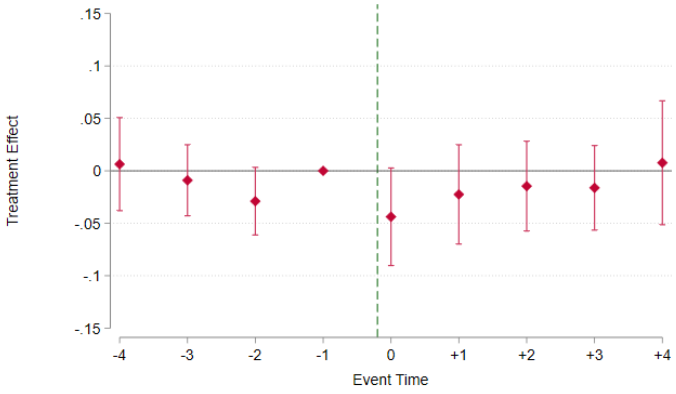
(B) TAXATION - FOREIGN SISTERS



(C) TAX:CAPITAL - LOCAL SISTERS



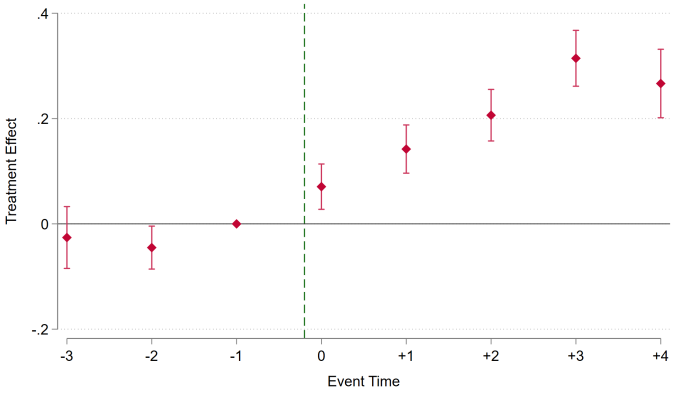
(D) TAX:CAPITAL - FOREIGN SISTERS



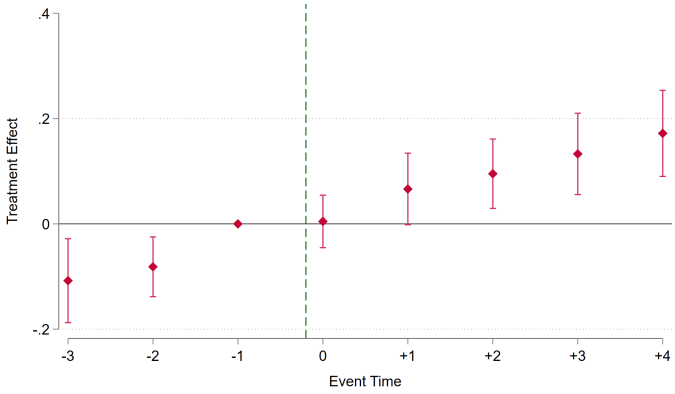
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using equation (9). The sample is restricted to sister firms of MNE groups in which all affiliates operated in jurisdictions that had implemented ESR ("Fully Covered" MNEs). The outcomes of such local and foreign sisters are compared against control firms. Taxation is measured 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 with 20 repetitions 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.IX: **FOREIGN COUNTRIES WITHOUT ESR**

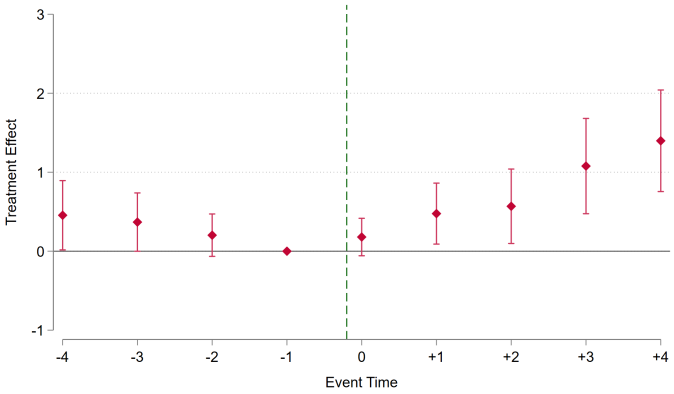
(A) **INVESTMENT - HIGH TAX**



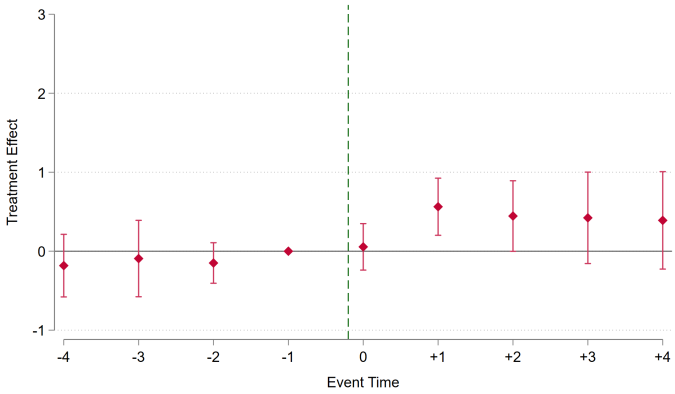
(B) **INVESTMENT - LOW TAX**



(C) **DEBT - HIGH TAX**



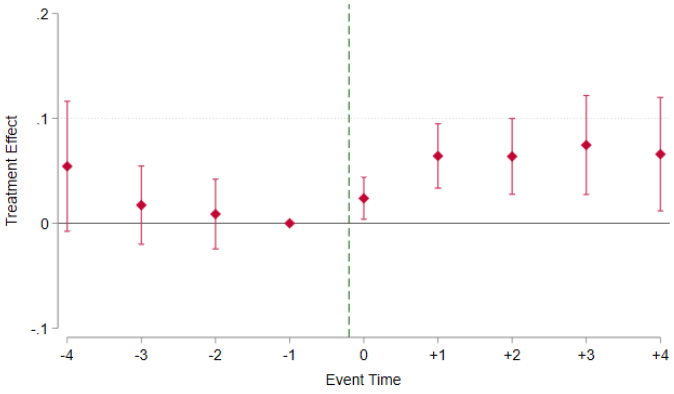
(D) **DEBT - LOW TAX**



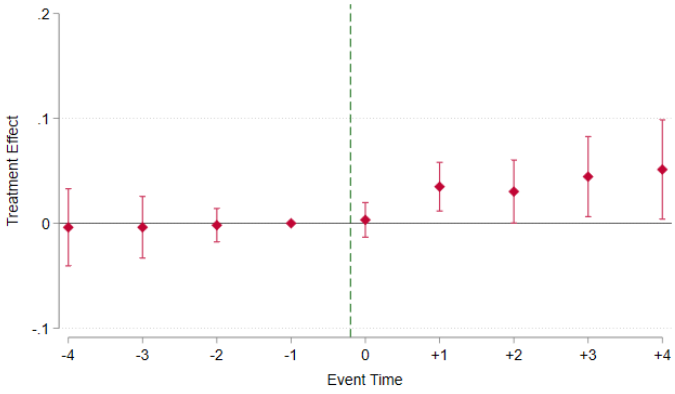
Notes: This figure presents dynamic firm-level treatment effects of ESR estimated using equation (9). The sample includes control firms and sister affiliates of treated firms located in jurisdictions that never adopted the policy. Sister affiliates are further classified as high-tax or low-tax based 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 over 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 with 20 repetitions 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.X: **FOREIGN COUNTRIES WITHOUT ESR**

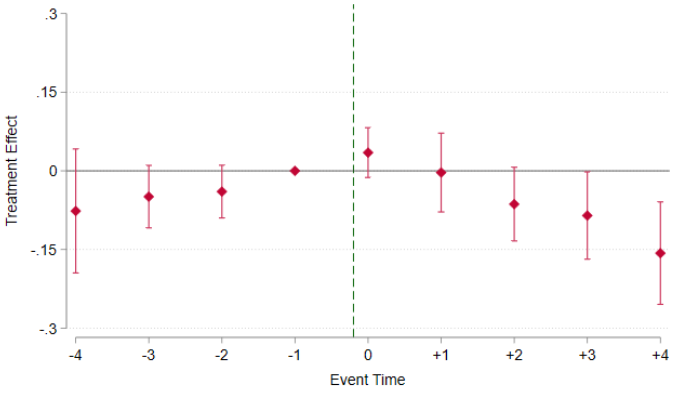
(A) **TAXATION - HIGH TAX**



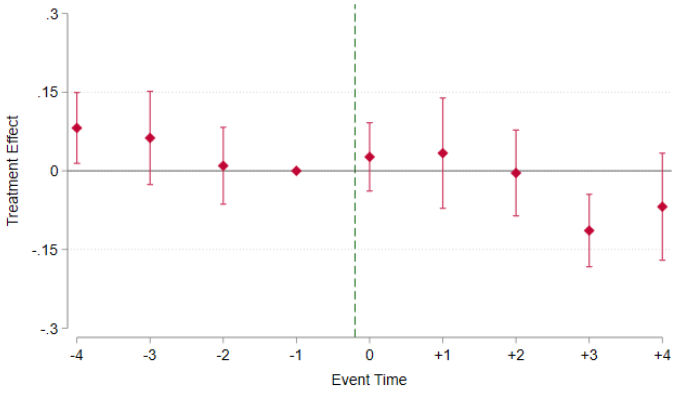
(B) **TAXATION - LOW TAX**



(C) **TAX:CAPITAL - HIGH TAX**



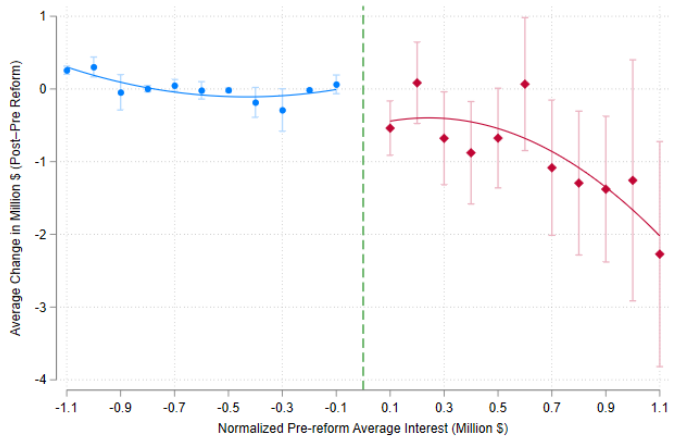
(D) **TAX:CAPITAL - LOW TAX**



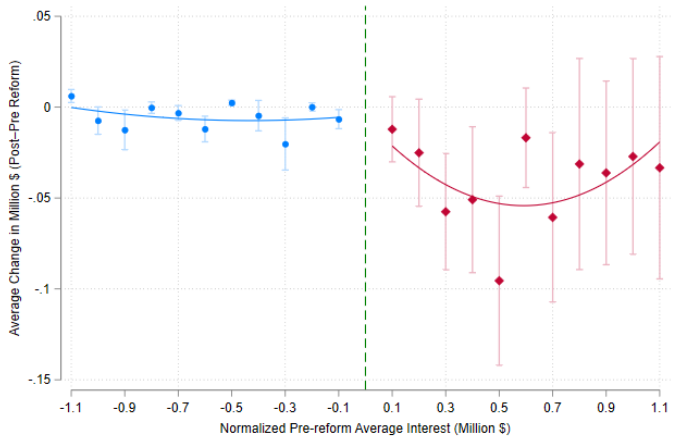
Notes: This figure presents dynamic firm-level treatment effects of ESR estimated using equation (9). The sample includes control firms and sister affiliates of treated firms located in jurisdictions that never adopted the policy. Sister affiliates are further classified as high-tax or low-tax based on whether the average corporate tax rate in their jurisdiction is above or below the global minimum rate of 15%. Taxation is measured 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 with 20 repetitions 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.XI: **REGRESSION DISCONTINUITY - TAX AVOIDANCE**

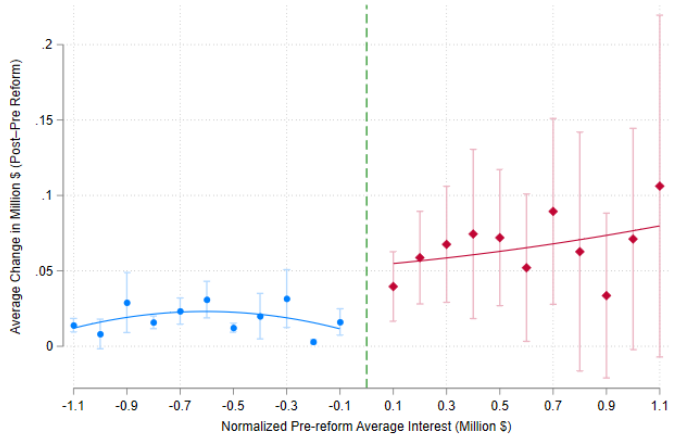
(A) **LEVEL OF DEBT**



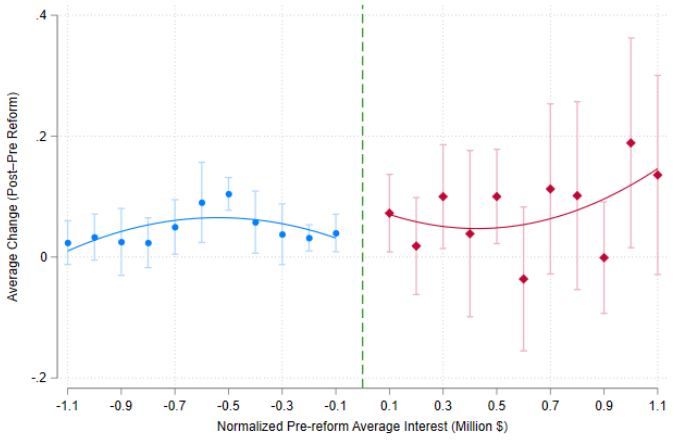
(B) **INTEREST EXPENSE**



(C) **TAXATION**



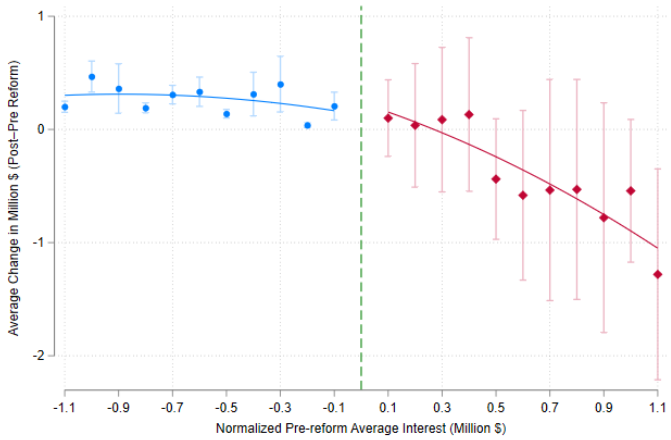
(D) **TAX:CAPITAL**



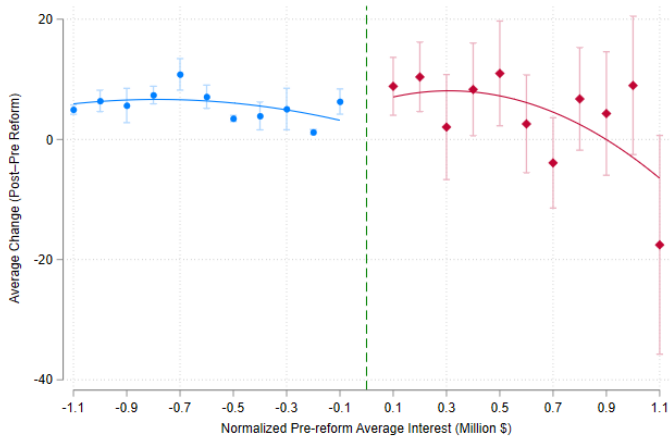
Notes: This figure plots the average difference in the mean pre- and post-reform outcomes around the de-minimis cutoff. The running variable is the difference between pre-reform average interest expenses and the country-specific de-minimis threshold. Since this threshold varies across countries, I normalize it across countries to 0. 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%. Additionally, I fit quadratic polynomials separately on each side of the policy cutoff.

FIGURE F.XII: **REGRESSION DISCONTINUITY - REAL EFFECTS**

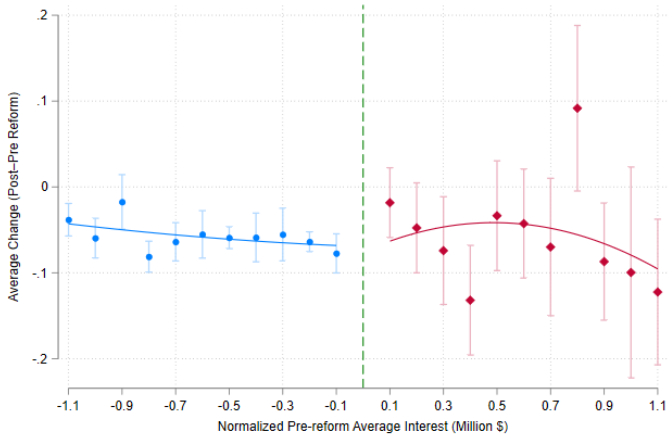
(A) **CAPITAL**



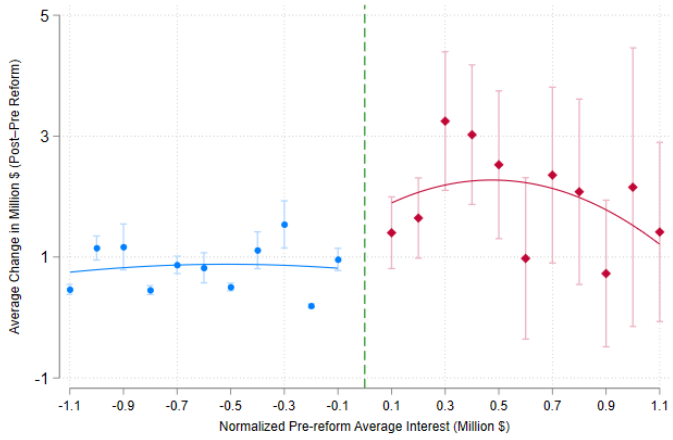
(B) **EMPLOYMENT**



(C) **INVESTMENT**

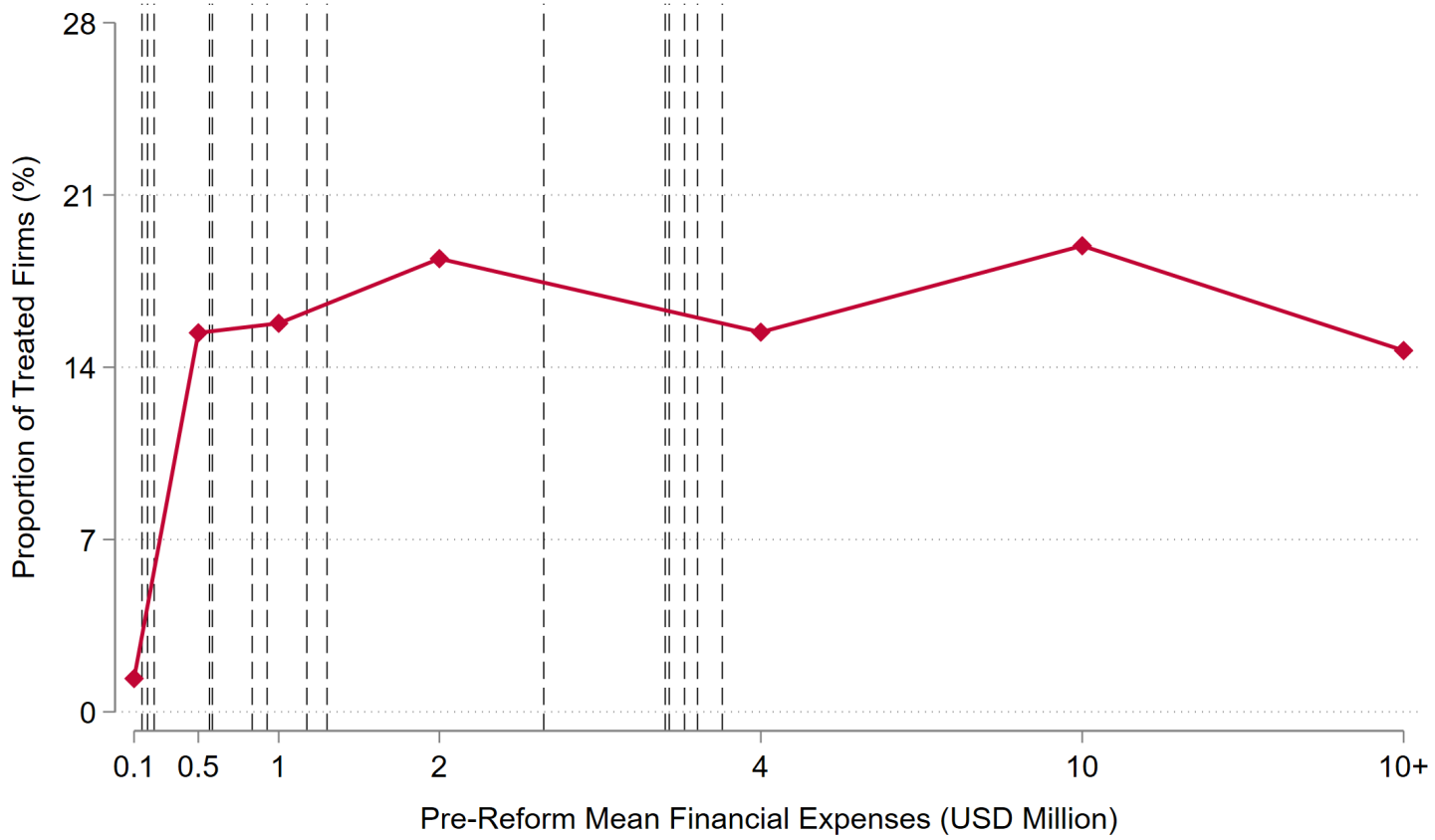


(D) **EQUITY**



Notes: This figure plots the average difference in the mean pre- and post-reform outcomes around the de-minimis cutoff. The running variable is the difference between pre-reform average interest expenses and the country-specific de-minimis threshold. Since this threshold varies across countries, I normalize it across countries to 0. 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%. Additionally, I fit quadratic polynomials separately on each side of the policy cutoff.

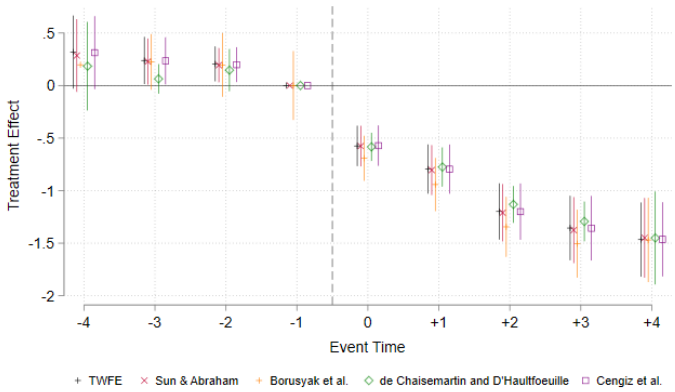
FIGURE F.XIII: **DISTRIBUTION OF PRE-REFORM AVERAGE INTEREST EXPENSE**



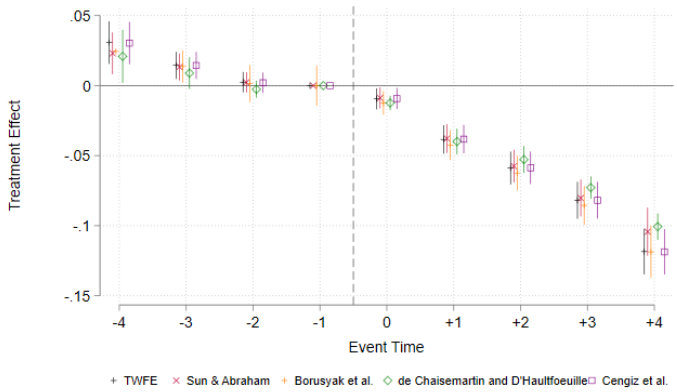
Notes: This figure shows the distribution of pre-reform average interest expenses for treated firms. For countries where ESR applies to net interest expenses, the figure reports net values; for countries applying the rule to gross interest expenses, it reports gross values. All amounts are expressed in USD millions. The black dashed lines indicate country-specific de minimis thresholds. Averages are computed over the three pre-reform years. Slovak firms are excluded, as Slovakia's ESR did not include a de minimis threshold.

FIGURE F.XIV: **EFFECTS OF ESR - TREATED FIRMS**

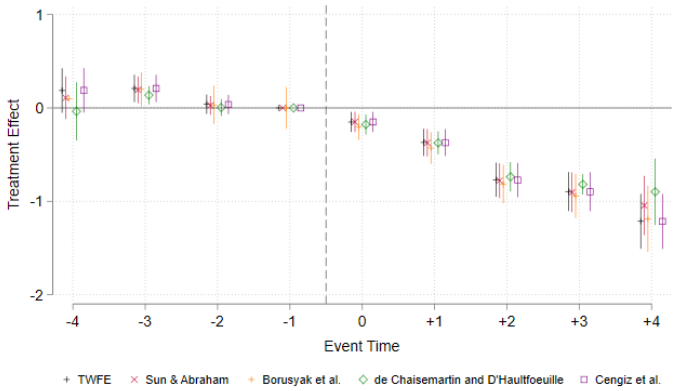
(A) **LEVEL OF DEBT**



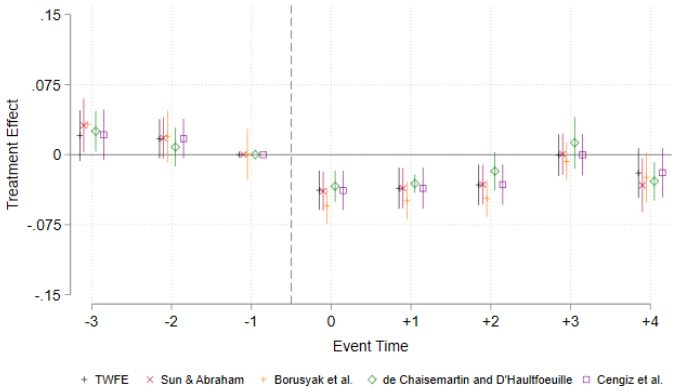
(B) **INTEREST EXPENSE**



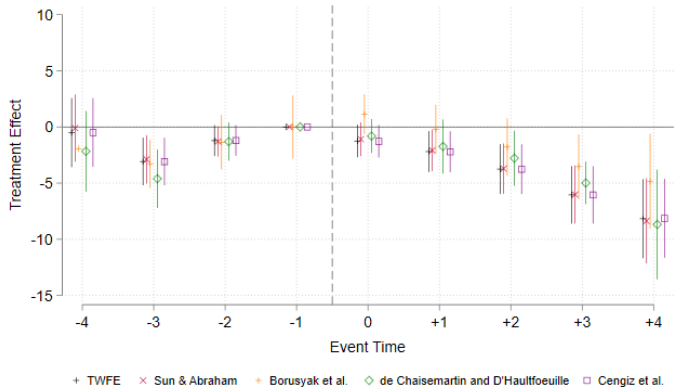
(C) **CAPITAL**



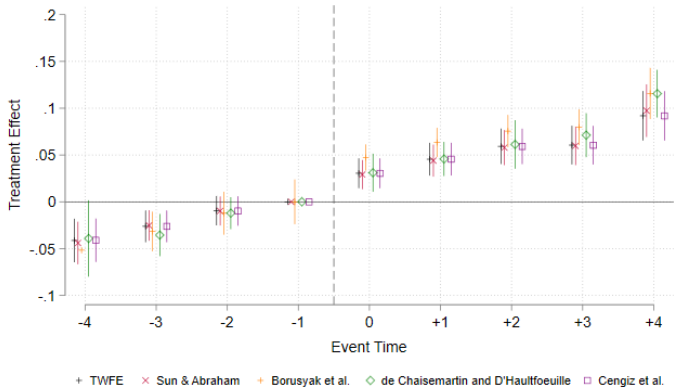
(D) **INVESTMENT**



(E) **EMPLOYMENT**



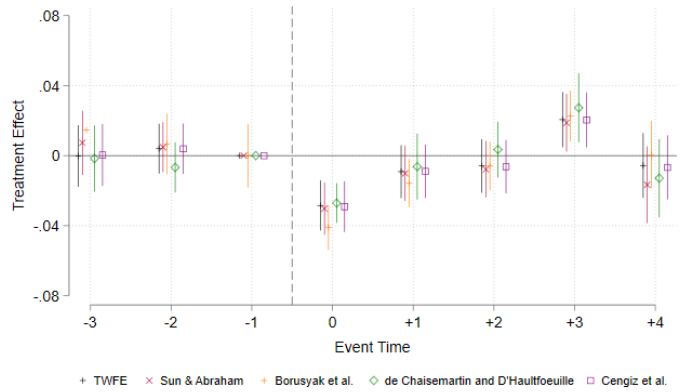
(F) **TAXATION**



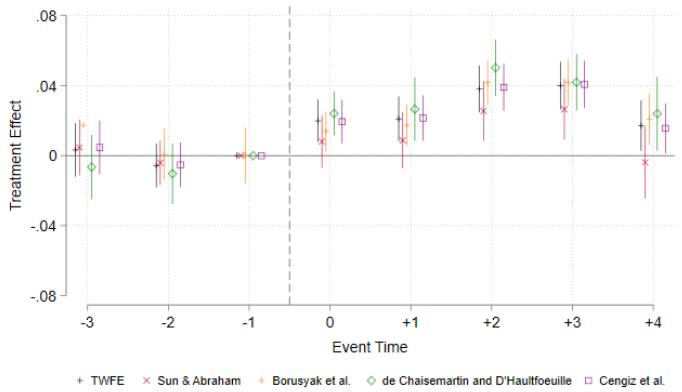
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using different DiD specifications. Panels A, B, C, and F present outcomes in USD millions. Panel D shows the investment rate, defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Panel E reports effects on employment, measured as the number of employees. The outcomes of treated firms are compared with that of control firms; sister firms are dropped from the analysis. All specifications include include firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. 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.

FIGURE F.XV: **REALLOCATION OF INVESTMENTS**

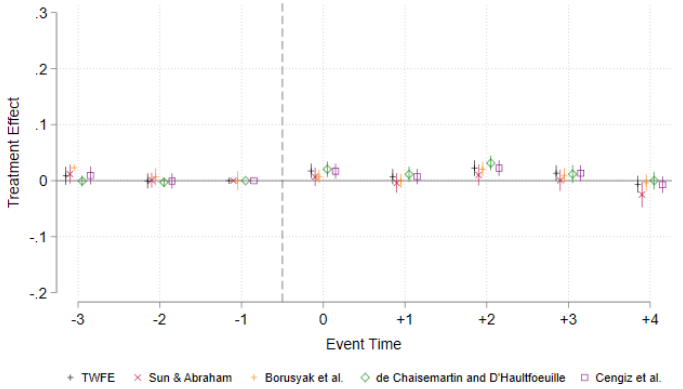
(A) **LOCAL SISTERS**



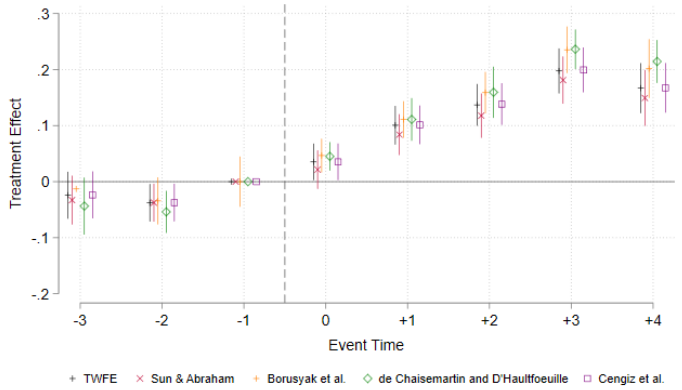
(B) **FOREIGN SISTERS**



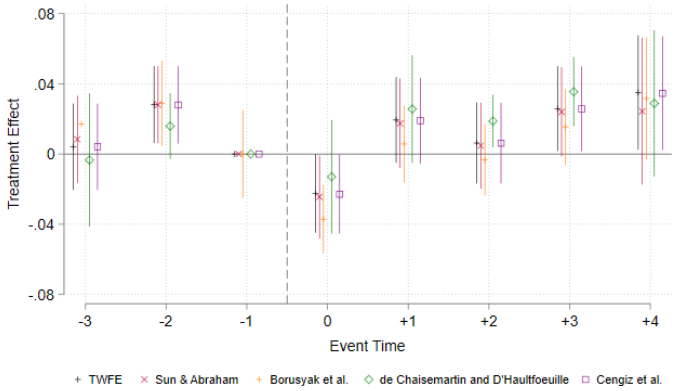
(C) **FOREIGN COUNTRIES WITH ESR**



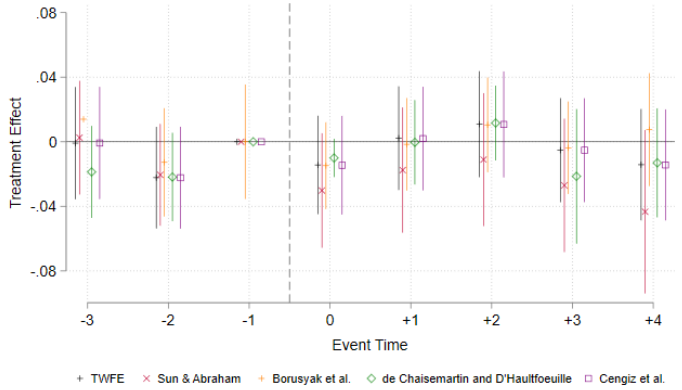
(D) **FOREIGN COUNTRIES WITHOUT ESR**



(E) **LOCAL SISTERS OF FULLY COVERED MNES**



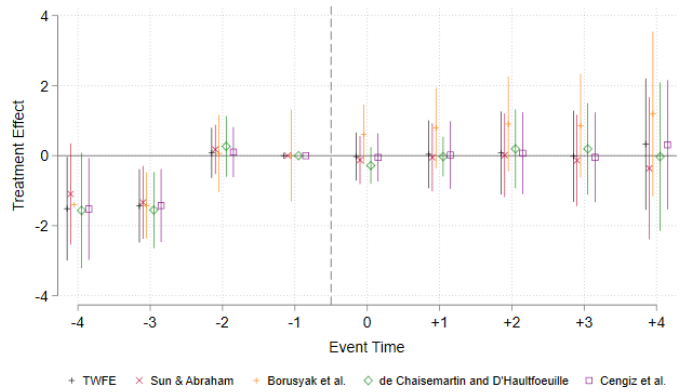
(F) **FOREIGN SISTERS OF FULLY COVERED MNES**



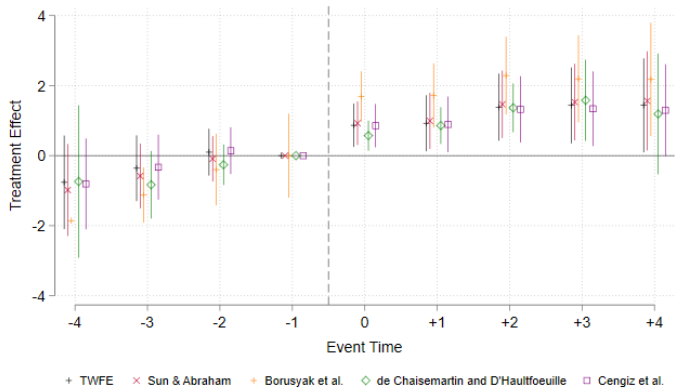
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using different DiD specifications. Investments are defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. The outcomes of sister firms are compared 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. Panels E and F further restrict the sample to MNE groups in which all affiliates operated in jurisdictions that had implemented ESR ("Fully Covered" MNEs), with effects shown for local and foreign sisters, respectively. All specifications include firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. 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.

Figure F.XVI: Effect on Employment

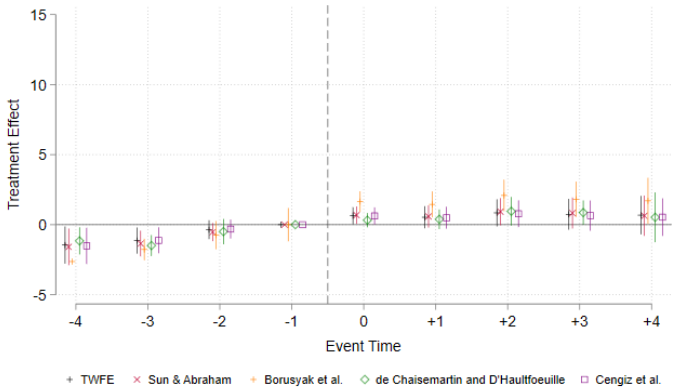
(A) Local Sisters



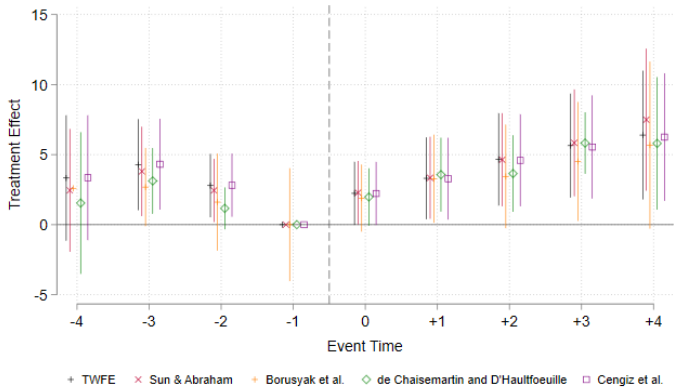
(B) Foreign Sisters



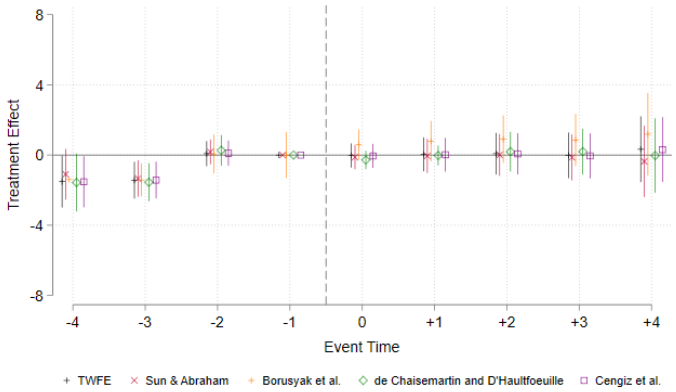
(C) Foreign Countries with ESR



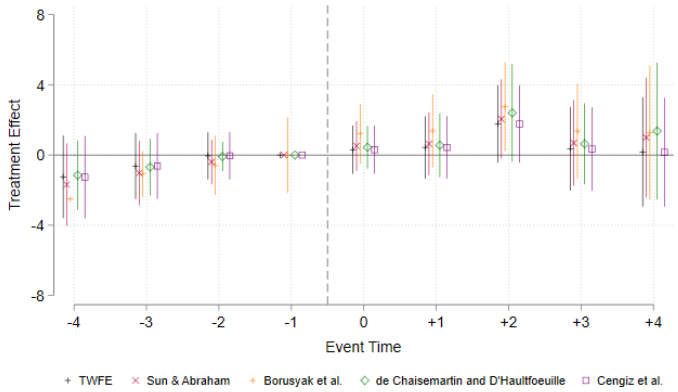
(D) Foreign Countries without ESR



(E) Local Sisters of Fully Covered MNEs



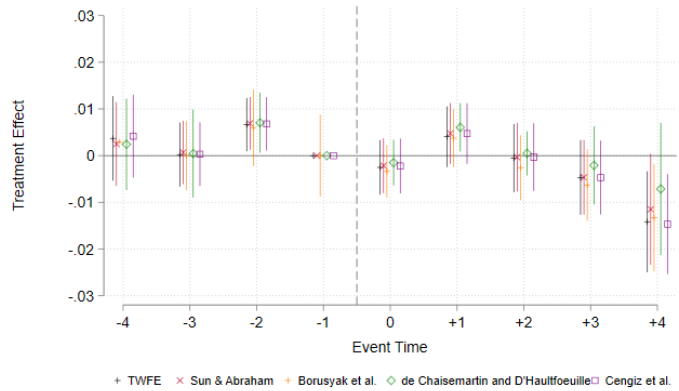
(F) Foreign Sisters of Fully Covered MNEs



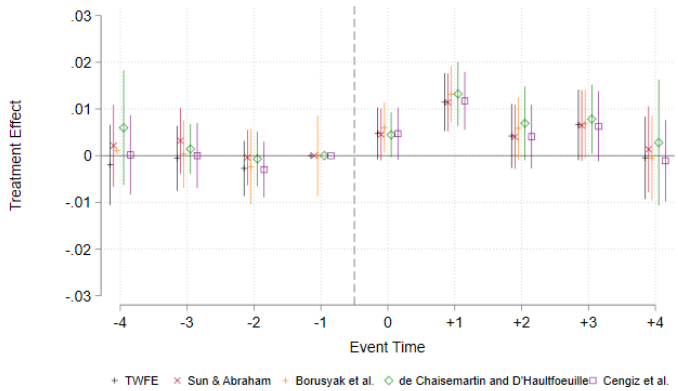
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using different DiD specifications. Employment is defined as the number of employees. The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report 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. Panels E and F further restrict the sample to MNE groups in which all affiliates operated in jurisdictions that had implemented ESR ("Fully Covered" MNEs), with effects shown for local and foreign sisters, respectively. All specifications include firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. 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.

FIGURE F.XVII: **EFFECT ON TAXATION**

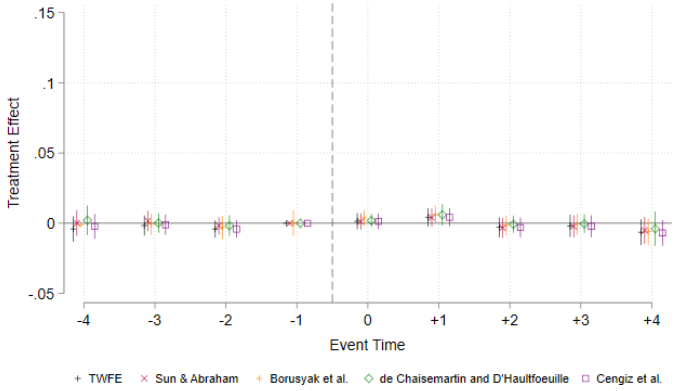
(A) **LOCAL SISTERS**



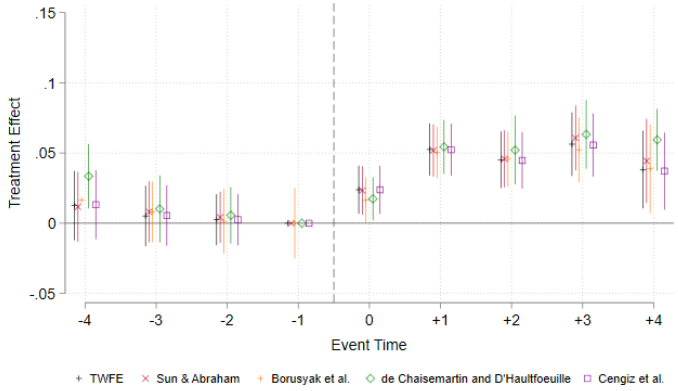
(B) **FOREIGN SISTERS**



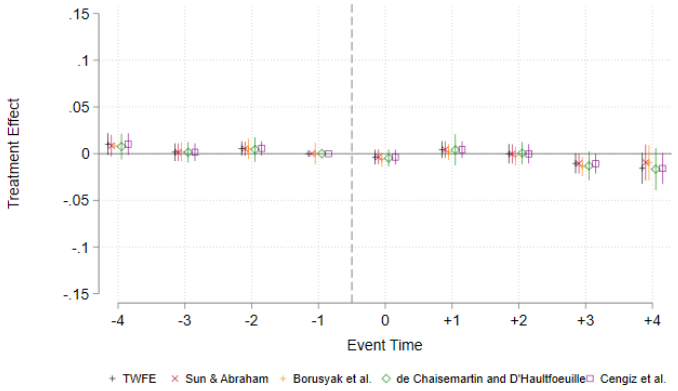
(C) **FOREIGN COUNTRIES WITH ESR**



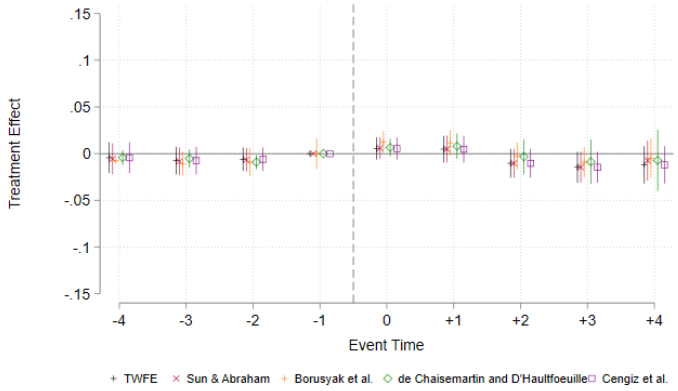
(D) **FOREIGN COUNTRIES WITHOUT ESR**



(E) **LOCAL SISTERS OF FULLY COVERED MNES**



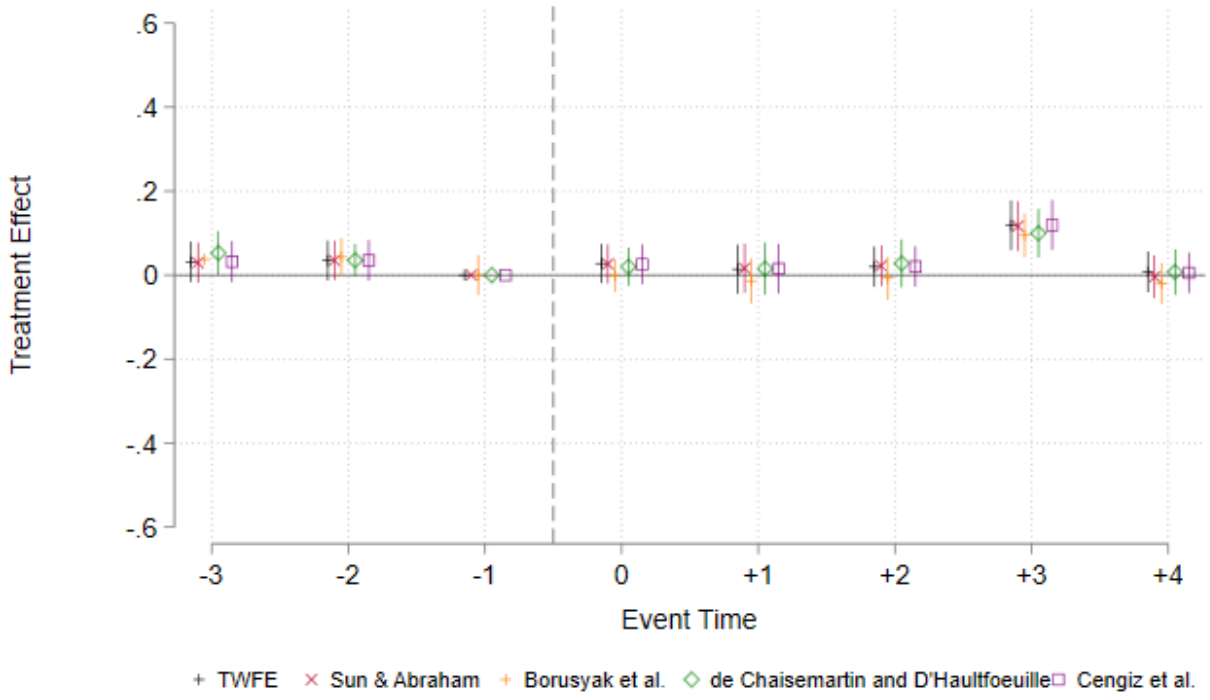
(F) **FOREIGN SISTERS OF FULLY COVERED MNES**



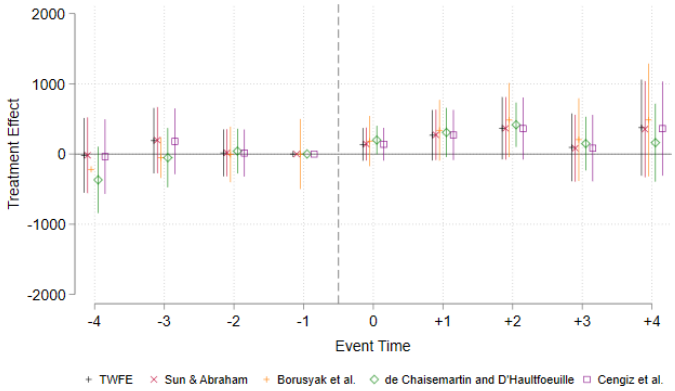
Notes: This figure reports dynamic firm-level treatment effects of ESR (in USD million) estimated using different DiD specifications. The outcomes of sister firms are compared with that of control group; treated firms are dropped from the analysis. Panels A and B report effects on local and foreign sister affiliates, respectively. Panels C and D restrict the sample to foreign sisters whose home country did and did not introduce ESR, respectively. Panels E and F further restrict the sample to MNE groups in which all affiliates operated in jurisdictions that had implemented ESR ("Fully Covered" MNEs), with effects shown for local and foreign sisters, respectively.All specifications include firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. 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.

FIGURE F.XVIII: **EFFECT OF ESR - GROUP LEVEL**

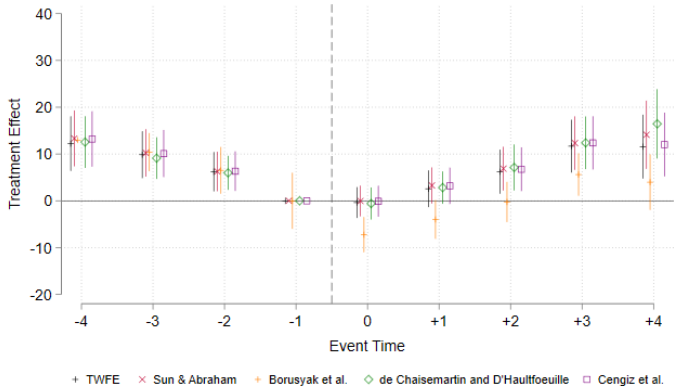
(A) **INVESTMENT**



(B) **EMPLOYMENT**



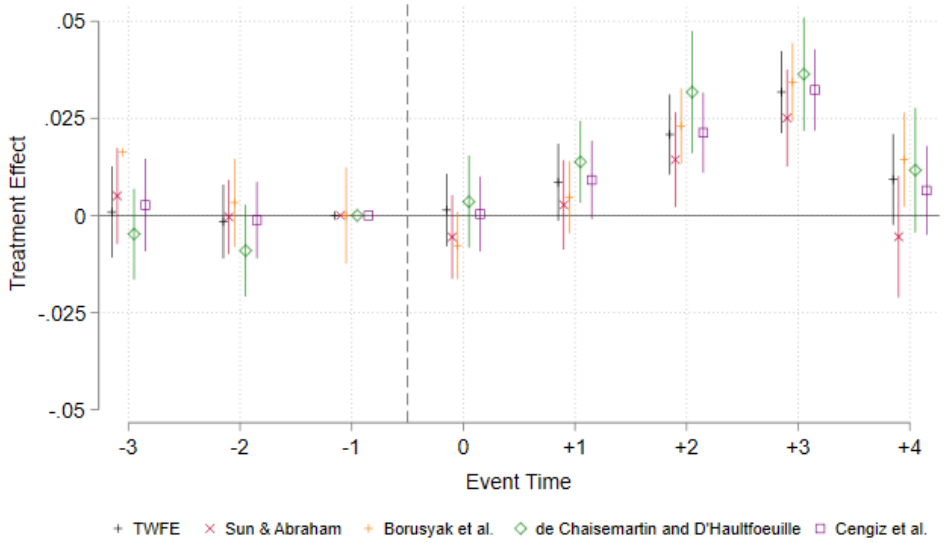
(C) **TAX**



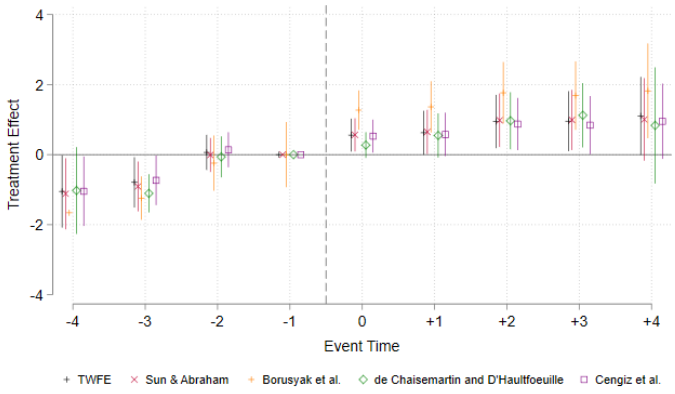
Notes: This figure reports dynamic group-level treatment effects of ESR estimated using different DiD specifications. Panel A reports the effect on investment defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Panel B reports the effect on employment, defined as the number of employees, and Panel C reports the effect on taxation measured in USD million. All specifications include group fixed effects, year fixed effects, and time-varying jurisdiction-level controls. 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.

FIGURE F.XIX: **EFFECT OF ESR - SISTER AFFILIATES**

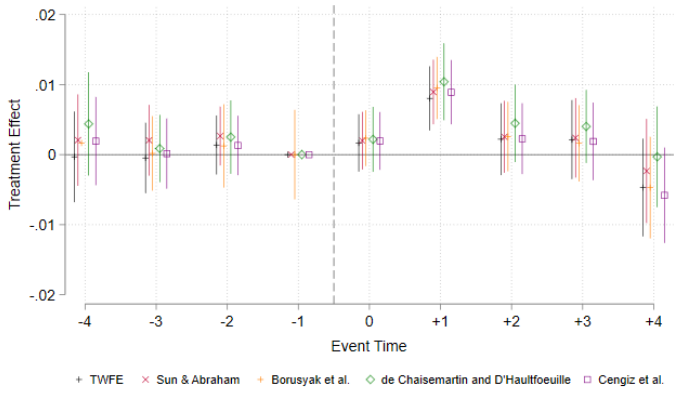
(A) **INVESTMENT**



(B) **EMPLOYMENT**



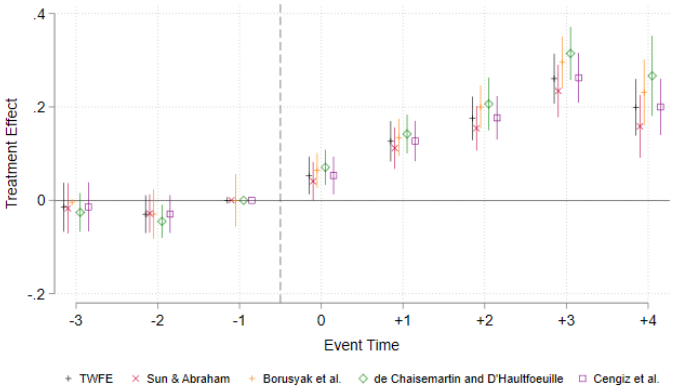
(C) **TAXATION**



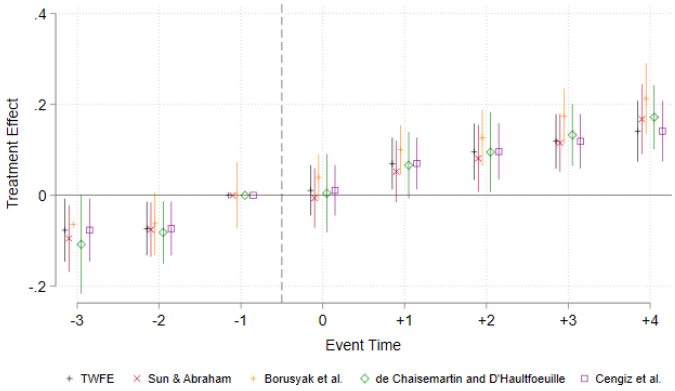
Notes: This figure reports dynamic firm-level treatment effects of ESR estimated using different DiD specifications. Panel A reports the effect on investment defined as gross investment over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Panel B reports the effect on employment, defined as the number of employees, and Panel C reports the effect on taxation measured in USD million. The outcomes of sister firms are compared with that of control firms; treated firms are dropped from the analysis. All specifications include firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. 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.

FIGURE F.XX: **FOREIGN COUNTRIES WITHOUT ESR**

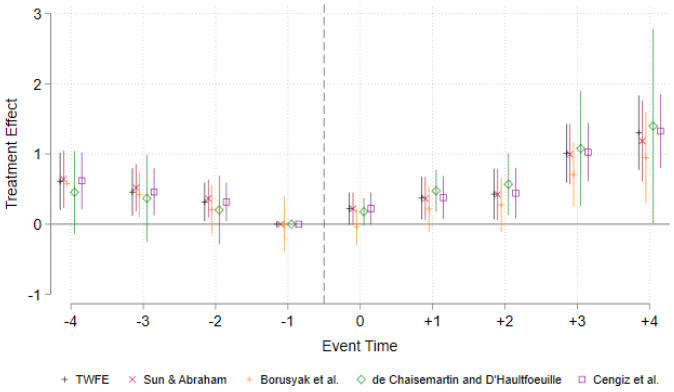
(A) **INVESTMENT - HIGH TAX**



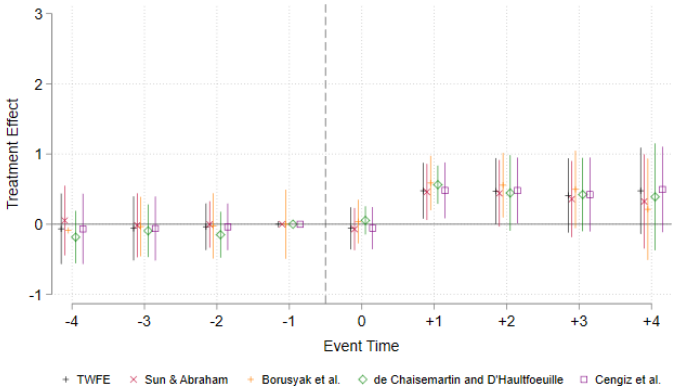
(B) **INVESTMENT - LOW TAX**



(C) **DEBT - HIGH TAX**



(D) **DEBT - LOW TAX**



Notes: This figure presents dynamic firm-level treatment effects of ESR estimated using different DiD specifications. 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 over lagged capital stock; because of the lag structure, estimates are reported from $t^* - 3$ onward. Debt is reported in USD million. All specifications include firm fixed effects, year fixed effects, and time-varying jurisdiction-level controls. 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.

G Additional Tables

TABLE VII: **COUNTRY STATISTICS**

Country (1)	Firms (2)	Groups (3)	Available Financials (4)	Country (1)	Firms (2)	Groups (3)	Available Financials (4)
Austria	1.9	2.4	36.3	*South Korea*	1.0	1.6	68.9
Australia	3.4	4.0	58.8	*Sri Lanka*	0.0	0.0	66.7
Bosnia	0.8	1.6	9.2	*Luxembourg*	0.2	0.5	16.5
Belgium	5.8	6.6	33.7	*Latvia*	2.2	4.4	9.0
Bulgaria	2.6	4.5	13.3	*Morocco*	0.8	1.2	45.0
Brazil	0.4	0.6	52.6	*Montenegro*	0.3	0.6	10.4
Czech Republic	5.9	9.5	15.7	*Macedonia*	0.7	1.4	4.3
Algeria	0.1	0.2	53.3	*Netherlands*	0.3	0.6	46.4
Spain	1.1	2.0	29.7	*Norway*	0.0	0.0	33.3
Finland	1.5	2.2	45.8	*New Zealand*	0.8	1.7	41.4
France	21.5	18.7	18.9	*Philippines*	0.9	1.2	67.2
United Kingdom	5.1	6.3	45.4	*Poland*	5.1	8.4	27.2
Georgia	0.1	0.1	20.3	*Portugal*	4.3	5.3	24.6
Greece	0.5	0.7	47.2	*Romania*	8.0	16.3	7.7
Croatia	1.7	3.4	14.1	*Serbia*	5.0	9.5	6.9
Hungary	2.4	4.2	23.1	*Sweden*	5.1	5.1	43.7
Ireland	3.0	3.3	38.1	*Slovenia*	1.5	2.4	16.1
India	0.3	0.5	51.8	*Slovakia*	0.7	1.5	2.1
Iceland	0.1	0.1	43.0	*Ukraine*	4.9	5.6	7.1
Japan	0.2	0.3	78.6				
Total Entities	172549	73038	8584	*Total Entities*	172549	73038	8584

Notes: This table reports country coverage for the estimation sample. In total, the sample comprises 172,549 firms belonging to 73,038 multinational groups, of which consolidated financial statements are available for 8,584 groups. Column 1 lists jurisdictions. Column 2 reports the share of firms in the sample located in each jurisdiction. Column 3 shows the share of MNE groups with at least one affiliate in that jurisdiction. Column 4 reports the share of those groups in Column 3 for which consolidated financial statements are observed. While the firm shares in Column 2 sum to one hundred, the shares in Columns 3 and 4 do not, as MNE groups typically operate in multiple jurisdictions.

TABLE VIII: **EFFECT OF ESR - GROUP LEVEL**

Outcomes (y_i):	Investment	Employment	Taxation	Tax:Capital
	(1)	(2)	(3)	(4)
$Failed_i \times Post_t$	0.04*	253.91	6.88***	34.89***
	(1.79)	(1.56)	(3.39)	(5.43)
Pre-Reform Mean	0.13	18992.46	73.39	206.27
Percentage Change	26.87%	1.34%	9.38%	16.91%
Observations	61,454	98,303	127,941	126,805
Switchers	1,785	2,560	3,599	3,464

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR at the group level. *Investment* is defined as the ratio of gross investment spending scaled by lagged fixed assets, *Employment* as the number of employees and *Taxation* is measured in USD million. Pre-Reform Mean is the sample mean of the treated groups in pre-reform periods. All specifications include group fixed effects, year fixed effects, and time-varying jurisdiction-level controls. The standard errors are robust, bootstrapped with 200 repetitions, and clustered at the group level. Observations is the number of observations used in the estimation, and Switchers is number of firms for which treatment status changed from 0 to 1.

TABLE IX: **SISTER AFFILIATES IN FOREIGN COUNTRIES WITHOUT ESR**

Outcomes (y_i):	Investment		Employment		Taxation		Tax:Capital	
	Low-Tax	High-Tax	Low-Tax	High-Tax	Low-Tax	High-Tax	Low-Tax	High-Tax
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Failed_i \times Post_t$	0.08***	0.18***	6.14***	2.32	0.03**	0.05***	-0.02	-0.03
	(4.02)	(12.64)	(2.74)	(1.40)	(2.38)	(4.05)	(0.56)	(1.20)
Pre-Reform Mean	0.19	0.16	105.13	167.49	0.24	0.52	0.47	0.92
Percentage Change	43.56%	113.77%	5.84%	1.38%	12.71%	10.54%	-3.69%	-3.61%
Observations	1,385,522	1,394,639	1,179,352	1,144,934	1,565,228	1,571,334	1,467,300	1,473,205
Switchers	2,760	4,952	2,990	3,073	3,313	5,846	3,313	5,844

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR on investments (measured as gross investment scaled by lagged fixed assets), employment (measured as number of employees) and taxation (measured in USD million) in 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. 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 bootstrapped with 20 repetitions, clustered at the firm level.

TABLE X: **REGRESSION DISCONTINUITY - TAX AVOIDANCE**

Outcomes (y_i):	Debt	Interest	Taxation	Tax:Capital
	(1)	(2)	(3)	(4)
β	-0.044	-0.026***	0.042***	-0.002
	(0.48)	(5.47)	(6.58)	(0.05)
γ^a	-0.984***	-0.010	0.035***	0.033
	(5.40)	(1.09)	(2.67)	(0.37)
Observations	33,891	28,199	37,621	36,541

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports regression discontinuity estimates of firm-level outcomes using a bandwidth of USD 1.1 million around the policy threshold. 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. Linear polynomials in the running variable are fitted separately on each side of the threshold. *Debt*, *Interest*, and *Taxation* are measured in USD million. β denotes the discontinuity estimate, γ the slope coefficient of the fitted curve above the cutoff, and "Observations" the number of firms included in the estimation.

TABLE XI: **REGRESSION DISCONTINUITY - REAL EFFECT**

Outcomes (y_i):	Capital	Employment	Investment	Equity
	(1)	(2)	(3)	(4)
β	0.173**	7.996***	0.030	1.619***
	(2.11)	(6.03)	(1.33)	(12.06)
γ^a	-0.862***	-5.781**	-0.006	0.060
	(5.29)	(2.12)	(0.14)	(0.22)
Observations	40,424	30,035	36,961	40,637

t-value in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports regression discontinuity estimates of firm-level outcomes using a bandwidth of USD 1.1 million around the policy threshold. 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. Linear polynomials in the running variable are fitted separately on each side of the threshold. *Capital* and *Equity* are measured in USD million, *Investment* as the ratio of gross investment spending scaled by lagged fixed assets, and *Employment* as the number of employees. β denotes the discontinuity estimate, γ the slope coefficient of the fitted curve above the cutoff, and "Observations" the number of firms included in the estimation.