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 Rules (ESR)—an anti-avoidance measure adopted by over 45 jurisdictions between 2015 and 2023—that limit profit shifting through debt channels but increase 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 reduce profit shifting and tax avoidance but lower investments in affected subsidiaries. However, MNEs reallocate these investments to other subsidiaries, maintaining overall group-level investments. This reallocation, primarily within local subsidiaries, corrects prior capital misallocation and raises group-level tax revenue. These findings suggest that coordinated and well-targeted anti-tax avoidance rules can reduce tax avoidance without stifling overall investment.

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

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I Introduction

Profit shifting by multinational enterprises (MNEs) undermines economic fairness, placing a disproportionate share of the corporate tax burden on domestic businesses and intensifying economic inequality.[1] These activities cost countries USD 100-240 billion in lost revenue annually, equivalent to 4-10% of global corporate income tax revenue (OECD, 2024). In response, governments worldwide have implemented anti-tax avoidance measures to curb MNE profit shifting. However, these policies often have unintended effects, such as increased capital costs (Bilicka *et al.*, 2022), reduced investment (Serrato, 2018), and potential declines in corporate tax revenue (Djankov *et al.*, 2010). Therefore, it remains an empirical question: can anti-tax avoidance policies effectively curb profit shifting without stifling investment and economic growth?

In this paper, I address this issue by analyzing the causal impacts of Earnings Stripping Rules (ESR), a key anti-avoidance measure that targets profit shifting through debt channels (OECD, 2015a). Under most tax systems, interest income is taxable, whereas interest expenses are deductible against corporate tax liabilities (de Mooij & Hebous, 2018). Consequently, MNEs can exploit these provisions to shift profits to lower-tax jurisdictions by making tax-deductible interest payments from high-tax subsidiaries to low-tax subsidiaries (Bilicka *et al.*, 2022).[2]

The ESR policy, recommended by the OECD in 2015 and adopted by more than 45 jurisdictions between 2015 and 2023 (OECD, 2023), caps a firm's net interest deductions at 30% of its Earnings Before Interest, Tax, Depreciation, and Amortization (EBITDA), disallowing tax deductions beyond this threshold (OECD, 2015a). The policy thus creates a trade-off between tax revenue goals and economic expansion: while ESR can increase tax revenues by limiting interest deductions (Beer *et al.*, 2020), it may also raise the user cost of debt-financed capital (Mooij & Liu, 2020) and hence, discourage investments by MNEs (Serrato, 2018).

To analyze this trade-off, I empirically estimate the causal impacts of ESR by employing a staggered difference-in-difference framework and comparing tax-revenue and investment outcomes, measured in USD, between the treated and control groups. 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, I identify firms as "treated affiliates" if, in the baseline year — the year preceding ESR implementation — they fail the country-specific ESR, i.e., firms with net interest deduction in excess of 30% of EBITDA, rendering a portion of their interest non-deductible. Thus, the treatment in this study is not the introduction of ESR but rather

[1] Please see Clausing *et al.* (2021), Piketty *et al.* (2018), Akcigit & Ates (2023), and Bachas *et al.* (2023) for a detailed discussion on these impacts.

[2] 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).

whether an affiliate exceeds this threshold. Since the reform may prompt reallocation of assets and operations within groups from treated affiliates to non-treated affiliates, I identify "sister affiliates" — those in the same group as treated affiliates but not exceeding the ESR threshold — as a separate category for comparison (Liu, 2020). Affiliates unrelated to treated affiliates, hence belonging to different MNE groups, serve as "control affiliates". I then conduct two analyses: one comparing treated affiliates to control affiliates to measure the direct effects of ESR, and another comparing sister affiliates to control affiliates to assess potential reallocation effects within MNE groups.

To evaluate the overall impact of ESR at the group level, I compare outcomes between MNEs with at least one treated affiliate against those composed solely of control affiliates. This approach isolates the group-level impact of ESR, accounting for any intra-group reallocation. Across all analyses — treated versus control affiliates, sister versus control affiliates, and treated versus control groups — the identification assumption requires that, in the absence of ESR enforcement, the outcomes in these groups would have evolved similarly. I validate this assumption by conducting event studies on 60 different outcome variables across the three specifications. The results consistently confirm parallel trends in outcomes prior to the ESR enactment, providing robust support for the validity of the identification assumption in my empirical analysis.

I extract detailed balance sheet, financial, and incorporation information for MNEs from the ORBIS database for the years 2010 to 2021. This cross-country panel data allows me to track firms over time and examine trends and variations in their financial behaviors. ORBIS is widely used in studies examining MNE investments due to its two key strengths: the availability of ownership information, which allows for the identification of related and unrelated affiliates, and the distinction between unconsolidated financial statements at the firm level and consolidated financials at the group level (Alviarez *et al.*, 2023).[3]

The empirical analysis at the affiliate level reveals that ESR implementation reduces profit shifting and tax avoidance, albeit at the expense of lower investments in treated affiliates. Treated firms decrease their interest expenses, on average, by 23% compared to control affiliates, resulting in an increased profit before tax of 87% and increased tax liabilities of 31%.[4] However, the restrictions on interest deductions also raise the cost of debt, leading firms to reduce their debt holdings by over 21% which in turn decreases capital stock by 16% and lowers investment spending by 31%. The observed 5.5% decline in firms' revenue indicates that this reduction reflects a real economic response rather than a mere reporting

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

[4]While some jurisdictions disallow net-interest deductions (i.e., interest deductions net of interest income), others disallow gross-interest deductions. In the net-interest regime, affiliates can also optimize interest income to offset the reform's impact. Consequently, when estimating averages across all rule-enforcing jurisdictions — both gross and net interest regimes — the decrease in interest expenses and the increase in the tax base are not one-to-one.

adjustment.

However, MNEs respond by reallocating investments to sister affiliates, which not only offsets the declines in investment and revenue at the treated affiliates but also increases tax revenues at these sister affiliates. This reallocation boosts assets by 4.2%, investment spending by 13%, and tax liabilities by 13% at these sister affiliates relative to control firms post-reform. Additionally, the 2.3% increase in revenues at these affiliates suggests that capital from treated affiliates is redirected to the affiliates where it is more productive. While this reallocation could be to sister affiliates in no-ESR jurisdictions, I find that the majority of investments shift to within-country sister affiliates below the ESR threshold, indicating limited capital flight and a preference for domestic reallocation where feasible.

Consequently, at the group level, while overall investments remain unchanged post-reform, group revenue rises by 23%, indicating that ESR encourage MNEs to allocate capital to affiliates with the highest marginal product, effectively correcting prior misallocations made for profit-shifting. Additionally, the 20% increase in tax liabilities highlights the reform's effectiveness in raising tax contributions without harming MNE performance. I also observe an increase in third-party debt holdings at the group level, with incorporation analysis revealing that MNEs use this debt to establish new affiliates in ESR-enforcing jurisdictions. This strategy likely leverages provisions like group taxation, which allow affiliates to share interest deductions — if one affiliate cannot deduct interest, another can — thereby enabling partial circumvention of ESR's limitations.

Heterogeneity analysis reveals that MNEs respond differently to the ESR based on the strictness of the rules. Affiliates under the gross-interest regime face stricter ESR than those in the net-interest regime, as the latter can optimize interest income to mitigate the reform's impact. Consequently, gross-interest affiliates demonstrate more pronounced reductions in interest expenses and greater investment adjustments. Group escape clauses further soften the policy's impact, allowing firms to share interest expenses through group taxation. As a result, affiliates operating under these rules experience smaller decreases in interest expenses and greater reallocations of debt to domestic sister affiliates. Furthermore, the analysis highlights a pronounced home bias, with domestic affiliates incurring higher taxes than their foreign counterparts, despite similar reductions in interest expenses.

This study underscores that anti-avoidance policies can indeed increase tax revenues without stifling investment, particularly when implemented in a coordinated manner. By aligning tax rules and enforcement strategies, countries can enhance tax revenues in jurisdictions where economic activity occurs, thereby mitigating the risk of capital flight and investment decline. However, policymakers must exercise caution. While coordinated efforts can be effective, overly restrictive policies — such as those seen in Uganda — can deter

investments and negate potential gains (Bashir *et al.*, 2024).[5] Conversely, the absence of such policies may lead to increased profit-shifting and tax avoidance through debt channels in high-tax jurisdictions that lack adequate safeguards. Thus, the key takeaway is that well-designed, coordinated anti-avoidance measures can effectively capture additional revenues while preserving investment, but vigilance is essential to strike the right balance.

This paper contributes to the growing literature on the effects of anti-tax avoidance measures on the real economic activities of MNEs by analyzing the global movement of profits, investments, and debt within these corporations (see Johannesen, 2014, Serrato, 2018, Clifford, 2019, and Liu, 2020). It demonstrates that coordinated anti-avoidance policies can enhance tax revenues without adversely affecting investments, aligning with the objectives of the OECD's BEPS initiative, particularly Pillars 1 and 2 (OECD, 2023). Through a comprehensive examination of the OECD's BEPS Action 4 recommendations, the research also reveals heterogeneous responses to anti-avoidance policies across countries and highlights the role of escape clauses, such as group taxation (Andresen & Thorvaldsen, 2023). Furthermore, it underscores the necessity for careful policy design to avoid excessive restrictions that could undermine investment incentives, thus enriching the ongoing discourse on effective global tax policy (OECD, 2024).

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 & Thorvaldsen, 2022), the UK (Bilicka *et al.*, 2022), Finland (Harju *et al.*, 2023), and the US (Carrizosa *et al.*, 2023) — they tend to focus narrowly on intra-group loans and assess each jurisdiction in isolation. In contrast, this research provides a cross-country analysis of rules targeting all forms of debt, allowing for tracking of capital and investment spending across sister affiliates 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 affiliates, they do not result in a complete exit from the enforcement country (see Desai *et al.*, 2004, Huizinga *et al.*, 2008, Huizinga & Laeven, 2008, Mintz & Weichenrieder, 2010, and Buettner *et al.*, 2012). Instead, investments are often reallocated to existing domestic sister affiliates or redirected to new entities established within the same jurisdiction.

The remainder of this paper is organized as follows: Section II provides the contextual

[5] Uganda introduced Thin Capitalization Rules in 2015 and switched to the ESR regime in 2018 following OECD recommendations. However, unlike most ESR-enforcing jurisdictions, Uganda implemented a much stricter version by limiting gross (rather than net) interest expenses, excluding any *de minimis* threshold (typically 3 million EUR in other jurisdictions), and disallowing escape clauses such as group taxation or equity tests. Consequently, as Bashir *et al.* (2024) find, while the ESR reduced debt and interest expenses of MNE affiliates, it did not increase taxable revenues due to poor targeting, instead led to reduced MNE revenues, employment, and economic activity.

details of Earnings Stripping Regulations, Section III derives a theoretical framework to analyze the impact of ESR and to formulate testable predictions, Section IV describes the data and Section V details the research design, Section VI documents a few stylized facts, Section VII presents the empirical results, and lastly, Section VIII offers conclusions and final thoughts.

II Policy Background

MNEs can finance their affiliates' capital through debt or equity, with debt comprising internal or external borrowings (Huizinga *et al.*, 2008; Møen *et al.*, 2019). Equity-financed capital faces double taxation - first on profits and again on capital gains, while debt financing offers tax advantages since interest expenses are tax-deductible (Mooij & Hebous, 2017). This difference allows MNEs to exploit the deductibility of interest expenses across jurisdictions with varying tax rates, a practice known as *tax-motivated debt bias* (De Mooij, 2012). This involves borrowing in high-tax jurisdictions from affiliates in low-tax jurisdictions or tax havens, facilitating tax-free profit shifting.

To counteract the abuse of interest deductions, jurisdictions have historically implemented Thin Capitalization Rules (TCR), which set predefined debt-to-equity ratios — ranging from 0.3:1 in Brazil to 6:1 in Switzerland — beyond which interest expenses are non-tax deductible (Mooij & Hebous, 2017). However, as corporate structures evolved, TCR became less effective in curbing excessive interest deductions (Johannesen, 2014; Crivelli *et al.*, 2016). The challenge arises from the different rules for classifying debt and equity across jurisdictions enabling the creation of hybrid financial instruments, treated as equity in one jurisdiction and debt in another. This facilitates tax avoidance by offsetting tax-deductible interest expenses in the host jurisdiction against tax-favored dividend income in the home jurisdiction.

Additionally, TCR often target only intra-group loans and include a difficult-to-enforce arm's-length principle.[6] As corporate structures become more complex, tracking related party debt (i.e., intra-group borrowings) becomes challenging, especially since two distinct MNE groups may be owned by a single non-corporate entity. Consequently, what appears as external borrowing could be intra-group borrowing. Moreover, financial payments such as loan fees, guarantee fees, and financial lease charges — treated as tax-deductible like interest payments — create further opportunities for tax avoidance.

Recognizing the need for a change, the OECD initiated a comprehensive discussion in 2013 by releasing a prospective Earnings Stripping Rules (ESR) discussion paper, soliciting input from various stakeholders. After thorough consideration, the OECD introduced

[6]This principle requires that transactions between related parties be conducted as if they were between unrelated parties, ensuring market-consistent terms.

these regulations in 2015 as Action 4 of its Base Erosion and Profit Shifting (BEPS) initiative (OECD, 2013; OECD, 2015b).

The Action 4 report established a robust and enforceable framework by linking an entity's net financial deductions to its taxable income. Unlike previous policies that indirectly targeted excessive interest deductions by limiting a company's leverage (i.e., debt-to-equity ratio), ESR directly limited the deductions themselves. The OECD recommends ESR in the following format: (a) net financial deductions above a de minimis threshold of EUR 3 million are deductible only up to 30% of a company's EBITDA, (b) the rule applies to all types of debt, including third-party debt, (c) net financial deductions encompass all payments economically equivalent to interest, not just interest from debt, (d) a company can opt to be taxed on a group basis, allowing for group escape.

The policy rollout is staggered across countries with Germany and Italy being the pioneers and enacting these laws in 2008. South Africa followed in 2014, with Spain and Slovakia adopting them in 2015. The UK, the US, and Vietnam embraced these laws in 2017, and Albania, Argentina, India, Poland, and Uganda did so in 2018. Following the Action 4 recommendations, the European Union (EU) established the Anti-Tax Avoidance Directive (ATAD) in July 2016. This directive required all EU jurisdictions to adopt ESR by December 31, 2018, with non-compliance risking infringement procedures (Deloitte, 2021). Consequently, nearly all EU jurisdictions introduced these laws in 2019.

In some jurisdictions, ESR were the first interest limitation rules, while in others, they replaced or coexisted with TCR rules. Across the ESR-enforcing countries, there is heterogeneity in the de minimis threshold amount, the allowance for carrying forward disallowed interest expenses and unused capacity, the nature of interest expenses (i.e., net or gross), the allowance of group-escape provisions, and their application to domestic groups and standalone companies. I provide more details of the jurisdiction-level heterogeneity in ESR implementation in Appendix A.

III Conceptual Framework

This section establishes a simple theoretical framework for understanding the tax optimization behavior of MNEs operating across jurisdictions with different tax structures, based on models by Mintz & Smart (2004), Huizinga *et al.* (2008) and Møen *et al.* (2019). The model explains how implementing ESR rules in high-tax jurisdictions reduces MNEs' incentives to excessively leverage their affiliates in such locations, leading to lower debt levels and reduced interest expenses (Bilicka *et al.*, 2022). Additionally, as debt is one of the major capital-financing channels, declining debt levels increase the equity cost of capital leading to disruption of the financing and investment landscape (Biermann & Huber, 2023).

Consider an MNE having a network of affiliates, $i \in 1, 2, ...n$, located across $p \in 1, 2, ..., N$ countries, each with a distinct statutory corporate income tax rate, t_p. The MNE invests in each affiliate, i, capital k_i, which results in the cash-flow, $y_i = f(k_i)$. The investment k_i can be made through either equity or internal debt.[7] The key distinction between internal debt and equity lies in the tax deductibility of the former, making it a tax-advantageous financing option.[8] I denote the fraction of capital k_i financed through internal debt as λ_i, with the complementary share financed through equity being $(1 - \lambda_i)$. Furthermore, I incorporate agency costs into my model to reflect potential conflicts between equity and debt claimants which may remain unresolved through contractual means.

Thus, the profit function guiding the MNE's strategic operations is represented as follows:

$$\pi(k) = max \sum_{i=1}^{n} \left\{ (1 - t_p) f(k_i) - r(1 - \lambda_i) k_i - \iota_i (1 - \rho_p \, t_p) \lambda_i k_i - \frac{\eta}{2} \lambda_i^2 k_i \right\} \tag{1}$$
$$\text{s.t.} \quad \sum \iota_i (1 - \rho_p \, t_p) \lambda_i k_i = 0,$$

Here r denotes the equity cost of capital, ι_i represents the interest rate on internal debt, and the final term, $\frac{\eta}{2} \lambda_i^2 k_i$, captures the agency cost of debt.[9] Importantly, the parameter $\rho \in [0, 1]$, signifies the extent of tax deductibility on interest expenses, with $\rho = 1$ indicating full tax deductibility and $\rho = 0$ indicating no tax deductibility. The optimization constraint ensures that intra-group lending and borrowing balance out.

Note that in the model above, while borrowing incurs costs, lending from an affiliate does not. Consequently, the affiliate in the jurisdiction with the lowest tax rate, indexed as 1, is identified as the *tax haven* affiliate.[10] The optimization problem is thus configured as follows:

$$\pi(k) = max \sum_{i=2}^{n} \left\{ (1 - t_p) f(k_i) - r(1 - \lambda_i) k_i - \iota_i (1 - \rho_p \, t_p) \lambda_i k_i - \frac{\eta}{2} \lambda_i^2 k_i \right\} \tag{2}$$
$$+ (1 - t_1) f(k_1) - r(k_1) + \sum_{i=2}^{n} \iota_i (1 - t_1) \lambda_i k_i,$$

The MNE maximizes its after-tax profits by optimizing: (1) the amount of capital k_i invested in each affiliate, (2) the fraction of k_i financed through internal debt, λ_i, and (3) the associated

[7] External debt, though important for financial analysis, is not relevant for profit shifting motivations. Therefore, it is dropped from the analysis. However, the analysis can easily be extended to incorporate external debt as well (see Buettner *et al.* (2012); Møen *et al.* (2019)).

[8] Note that internal debt also entails fixed repayment schedules and interest obligations, thereby potentially limiting the corporation's adaptability to volatile market conditions. In contrast, equity financing affords a greater flexibility without fixed repayments and interest costs but exposes the subsidiary to liquidity risks during financial downturns or if the parent company withdraws funding.

[9] This agency cost follows an increasing and convex profile, i.e., $c^{'} > 0$ and $c^{''} > 0$.

[10] This affiliate exclusively issues all internal debt within the MNE's network, transacting with the remaining $n - 1$ affiliates.

interest expenses, proxied by the interest rate ι_i. 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

Maximizing (2) with respect to λ_i yields the optimal borrowing function as follows:

$$\lambda_i^* = \frac{r + \iota_i(\rho_p t_p - t_1)}{\eta} \tag{3}$$

Suppose that country p initially permitted full deductibility of interest expenses (i.e., $\rho_p = 1$). When the country introduces ESR, the tax deductibility of interest expenses becomes limited (i.e., $\rho_p < 1$). Since $\frac{\partial \lambda_i}{\partial \rho_p} = \frac{\iota_i t_p}{\eta} > 0$, the borrowings of affiliates in this country will decrease. Therefore,

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 Interest Expenses

Additionally, maximizing (2) with respect to ι_i, yields the following expression:

$$\frac{\partial \pi}{\partial \iota_i} = (\rho_p \; t_p - t_1)\lambda_i \; k_i \tag{4}$$

The profit function shows a positive correlation with the parameter ρ_p with respect to the interest rates, ι_i. Therefore, a reduction in ρ_p due to the introduction of ESR results in decreased net profits for each additional dollar allocated to loan repayment. Consequently,

Prediction 2: *With the implementation of ESR, interest expenses associated with internal debt will decrease.*

III.3 Effect on Investments

Finally, maximizing (2) with respect to capital k_i yields the following result:

$$f^{'}(k_i) = r(1 - \lambda_i) + \tau_i \tag{5}$$

where

$$\tau_i = \frac{r(1 - \lambda_i)t_p - (\rho_p \; t_p - t_1)\iota_1 \lambda_i + \gamma_i}{1 - t_p} \qquad \& \qquad \gamma_i = \frac{\eta}{2}\lambda_i^2 \tag{6}$$

In the above equation, the user cost of capital, τ_i, is negatively correlated with the deductibility of interest expenses, ρ_p. Consequently, when a country introduces ESR, ρ_p de-

creases, resulting in an increase in τ_i. From equation (5), it is evident that there is an inverse relationship between investments and the user cost of capital; thus the increase in τ_i leads to a decline in investments. Therefore,

Prediction 3: *Following the enforcement of ESR, investments within the affected affiliates will decrease.*

Using the data mentioned in detail below, I empirically test the predictions formulated in this section.

IV Data on MNE Finances & Incorporation

To estimate the causal effects of ESR, I extract detailed balance sheet, financial, and incorporation information for MNEs at both the affiliate and group levels from the ORBIS database for the years 2010-2021. Supplied by Moody's, ORBIS is a comprehensive company database that provides entity-level financial data for over 489 million companies worldwide, along with historical information on company location, industry classification, and ownership structures dating back to June 1989. To record profits generated by multinationals in their subsidiaries, ORBIS relies on information from public business registries. However, in a few jurisdictions, public registries either do not exist (e.g., Bermuda) or lack income information (e.g., the United States and Switzerland). Consequently, profits booked by multinationals in these jurisdictions are not visible in ORBIS (Tørsløv *et al.*, 2023).

Despite these coverage limitations, my estimation results remain unbiased for two reasons. First, the data for the majority of ESR-enforcing countries is visible in ORBIS, allowing accurate identification of treatment effects at the affiliate level (see Appendix Table E.I and Figure F.I for geographical coverage of affiliate-level sample data). Second, group-level consolidated financial reports include the financials of all subsidiaries, including those in tax havens (see Appendix Figure F.II for geographical coverage of group-level sample data). At most, these coverage issues may lead to an underestimation of the true effects at the affiliate level.

IV.1 Financial Data

For this project, I focus on MNEs only, defined as networks of affiliates under the same global ultimate owner (GUO) that includes at least one affiliate domiciled outside the GUO's jurisdiction. Since a firm only fails the ESR test if its interest expense:EBITDA exceeds a specific threshold, it is essential to gather information on both variables to accurately identify the firms that fail the ESR test from those that do not. Consequently, when collecting data from the ORBIS cluster, I ensure that both the *financial expenses* and *EBITDA* columns are

simultaneously non-empty. Moreover, I exclude observations with missing jurisdiction information, industry information, or negative asset values. To mitigate potential biases from duplicate entries, I retain only the latest reported financials (Kalemli-Ozcan *et al.*, 2015), avoiding issues similar to those identified in Clausing (2016).[11] I detail data cleaning steps in Appendix B and provide the definitions of key variables of interest in Appendix B.1.

Furthermore, to identify the group-level effects of the reform, I restrict the affiliate-level analysis to only the firms whose GUO also reports consolidated group-level financials. Additionally, I exclude intermediate owners to avoid double counting, retaining only the highest company in each ownership chain. The analysis is confined to four periods preceding the reform and five post-treatment periods. Following a series of data cleaning steps detailed in Appendix B, my cleaned affiliate-level dataset comprises an unbalanced panel of approximately 560,000 observations from about 95,000 affiliates belonging to 10,250 groups. At the group level, the cleaned dataset contains 65,000 observations from these 10,250 groups. I provide summary statistics at the affiliate level in Table I, at the group level in Table II, and industry classification statistics in Appendix Table E.II.

IV.2 Incorporation Data

For the extensive margin analysis of the policy, I utilize the detailed incorporation and ownership information from the ORBIS database, which provides comprehensive data on new incorporations (i.e., firms established), their global ultimate owners (GUOs), the jurisdictions and the dates of incorporation. For the years 2010-2021, the database contains information on over 43 million incorporations. However, for the purposes of this project, I retain the incorporation data of only the 10,250 MNE groups identified earlier. The cleaned sample consists of a repeated cross-section of more than 68,000 incorporations across 157 jurisdictions by over 8,500 groups. The geographical coverage of this cleaned incorporation data is presented in Appendix Figure F.III.

IV.3 Jurisdiction-Level Policy Data

To track jurisdiction-specific policy reforms, I obtain accurate and up-to-date information on policy data from various sources, including official fiscal acts, OECD reports, and annual auditing reports by firms like PWC, Deloitte, KPMG, and EY among others. To implement the analysis, I adopt the broader definitions and scope of financial expenses as advised by Action 4 of the OECD's BEPS initiative, applying them to both related-party and third-party loans. Consequently, I consider the policy effective in a jurisdiction not in the year the 30% of EBITDA rule was introduced, but when these broader definitions were adopted. I adjust for

[11] For a detailed discussion on these biases please see Blouin & Robinson (2023).

partial implementations by defining policy years accordingly and exclude jurisdictions with limited ESR scope and those with incomplete data. I also abstract from exemption clauses due to the inability to differentiate types of interest expenses in my data. Jurisdictions with reforms predating my dataset and those without data availability are also excluded. I provide additional jurisdiction level ESR details in Appendix A.

IV.4 Data on Jurisdiction-Level Controls

Finally, to control for jurisdiction-specific business cycles effects, I retrieve the data on global statutory corporate tax rates from Enache (2023), yearly exchange rate data from Treasury (2024), and population and GDP per capita statistics from the World Bank's World Development Indicator Database.

V Research Design

Given the staggered rollout of ESR across jurisdictions, I use a staggered difference-in-differences (DiD) framework to work out the causal impacts of the policy. Since ESR impacts firms with financial borrowings exceeding a threshold (e.g., 30% of EBITDA), I leverage the similarity between firms just below the threshold (*control* firms) and those just above it (*treated* firms). To avoid potential endogenous responses, I identify *treatment* and *control* units in the baseline year, i.e., t^**-1* where t^* is the policy year, though anticipation effects may occur due to policy announcements.[12] I provide additional details on treatment identification in Appendix C.1.

While the ESR directly impacts the firms that fail the (interest expenses:EBITDA) ratio test, it could induce reallocation effects as well. Specifically, the reform may result in the reallocation of investments and operations from treated firms to their sister affiliates (Liu, 2020, Bilicka *et al.*, 2022). Such reallocation is of interest in itself and has important policy and welfare implications. I therefore separately estimate this reallocation and for that purpose classify the sample into three distinct groups:

- *Treated Firms*: Firms that fail the jurisdiction-specific ESR in the baseline year. For example, for the UK's case, treated firms wii include those with net financial expenses exceeding 2 million GBP and 30% of EBITDA in 2016.[13]

[12] Anticipation effects can arise from the OECD announcement in 2015, the Anti-Tax Directive Agreement (ATAD) by the European Union (EU) in 2016, or jurisdiction-specific announcements when the policy is announced but not yet enforced. However, in Appendix C.2, I address announcement effects of the OECD and ATAD announcements. Moreover, in Appendix C.3.1 and C.3.2, I non-parametrically rule out selection into the treatment.

[13] Note that the UK introduced the ESR measures in 2017. Therefore, I identify the treatment status of UK affiliates in 2016.

- *Sister Firms*: Sister affiliates of treated firms that do not fail the ESR in the baseline year. Continuing with the UK example, these firms will include UK affiliates that do not fail the UK test in 2016 and non-UK affiliates that do not fail the ESR test of their respective jurisdictions in their baseline years.
- *Control Firms*: Firms that neither fail the ESR nor are affiliated with treated firms. This classification ensures a control group free from policy responses.

To causally estimate the dynamic impact of ESR, I use the DiD specification by de Chaisemartin & D'Haultfoeuille (2020), allowing for unbalanced panel data with different treatment periods. The baseline regression model is:

$$y_{ist} = \alpha_i + \mu_t + \sum_{\substack{p=-4 \\ p\neq -2}}^{4} \beta_p \, treat_i \times \mathbb{1}(t - t_s^* = p) + X_{st}^{'} \delta + \lambda_{is}^{'} f(t) + \varepsilon_{ist} \tag{7}$$

Here y_{ist} represents the outcome for firm i in jurisdiction s during year t. The equation includes firm fixed effects (α_i) and year fixed effects (μ_t), while $treat_i$ denotes the treatment indicator. The parameter t_s^* indicates the policy implementation year, and X_{st} consists of time-varying jurisdiction-level controls such as tax rates, population, and GDP per capita. The term $f(t)$ is a linear trend vector (i.e., $f(t) = t$) of firm-specific time trends λ_{is}.[14] The unobserved error component is represented by ε_{ist}. To estimate the average effects of the policy, I replace the treatment-by-event-time interactions with a single treatment-by-after interaction. The standard errors for both the models are calculated using the bootstrap method. To address potential anticipation effects, the reference year for treatment effects is set to $t^* - 2$.[15]

When estimating the policy effects on treated firms, I exclude sister affiliates and compare the outcomes against control affiliates. Conversely, for reallocation effect analysis,I

[14]Including firm-specific linear trends helps account for differential pre-treatment trends among firms. Firms with higher leverage, indicated by a higher debt-to-equity ratio, tend to borrow more each year for operational expansion. As a result, their interest expenses grow at a faster rate compared to firms that do not rely as heavily on debt. These dynamics lead to different growth trajectories for debt and interest expenses among firms even before policy implementation. By incorporating these trends, I mitigate potential biases arising from these pre-existing differences, ensuring a more accurate estimation of the policy's impact (Freyaldenhoven *et al.*, 2019). I provide additional details in Appendix C.3.

[15]In all ESR-enforcing jurisdictions, the policy announcement typically occurs in year $t^* - 1$, with announcements early in the year and firms reporting financials at year-end. This timing suggests that, without anticipation effects, the announcement is unlikely to prompt immediate financial responses within the same year due to operational challenges. However, strategic decisions like investments and asset financing can be influenced within the announcement year. For estimation, while financial parameters maintain a linear trend in $t^* - 1$, strategic responses deviate from this pattern, rejecting parallel counterfactual trends if referenced to $t^* - 1$. Using $t^* - 2$ as the reference year ensures accurate trend fitting for all parameters, leading to parallel counterfactual trends, non-significant coefficients for financial parameters in $t^* - 1$, and significant ones for strategic variables. Although identifying treatment in $t^* - 2$ is possible, it introduces complexities and potential confounding factors like BEPS and ATAD announcements. Given these challenges, identifying treatment in $t^* - 1$ and referencing $t^* - 2$ is the most efficient method, ensuring robustness by avoiding mean reversion, selection bias, or announcement effects. I provide additional details in Appendix C.5.

drop treated affiliates, and compare sister affiliates' outcomes against control affiliates. This consistently defined control group facilitates a clean analysis of reallocation.[16] For group-level analysis, an MNE is considered treated if at least one of its affiliates is treated at the firm level. In both reallocation and group-level analyses, I assign the earliest treatment year for any firm in a group to all sister affiliates for consistency. Given the presence of negative values in outcome variables like *profits before tax*, *taxation*, and *net interest expense*, I report treatment effects in levels (i.e., nominal USD millions) and also normalize them to the mean of the treatment group in the reference year ($t^* - 2$) for percentage interpretation.[17] I provide additional details in Appendix C.4. To reduce sensitivity to outliers, I trim the data at the 1st and 99th percentiles.

V.1 Locational Responses

To evaluate extensive margin responses, I measure the probability of MNE incorporation in a specific jurisdiction by taking the ratio of MNE incorporations in that jurisdiction to the total number of global incorporations in a given year. This probability reflects the attractiveness of a jurisdiction for establishing new affiliates or expanding existing operations. Using the DiD model from equation (7), setting the treatment date as the earliest year in which any firm in the group was treated, and winsorizing the data at 1%, I first analyze whether treated groups, on average, incorporate more or fewer affiliates post-reform relative to the control group at a global level. Since the incorporations data is a repeated cross-section data, I exclude firm-specific linear trends while estimating equation (7). Following this, I investigate the specific jurisdictions to identify where incorporations have increased or decreased. However, it is important to acknowledge that this approach may underestimate the true extent of locational responses, as the incorporation data does not account for mergers and acquisitions.

VI Stylized Facts

Before presenting the empirical results, I first present a few stylized facts based on the cleaned sample data of MNE affiliates to highlight key insights into the broader policy im-

[16] A potential issue with the method proposed by Chaisemartin *et al.* (2024) is its inability to compare outcomes between treated and never-treated groups, as it only assesses units whose treatment status changes over time against those whose status remains unchanged. Consequently, the control group is not identical across the two specifications (i.e., treated versus control and sister versus control), potentially leading to misidentification of reallocation effects. However, I address this concern in Appendix H and provide evidence that the reallocation effects estimated are not confounded by such discrepancies.

[17] The taxation variable in ORBIS encompasses all types of taxes, including those that are paid, accrued, and deferred. Consequently, the observed negative values may arise from tax refunds or credits, loss carryforwards, adjustments or corrections, and deferred taxes.

plications on tax-avoidance and profit-shifting behaviors.

VI.1 ESR Targets Aggressive Profit Shifters

A common pattern observed in studies on tax avoidance is that firms engaging in the most aggressive profit shifting often report zero or negative taxable profits (Johannesen *et al.*, 2020; Bilicka, 2019; Dharmapala & Hebous, 2018). Since the OECD introduced ESR to limit *Base Erosion* and *Profit Shifting* (*BEPS*), I plot the distribution of taxable profits scaled by total assets for each of the three groups of affiliates and use it as a proxy to measure profit shifting in Figure Ia.[18]

I note that the proportion of treated affiliates reporting zero taxable profits, something I term *zero-bunching*, is almost twice as that of sister and control affiliates. Moreover, the treated affiliates also have a disproportionately higher number of firms reporting negative accounting profits. Since all three groups comprise MNE affiliates only, the figure highlights that the reform targets the group of firms that engage most aggressively in profit shifting.

Furthermore, in the Appendix Figure F.I, I plot the before- and after-treatment distribution of taxable profits with respect to total assets for each group of affiliates separately. While the distribution of the control and sister firms remains largely unchanged across both periods, I observe a sharp decrease in the proportion of *zero-bunching* treated firms post-reform and an increase in the proportion of treated firms reporting positive profits relative to pre-reform periods. This figure suggests, at least non-parametrically, that the ESR reforms were able to curb profit shifting activities at the affiliate level.

VI.2 ESR Induces Bunching at Exemption Thresholds

As detailed in Section 2, ESR policies impact firms only if they fail to meet jurisdiction-specific rules. These rules set an exemption threshold, typically defined as an *interest expense:EBITDA* ratio, below which firms are exempt from ESR stipulations. Given these sharp exemption thresholds, firms may bunch just below limits to avoid ESR provisions (Saez, 2010; Chetty *et al.*, 2011; Kleven & Waseem, 2013).[19] To test this hypothesis, I normalize deduction thresholds across all jurisdictions to zero and restrict my sample to firms that would have been affected by ESR if their *interest expense:EBITDA* ratio exceeded the threshold.[20] If ESR induces bunching at these exemption thresholds, post-treatment I expect to see

[18]The data used in this study are derived from the financial statements of companies, focusing on profits before tax. Although "profits before tax" and "taxable profits" are distinct terms in accounting literature, they are used interchangeably in this paper to mean profits before tax.

[19]For firms with positive tax liabilities, this ratio threshold acts as a tax kink, while for those with negative tax liabilities, it serves as a tax notch.

[20]For instance, in jurisdictions like Uganda with only an *interest expense:EBITDA* threshold, I include all MNE affiliates. In India, where a *de-minimis* threshold also exists, I include only firms that fail the *de-minimis* test. In

an excess of firms just below this normalized threshold and a reduced mass above it.

The results of this exercise, presented in Figure Ib, reveal notable bunching not only local to the normalized threshold but also a broader shift in the distribution far from the threshold. In other words, there is an excess mass at all points below the threshold and a reduced mass at all points above it, indicating significant distortion caused by ESR. Additionally, in Appendix Figure F.II, I present a placebo test distribution for *all* firms in the sample. Here I do not restrict the sample to only the firms that would have failed the test but instead expand it to every firm within the cleaned sample. The figure shows no structural shift in the *interest expense:EBITDA* ratio for the original sample. Thus, the figure confirms the absence of a structural shift in the distribution of *interest expense:EBITDA* in the entire sample that may have driven this bunching phenomenon in the subsample of firms post-reform.

VI.3 ESR Leads to a Reduction in Debt Bias

Corporate policies that allow interest deductibility create a tax-motivated preference for financing capital through debt rather than equity or retained earnings (De Mooij, 2012; Mooij & Hebous, 2017). This debt bias results in higher leverage among MNE affiliates, as the tax benefits of debt outweigh the costs associated with other financing methods. In Appendix Figures F.IIIa, F.IVa, and F.Va, I plot the distribution of the proportion of assets financed through debt, equity, and retained earnings, respectively, for all three groups of firms. The figures reveal that treated firms have a significantly higher proportion of their capital financed through debt compared to sister and control firms thus confirming the hypothesis of a debt bias in the capital structure of treated firms. Moreover, the analysis in Appendix Figures F.IIIb, F.IVb, and F.Vb which displays the pre- and post-reform distribution of capital financing for the treated group, highlights a notable decrease in debt financing and a corresponding increase in equity and retained earnings financing among treated firms. These results underscore the effectiveness of the reform in reducing the debt bias, as the affected firms shift towards more balanced financing methods.

VII Empirical Results

I now present the causal estimates of the ESR policy and empirically test the predictions developed in Section III. First, I discuss the average treatment effect of the reform on tax avoidance, investments, and other outcomes at the affiliate level. Following this, I examine potential reallocation effects of the policy and then analyze its impact at the group level. I

Japan, which has an additional *debt* test, I restrict the sample to firms failing both the *de-minimis* and *debt* tests. This subgroup has the highest incentive to bunch just below the *interest expense:EBITDA* threshold.

also present evidence on the locational impacts of the policy and conclude by discussing the heterogeneity and robustness of my results.

VII.1 Effects of ESR at Firm Level

In this section, I evaluate the causal impact of ESR on firm-level outcomes by comparing treated firms against control firms, excluding sister affiliates from my analysis.

VII.1.1 ESR Leads to Reduction in Profit Shifting & Tax Avoidance

The ESR reform specifically targets financial expenses to limit profit shifting and tax avoidance. In Figure III and Table IV, I observe a substantial reduction in these expenses by approximately 1.36 million USD, representing a 23% reduction from the pre-baseline mean.[21] This decrease may result from a reduction in interest rates on intra-group loans, a revision in the debt payment schedules, or cutbacks on non-debt-related financial expenses.[22] Importantly, if all these operations are conducted within the MNE group, they serve as avenues for tax avoidance and profit shifting. Thus, a reduction in financial expenses signifies an average reduction across all these components, not just interest expenses, highlighting the robustness of the reform.

Since ESR sets a limit on the financial expenses:EBITDA ratio, a firm can claim higher financial deductions if it has higher EBITDA. Table IV reveals that EBITDA for treated firms does in fact increase by nearly 20%.[23] This increase in EBITDA can result from operational expansion (higher revenue) or improved operational efficiency (cost reduction). On the revenue side, I find no evidence of increased revenue; instead, reported revenue decrease by 5.5% after the reform (discussed in the next section). Given the revenue contraction, the increase in EBITDA is probably due to operational efficiency, though I lack data on key variables like the cost of goods sold and gross profit to test this claim. The combination of increase in EBITDA and reduction in financial expenses leads to an 87% reduction in the interest expense:EBITDA ratio for treated firms, as shown in Appendix Table E.III. This significant reduction highlights the incentives for treated firms to lower this ratio to avoid failing these tests in subsequent years.

Additionally, the reduction in financial deductions is expected to be accompanied by a corresponding increase in taxable revenues. Figure III and Table IV show that the taxable

[21] Note that the number of *Observations* in the average effects exceeds the raw sample size. This discrepancy arises because the methodology of Chaisemartin *et al.* (2024) computes the average effect using an average of the effective sample sizes in each of the DiD comparisons. The effective sample size for each comparison can vary due to the differing numbers of treated and control units and the variations in the timing of treatments. Consequently, some comparisons may include overlapping observations.

[22] for example, advisory fees, guarantee fees, finance lease charges, loan agreement fees, etc.

[23] Note the increase in standard errors of the dynamic estimates in later years of the reform. I discuss these large standard errors in Appendix D.1

profits for the treatment group increase by more than 87%, taxation by 31%, and net-of-tax profits by 120% of the pre-baseline mean. Note that the taxable profits are not bounded below by zero (my data also include loss-making firms), and taxation refers to all taxes whether paid, accrued, or deferred. Therefore, the substantial increase in the taxable profits and taxation is likely due to the average effects of both profitable and loss-making firms and the impact of taxes paid and losses carried forward, respectively. Although the dynamic effects of the policy may suggest selection into the treatment due to statistically significant non-zero coefficients of profit before tax and financial expense in $t^* - 1$, I reject the hypothesis of treatment selection in Appendix C.3.1 and C.3.2.

VII.1.2 ESR Leads to Reduction in Investments

As the deductibility of interest expenses creates a tax-motivated debt bias among firms engaged in tax avoidance and profit shifting through debt channels, the introduction of ESR is expected to reduce this bias due to the increased net cost of financing assets through debt. Figure IIa illustrates a notable decrease in the debt holdings of affected affiliates following the reform. Parametric estimates in Table III corroborate this observation, revealing a reduction in debt by 21% relative to the pre-baseline mean. Additionally, Table E.III in the Appendix shows a 15% reduction in the debt-to-equity ratio and a 19% decrease in the proportion of capital financed through debt (i.e., debt-to-assets ratio) for the treated firms. Collectively, these results robustly demonstrate the reform's success in mitigating tax-motivated debt bias thus empirically supporting Prediction 1 outlined in III.1.

Additionally, as noted in Section III.3, the reduction in debt levels may disrupt the asset portfolios and investment activities of affected firms. Figure II empirically validates this prediction, illustrating a notable decline in the capital and asset portfolios of treated firms post-reform. Table III quantitatively reveals that various types of assets, including tangible, intangible, and fixed assets, decrease by at least 10% on average relative to the pre-baseline mean. Furthermore, gross investment spending by treated affiliates decreases by 31% translating into a 19% drop in investments, as detailed in Appendix Table E.III. Since equity in treated affiliates increases in the later periods of the treatment (see Figure IIb), it leads to higher investment spending by treated affiliates in later reform years. One concern with the investment spending graph is that it may reflect the business cycle effect rather than causal effect of the policy. I address this concern in Appendix C.3.2 and provide evidence that my results are not confounded by mean reversion or business cycles effects. Since my measure of investments is based on the stock of fixed assets rather than cash flow or capital expenditures, it is possible that the reduction in investments is due to asset revaluation rather than actual reductions. However, the 5.5% decline in revenue (see Table IV) indicates that the investment response is likely a real response rather than a mere reporting response.

The data further suggests that firms may have anticipated the reform's impact, as I observe a decrease in assets and capital even in the baseline year prior to the reform. This could be due to the announcement effect of jurisdiction-specific reforms, the binding ATAD agreement, or the announcement of OECD reforms. In Appendix C.2, I reject the hypothesis that the ATAD or BEPS announcement had any anticipation effect and posit that the jurisdiction-specific announcement may have served as an anticipation signal. These findings align with literature indicating that firms often begin adjusting their strategies in anticipation of regulatory changes, even before the formal implementation of such reforms. I discuss it in greater detail in Appendix C.5.

VII.2 Reallocation Effects of ESR

In this section, I examine the reallocation effects of the ESR reform by excluding the treated affiliates from the sample and comparing the outcomes of sister affiliates against the control group. Since the control group remains constant across both analyses, this allows me to directly compare the difference-in-differences estimates for treated affiliates with those of sister affiliates. Note that I assign the start of treatment for the treated affiliate in a group to all the sister affiliates. Consequently, the results presented may underestimate the true reallocation effects of the policy, as they do not account for any potential variation in the timing or intensity of the treatment across different sister affiliates.

VII.2.1 Reallocation of Assets & Investments

Table V demonstrates a significant increase in assets and investment spending among sister affiliates following the ESR implementation: tangible fixed assets increase by 4.2%, intangible assets by 13.5%, and investment spending by 13.3% relative to the pre-baseline mean of the sister affiliates. Given the concurrent decline in assets and investments at the treated affiliates (see Section VII.1.2), this growth in sister affiliates' assets and investments indicates a strategic intra-group reallocation of investments. The simultaneous increase in reported revenue further suggests that capital was initially misallocated to treated affiliates due to profit-shifting incentives. Post-reform, this capital is reallocated to more productive affiliates. These higher revenues thus lead to increased taxation even at sister affiliates (see Figure IV). These changes are not direct effects of the ESR, as the policy does not target these sister affiliates, i.e., these firms do not fail the test in their baseline years. Instead, the increases can be attributed to positive reallocation effects from the reform. Since sister affiliates of a company can be located globally, I now investigate *where* the reallocation of investments actually occurs.

VII.2.2 Reallocation to Local Affiliates

I categorize sister affiliates into two broad groups: local affiliates and foreign affiliates. The initial analysis considers two opposing behavioral patterns induced by the ESR reform: (1) the introduction of ESR may render the entire jurisdiction less attractive for MNE operations, leading to a significant reallocation towards foreign affiliates; or (2) the relative ease of reallocating resources within the jurisdiction, better understanding of the national tax code, etc., may encourage a shift to local affiliates. Notably, since local sister affiliates have not failed the ESR test, they possess some debt capacity until such a failure occurs. Consequently, reallocating resources to these affiliates could prove to be a more strategic choice. To examine this hypothesis, I first restrict the sample to local affiliates and compare outcomes with the control group. Subsequently, the analysis shifts to foreign affiliates, where I again compare outcomes with the control group. By evaluating the responses from these two analyses, it becomes possible to identify any differential behaviors. Figure XI illustrates that the latter response dominates, indicating that debt, capital, and assets are reallocated more toward local affiliates than foreign affiliates. This shift in investment and capital further enhances the reported revenues of local affiliates, resulting in increased taxation for these entities compared to their foreign counterparts. These findings underscore the effectiveness of ESR in strengthening jurisdiction-level tax revenue collection by raising additional taxes not only from treated affiliates but also from their local sister affiliates.

VII.2.3 Reallocation to Foreign Affiliates within ESR-Enforcing Jurisdictions

Next, I shift the focus to foreign affiliates of treated firms, where I classify them into foreign affiliates located in jurisdictions with ESR rules and in jurisdictions without these policies. I anticipate two opposing behavioral patterns: if the introduction of ESR makes the entire jurisdiction less attractive for MNE operations, a reallocation of assets and investments to jurisdictions without such policies would be expected. Conversely, if reallocations occur within ESR-implementing jurisdictions, it may indicate non-negative reallocations or optimization behaviors by MNEs utilizing group escape clauses to mitigate the reform's impact.[24] To test these hypotheses, the initial analysis examines foreign sister affiliates located solely in ESR-enforcing jurisdictions, excluding those in non-ESR jurisdictions. The second analysis centers exclusively on foreign sister affiliates in non-ESR jurisdictions, maintaining the same control group as in previous estimations. Again, the comparison of the responses from these two analyses helps me identify any differential behavior. Appendix Figures G.I and G.IIa reveal that debt, capital, and assets are reallocated to sister affiliates within ESR-enforcing jurisdictions, rather than to non-ESR jurisdictions. The corresponding increase

[24] I discuss this in more detail in Section VII.3.

in reported revenue supports the notion that this is a real response rather than merely a reporting adjustment. Notably, despite a lack of debt reallocation to sister affiliates in non-ESR jurisdictions, these affiliates report higher interest expenses, leading to an increased interest-to-EBITDA ratio. Consequently, the taxable profits of these affiliates shrink, suggesting that profit-shifting and tax avoidance practices of MNEs may have shifted toward these affiliates. This observation highlights a critical policy implication: jurisdictions lacking ESR may find themselves at a disadvantage and should consider adopting such measures in the future.

Finally, it is important to note that if all the existing affiliates of an MNE face capacity constraints — meaning that additional debt could result in failing the ESR test — then part of the debt and investments may be redirected towards establishing new affiliates and expanding MNE operations. This dynamic is discussed in greater detail in the Section VII.4.

VII.3 Group-Level Consequences of ESR Policies

Given the limitations in fully observing the financials of all affiliates within an MNE group (Tørsløv *et al.*, 2023), the true extent of reallocation to sister affiliates, as discussed in the previous section, is likely only partially identified. Therefore, to gain a comprehensive understanding of the implications of the ESR policy, I now analyze the group level responses to ascertain whether the effects of the reform at the affiliate level are absorbed or magnified at the group level. I compare the consolidated financial statements reported by the global ultimate owners of the treated firms with those of control firms. I define the treatment year as the earliest year in which any affiliate within the treated group was subject to the ESR reform.

VII.3.1 ESR Increases Group-Level Tax Liabilities

From Table VII, I observe that while financial deductions increase post-reform at the whole group level, these deductions do not lead to a reduction in taxable profits or tax liability. Instead, I find that the rate of increase in EBITDA outpaces the rise in interest deductions, resulting in a net positive effect on the taxable profits. Although the increase in the taxable profits is modest, at approximately 10%, it nonetheless contributes to a rise in corporate tax liability by around 20%. Since I observe an increase in taxation at firm level for both the treated affiliates and the sister affiliates, these results corroborate my earlier findings and highlight the effectiveness of ESR reforms in raising group-level tax receipts, even amid the reallocation of tax-avoidance practices to other jurisdictions. In other words, the ESR measures remain robust against intra-group reallocation activities in limiting tax avoidance.

VII.3.2 ESR Does Not Affect Group Level Investment

Tables VI and E.VI demonstrate that both gross investment spending and overall investments at the group level remain stable post-reform. This stability corroborates my earlier observation of intra-group investment reallocation from treated affiliates to their sister affiliates. Consequently, net of this intra-group reallocation, there is no evidence of reduced investments at the group level, highlighting the effectiveness of ESR. Although the reform leads to a reduction in investments at the treated affiliates, the negative investment shock does not extend throughout the entire group.

Additionally, the increase in group-level revenue, as reported in Table VII, supports this conclusion and provides evidence that the reform has not impaired the overall investment capacity and operational performance of MNE groups. On the contrary, the reform has corrected capital misallocation within MNE groups by directing capital to the most productive affiliates. Thus, the ESR not only increase tax revenue but also optimize capital allocation.

Moreover, Table VI indicates a significant increase in both assets and debt at the group level, at least 17% above the pre-baseline mean. This is notable given the reduction in debt and assets at the treated affiliate level and the debt reallocation to sister affiliates within ESR-enforcing jurisdictions. One might expect a decrease in consolidated debt levels (if not all the debt from treated affiliates gets reallocated) or no change (if all the debt gets reallocated). Therefore, the group-level increase appears counterintuitive. However, this behavior may suggest expansion activities, with additional debt used to enhance operations within existing affiliates or incorporate new affiliates into the group. Given the lack of evidence for an average increase in debt holdings across all sister affiliates — possibly due to these affiliates nearing the ESR threshold, limiting their capacity to absorb additional debt — (see Table V), it is plausible that the additional debt is allocated towards new affiliates. I test this hypothesis in the next Section.

VII.4 Extensive Margin

The results discussed above indicate that while debt decreases at the treated affiliates post-reform, overall group-level debt increases. If all restricted debt gets reallocated to sister affiliates, group-level debt holdings, net of intra-group reallocation, would remain unchanged. However, I do not find statistically significant evidence of increased debt holdings at the sister affiliates. Thus, the additional debt may get channeled to other sister affiliates not captured in the data, used for mergers and acquisitions, or employed for incorporating new affiliates. Although I lack comprehensive data to analyze mergers and acquisitions and cannot fully observe all MNE financial operations due to coverage issues (Tørsløv *et al.*, 2023) or data cleaning (Appendix B), I can empirically test the incorporation behavior of treated MNEs compared to control MNEs. If the number of incorporations by the treated group

increases post-reform, it could partly explain the higher debt holdings at the group level.

To calculate the probability of incorporation, I divide the number of MNE incorporations in a particular jurisdiction by the total number of worldwide MNE incorporations in a given year. After winsorizing the data at 1% and 99%, I estimate equation (7) without unit-specific linear trends and compare the incorporations of treated groups against control groups. Table VIII shows that the average worldwide incorporations by the treated group increase by at least 2.5% post-reform. This suggests that MNEs may use the excessive debt, which is limited for tax-deductible interest expenses after the reform at the affected affiliates, to finance the establishment of new affiliates.

Next, to identify *where* the incorporations occur, I categorize jurisdictions into two groups: (1) jurisdictions where treated MNEs have at least one treated affiliate, and (2) jurisdictions where treated MNEs do not have any treated affiliates. I then re-estimate equation (7) without unit-specific linear trends twice: once for the first group of jurisdictions and a second time for the remaining group. In both estimations, I retain the full set of worldwide incorporations by the control group to estimate the differential responses in the two sets of jurisdictions by the treated MNEs. I hypothesize two potential locational responses:

1. **Decreased Attractiveness for Profit-Shifting:** ESR may render a jurisdiction less attractive for profit-shifting, leading to reduced investments and a shift of operations to jurisdictions without such regulations.

2. **Increased Local Incorporations**: The ease of asset transfer or the provision of group escape clauses in ESR, allowing deductions up to the net interest of the jurisdiction-wide group, may incentivize MNEs to incorporate new subsidiaries within the affected jurisdiction.

Table VIII shows that the difference-in-differences coefficient for incorporations in affected jurisdictions is 7.3%, compared to 4.6% in unaffected jurisdictions. This means that the treated group is 7.3% more likely to establish a new affiliate in the affected jurisdictions, conditional on setting up a new affiliate. One reason for this higher likelihood could be the ease of transferring assets to within-jurisdiction affiliates. However, group escape clauses could potentially play an even more significant role.[25] These clauses allow an ESR-failing affiliate to benefit from the group's overall interest deductions, helping the failing affiliate circumvent ESR rules by leveraging the group's interest deduction capacity.

To test this, I subdivide the sample of incorporations in affected jurisdictions into two groups: (1) jurisdictions with group escape clauses and (2) jurisdictions without them. Fig-

[25] If an MNE sets up new affiliates and allocates substantial debt to them, ensuring the interest deductions of these new entities do not exceed the *de-minimis* threshold, the ESR-failing affiliate can benefit from the overall groups interest deductions. This allows the failing affiliate to circumvent the ESR rules by leveraging the groups broader interest deduction capacity.

ure VIIf shows that the treated groups increase their incorporations into jurisdictions with group escape clauses more than into those without them. This strategy enables new entities to escape ESR due to the *de minimis* threshold while allowing the existing affected affiliate to escape ESR due to group taxation.

Finally, I explore an other dimension of heterogeneous responses, by comparing incorporation patterns based on whether the failing jurisdiction is the home jurisdiction of the MNE group or a foreign jurisdiction. Figure VIIe shows that incorporations are higher in the home jurisdiction if the MNE fails ESR there compared to a foreign jurisdiction highlighting home bias in the MNE operations. I test this hypothesis in more detail in the next section.

VII.5 Heterogeneity in Policy Impact

Given the heterogeneity in ESR policy elements across different jurisdictions, this project offers a unique opportunity to analyze how firms respond to different levels of policy strictness. I categorize the treated units into two sub-groups based on the degree of policy heterogeneity and perform the analysis twice: first, comparing the first sub-group of treated firms against the full sample of control firms, and second, comparing the second sub-group of treated firms against the same control group. In both estimations, I scale the outcomes by the mean pre-baseline of the respective treated group. By overlaying the dynamic responses from the two groups of treated firms, I can visually identify and analyze their differential responses to varying levels of policy strictness.

VII.5.1 Response to Gross vs. Net Interest Deduction Limits

Jurisdictions vary in their approach to restricting the deductibility of interest expenses. Some disallow deductions if gross financial expenses exceed a set threshold, while others apply the rule to net financial deductions, where financial income is subtracted from financial expenses. The net financial deductions rule is more lenient than the gross financial expense rule, leading to stronger responses under the gross rule.

Figure VIII presents the results of this analysis. I observe that the interest expenses of treated affiliates subject to the gross threshold decrease significantly more than those of affiliates subject to the net threshold. This reduction leads to a relatively greater in the taxable profits and consequently higher taxation for affiliates facing the gross expense rule. Additionally, real responses, such as the reduction in tangible fixed assets, increase in equity, and decline in revenues, are more pronounced for affiliates under the gross financial expense rule compared to those under the net financial expense rule.

VII.5.2 Impact of Group Escape Provisions

Jurisdictions that provide group escape clauses allow an ESR-affected affiliate (i.e., one with net interest deductions exceeding 30% of EBITDA) to deduct up to the level of net interest for its jurisdiction-wide or worldwide group. This creates stronger incentives for intra-group reallocation of debt and investments, as higher interest deductions at the affected affiliate can be offset against higher deductions at sister affiliates.

Figure IX shows that the reduction in debt and assets of affiliates in group escape regimes is significantly greater than for those without such provisions. Additionally, the reduction in capital and investments is also more pronounced for affiliates operating under a group escape regime, indicating substantial intra-group reallocation of debt and investments.

Since most of the reallocation from treated affiliates occurs toward local sister affiliates (see Section VII.2.2) and the majority of new incorporations are in ESR-enforcing jurisdictions with group escape regimes (see section VII.4), this analysis highlights the critical role that group escape clauses play by facilitating stronger reallocation of capital and investments to domestic affiliates (Andresen & Thorvaldsen, 2023).

Moreover, as the group escape clauses allow an ESR-failing entity to be tested on a group basis rather than affiliate basis, the failing entity does not need to significantly reduce its interest expenses, as the allowed deduction limit increases. This is evident in Figure IX, where the reduction in interest expenses for entities under group escape regimes is noticeably less than for those without these provisions. Consequently, these affiliates also end up paying relatively less in taxes.

VII.5.3 Domestic vs. Foreign Affiliates

Prior literature suggests a home bias in reactions to anti-avoidance policies, where responses are stronger for foreign affiliates and less pronounced for domestic affiliates (Bilicka *et al.*, 2022). I categorize affiliates as foreign or domestic based on the headquarters jurisdiction of the entire group and estimate the differential responses of the two groups. The results are presented in Figure X.

I observe that the reduction in debt and capital is much more pronounced for foreign affiliates compared to domestic affiliates, highlighting the volatility of foreign affiliates. The reduction in interest expenses is similar for both groups, indicating the policy's effectiveness in limiting profit shifting through the interest channel. However, I find that the taxation of domestic affiliates is significantly higher than that of foreign affiliates. Since the reduction in interest expenses is comparable across both groups, the lack of change in taxation for foreign affiliates suggests that these firms may be engaging in alternative tax avoidance strategies.

Finally, the greater increase in equity for domestic affiliates compared to foreign affiliates, along with the other results discussed, supports the hypothesis of home bias in re-

sponse to anti-tax avoidance policies among MNE affiliates.

VII.6 Robustness Checks

I verify the robustness of my baseline results by estimating the causal impacts of the policy using alternative staggered difference-in-differences methodologies in Appendix H. I implement two changes to the baseline estimation: (1) I designate $t^* - 1$ as the reference year and (2) I estimate the responses once without controlling for unit-specific linear trends and then again by fitting a linear pre-baseline trend using the years $t^* - 4$, $t^* - 3$, and $t^* - 2$, extrapolating it to post-treatment years, and differencing out the first-stage estimates from this trend.

These two-stage estimates allow me to directly test whether my baseline results are influenced by the choice of the baseline year (i.e., $t^* - 1$ or $t^* - 2$) and to assess whether the dynamic event study estimates from different methodologies remain similar before and after controlling for pre-baseline trends. The alternative estimation designs I use include the canonical two-way fixed effects (TWFE) approach, as well as methods proposed by Sun & Abraham (2021), Borusyak *et al.* (2024), Callaway & SantAnna (2021), and Cengiz *et al.* (2019).

The results of this exercise, presented in Figures H.I to H.XVIII, show that the dynamic estimates from all these estimators closely align. Furthermore, the similarity of the TWFE estimator with the other estimators indicates that my dynamic estimates do not suffer from negative estimation weights, as noted by Roth *et al.* (2023).

Additionally, I verify the robustness of my results by trimming and winsorizing the data at 1%, 2.5% and 5%. I also assess different ownership thresholds, including 50%, 75%, and 100%, as well as the inclusion of U.S. MNEs in the estimation data.[26] To ensure my findings are not confounded by mean reversion due to business cycles, I conduct further checks in Appendix C.3.2. I also confirm that my baseline results are not driven by any specific industry or jurisdiction, ensuring the generalizability of my findings.

VIII Conclusion

While tax avoidance and profit shifting by MNEs results in annual tax revenue losses of USD 100-240 billion, policies designed to curb such behavior often increase the cost of capital, leading to reduced corporate investments. This raises a critical question: can anti-tax avoidance measures effectively curb profit shifting without stifling investment and economic growth? I empirically answer this question by analyzing the causal impacts of Earnings Stripping Rules (ESR), announced by the OECD in 2015 and introduced by more than

[26] Please see Appendix D.2 for a discussion on the exlusion of these MNEs in the baseline estimation.

45 jurisdictions to date. ESR limit profit shifting through debt channels by capping interest expenses at 30% of corporate taxable earnings. Although ESR can increase tax revenues by restricting interest deductions, they also raise the cost of debt-financed capital, potentially reducing investments by MNEs. To study this tradeoff, I extract detailed financial data of MNEs from the ORBIS database for the years 2010-2021 and causally identify the effects of ESR on MNE operations and investments.

The empirical analysis reveals that the ESR implementation effectively reduces profit shifting and tax avoidance, albeit at the expense of lower investments in treated affiliates. However, MNEs compensate by reallocating their investments to other affiliates, leaving the overall group-level investments unaffected. I also find that the majority of investments shift to within-country sister affiliates below the ESR threshold, indicating limited capital flight and a preference for domestic reallocation where feasible. Moreover, the increase in group-level revenue and taxation indicates that ESR prompt MNEs to invest where capital is most productive, correcting previous capital misallocation. Hence, the introduction of ESR results in larger tax revenues without a significant decrease in investments. Additionally, the findings suggest intensified profit shifting through debt channels in countries without such policies.

The findings of my paper highlight that well-targeted anti-avoidance policies increase tax revenues without stifling investment when implemented in a coordinated manner. By aligning tax rules and enforcement strategies, countries can enhance tax revenues in jurisdictions where economic activity occurs, thereby mitigating the risk of capital flight and investment decline. However, jurisdictions which adopt much stricter versions of the policy, may inadvertently reduce investments due to higher capital costs. Conversely, the absence of such policies in high-tax jurisdictions may lead to an increase in profit-shifting and tax avoidance through debt channels in such locations. Therefore, a coordinated global approach to implementing largely homogeneous anti-tax avoidance policies is essential for ensuring fairness and efficiency in international taxation.

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IX Tables

TABLE I: **SUMMARY STATISTICS I - AFFILIATE LEVEL**

	Treated			Sister			Control		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Debt & Assets									
Debt	65.232	141.353	19,443	17.778	76.016	215,015	12.065	57.723	319,625
Equity	66.522	177.653	18,393	47.916	162.656	212,420	31.000	118.797	317,601
Capital	24.265	54.911	19,714	11.755	39.190	214,475	7.140	28.860	318,782
Fixed Assets	128.786	291.193	19,706	56.396	214.285	214,607	34.104	155.044	319,423
Tangible Assets	49.346	108.374	19,455	21.777	73.212	208,340	12.513	50.549	311,115
Intangible Assets	4.311	15.304	19,300	2.439	10.700	207,754	1.636	8.270	309,366
Total Assets	219.878	473.815	19,690	120.140	373.652	210,708	69.990	258.385	318,012
Gross Investments	4.722	15.020	16,020	3.114	11.661	159,263	2.107	8.863	262,005
Financials									
Revenue	134.933	303.119	19,705	112.807	291.721	207,044	68.192	204.647	297,827
EBITDA	10.234	27.172	19,066	7.805	24.135	212,078	4.905	17.515	317,343
Interest Expense	5.772	11.129	19,377	1.597	6.247	214,869	0.898	4.268	319,837
Tax Base	3.020	19.775	18,619	5.682	20.484	212,087	3.716	15.231	317,136
Taxation	0.948	3.737	17,865	1.168	3.816	200,180	0.785	2.875	301,756
Net Profits	2.312	17.014	18,482	4.533	17.010	210,296	2.957	12.682	314,828
Ratios									
Debt:Equity	2.406	4.763	19,749	1.032	3.257	215,750	0.913	2.890	318,511
Debt:Assets	0.318	0.325	19,779	0.145	0.244	215,671	0.141	0.219	318,488
Interest:EBITDA	0.209	1.190	18,628	0.079	0.654	213,202	0.069	0.591	316,234
Net Interest:EBITDA	0.175	1.379	19,012	0.016	0.847	213,478	0.017	0.791	315,574
Tax:Assets	0.004	0.017	18,719	0.015	0.026	202,042	0.017	0.026	298,925
Investment Rate	0.300	1.127	16,658	0.412	1.290	159,582	0.489	1.374	258,185

Notes: This table provides summary statistics of important variables at the affiliate level. The sample is restricted to MNE affiliates only and the Treated group includes MNE affiliates that failed jurisdiction-specific ESR test at the baseline year. The Sister group includes firms that do not fail the jurisdiction-specific ESR test at the baseline but were sister affiliates of treated firms. The Control group includes MNE affiliates that neither themselves failed any jurisdiction-specific ESR test in the baseline year nor were sister affiliates to any treated firm. SD is the standard deviation and N is the number of observations. *EBIT* is Earnings Before Interest and Tax, *EBITDA* is Earnings Before Interest, Tax, Depreciation and Amortization, *Tax Base* is the profit before taxes, and *Net Profits* are profits after tax. *Gross Investments* are defined as the current year change in investment spending in fixed assets, *Net Investments* are defined as Gross Investments net of depreciation, and *Investment Rate* is defined as the *Gross Investments* scaled by lagged *Fixed Assets*. All the variables are trimmed at 1% and 99%. All the variables apart from *Investment Rate* and *Ratios* are reported in millions of USD. The sample includes 4 pre-treatment years and 5 post-treatment years.

TABLE II: **SUMMARY STATISTICS II - GROUP LEVEL**

	Treated			Control		
	Mean	SD	N	Mean	SD	N
	(1)	(2)	(3)	(4)	(5)	(6)
Debt & Assets						
Debt	1630.216	3236.378	9,149	312.455	1270.590	55,042
Equity	2216.787	3916.817	8,909	543.564	1682.457	54,610
Capital	261.302	535.254	9,221	73.406	250.045	53,849
Fixed Assets	3806.344	7433.688	9,135	723.361	2705.551	54,392
Tangible Assets	1588.246	3075.671	8,934	322.603	1237.284	53,699
Intangible Assets	637.376	1442.524	8,914	128.376	579.585	53,970
Total Assets	6366.446	11840.352	9,135	1298.005	4405.167	54,402
Gross Investments	288.456	667.412	7,218	61.907	269.279	44,725
Financials						
Revenue	4212.869	7435.151	9,021	977.920	2934.886	53,177
EBITDA	481.751	919.340	8,971	112.478	385.421	54,572
Interest Expense	94.945	188.622	9,106	18.549	73.703	54,439
Tax Base	259.657	541.843	8,877	67.307	238.071	54,646
Taxation	70.312	142.303	8,828	17.358	60.295	54,245
Net Profits	191.193	412.540	8,865	50.072	181.379	54,546
Ratios						
Debt:Equity	1.122	1.526	9,466	0.784	1.306	54,701
Debt:Assets	0.280	0.177	9,509	0.208	0.171	54,666
Interest:EBITDA	0.275	0.593	9,328	0.158	0.462	54,214
Net Interest:EBITDA	0.134	0.624	9,369	0.063	0.521	54,173
Tax:Assets	0.012	0.013	9,476	0.016	0.016	53,583
Investment Rate	0.219	0.585	7,721	0.291	0.708	44,184

Notes: This table provides summary statistics of important variables at the group level. The sample is restricted to the consolidated reports of only the groups for which unconsolidated reports at the affiliate level are identified in the sample as well. Treated group includes MNEs that had at least one treated affiliate anywhere in the world and vice versa for the Control group. SD is the standard deviation and N is the number of observations. *EBIT* is Earnings Before Interest and Tax, *EBITDA* is Earnings Before Interest, Tax, Depreciation and Amortization, *Tax Base* is the profit before taxes, and *Net Profits* are profits after tax. *Gross Investments* are defined as the current year change in investment spending in fixed assets, *Net Investments* are defined as Gross Investments net of depreciation, and *Investment Rate* is defined as the *Gross Investments* scaled by lagged *Fixed Assets* All the variables are trimmed at 1% and 99%. All the variables apart from *Investments* and *Ratios* are reported in millions of USD. *Investments* are reported as the ratio of gross investment spending and beginning of year net-book value of tangible assets. The sample includes 4 pre-treatment years and 5 post-treatment years.

TABLE III: **CAPITAL & INVESTMENTS - TREATED AFFILIATES**

Dep. Var	Debt	Equity	Capital	Fixed Assets	Tangible Assets	Intangible Assets	Total Assets	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Failed_i \times Post_t$	-14.19***	-3.69	-3.96***	-15.25***	-5.70***	-0.95**	-25.85***	-1.82***
	(2.57)	(2.54)	(0.89)	(2.85)	(1.49)	(0.47)	(5.07)	(0.46)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	67.52	65.48	24.08	130.86	49.49	4.30	217.10	5.86
Observations	677,593	670,422	677,222	677,712	660,011	652,877	674,781	571,463
Switchers	11,923	10,733	12,147	12,154	11,833	11,678	12,127	8,736
Percentage Change	21.02	5.64	16.45	11.65	11.52	22.16	11.91	31.03

Standard errors in parentheses

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

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the affiliate level. The outcomes of treated affiliates are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the treated affiliates in the reference year. I include firm and year fixed effects, firm-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust 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: **OPERATIONS - TREATED AFFILIATES**

Dep. Var	Revenue	EBITDA	Interest	Tax Base	Taxation	Net Profits
	(1)	(2)	(3)	(4)	(5)	(6)
$Failed_i \times Post_t$	-7.35**	1.76***	-1.36***	1.75***	0.23*	2.01***
	(3.39)	(0.61)	(0.35)	(0.60)	(0.13)	(0.54)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	134.48	8.60	5.85	2.01	0.75	1.67
Observations	633,585	667,348	677,280	666,777	624,838	660,755
Switchers	12,131	11,011	11,673	10,594	10,063	10,498
Percentage Change	5.46	20.42	23.18	87.19	31.00	120.04

Standard errors in parentheses

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

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the affiliate level. The outcomes of treated affiliates are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the treated affiliates in the reference year. I include firm and year fixed effects, firm-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust and clustered at firm level. *Interest* is the gross financial expenses, *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 V: CAPITAL & INVESTMENTS - SISTER AFFILIATES

Dep. Var	Debt	Equity	Capital	Fixed Assets	Tangible Assets	Intangible Assets	Total Assets	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Failed_i \times Post_t$	-0.03	1.56	0.08	0.40	0.93**	0.33***	2.20	0.42***
	(0.90)	(1.03)	(0.31)	(1.30)	(0.38)	(0.08)	(2.22)	(0.12)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	18.95	48.24	12.43	58.12	21.99	2.45	125.00	3.16
Observations	778,572	770,549	777,850	778,440	757,963	750,385	773,489	648,569
Switchers	110,354	108,229	110,297	110,386	107,449	106,794	108,398	83,234
Percentage Change	0.17	3.24	0.67	0.68	4.23	13.57	1.76	13.36

Standard errors in parentheses

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

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the affiliate level. The outcomes of sister affiliates are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the sister affiliates in the reference year. I include firm and year fixed effects, firm-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust 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 VI: CAPITAL & INVESTMENTS - GROUP LEVEL

Dep. Var	Debt	Equity	Capital	Fixed Assets	Tangible Assets	Intangible Assets	Total Assets	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Failed_i \times Post_t$	255.43***	357.45***	31.67***	634.68***	331.41***	66.35**	1153.86***	10.89
	(77.74)	(106.07)	(9.89)	(151.92)	(70.18)	(30.89)	(263.43)	(38.43)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	1495.79	2062.78	244.83	3461.87	1455.23	569.19	5913.39	232.64
Observations	120,701	119,078	119,493	118,971	117,491	118,408	118,752	93,928
Switchers	5,118	4,931	5,216	5,120	5,019	4,979	5,139	3,358
Percentage Change	17.08	17.33	12.94	18.33	22.77	11.66	19.51	4.68

Standard errors in parentheses

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

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the group level. The outcomes of treated groups are compared with the control group. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the treated groups in the reference year. I include group and year fixed effects, group-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust and clustered at group level. *Observations* is the number of observations used in the estimation, and *Switchers* is the number of MNE groups for which treatment status changed from 0 to 1.

TABLE VII: **OPERATIONS - GROUP LEVEL**

Dep. Var	Revenue (1)	EBITDA (2)	Interest (3)	Tax Base (4)	Taxation (5)	Net Profits (6)
$Failed_i \times Post_t$	902.06***	95.18***	14.33**	23.61	12.65***	21.00
	(192.54)	(28.12)	(5.89)	(18.62)	(4.31)	(16.38)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	3830.09	440.81	87.54	233.51	63.43	168.81
Observations	117,217	118,111	117,841	118,136	116,299	117,801
Switchers	5,064	4,836	4,968	4,682	4,583	4,687
Percentage Change	23.55	21.59	16.37	10.11	19.94	12.44

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the group level. The outcomes of treated groups are compared with the control group. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the treated groups in the reference year. I include group and year fixed effects, group-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust and clustered at group level. *Interest* is the gross financial expenses, *Observations* is the number of observations used in the estimation, and *Switchers* is the number of MNE groups for which treatment status changed from 0 to 1.

TABLE VIII: **NEW INCORPORATIONS - GROUP LEVEL**

Dep. Var	Global (1)	Affected Countries (2)	Unaffected Countries (3)
$Failed_i \times Post_t$	0.025***	0.073***	0.046***
	(0.009)	(0.015)	(0.010)
Firm FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	63,341	55,859	67,994
Switchers	10,317	1,033	8,463

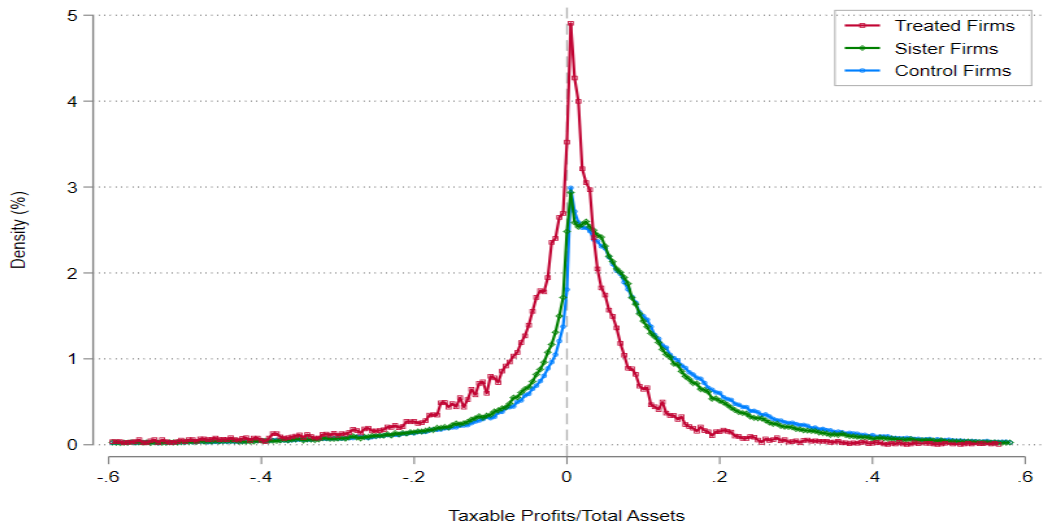
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR on the group level new incorporations. The incorporations of treated groups are compared with the control group. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The estimates are reported in percentage changes. I control for group and year fixed effects, and time varying jurisdiction level controls. The standard errors are robust and clustered at the group level. *Observations* is the number of observations used in the estimation and *Switchers* is the number of MNE groups for which treatment status changed from 0 to 1.

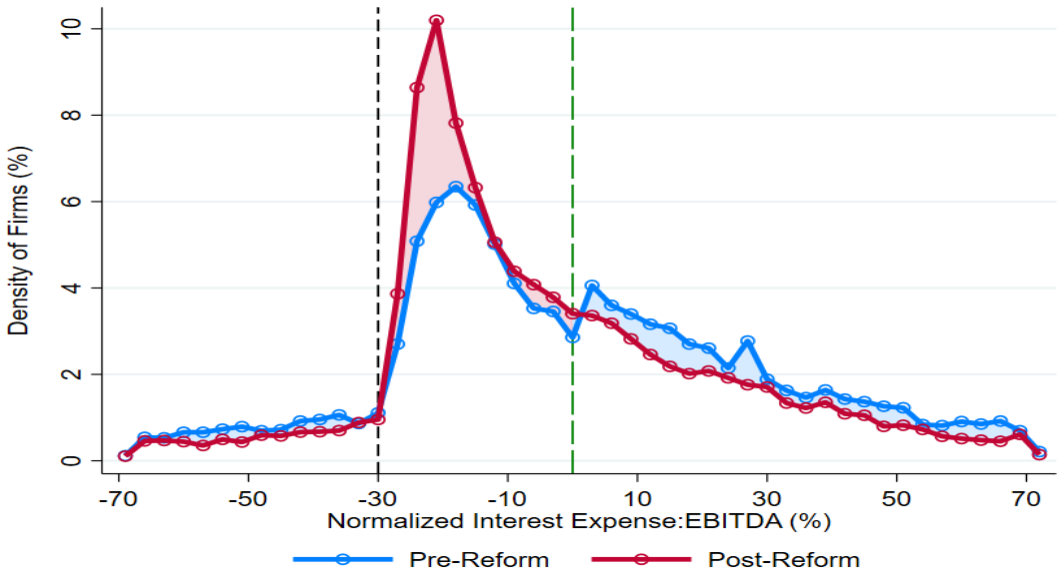
X Figures

FIGURE I: **STYLIZED FACTS - AFFILIATE LEVEL**

(A) **PROFIT SHIFTING**

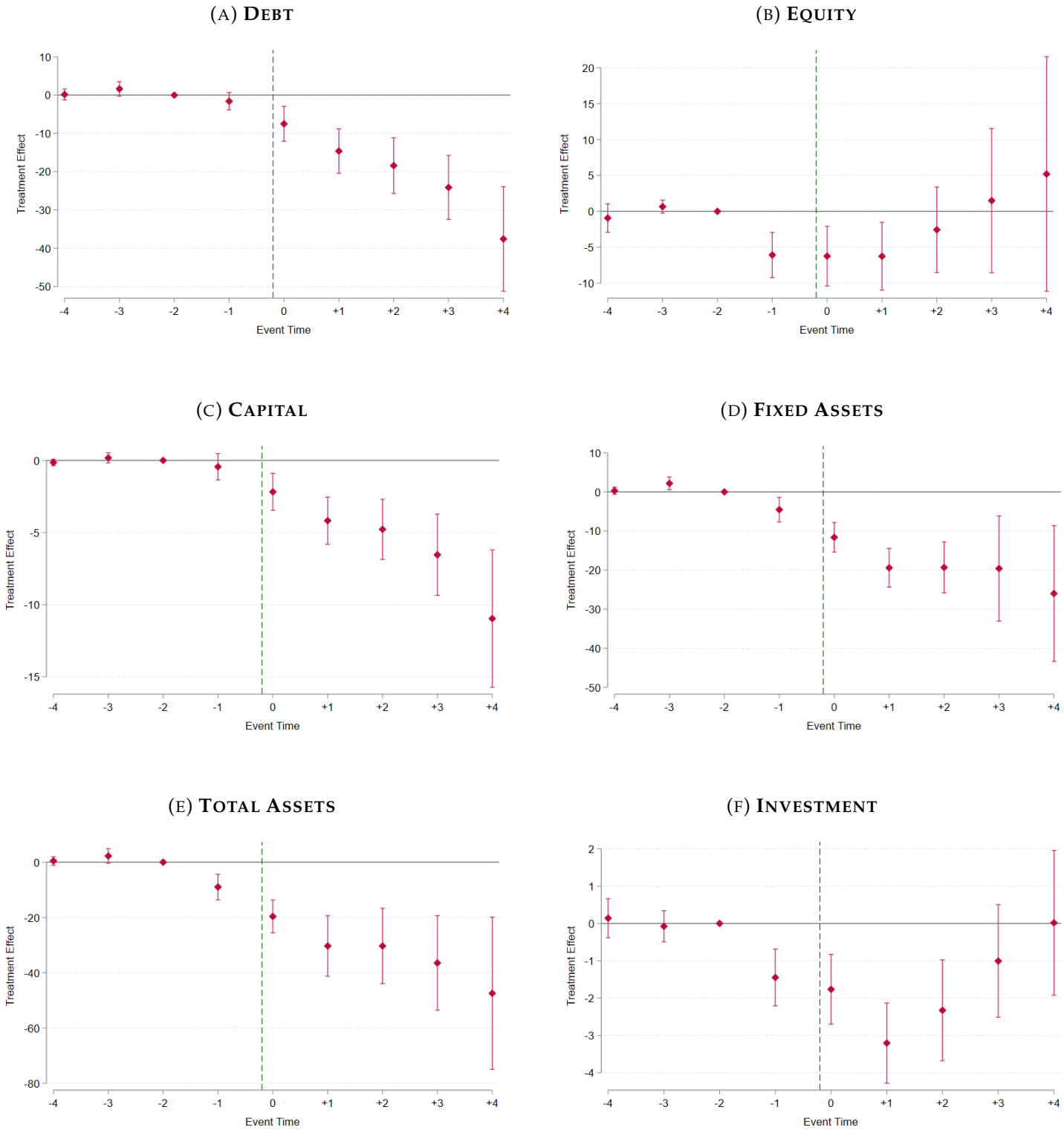


(B) **BUNCHING AT EXEMPTION THRESHOLD**



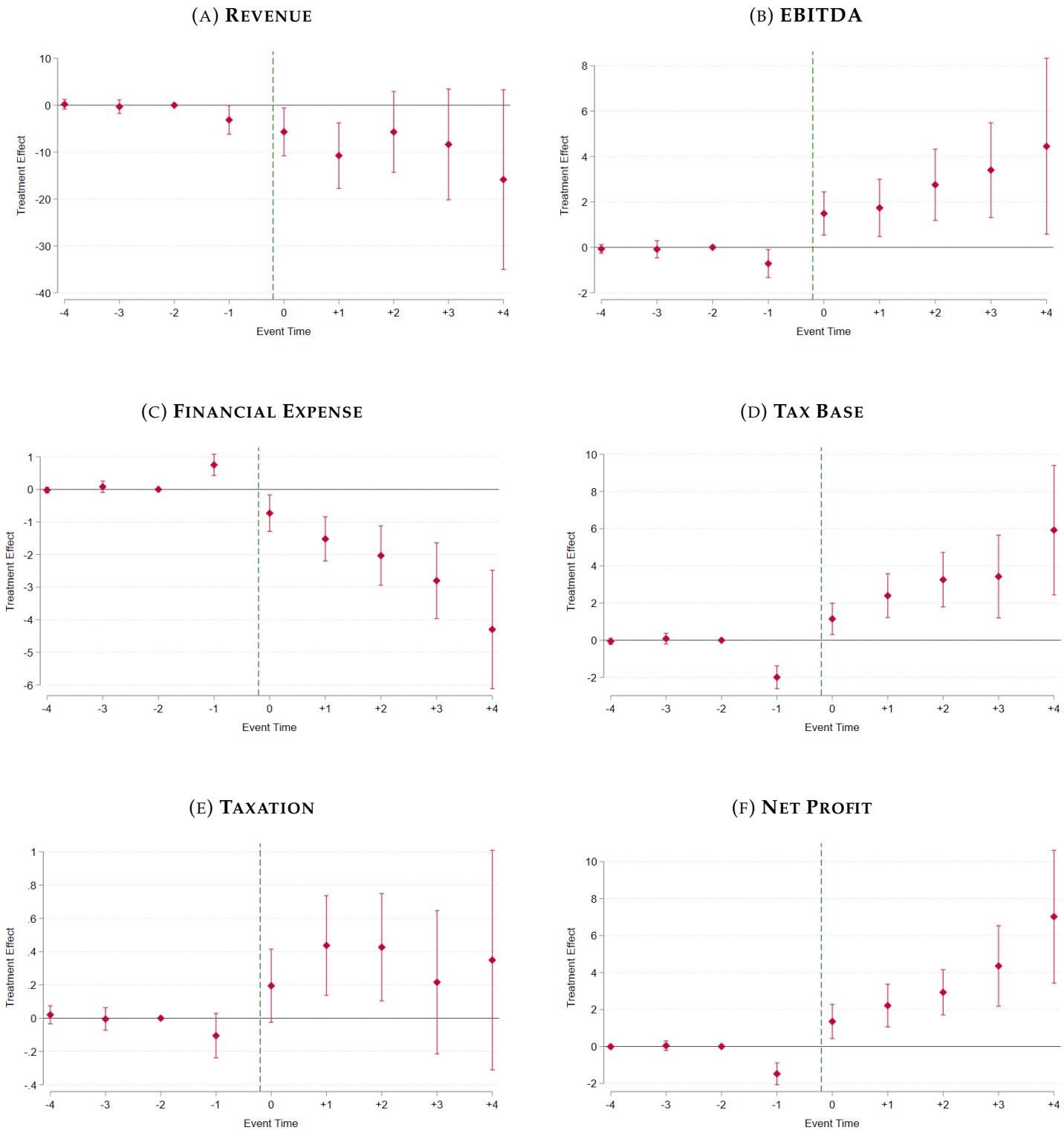
Notes: Panel (A) plots the pooled distribution of the ratio of taxable profits to total assets for treated affiliates, sister affiliates, and control affiliates. The ratio is winsorized at the 1st and 99th percentiles, restricted between -0.6 and 0.6, and divided into 200 bins. Panel (B) displays the pre and post-treatment pooled distribution of the normalized interest expense-to-EBITDA ratio of the affiliates that failed the jurisdiction-specific *de-minimis* threshold and the *debt-to-equity* test, if applicable. The bin size is 0.03, and the range of the ratio is between -0.7 and 0.7. In both the panels, the sample is restricted to 4 pre-treatment years and 5 post-treatment years.

FIGURE II: **CAPITAL & INVESTMENTS - TREATED AFFILIATES**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of treated affiliates are compared with that of control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

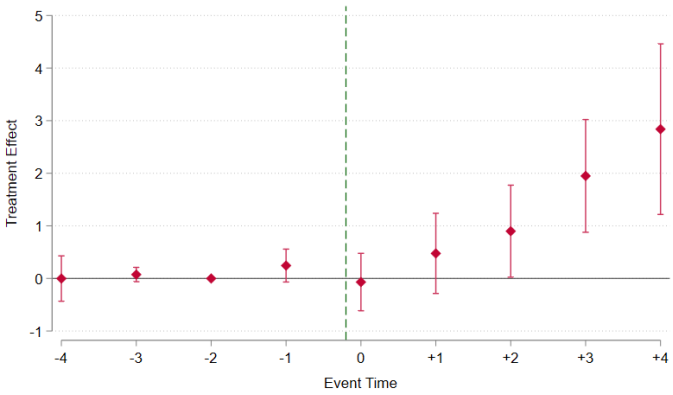
FIGURE III: **OPERATIONS - TREATED AFFILIATES**



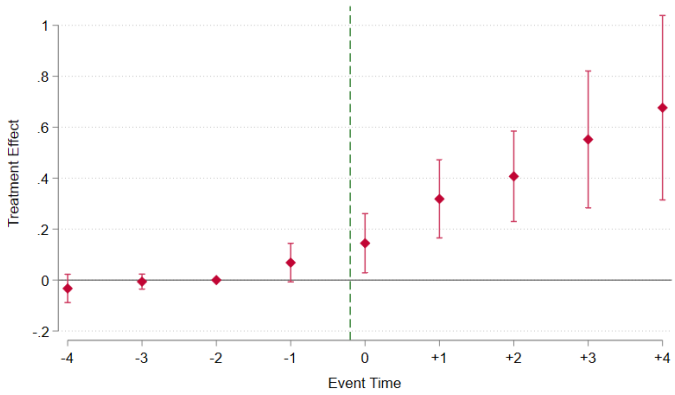
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of treated affiliates are compared with that of control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate 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: **INVESTMENT & OPERATIONS - SISTER AFFILIATES**

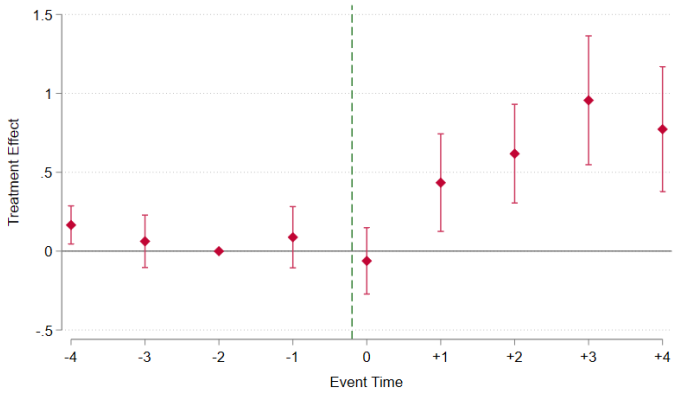
(A) **TANGIBLE ASSETS**



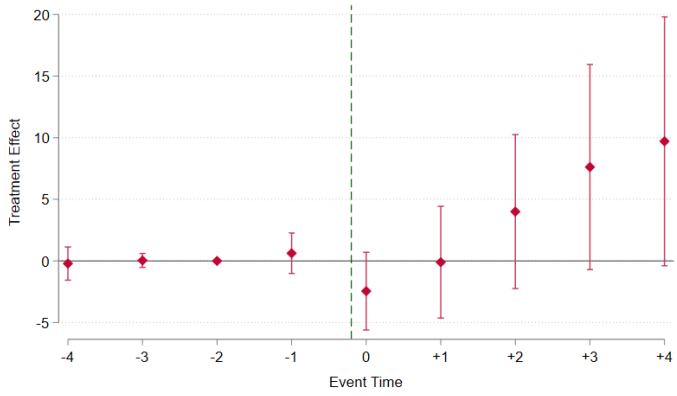
(B) **INTANGIBLE ASSETS**



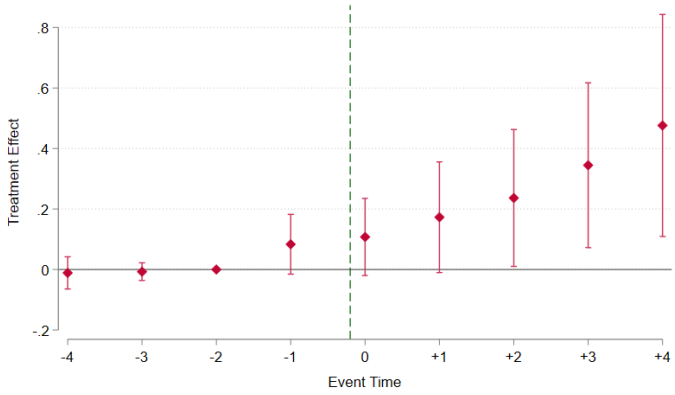
(C) **INVESTMENT**



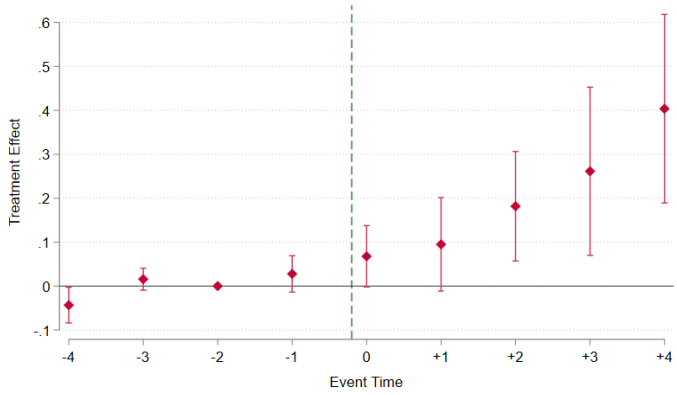
(D) **REVENUE**



(E) **FINANCIAL EXPENSE**



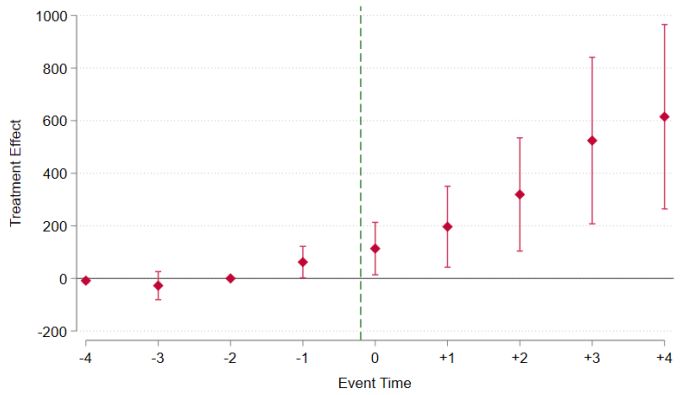
(F) **TAXATION**



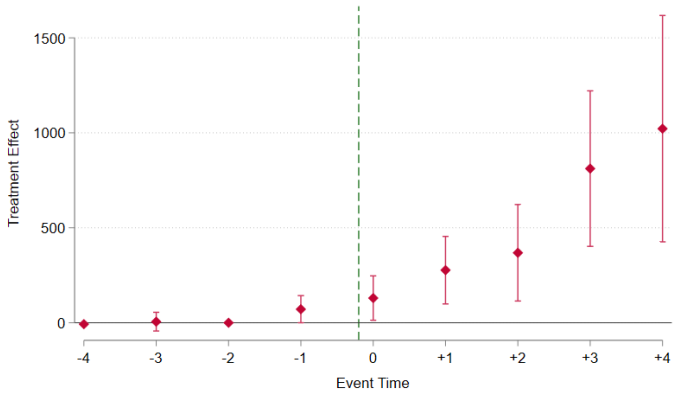
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of sister affiliates are compared with that of control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate 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: **CAPITAL & INVESTMENTS - GROUP LEVEL**

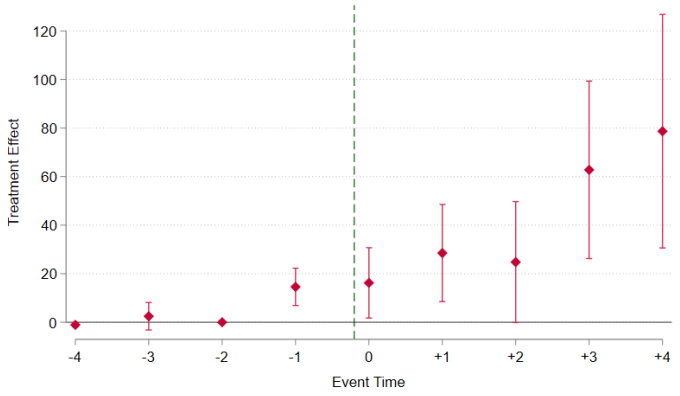
(A) **DEBT**



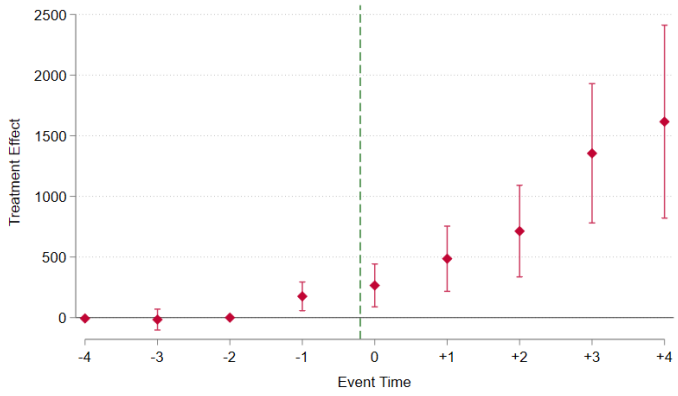
(B) **EQUITY**



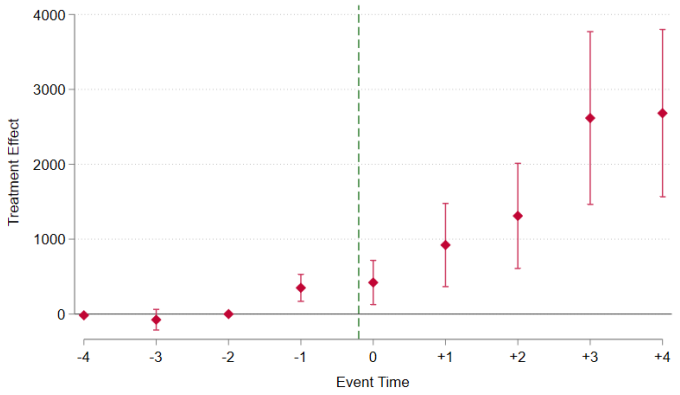
(C) **CAPITAL**



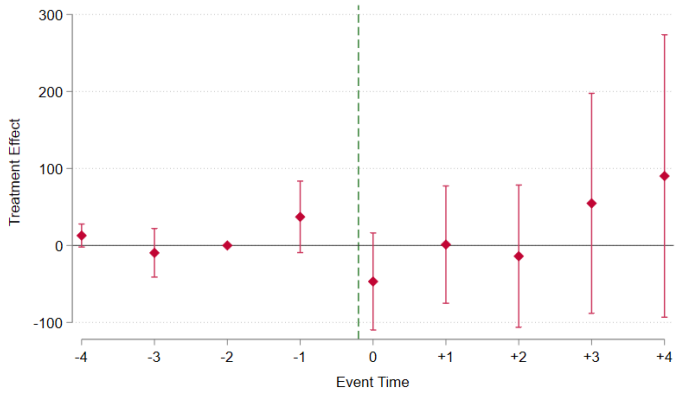
(D) **FIXED ASSETS**



(E) **TOTAL ASSETS**

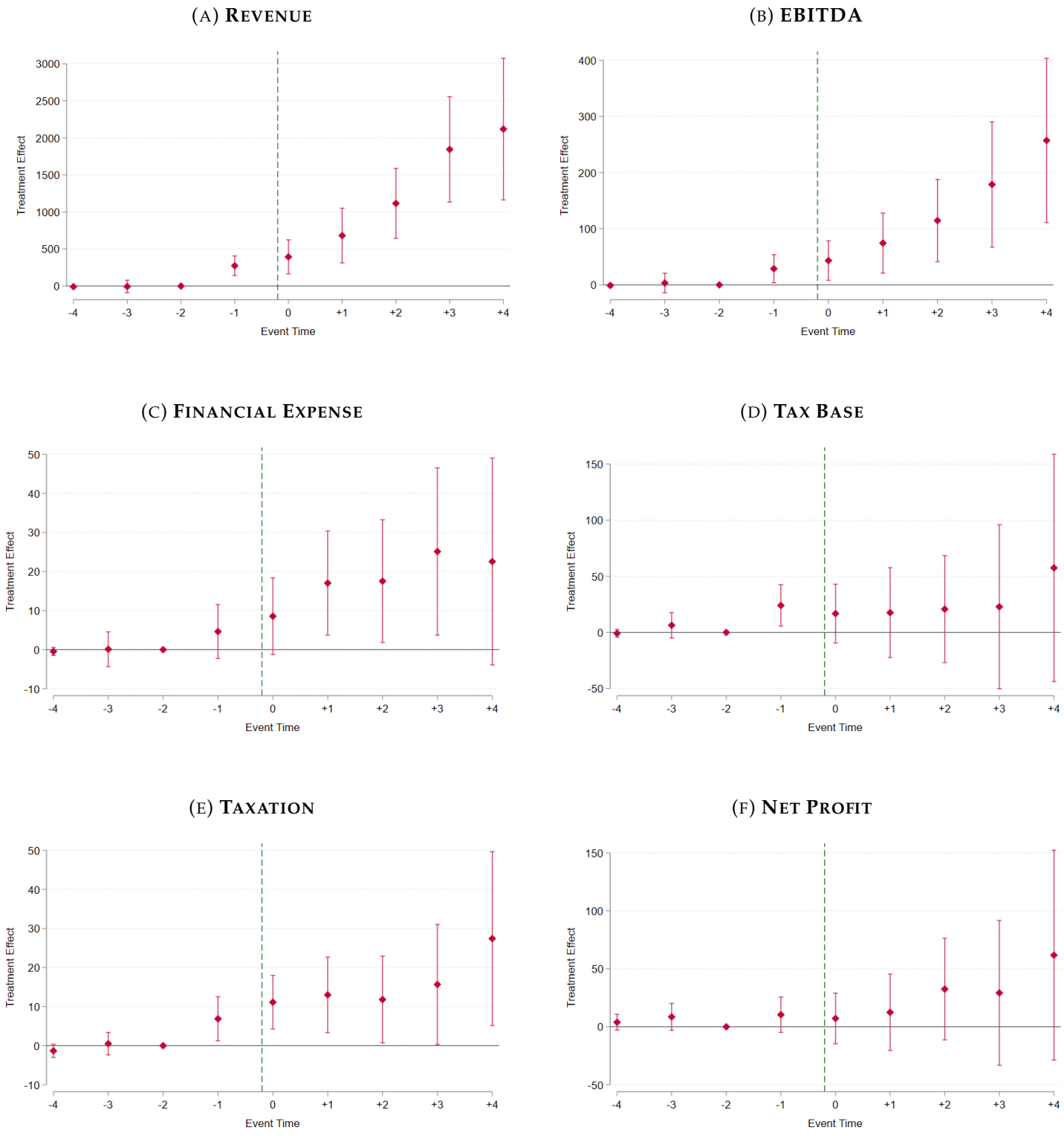


(F) **INVESTMENT**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the MNE group level. The outcomes of treated groups are compared with that of control group. The reference year is $t^* - 2$ and I include group and year fixed effects, group-specific linear trends, 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 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.

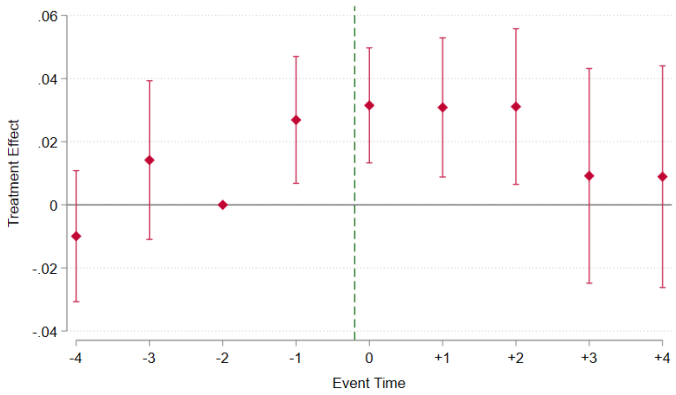
FIGURE VI: **OPERATIONS - GROUP LEVEL**



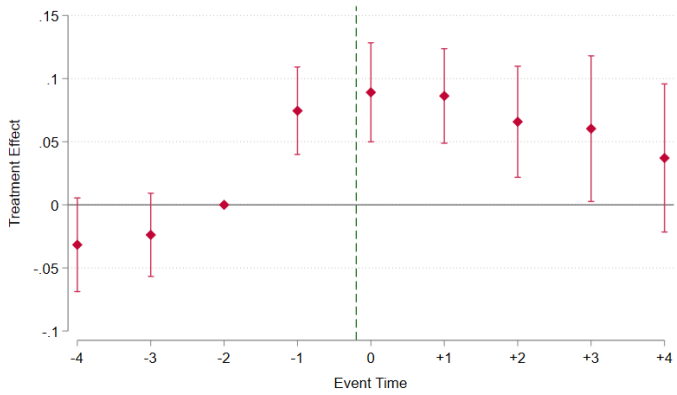
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the MNE group level. The outcomes of treated groups are compared with that of control group. The reference year is $t^* - 2$ and I include group and year fixed effects, group-specific linear trends, and time-varying jurisdiction-level controls. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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 VII: **NEW INCORPORATIONS - GROUP LEVEL**

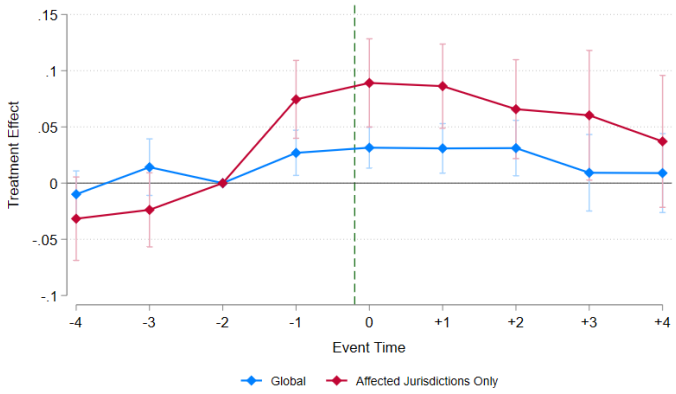
(A) **GLOBAL INCORPORATIONS**



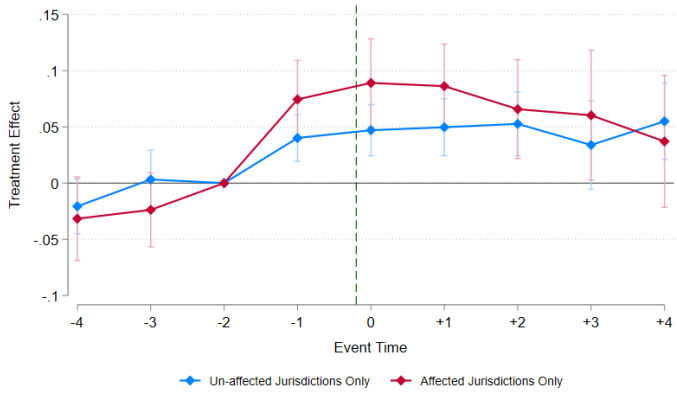
(B) **AFFECTED JURISDICTIONS ONLY**



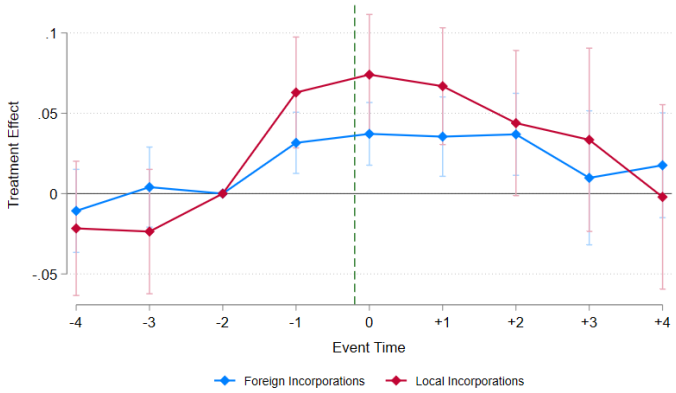
(C) **GLOBAL VS AFFECTED JURISDICTIONS**



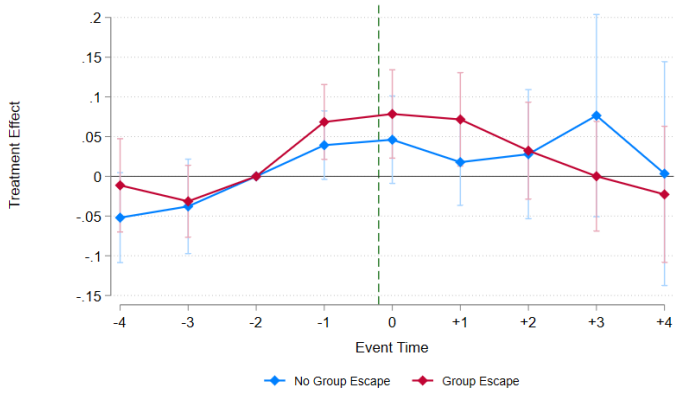
(D) **AFFECTED VS UNAFFECTED JURISDICTIONS**



(E) **LOCAL VS FOREIGN INCORPORATIONS**



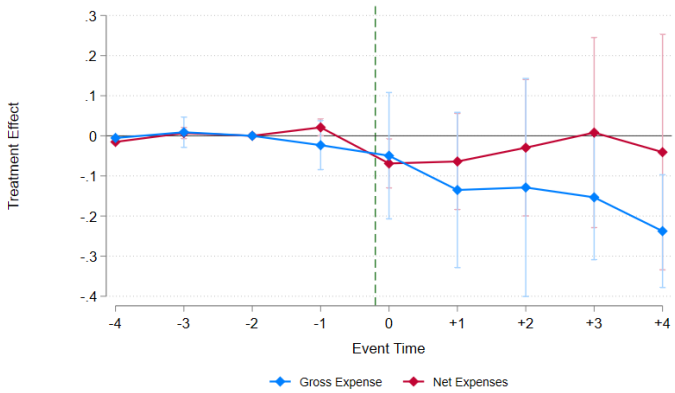
(F) **EFFECT OF GROUP ESCAPE**



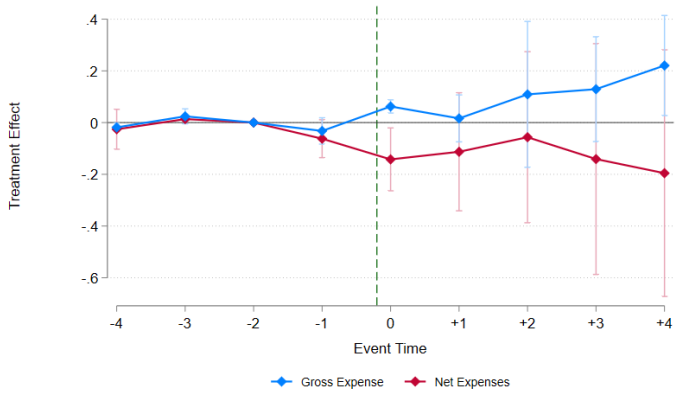
Notes: This graph plots the dynamic average treatment effects of ESR on the group level new incorporations. The incorporations of treated groups are compared with that of control group. The reference year is $t^* - 2$ and I include group fixed effects, year fixed effects and and time-varying jurisdiction-level controls. The estimates are reported in percentage changes and the standard errors are robust and clustered at group level. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction. In the top two panels, baseline dynamic diff-in-diff coefficients are plotted, while in the remaining panels, the baseline diff-in-diff coefficients are estimated for two subsamples and then overlaid in a single graph to aid in visually assessing the differential responses. I provide additional details in Results section (VII.4).

FIGURE VIII: **HETEROGENEITY IN DEDUCTIONS TYPE**

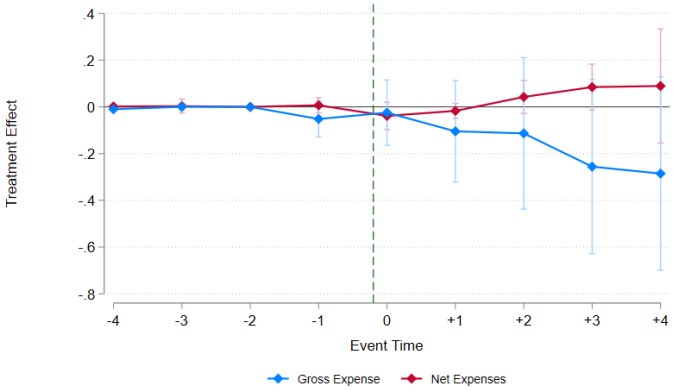
(A) **TANGIBLE ASSETS**



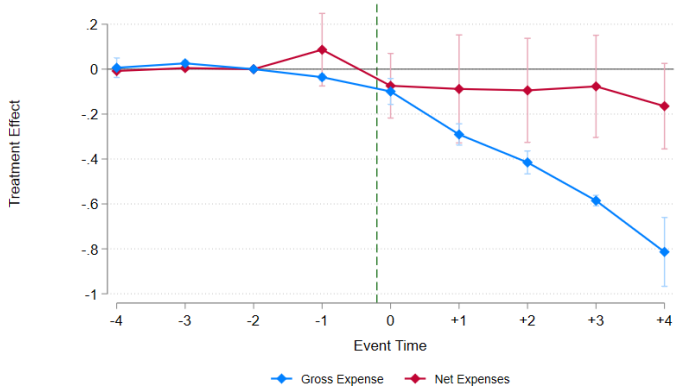
(B) **EQUITY**



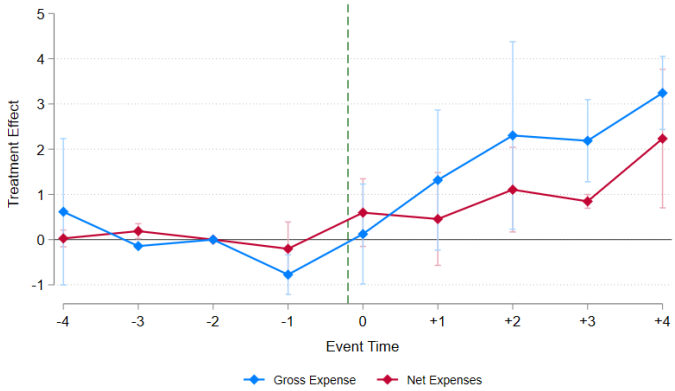
(C) **REVENUE**



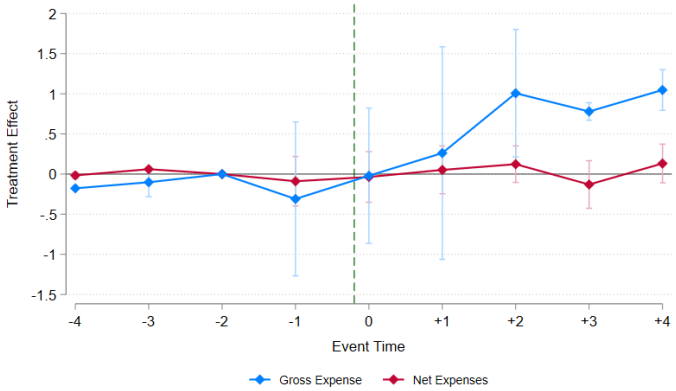
(D) **FINANCIAL EXPENSE**



(E) **TAX BASE**

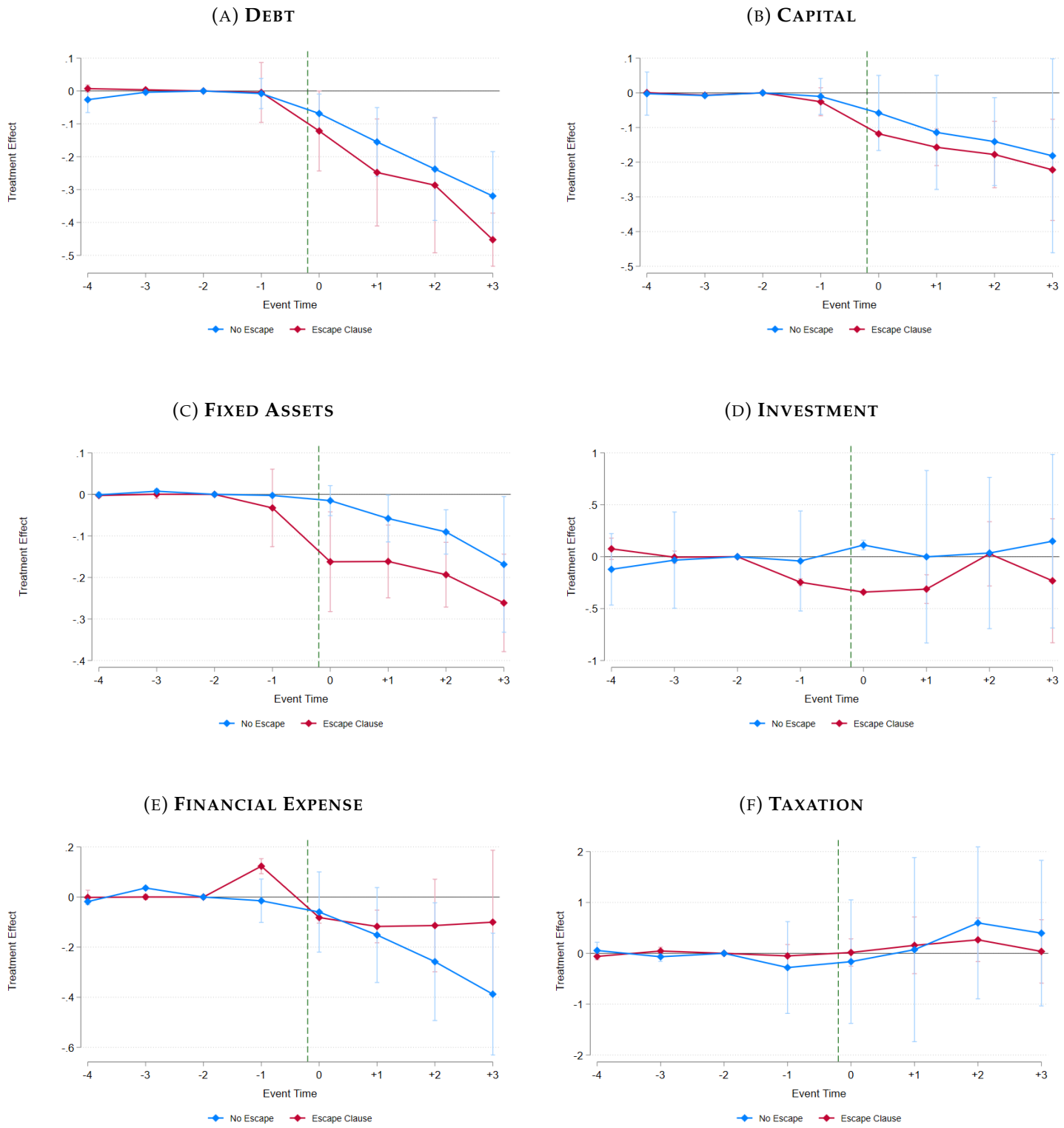


(F) **TAXATION**



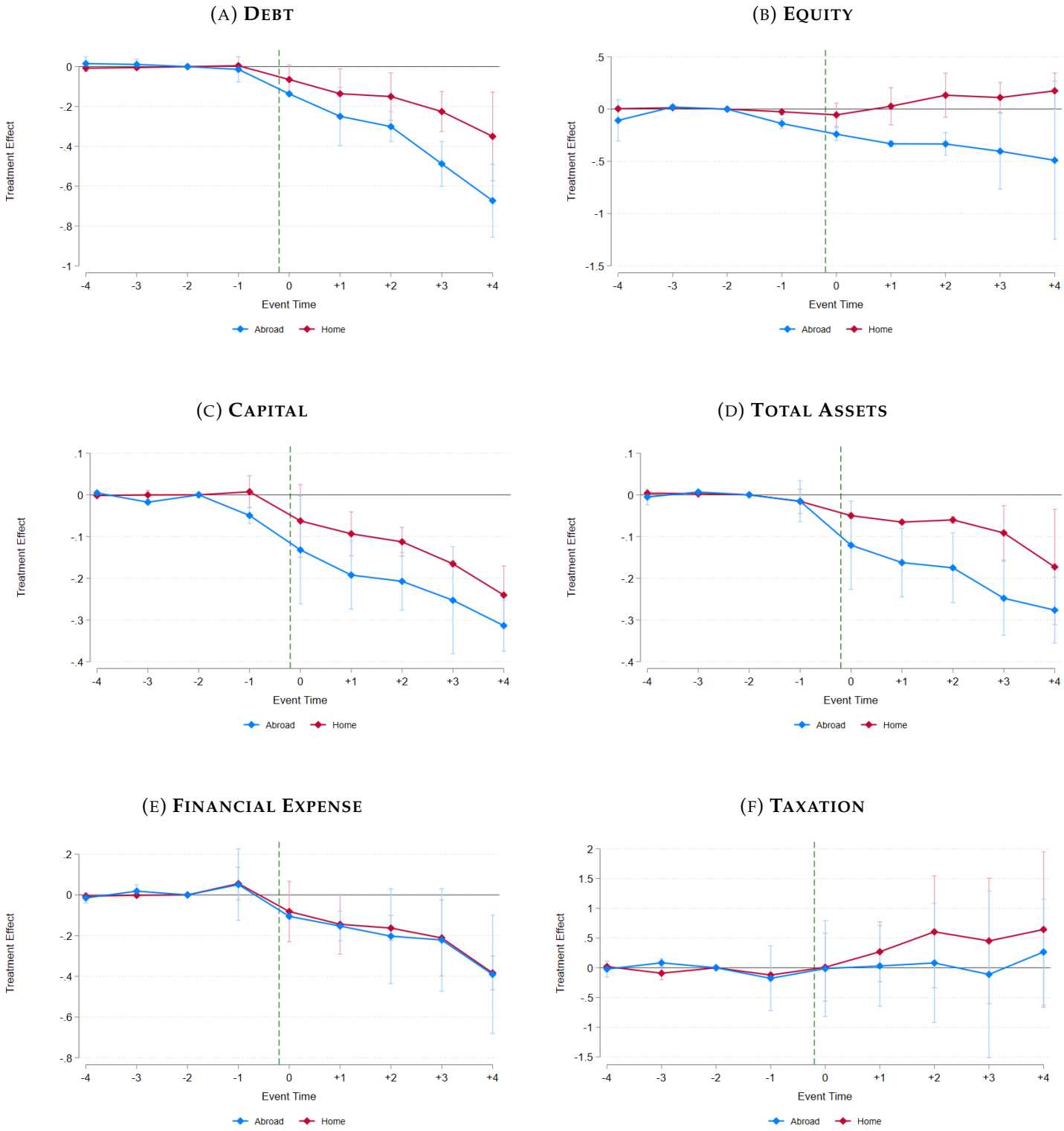
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of treated affiliates in jurisdictions with net interest expense rule are compared with the control group and 2) the outcomes of treated affiliates in jurisdictions with gross interest expense rule are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE IX: **HETEROGENEITY IN RESPONSES TO GROUP ESCAPE PROVISIONS**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of treated affiliates in jurisdictions with group escape provisions are compared with the control group and 2) the outcomes of treated affiliates in jurisdictions without these provisions are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

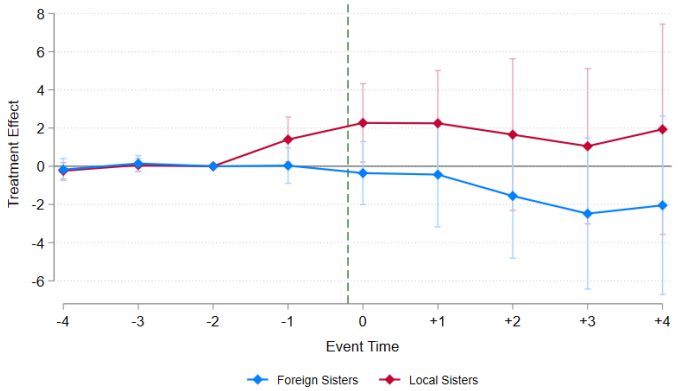
FIGURE X: **HETEROGENEITY IN RESPONSES OF DOMESTIC VS FOREIGN AFFILIATES**



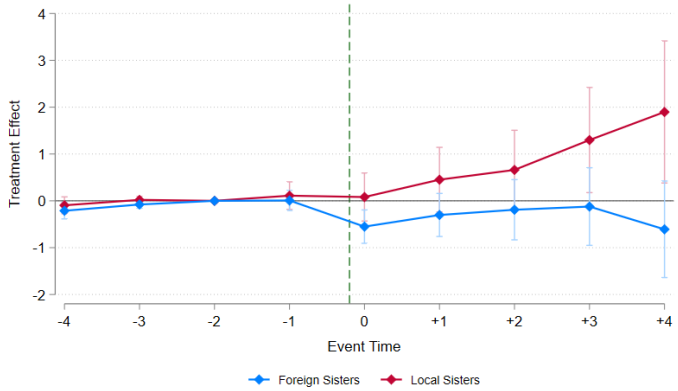
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of treated affiliates in home jurisdictions are compared with the control group and 2) the outcomes of treated affiliates in foreign jurisdictions are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE XI: **HETEROGENEITY IN REALLOCATION - LOCAL VS FOREIGN SISTER AFFILIATES**

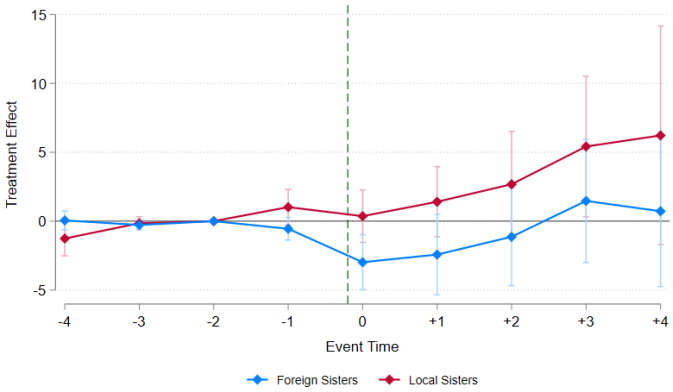
(A) **DEBT**



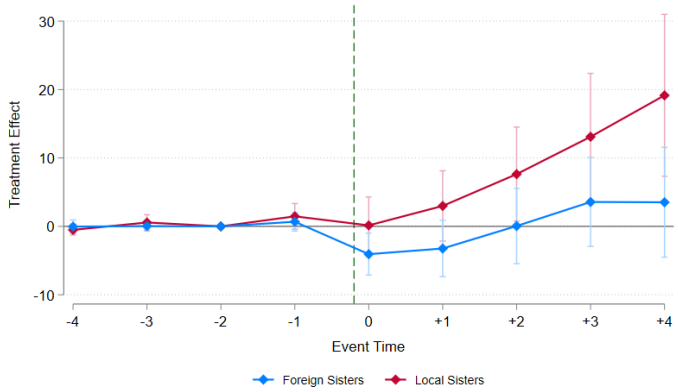
(B) **CAPITAL**



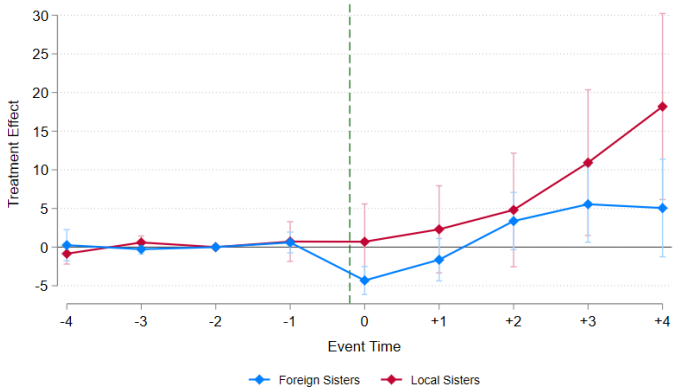
(C) **FIXED ASSETS**



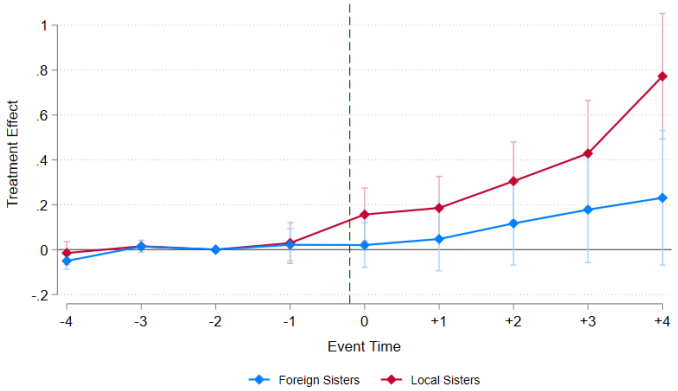
(D) **TOTAL ASSETS**



(E) **REVENUE**



(F) **TAXATION**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of local sister affiliates are compared with the control group and 2) the outcomes of foreign sister affiliates are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

Appendix

A Jurisdiction Level ESR Deatils

In this appendix, I provide a detailed overview of the Earnings Stripping Rules (ESR) implemented by various jurisdictions over the past decade. A key aspect of Action 4 of the OECD's Base Erosion and Profit Shifting (BEPS) initiative was the extension of the definition of financial expenses that also includes expenses that are economically equivalent to interest, such as financial lease charges, foreign exchange gains and losses, and service fees on financial products. Moreover, Action 4 recommended that these rules shall apply not only to related-party loans but also to third-party loans.[27] Additionally, two escape clauses, namely, *Equity Escape* and *Group Escape* were also recommended along with provisions to carry forward disallowed interest expenses and unused deductions capacity in future periods.[28]

As a result, I consider the policy to be effective in a jurisdiction not necessarily in the year when the 30% of EBITDA rule was first implemented but rather when these broader definitions, as advised by Action 4, were introduced. For instance, some jurisdictions initially implemented these rules only partially: Poland introduced ESR in 2015 alongside existing Thin Capitalization Rules (TCR) but allowed companies to choose which regime they wanted to be taxed under, Finland introduced ESR in 2014 but the scope was limited to related-party loans, Portugal and Greece introduced ESR in 2013 and 2014, respectively, but the rules were not fully aligned with OECD recommendations and the ATAD agreement, i.e., the definition of exceeding financial costs was neither robust nor broad, etc.[29] However, Poland revoked the right to election in 2018, Finland expanded the scope to include non-related-party debt in 2019, and Portugal and Greece adopted broader definition of financial expenses, i.e., including those economically equivalent to interest, in 2019. Therefore, I define the policy year as 2018 for Poland, and 2019 for Greece, Finland, and Portugal.

Moreover, some jurisdictions adjusted their thresholds after the policy introduction. For example, Romania introduced ESR in 2018, allowing net interest expenses up to 30% of EBITDA and setting a *de-minimis* threshold of EUR 1 million. However, for 2018, the deductions of excess borrowing costs were limited to 10% of tax-adjusted EBITDA for amounts exceeding EUR 200,000. As such, I define the treated firms in Romania based on the 2018

[27] This broader definition and scope of ESR represent a significant departure from earlier policies, targeting firms that engage in aggressive tax avoidance more comprehensively.

[28] Equity Escape allows a firm exemption from ESR if its equity ratio, i.e., equity:total assets is higher than that of the entire group. Similarly, Group Escape allow a firm that fails the ESR test to deduct up to the net interest on third party loans to EBITDA of the consolidated group.

[29] I define "exceeding" borrowing costs as "net" financial expenses, i.e., the difference between financial expenses and financial income.

rule when the reform was fully implemented, even though the policy parameters were not entirely aligned with OECD recommendations.

Since the dataset spans from 2010 to 2021 and Germany and Italy implemented these rules in 2008, the policy details for these two jurisdictions are not included here. Additionally, while South Africa and Ivory Coast introduced ESR in 2014 and 2019, respectively, data on firms from these jurisdictions is not available in the estimation sample. Therefore, their policy details are also excluded from this section. For all the remaining rule-enforcing jurisdictions, the policy details are mentioned below.

- **Albania:** introduced these rules from January 1st, 2018. The rule limited an entity's deductible expenses due to interest on loans from related parties to 30% of its net profit before interest, depreciation, and amortization.[30] The excess interest expenses can be carried forward by 5 years.

- **Argentina:** introduced these rules on January 1st, 2018. It allowed gross interest expense of 30% of EBITDA and set a *de-minimis* threshold of 1 million ARS. The excess interest expenses can be carried forward by up to 5 years while the unused capacity can be carried forward to up to 3 years. 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.[31]

- **Austria:** introduced these rules on January 1, 2021. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The excess interest expenses can be carried forward indefinitely. It also had a grandfathering clause for the loans issued prior to 17 June, 2016. The rule does not apply to stand alone companies. It also had a provision of group escape, i.e., if equity:assets is $\geq$ equity:assets of the group. Its considered equal if the taxpayer's equity over its total assets is lower by up to two percentage points of that of the group.

- **Belgium:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The excess interest expenses can be carried forward indefinitely. It also had a grandfathering clause for

[30] Since its difficult to establish the loans from related parties with the available data, I drop the analysis of Albania from this exercise.

[31] Although I cannot work out the exemption clause through the available data, I still use the data on Argentinian firms to identify the treated group as it is possible to do so given the baseline treatment definition.

the loans issued prior to 17 June, 2016. There are some other escape provisions as well.[32] However, I define the rule at the most basic and baseline definition.

- **Botswana:** introduced these laws on July 1st, 2019. The legislation is based on a fixed ratio rule which limits an entity's net interest deductions to 30% of its earnings before interest, taxes, depreciation and amortization (EBITDA) except for a company whose main business is banking or insurance.

- **Bulgaria:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The excess interest expenses can be carried forward indefinitely. A parallel 3:1 debt-to-equity ratio rule is also present.

- **Canada:** introduced these rules on October 1st, 2023 and, hence, is out of the scope of this study.

- **Costa Rica:** introduced these rules on January 1st, 2021. It allowed gross interest expense of 30% of EBITDA. The allowed percentage is to gradually decrease down by 2% each year to 20%.

- **Croatia:** introduced these rules on January 1st, 2019. The rules limited the deductibility of interest expenses to 30% of EBITDA or a EUR 3 million safe harbor limit. The excess interest expenses can be carried forward to up to three years. The rule doesn't apply to stand-alone or financial companies. An exclusion is provided in respect of loans used to finance long-term infrastructure projects. The excess interest expense can be carried forward to three years.

- **Cyprus:** introduced these rules on January 1, 2019. The rules limited the deductibility of net interest expense to the greater of 30% of EBITDA and EUR 3 million. There's also a grandfathering clause for the loans concluded before 17 June, 2016 and an exclusion from scope of long-term infrastructure projects which are considered to be in the general public interest. There is also a group-escape clause which kicks in if the ratio of taxpayer's equity to total assets is equal or not less than 2% points of that of the whole group. The excess interest expenses and unused capacity can be carried forward by up to 5 years.

[32] like 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.

- **Czech Republic:** introduced these rules on April 1, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 80 million CZK. The excess interest expenses can be carried forward indefinitely. The rule does not apply to stand alone companies. A parallel 4:1 debt-to-equity ratio rule is also present on related party loans only.[33]

- **Denmark:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 22313400 DKK. The excess interest expenses can be carried forward indefinitely. The rule also applies to stand alone companies. A parallel debt:equity of 4:1 and greater than 2.3% of tax values of certain qualifying assets (mainly operating assets) exist as escape clauses.[34]

- **Estonia:** introduced these rules on January 1, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The rule does not apply to stand alone companies. The group escape clause is activated if the company asks to have its tax liability calculated at the consolidated group level and if the loan is used to finance long-term public sector infrastructure projects and the project developer, borrowing costs, assets and income are all within the EU.[35]

- **Finland:** introduced these rules on January 1st, 2019. It allowed net interest expense of 25% of EBITDA and set a *de-minimis* threshold of 0.5 million EUR for related party loans and 3 million EUR for non-related parties.[36] The rule does not apply to stand alone companies. The excess interest expense could be carried forward indefinitely and the policy also entails a grandfathering clause for loans issued prior to 17/06/2016. The group escape clause is activated if the equity ratio of the company is higher than the consolidated equity ratio of the whole group.

- **France:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The excess interest expense could be carried forward indefinitely and the unused capacity can be carried forward to up to 5 years. 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[37], the internal debt is

[33] Since I cannot establish related and unrelated party loans from the available data, I apply this provision to all the taxpayers and only consider the firms that fail both of these rules.

[34] Due to data limitations, I ignore this last piece of rule.

[35] I ignore the last two provisions when identifying the affected firms due to data constraints as mentioned earlier.

[36] I define the rule at EUR 3 million to remain on the safe side.

[37] 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.

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.[38]

- **Gabon:** introduced these rules on January 1st, 2019. The rules limited the deductibility of related-party interest expenses to 25% of EBITDA provided that the debt:equity ratio is more than 1:1.5. Since the rule was only applicable to related party interest expense which I can not observe in the data, I drop the analysis of Gabonese affected firms.

- **Greece:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The rule does apply to stand alone companies. The excess interest expense could be carried forward indefinitely.

- **Hungary:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 939810000 HUF. The escape clause is activated if the taxpayer's standalone equity/asset ratio is higher than the group's ratio or the group's net interest/tax EBITDA ratio is higher than the taxpayer's standalone ratio.

- **Iceland:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 100 million ISK. The escape clause is activated if the taxpayer's equity ratio is no more than 2 percentage points below the equity ratio of the group to which it belongs.

- **India:** introduced these rules on April 1st, 2018. It allowed gross interest expense of 30% of EBITDA and set a *de-minimis* threshold of 10 million INR. The excess interest expense could be carried forward for up to 8 years. The escape clause is activated if the disallowed expense is the lower of 30% EBITDA and the interest paid/payable to the associated enterprises in the financial year.

- **Japan:** introduced these rules on April 1st, 2020. It allowed net interest expense of 20% of EBITDA and set a *de-minimis* threshold of 20 million JPY. The escape clause is activated if total ratio of group net interest expense to group adjusted income is 20%

[38]For the case of France, I ignore the case when debt:equity is greater than 1:5 and simply 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's no over-identification of treated firms subject to such laws.

or less.[39] Additionally, debt:equity of 3:1 needs to be failed first. The excess interest expense can be carried forward to up to 7 years.

- **Kenya:** introduced these rules on January 1st, 2022 and limited the deductibility of interest expense to 30% of EBITDA. The rule replaced the existing Thin Capitalization rules and did not set up any *de-minimis* threshold. Due to the timing of the policy, Kenya falls outside of the scope of the policy.

- **Korea:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA. This rule is applicable only if debt:equity is higher than 2:1. Moreover, this rule is for related parties debt only.[40]

- **Latvia:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. This rule is applicable only if the debt:equity is higher than 4:1.

- **Lithuania:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. Additionally, there are two more rules: 1. if the total amount of group of companies net interest expenses operating in Lithuania is less than the threshold, all of the interest expenses of each company is deductible.[41] 2. companies whose financial statements are included in a group consolidated financial statements and whose equity ratio is no more than 2% points lower than that of the group, they can deduct all expenses.

- **Luxembourg:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The policy also entails a grandfathering clause for loans issued prior to 17/06/2016 and does not apply to stand alone companies. The escape clause is activated if the equity ratio is less than by no more than 2 percentage points of that of the group. The excess interest expenses can be carried forward indefinitely and the unused interest capacity can be carried forward by up to 5 years.

- **Malaysia:** introduced these rules on January 1st, 2019. It allowed gross interest expense of 20% of tax-EBITDA and set a *de-minimis* threshold of 500,000 RM. The excess interest expenses can be carried forward indefinitely.

- **Malta:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million EUR. The policy also entails a grandfathering clause for loans issued prior to 17/06/2016 and does not apply

[39] I ignore this rule.
[40] Since I can't distinguish related party loans, I drop it from the analysis.
[41] I ignore this rule

to stand alone companies. The excess interest expenses could be carried forward indefinitely while the unused capacity can be carried forward to up to 5 years. There is also a detailed and sophisticated group escape clause which is not mentioned here for the purposes of brevity.

- **Mexico:** introduced these rules on January 1st, 2020. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 20 million MxP. The excess interest expenses could be carried forward up to 10 years. The rule is only applicable if debt:equity is greater than 3:1.
- **Mongolia:** introduced these laws on January 1st, 2020. It limited deductibility of interest expenses to related parties above 30% of EBITDA provided that the debt:equity is greater than 3:1. Since the law is wholly limited to related parties only, I drop Mongolian firms from the analysis.
- **Netherlands:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 1 million EUR. The excess interest expense can be carried forward indefinitely.
- **Nigeria:** introduced these rules on January 1st, 2020. It limited the deductibility of interest expense for tax purposes that are in excess of 30% of EBITDA. The excess interest expense can be carried forward to up to 5 years.
- **Norway:** introduced these rules on January 1st, 2019. It allowed net interest expense of 25% of EBITDA. The rule does apply to stand alone companies. If a company is not a part of a group, 5M NOK threshold applies. But if a company is a part of group, the 25M NOK threshold applies to the whole group. Furthermore, the rule wouldn't apply if a company's equity ratio is at least equal to that of the consolidated group or if the ratio of the consolidated Norwegian entities is at least as high as that of the global group.
- **Poland:** introduced these rules on January 1st, 2018. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 3 million PLN. The excess interest expense can be carried forward by up to 5 years.
- **Portugal:** introduced these rules on May 4th, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 1 million EUR. The excess interest expenses can be carried forward by up to 5 years and the unused interest capacity can be carried forward by up to 5 years as well.
- **Romania:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set 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. Therefore, I define the treated firms in Romania on the 2018 rule in 2017 as the baseline year.

- **Slovakia:** introduced these laws on January 1st, 2015 with interest deductions limited to 25% of EBITDA. No carry-forward for excess interest expense is available.
- **South Africa:** introduced these rules on January 1st, 2015. The law limited the deductibility of interest expense to 40% of EBITDA. The excess interest expense could be carried forward to the subsequent year. In ORBIS, there is only one observation for South African firms and that too has an *interest expense:EBITDA* of 0.06. Therefore, I don't present more details about the policy here but can be found in the associated files.
- **Spain:** introduced these rules on January 1st, 2012. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 1 million EUR. The excess interest expenses can be carried forward indefinitely. Additionally, the ratio can be applied to the whole group if the companies are taxed under tax consolidation regime.
- **Sweden:** introduced these rules on January 1st, 2019. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 5 million SEK. The excess interest expenses can be carried forward by up to 6 years. Additionally, the ratio can be applied to the whole group if the companies are taxed under tax consolidation regime.
- **Uganda:** introduced these rules on July 1st, 2018. It allowed gross interest expense of 30% of EBITDA. The excess interest expenses can be carried forward by up to 3 years. The rule is also applicable to stand alone companies.
- **UK:** introduced these rules on April 1st, 2017. It allowed net interest expense of 30% of EBITDA and set a *de-minimis* threshold of 2 million GBP. The excess interest expenses can be carried forward indefinitely. The rule does not apply to stand alone companies. Additionally, the group escape clause is activated if the net interest expense is less than net adjusted group interest. I ignore this group escape clause.
- **US:** introduced these rules on January 1st, 2018. The rules were implemented as section 163 (j) of Tax Cuts and Jobs Act (TCJA). The rule limited 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 USD 25 million or less in the previous three years.

- **Vietnam:** introduced these rules on May 1st, 2017. The rule limited the deductibility of gross interest expense to related parties to 20% of EBITDA and didn't have any *de-minimis* threshold nor any carry-forward provision for excess interest expense or unused capacity. Since I only have one Vietnamese observation in the baseline year in my dataset, I ignore the case of Vietnam for the purpose of this project. The reason is that I can't identify the related party loans in the dataset.
- **Zambia:** introduced these laws on January 1st, 2019. The rule limited the deductibility of gross interest expense to 30% of EBITDA. The rule didn't have any *de-minimis* threshold. The unused interest expense could be carried forward to up to 5 subsequent years.

B Data Cleaning

The data for this project was obtained directly from the ORBIS Historical database via Moody's online cluster. The data was extracted using PySpark and subsequently downloaded from the SQL server onto a local machine. The detailed code for this process is available in the supplementary code files, but the key steps are highlighted below:

- Initial Data Extraction: The first batch of downloads was conducted in December 2022. Data was extracted from the Detailed Financials Monthly table of the ORBIS database, specifically from the Industry Global Financials Ratios section. The selection included a few key variables, with the consolidation codes restricted to U1/U2 (unconsolidated) and C1/C2 (consolidated) for the years 2010-2022. Note that since I downloaded in December 2022, financial data for many companies for 2022 is incomplete.

- Jurisdiction, Shareholder, and Industry Data Integration: Jurisdiction information (*country_iso_code*) was extracted from the Firmographics Monthly table under Contact Info. Shareholder information (*shareholder_bvd_id*) and global ultimate owner data (*guo_25* and *guo_25c*) were obtained from the Detailed Ownership Monthly table of Links Current. Industry information (*nace_rev_2_main_section*) was also pulled from the Firmographics Monthly table of Industry Classifications. These datasets were then merged with the financial data to create a comprehensive dataset.

- Filtering Observations: Observations were retained only if they had non-empty reporting of both *EBITDA* and *financial_expenses*. These two variables are critical for determining whether an affiliate (and by extension, the entire group) failed the ESR test at the baseline. Duplicate observations were then removed based on *bvd_id_number*, *consolidation_code*, *filing_type* (e.g., local GAAP, US GAAP, IFRS), *closing_date*, *financial_expenses*, *financial_revenue*, and *EBITDA*. This filtering resulted in a total sample size of 38,582,931 observations. To manage memory, the data was divided into ten sub-stratas, each containing 4 million observations.

- Further Data Iterations: This process was repeated for the remaining financial variables in the database.

- Incorporations Data: For incorporations, data was extracted on the incorporated firm (*bvd_id_number*), jurisdiction of incorporation, date of incorporation, type of entity, and the global ultimate owner (*guo_25c*) of the incorporated subsidiary. Like the financial data, the incorporation data was restricted to the years 2010-2021.

After downloading the data from the SQL server in Moody's workspace in Excel format, it was loaded into STATA for further analysis. The analysis was restricted to MNEs, de-

fined as consolidated groups with affiliates in at least two different locations. Additionally, the analysis was limited to affiliates whose global ultimate owner (*guo_25c*) also reported consolidated financials, enabling consistent analysis of group-level effects of the reform.[42] Intermediate shareholders were excluded, retaining only the ultimate owner in consolidated financials.

The sample was further restricted to the most recent financial reporting of an affiliate/owner in a given year if multiple reports were available. Entities missing jurisdiction or industry information, or belonging to the public, financial, or real estate sectors, were excluded. Observations with negative values for assets, debt, revenue, or the number of employees were also removed. For rule-enforcing jurisdictions, if financials were reported in multiple currencies, only the national currency (in which the *de-minimis* threshold of the policy is determined) observations were retained, and others were dropped.

B.1 Definition of Variables

- **Debt.** Sum of long-term borrowings from credit institutions and bonds issued and short-term financial debts (e.g. to credit institutions + current portion of loan term debt, bonds, etc.).
- **Equity.** Total equity (capital plus other shareholders funds).
- **Capital.** Of which issued share capital (authorized capital).
- **Fixed Assets.** Total amount (after depreciation) of non current assets (intangible assets plus tangible fixed assets plus other non-current assets).
- **Tangible Assets.** All tangible assets such as machinery, buildings, land etc.
- **Intangible Assets.** All intangible assets such as formation expenses, research expenses, goodwill, development expenses and all other expenses with a long term effect.
- **Total Assets.** Sum of all non-current assets and current assets.
- **Gross Investment spending**: Changes in fixed capital assets plus depreciation, i.e., $K_t - K_{t-1} + depreciation$, where K_t denotes book value of the fixed asset in year t.
- **Investments (I_t)**: Ratio of current-year gross investment spending to beginning-of-year net fixed capital assets (Liu, 2020; de Mooij & Liu, 2020).

[42] The ORBIS database provides detailed information on controlling shareholders, global ultimate owner corporations, and non-corporate global ultimate owners, such as banks or utilities. For this project, ownership is matched at the highest corporation level, treating distinct ultimate owner corporations owned by a single utility as separate entities.

- **Revenue.** Total operating revenues (the sum of net sales, other operating revenues and stock variations) in a given year. The figures do not include VAT.
- **EBITDA.**[43] A measure of a firm's operating performance over a given year. It is calculated by excluding expenses related to interest, taxes, depreciation, and amortization from a firm's taxable earnings.
- **Financial Expense.** All financial expenses such as interest charges, write-off financial assets, etc. in a given year.
- **Profits Before Tax.** The sum of operating profit and financial profit of a firm in a given year.
- **Taxation.** All taxes of a firm in a given year (paid, accrued or deferred).
- **Net Profits.** Profit before taxes minus tax expense of a firm in a given year.
- **Debt:Equity.** A financial ratio that measures a firm's leverage by dividing its total debt by its total equity. This ratio indicates the relative proportion of debt and equity used to finance the company's assets.
- **Debt:Assets.** A financial ratio that measures a firm's proportion of assets financed through debt by dividing its total debt by its total assets.
- **Interest:EBITDA.** A financial ratio that measures a firm's ability to pay interest on its debt by dividing its financial expense by its EBITDA. Financial expense is the cost incurred by the company for borrowed funds, while EBITDA represents earnings before interest, taxes, depreciation, and amortization.
- **Net Interest:EBITDA.** A financial ratio that measures a firm's ability to pay interest on its debt by dividing its net financial expense by its EBITDA. Net financial expense is the cost incurred by the company for borrowed funds net of its financial revenue, while EBITDA represents earnings before interest, taxes, depreciation, and amortization.
- **Taxation:Assets.** The ratio of all taxes of a firm in a given year to the total amount of assets.

[43] Earnings before Interest, Tax, Depreciation, and Amortization.

C Empirical Methodology - Additional Details

C.1 Treatment Definition

For all the jurisdictions, I define treatment at the baseline year ($t^* - 1$) — the year preceding ESR implementation. However, there exist a couple of exceptions: Albania presented the ESR in November 2016, announced it on January 1st, 2017 and introduced it on January 1st, 2018. In order to rule out any potential endogeneity, I define the baseline year at 2016 rather than 2017. Similarly, India announced the rules on March 31st, 2017 and were applicable from April 1st, 2018. Here also, I define treatment units in 2016.

In cases where a jurisdiction implemented ESR with a limited scope — for example, applying only to related-party loans — and did not revise the laws by 2021, I exclude firms from that jurisdiction from my analysis. This is because I cannot reliably distinguish between related and third-party loans in my data. Examples of such jurisdictions include South Korea, Mongolia, and Vietnam. Additionally, since I cannot observe the entire universe of group members operating within a jurisdiction, I cannot determine with complete confidence whether a group escape clause was activated. Therefore, I disregard the group escape activation and instead focus on identifying heterogeneous responses among firms in jurisdictions with and without these group escape provisions.

Additionally, a few of the rule-enforcing jurisdictions, specially those in the EU region, exempted the interest paid on loan agreements concluded prior to 17 June, 2016 from the incidence of ESR rules - something called as a *grandfathering clause*. Similarly, interest claimed on loans used to finance public infrastructure projects are also exempt from the incidence of ESR measures in certain jurisdictions. However, since I cannot cleanly identify the interest expenses due on different types of loan arrangements, I abstract from these exemption clauses and consider the entire stock of interest expenses to be targeted by ESR.

To identify treated firms within each jurisdiction, I identify the firms that failed the ESR threshold test (i.e., percentage and *de-minimis* threshold) at the respective baseline years. In calculating the interest expense/ EBITDA ratio, only cases where EBITDA was strictly positive were included. For cases where EBITDA was negative or zero, the ESR test was automatically failed if interest expenses (net/gross based on the jurisdiction) were strictly positive. Similarly, where jurisdictions had debt rules alongside ESR rules, requiring the failure of both rules, the ratio was only calculated if equity was strictly positive. If equity was negative or zero, the test was failed automatically. For jurisdictions with group escape clauses, i.e., requiring the failing of the equity test along with the earnings test, the equity ratio, i.e., equity/total assets, was computed for the group and used as a benchmark for individual firms. Observations were excluded if the equity ratio of the entire group could not be calculated. Similarly, for jurisdictions like Finland and Norway that set the *de-minimis*

threshold for the entire group of affiliates within the jurisdiction (i.e., consolidation at the national level), I calculated the net interest expenses of all its related entities operating in Norway/Finland and set the *de-minimis* at the group level threshold 25 million NOK and 5 million SEK, respectively. The sample was then merged with jurisdiction-level time-varying controls, and unmatched observations were deleted. Finally, all currencies were converted to the average nominal USD exchange rate for the year to report all financials in USD.

C.2 BEPS & ATAD Announcement Effects

One particular source of endogeneity could be the announcement effects of BEPS & ATAD that could bias my results. The OECD announced BEPS Action 4 in June 2015 while the EU signed ATAD agreement in June 2016. While the BEPS Action 4 was more of a recommendation from OECD, the ATAD agreement was binding on all EU members. Thus, announcement effect could potentially induce behavioral responses for both the policies but they will likely be much stronger for ATAD agreement. Note that the potential anticipation response could be in two alternating ways: either the firms whose interest deductions are too large react strategically to off-load their debt and reduce their interest expenses just in time before the introduction of the policy in the national charter and escape the application of the rule, or the firms continue their debt growth trajectory and increase their interest expenses to shift profits and avoid taxes until the policy is actually enforced. If the first scenario holds then my control group would be reacting to the policy as well and hence, would not be suitable to estimate diff-in-diff. On the other hand, if the second case is true then there would be selection into treatment. Thus, if either of the hypothesis is true, my estimates will be biased. I address both of the potential compounds in the following analysis.

To evaluate the presence of announcement effects, I calculate counterfactual treatment units in all ESR-enforcing jurisdictions, i.e., firms in a jurisdiction X that would have been affected by the X-specific policy in all calendar years leading up to the baseline year, and compare these with the actual treated units. For instance, since the UK announced the reform in 2017, I identify British firms that would have failed the UK ESR test in the years 2013, 2014, 2015 and 2016 (the baseline year). These firms constitute my counterfactual treatment units. Concurrently, I track whether the actual UK treated firms (those that failed the UK ESR in the baseline year, i.e., 2016) also failed the UK ESR in those respective years. By comparing the number of counterfactual treatment units with the actual treatment units, I assess the potential impact of the announcement effects on my results.

Figure C.I presents the findings of this analysis. In C.Ia, the blue line represents the counterfactual treatment units, showing the number of firms that would have been treated across all ESR-enforcing jurisdictions before the policy's introduction in each calendar year. The red line, on the other hand, indicates the number of actual treated units that also failed the test in each pre-policy year. If the BEPS announcement in 2015 or the ATAD agreement

in 2016 had induced any behavioral responses, I would expect to see significant changes in the number of firms that would have been treated in years 2015, 2016, and 2017. However, the data show a stable and constant trend following both policy announcements, i.e., the number of counterfactual treatment units neither significantly increase nor decrease in either of the years after the policy announcements. In other words, I do not find enough evidence of control group reacting to the policy reform thereby validating my choice of control group. Through this analysis, I can also reject the hypothesis of announcement effects at least in a non-parametric sense.

C.3 Discussion on Trends Inclusion

In my baseline estimation methodology, I control for unit-specific linear time trends to account for the unobserved linear differences across the treatment and control observations. The rationale is that firms with higher leverage, indicated by a higher debt-to-equity ratio, tend to borrow more each year for operational expansion. As a result, their interest expenses grow at a faster rate compared to firms that do not rely as heavily on debt. These dynamics lead to different growth trajectories for debt and interest expenses among firms even before policy implementation. Since financial expenses and (indirectly) debt determine the treatment status of firms in the baseline year, the differential trends in these high and low leveraged firms necessitate controlling for them to achieve pre-treatment parallel trends and thus unbiased estimation results. One way to account for these trends could be through controls. However, all the firm-level variables in my data react strongly to the treatment introduction and thus would be *bad controls*. Hence, I resort to directly controlling for these firm-specific trends in the main estimation methodology. By incorporating these trends, I mitigate potential biases arising from these pre-existing differences, ensuring a more accurate estimation of the policy's impact (Freyaldenhoven *et al.*, 2019). However, note that under such a specification, the interpretation of the coefficients becomes relative to the counterfactual trend. In other words, the intuition is that had there been no ESR, the treatment units would have continued evolving as their counterfactual pre-trend.

With an inclusion of the linear time trends, my estimates could be biased if 1) there's selection into the treatment, i.e., since I identify treatment in the baseline year when the policy is already announced but not yet enforced, firms may have an incentive to record large interest expenses to benefit from the last year of interest-expenses related amnesty and thus be recorded as treated units in my study or 2) there might be an effect of dynamics like real business cycles where firms record high interest expenses in some periods followed by low interest expenses in subsequent periods, i.e., mean reversion. If mean reversion due to business cycles are in fact the case in my treated units, then controlling for pre-trends would worsen the estimated coefficients and potentially overestimate the response. I empirically test for both of the above mentioned dynamics and argue that my results are not likely to be

biased because of either of the two.

C.3.1 Discussion on Treatment Selection

To test for selection, I extend the framework developed in Section C.2. The red line in Appendix Figure C.Ia, which plots the number of actual treated units that failed the policy in all calendar years leading up to the baseline years, shows a constant upward trend across all the time periods. Since I also include the baseline year in this analysis, the exercise is a non-parametric test of selection into the treatment. Furthermore, in Appendix Figure C.Ib, the green line plots the share of treated units within the counterfactual units. Notably, I find that this share increases at a constant rate, with no abrupt jumps at the announcement dates of ESR, thus rejecting the hypothesis of selection into treatment non-parametrically. Moreover, the treated units constitute more than 50% of the counterfactual units in all the years after BEPS announcement further highlighting the constant treatment composition. While the discontinuous decrease in the number of counterfactual units after 2017 may appear perplexing, this decline primarily occurs because more than 50% of the actual treated firms were domiciled in jurisdictions that implemented ESR before 2018, thus reducing the number of firms in the estimation subsample in years 2018 and onward.

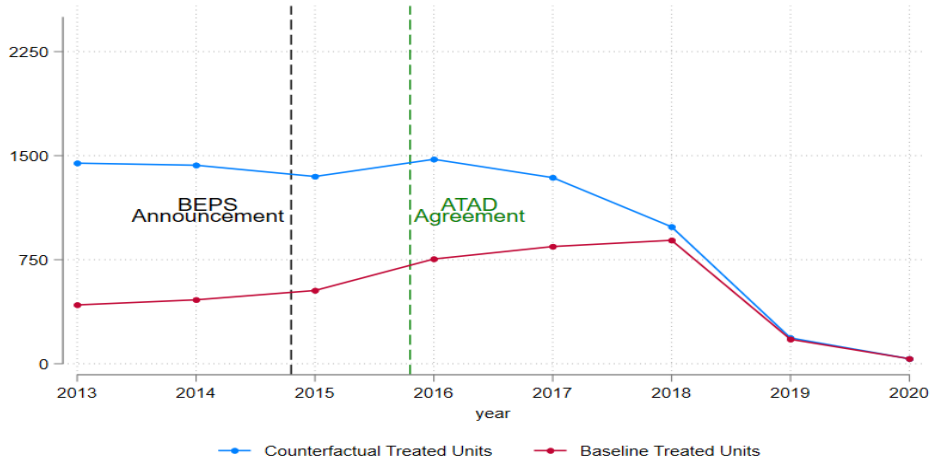
C.3.2 Discussion on Mean Reversion

For the test of mean reversion, note that the outcome variables that determine the treatment status of affiliates across different jurisdictions are gross financial expenses, net financial expenses, EBITDA, net financial expenses:EBITDA, gross financial expenses:EBITDA and debt:equity. Thus, if there is indeed mean reversion, then irrespective of the period I identify treatment status in, the responses shall be broadly similar. In other words, for all the possible baseline years, i.e., $t^* - 1$, $t^* - 2$, etc., the respective treated firms would exhibit a U-shape reporting behavior for these policy variables.

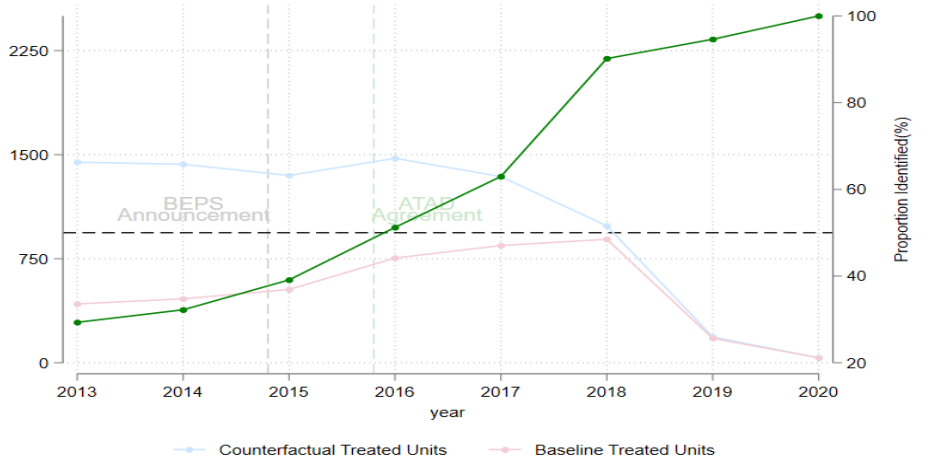
To test for this dynamic, I construct three additional versions of my panel data set covering the same number of years, but starting in earlier calendar years and using treatment definitions based on earlier years (i.e., $t^* - 2$, $t^* - 3$ and $t^* - 4$) as well. In each panel data set with a treatment definition based on earlier years, I re-estimate equation (7) but without the inclusion of unit-specific linear trends. In Figure C.II, I plot each of these placebo-in-time estimates setting event time equal to zero in the last year of the treatment definition for each panel. Each of the figure shows that there is no conceivable pattern or discernible trend across different years of the treatment definition. This suggests that the responses estimated under the baseline empirical methodology are likely a response to the ESR reforms and the estimates of the study may not necessarily be biased due to mean reversion or selection into the treatment. Thus, controlling for unit-specific linear trends does not bias my results.

FIGURE C.I: **ANNOUNCEMENT EFFECTS & TREATMENT SELECTION**

(A) **ANNOUNCEMENT EFFECTS**



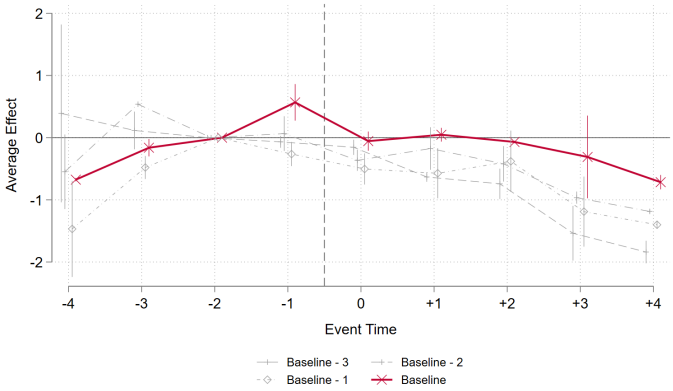
(B) **SELECTION INTO TREATMENT**



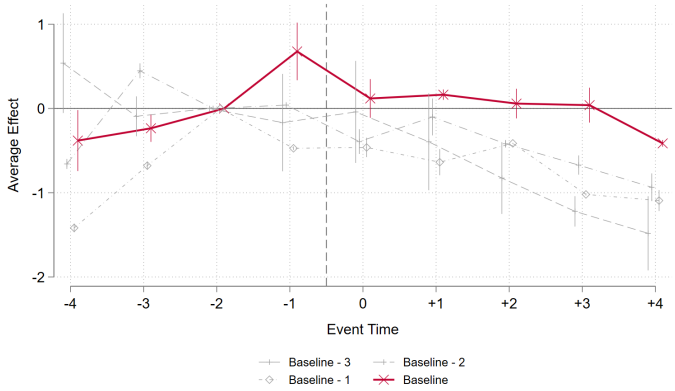
Notes: Panel (A) plots the number of counterfactual treatment units, i.e., firms that would have failed the ESR test in each calendar year leading up to the baseline year, against the actual treated units that would have failed the ESR test in all pre-reform years. Panel (B) plots the share of actual treated units in counterfactual treatment units in each year. Note that Indian firms are excluded from the analysis. This is because the share of Indian firms in actual treated units is close to 25%. Thus, inclusion of these firms in the estimation renders the graph non-representative.

FIGURE C.II: **TEST FOR MEAN REVERSION**

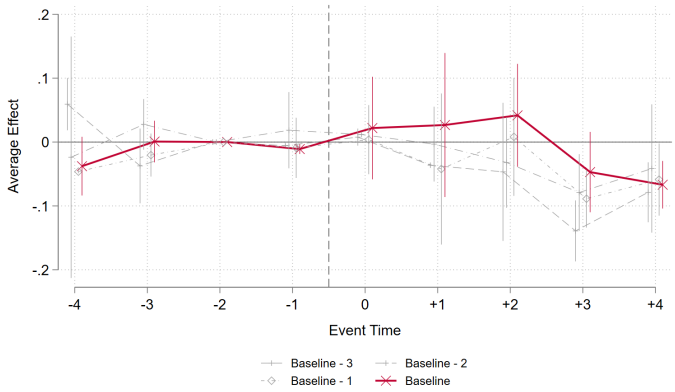
(A) **GROSS INTEREST EXPENSES**



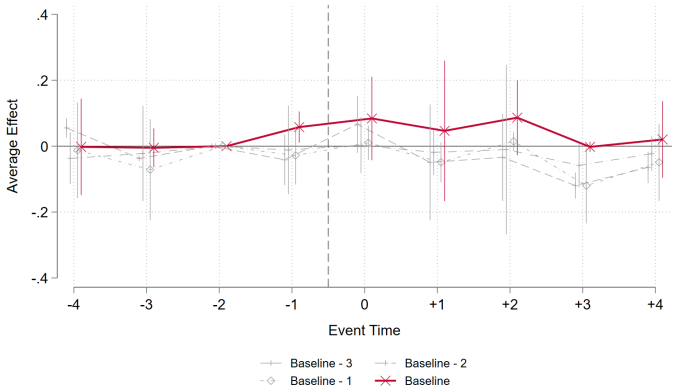
(B) **NET INTEREST EXPENSES**



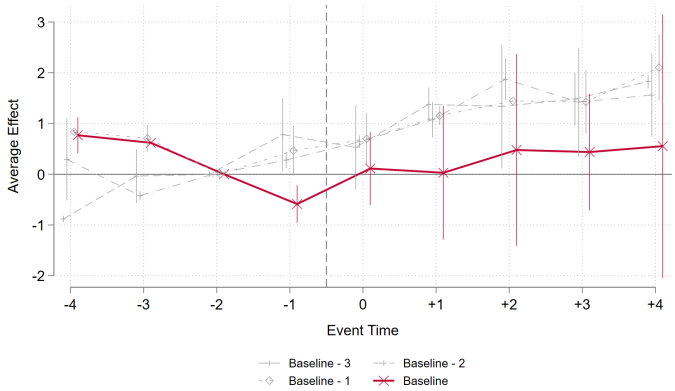
(C) **GROSS INTEREST:EBITDA**



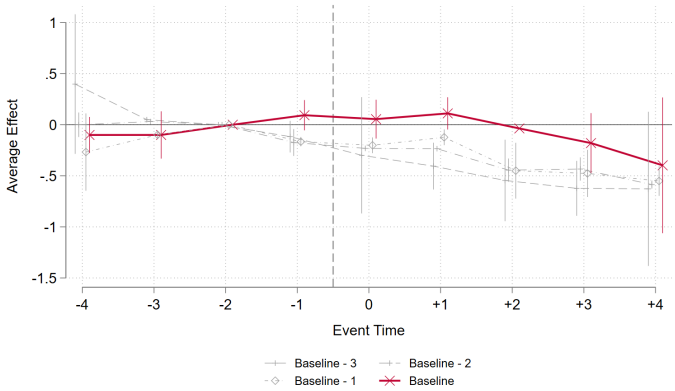
(D) **NET INTEREST:EBITDA**



(E) **EBITDA**



(F) **DEBT:EQUITY**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level for different baseline years. The outcomes of treated affiliates are compare with that of control group; sister affiliates are dropped from the analysis. In addition to the estimation at the original baseline year, i.e., $t^* - 1$, I construct three additional versions of my panel data set covering the same number of years, but starting in earlier calendar years and using treatment definitions based on these years (i.e., $t^* - 2$, $t^* - 3$ and $t^* - 4$). In each panel data set, I then re-estimate equation (7) but without the inclusion of unit-specific linear trends. I plot each of these placebo-in-time estimates setting event time equal to zero in the last year of the treatment definition for each panel. The effects of treatment at the original baseline year are displayed in bold red color and for others in grey color. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the black dashed line depicts the year of policy introduction.

C.4 Discussion on Estimating in Levels

Since my data features negative values for important variables, like profits before taxation, net interest expense, etc., following the recommendations of Chen & Roth (2023), I report all the treatment effects in nominal USD millions and compare them to the pre-baseline mean of the treated group for a percentage interpretation. Unfortunately, under my current setting I can not log transform the potential outcomes. The issue with log-transformation of variables with frequent zeros and negative values, as noted by Chen & Roth (2023), is that of scale variance, i.e., the results in such case are sensitive to the units of the outcome (Aihounton & Henningsen, 2021; de Brauw & Herskowitz, 2021; Mullahy & Norton, 2022). In order to overcome this issue, one can either use a Poisson QMLE regression (Santos Silva & Tenreyro, 2006; Brockmeyer & Hernandez, 2016) or normalize the outcome variable on some suitable pre-determined characteristic (Chen & Roth, 2023). However, neither of the two approaches can be used in my case. The reason for being unable to make use of first recommendation is that I use the STATA command developed by Chaisemartin *et al.* (2024) as the baseline difference-in-difference which is not compatible with the Poisson transformation or QMLE regression. Similarly for the second recommendation, almost all of the balance sheet and financial variables strongly react to the reform. Therefore, due to data limitation, I am unable to come up with a suitable pre-determined characteristic with which to scale the variables.

C.5 Discussion on $t^* - 2$ as the Reference Year

In all the ESR-enforcing jurisdictions, the policy announcement typically occurs in year $t^* - 1$, with announcements made early in the year, while firms report their financials at year-end. This timing suggests that, conditional on no anticipation effect, the announcement is unlikely to prompt immediate financial responses within the same year, as altering firm operations mid-year is challenging. However, announcements can influence long-term strategic decisions, such as investments and asset financing, even within the announcement year.

The effect it has on my estimation is that while the policy parameters, like gross financial expenses, net financial expenses, EBITDA, net financial expenses:EBITDA, gross financial expenses:EBITDA, etc., follow pre-announcement trends even in $t^* - 1$, strategic responses deviate from this pattern in the announcement year, i.e., $t^* - 1$.[44] Hence, referencing $t^* - 1$ would reject the null hypothesis of parallel counterfactual trends for these strategic variables. However, using $t^* - 2$ as the reference year allows me to fit the trend accurately up to $t^* - 2$ for all the parameters, i.e., financial as well as strategic, and results in parallel counterfactual trends. With this adjustment, the coefficients of policy parameters remain

[44] The policy parameters maintain a linear trend since they relate to operational reporting.

non-significant in $t^* - 1$ (since they continue following the fitted linear trend), and that of strategic variables become significant in $t^* - 1$ (since they deviate from the fitted linear trend). Since adjustments made in $t^* - 1$ due to policy announcement also reflect a policy effect, my average treatment effects for these strategic variables will now have these effects recorded as well thanks to referencing with respect to $t^* - 2$.

While in principle I could identify the treatment status even in $t^* - 2$ and avoid any potential biases akin to the current methodology, doing so introduces its own complexities. With $t^* - 1$, I only address jurisdiction-level announcements, whereas $t^* - 2$ could potentially correlate with BEPS and ATAD announcements. Additionally, as detailed in Section A, some jurisdictions initially implemented these rules partially and later aligned their policies with BEPS recommendations. For instance, Greece adopted these rules in 2014, revised them in 2017, and aligned with BEPS in 2019. Similar scenarios occurred in other jurisdictions as well. Therefore, even with treatment identification in $t^* - 2$ my results could be confounded due to announcement or anticipation effects.

Given these complexities, identifying treatment in $t^* - 1$ and referencing $t^* - 2$ is the most efficient method to identify the effects. Moreover, since I do not find evidence of mean reversion, selection bias, or BEPS/ATAD announcement effects in the treated units with my baseline estimation, my baseline treatment sample is more robust than any other potential treatment samples.

D Empirical Results - Additional Details

D.1 Discussion on Larger Standard Errors in Later Period

Since I include unit-specific linear trends in my empirical estimation methodology to control for linear trend differences in key policy variables, like gross financial expenses, net financial expenses, EBITDA, net financial expenses:EBITDA, gross financial expenses:EBITDA, etc., my baseline estimation always have unit-specific trends across all the other outcomes of interest as well to ensure consistency. A drawback of controlling for these trends is larger standard errors for estimated coefficients specially for later years in the policy where the number of sample observations and the treated units significantly decrease.

D.2 The Case of US MNEs

ORBIS database is known for its lack of coverage of financials of US companies. It is true for affiliates of non-US and US MNEs operating in the US along with the consolidated financials of US headquartered non-publicly listed MNE groups. This is because the US doesn't require companies to publish their annual balance sheet and financial statement. A direct consequence of this lack of coverage is that the entire analysis offered in this study is for non-US territories. While the sample data does include the data of non-US domiciled affiliates of US MNEs, I drop this subsample of data because of two reasons: 1) I restrict the data to only those affiliates for whose ultimate group owners I have consolidated financials data to comprehensively examine the group level effects 2) the US introduced ESR through 163j in its TCJA of 2017. Therefore, the mere imposition of ESR in the US may induce shocks to the whole US group even affecting non-US affiliates. As a result, including these non-US affiliates of US companies into the analysis, I may erroneously regard them as control firms while in reality they may suffer from reallocation effects of the ESR.

E Additional Tables

TABLE E.I: **JURISDICTION STATISTICS**

Jurisdiction	Affiliates			Groups	
	Treated	Control	Sister	Treated	Control
Albania	0.173	0.022	0.038	0.000	0.000
Algeria	0.000	0.095	0.160	0.000	0.000
Argentina	0.000	0.004	0.016	0.000	0.000
Armenia	0.000	0.002	0.000	0.000	0.000
Australia	0.000	1.708	2.011	0.365	1.273
Austria	1.248	1.582	1.597	3.355	2.231
Bahrain	0.000	0.002	0.000	0.000	0.011
Barbados	0.000	0.002	0.000	0.000	0.000
Belgium	1.422	5.561	3.664	2.115	2.919
Bermuda	0.000	0.002	0.003	0.365	0.428
Bosnia and Herzegovina	0.000	0.153	0.152	0.000	0.034
Brazil	0.000	0.282	0.520	0.146	0.225
British Virgin Islands	0.000	0.002	0.000	0.146	0.124
Bulgaria	0.000	0.695	0.779	0.000	0.101
Canada	0.000	0.000	0.000	1.386	1.476
Cayman Islands	0.000	0.000	0.000	0.292	0.338
Chile	0.000	0.000	0.000	0.000	0.079
China	0.000	0.000	0.000	0.729	1.983
Colombia	0.000	0.002	0.008	0.000	0.034
Croatia	0.104	0.657	0.563	0.073	0.146
Cyprus	0.000	0.000	0.000	0.146	0.090
Czech Republic	0.347	1.804	2.549	0.219	0.113
Denmark	0.763	3.726	2.019	1.896	4.519
Egypt	0.000	0.000	0.000	0.000	0.045
El Salvador	0.000	0.002	0.003	0.000	0.000
Estonia	0.000	0.000	0.000	0.000	0.011
Finland	1.526	2.660	1.396	3.282	3.448
France	4.265	11.574	19.135	5.106	4.586
Georgia	0.000	0.016	0.019	0.000	0.000
Germany	0.000	6.980	7.066	8.242	12.632
Gibraltar	0.000	0.000	0.000	0.000	0.011
Greece	1.318	0.513	1.026	1.167	0.518
Hong Kong	0.000	0.000	0.000	0.219	0.158
Hungary	0.243	0.934	1.713	0.438	0.304
Iceland	0.000	0.076	0.060	0.000	0.124
India	24.688	1.941	3.180	8.972	1.702
Indonesia	0.000	0.000	0.000	0.000	0.034
Iran	0.000	0.004	0.000	0.000	0.011
Ireland	0.000	0.837	0.704	1.021	1.702
Israel	0.000	0.000	0.000	0.948	0.439
Italy	0.000	10.535	5.307	3.793	9.173
Jamaica	0.000	0.000	0.000	0.000	0.023
Japan	5.756	5.010	6.933	12.983	7.483
Jordan	0.000	0.002	0.000	0.000	0.011
Kenya	0.000	0.000	0.000	0.000	0.011
Kuwait	0.000	0.000	0.000	0.073	0.056
Latvia	0.000	0.442	0.336	0.000	0.113
Liberia	0.000	0.000	0.000	0.000	0.011
Liechtenstein	0.000	0.002	0.000	0.000	0.034
Lithuania	0.000	0.002	0.000	0.000	0.023
Luxembourg	0.069	0.333	0.211	1.167	0.530
Malaysia	0.555	0.011	0.030	0.802	0.597
Malta	0.000	0.015	0.022	0.000	0.068
Marshall Islands	0.000	0.000	0.000	0.146	0.056
Mauritius	0.000	0.000	0.000	0.073	0.045
Mexico	0.000	0.096	0.087	0.292	0.079
Montenegro	0.000	0.047	0.076	0.000	0.011
Morocco	0.000	0.495	0.633	0.000	0.023
Netherlands	0.208	0.572	0.804	2.261	4.654
New Zealand	0.000	0.375	0.455	0.146	0.372
Nigeria	0.000	0.000	0.000	0.000	0.011
North Macedonia	0.000	0.078	0.049	0.000	0.056
Norway	2.705	4.797	2.744	2.042	3.584
Oman	0.000	0.000	0.000	0.073	0.011
Pakistan	0.000	0.015	0.035	0.000	0.045
Philippines	0.000	0.652	0.750	0.000	0.045
Poland	4.300	4.309	4.571	1.751	0.958
Portugal	0.763	1.906	1.632	0.875	1.082
Qatar	0.000	0.000	0.000	0.000	0.034
Romania	9.605	1.535	1.624	0.146	0.011
Russia	0.000	0.056	0.049	0.073	0.192
Saint Kitts and Nevis	0.000	0.002	0.000	0.000	0.000
Saudi Arabia	0.000	0.000	0.000	0.146	0.034
Serbia	0.000	0.612	0.579	0.000	0.056
Singapore	0.000	0.000	0.000	0.073	0.338
Slovakia	7.594	0.726	1.123	1.313	0.158
Slovenia	0.000	0.368	0.390	0.073	0.068
South Korea	3.779	1.888	2.904	5.470	1.747
South Africa	0.000	0.000	0.000	0.146	0.361
Spain	11.650	7.644	7.685	7.586	4.936
Sri Lanka	0.000	0.040	0.022	0.073	0.045
Sweden	10.298	7.178	4.276	7.002	7.719
Switzerland	0.000	0.007	0.000	1.751	0.969
Taiwan	0.000	0.433	0.100	0.875	1.634
Tanzania	0.000	0.004	0.008	0.000	0.000
Thailand	0.000	0.490	0.498	0.584	0.462
The Bahamas	0.000	0.000	0.000	0.000	0.011
Tunisia	0.000	0.002	0.000	0.000	0.034
Turkey	0.000	0.005	0.000	0.292	0.214
Ukraine	0.000	0.515	0.536	0.000	0.011
United Arab Emirates	0.000	0.000	0.000	0.146	0.023
United Kingdom	6.623	6.963	7.160	6.929	9.894
Venezuela	0.000	0.000	0.000	0.000	0.011
Vietnam	0.000	0.002	0.000	0.000	0.023
Total Entities	2884	54932	36954	1371	8874

Notes: This table provides affiliate and group level jurisdiction statistics of the estimation sample. The jurisdiction classification is based on ORBIS reporting. In front of each jurisdiction, the percentage of affiliates belonging to treated, control, and sister groups is reported and the percentage of the MNEs, treated and control, headquartered in that jurisdiction is reported. The total number of firms in each classification are reported at the end of the table.

TABLE E.II: **SUMMARY STATISTICS I - INDUSTRY TYPES**

Industry	*Affiliates*		
	Treated	*Control*	*Sister*
Accomodation	4.265	2.509	2.727
Agriculture	1.352	1.669	1.044
Construction	11.096	7.866	9.470
Information	7.004	11.572	8.526
Manufacturing	34.917	32.063	27.469
Mining	2.288	0.983	1.918
Other Service	0.659	0.925	0.863
Power	11.581	2.789	12.862
Transportation	7.247	7.293	8.093
Wholesale	19.591	32.331	27.028
Total Entities	2884	54932	36969

Notes: The table provides affiliate level industry statistics of the estimation sample. The industry classification is based on ORBIS reporting. In front of each industry, the percentage of affiliates belonging to treated, control, and sister groups is reported. The total number of firms in each classification are reported at the end of the table. *Wholesale* contain Retail sector as well, *Power* is related to Hydrocarbons and Electricity, and *Transportation* entails Communication as well.

TABLE E.III: **FINANCIAL RATIOS - AFFECTED AFFILIATES**

Dep. Var	Debt:Assets (1)	Debt:Equity (2)	Interest:EBITDA (3)	Net Interest:EBITDA (4)	Tax:Assets (5)	Investment Rate (6)
$Failed_i \times Post_t$	-0.07***	-0.39*	-0.21**	-0.14	0.01***	-0.08
	(0.01)	(0.22)	(0.09)	(0.13)	(0.00)	(0.05)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	0.34	2.63	0.24	0.17	0.00	0.39
Observations	674,670	670,228	653,430	653,529	613,175	553,865
Switchers	11,997	11,614	9,959	10,544	11,038	9,599
Percentage Change	19.45	14.98	87.21	81.32	348.08	19.40

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the affiliate level. The outcomes of treated affiliates are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the treated affiliates in the reference year. I include firm and year fixed effects, firm-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust 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 E.IV: **OPERATIONS - REALLOCATION EFFECTS**

Dep. Var	Revenue (1)	EBITDA (2)	Interest (3)	Tax Base (4)	Taxation (5)	Net Profits (6)
$Failed_i \times Post_t$	2.73	-0.10	0.22**	0.05	0.15***	-0.10
	(2.63)	(0.25)	(0.10)	(0.32)	(0.05)	(0.31)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	116.45	7.98	1.80	5.50	1.17	4.34
Observations	730,827	765,641	777,966	765,339	715,107	758,326
Switchers	106,915	106,772	109,810	106,530	97,862	105,473
Percentage Change	2.35	1.21	12.16	0.92	13.26	2.20

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the affiliate level. The outcomes of sister affiliates are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the sister affiliates in the reference year. I include firm and year fixed effects, firm-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust and clustered at firm level. *Interest* is the gross financial expenses, *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 E.V: **FINANCIAL RATIOS - REALLOCATION EFFECTS**

Dep. Var	Debt:Assets (1)	Debt:Equity (2)	Interest:EBITDA (3)	Net Interest:EBITDA (4)	Tax:Assets (5)	Investment Rate (6)
$Failed_i \times Post_t$	-0.01***	0.03	-0.00	-0.01	0.00***	0.02
	(0.00)	(0.08)	(0.03)	(0.03)	(0.00)	(0.03)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	0.15	1.04	0.09	0.02	0.02	0.42
Observations	775,782	770,689	752,121	752,464	703,818	631,050
Switchers	110,599	109,534	105,993	106,837	99,265	84,271
Percentage Change	9.09	2.77	2.34	37.23	25.65	4.98

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the affiliate level. The outcomes of sister affiliates are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the sister affiliates in the reference year. I include firm and year fixed effects, firm-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust 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 E.VI: **FINANCIAL RATIO - GROUP LEVEL**

Dep. Var	Debt:Assets (1)	Debt:Equity (2)	Interest:EBITDA (3)	Net Interest:EBITDA (4)	Tax:Assets (5)	Investment Rate (6)
$Failed_i \times Post_t$	-0.01	-0.04	0.02	-0.03	0.00**	0.00
	(0.01)	(0.07)	(0.05)	(0.05)	(0.00)	(0.06)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-Baseline Mean	0.28	1.18	0.29	0.16	0.01	0.20
Observations	119,779	119,078	115,641	115,671	114,272	92,130
Switchers	5,352	5,288	5,051	5,110	5,274	3,894
Percentage Change	1.80	3.10	5.70	16.59	16.01	1.53

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

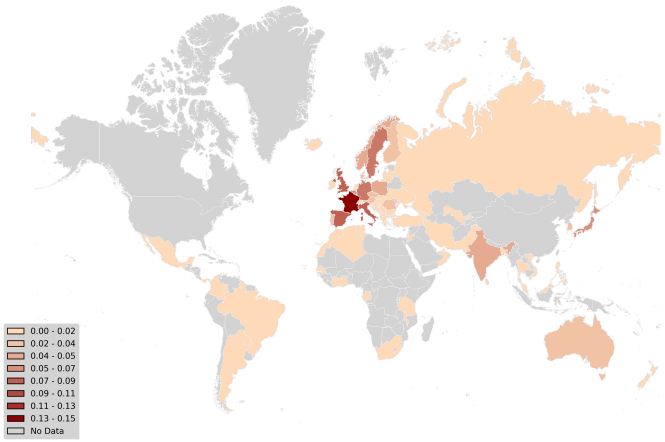
Notes: This table provides the average treatment effects of ESR (in nominal USD millions) at the group level. The outcomes of treated groups are compared with the control group. The reference year is $t^* - 2$ and $Post_t$ includes $t^* - 1$ to $t^* + 4$. The *Pre-Baseline Mean* is the sample mean of the treated groups in the reference year. I include group and year fixed effects, group-specific linear trends, and time-varying jurisdiction-level controls. The standard errors are robust and clustered at group level. *Observations* is the number of observations used in the estimation, and *Switchers* is the number of MNEs for which treatment status changed from 0 to 1.

F Additional Figures

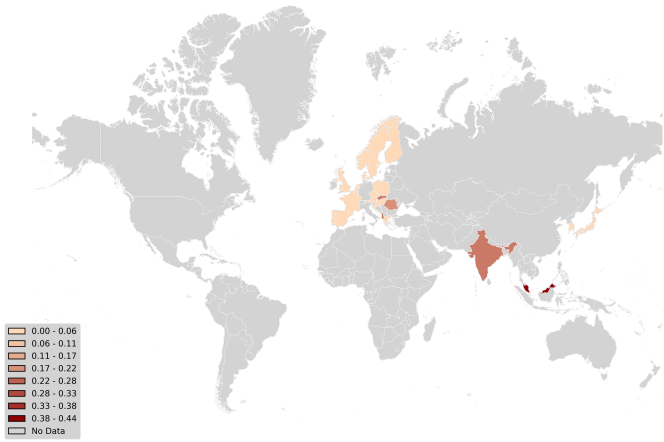
F.1 Geographical Coverage

FIGURE F.I: **GEOGRAPHICAL COVERAGE OF AFFILIATES**

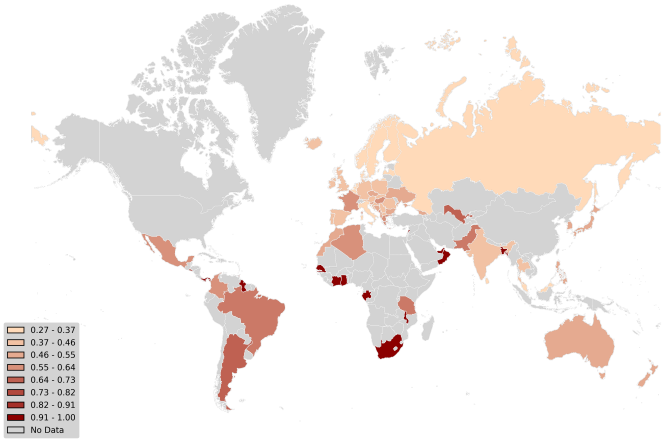
(A) **ALL AFFILIATES**



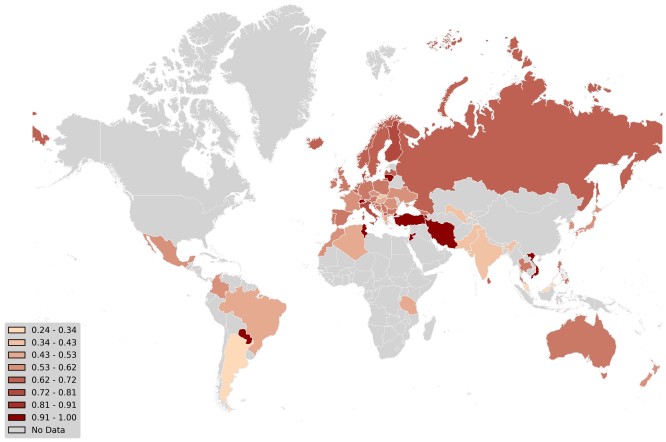
(B) **TREATED AFFILIATES ONLY**



(C) **SISTER AFFILIATES ONLY**



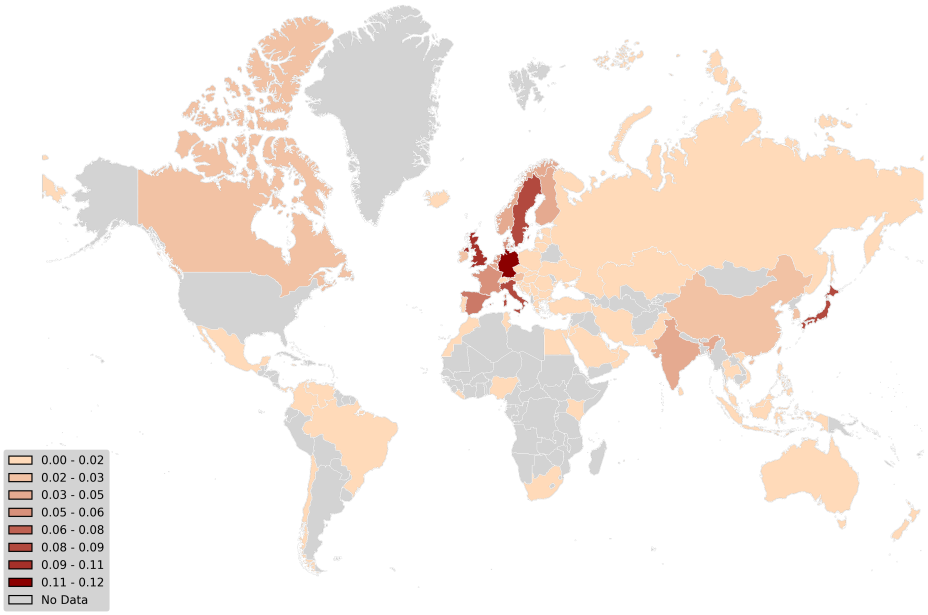
(D) **CONTROL AFFILIATES ONLY**



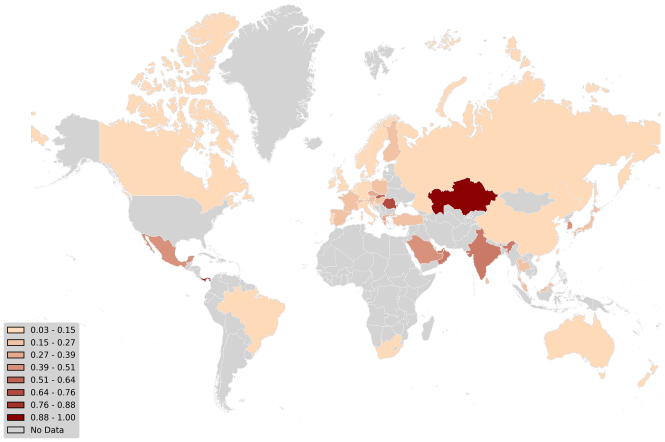
Notes: This figure plots the geographical distribution of the sample of MNE affiliates available in the data. Panel (A) plots the jurisdiction-wise share of affiliates in the entire sample. Panels (B), (C), and (D) plot the jurisdiction-wise share of treated affiliates, sister affiliates, and control affiliates, respectively, in the entire sample of affiliates within that specific jurisdiction.

FIGURE F.II: **GEOGRAPHICAL COVERAGE OF MNE GROUPS**

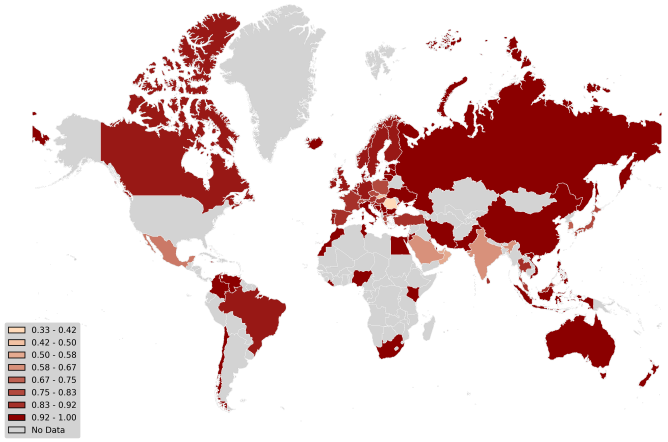
(A) **ALL MNES**



(B) **TREATED MNES ONLY**



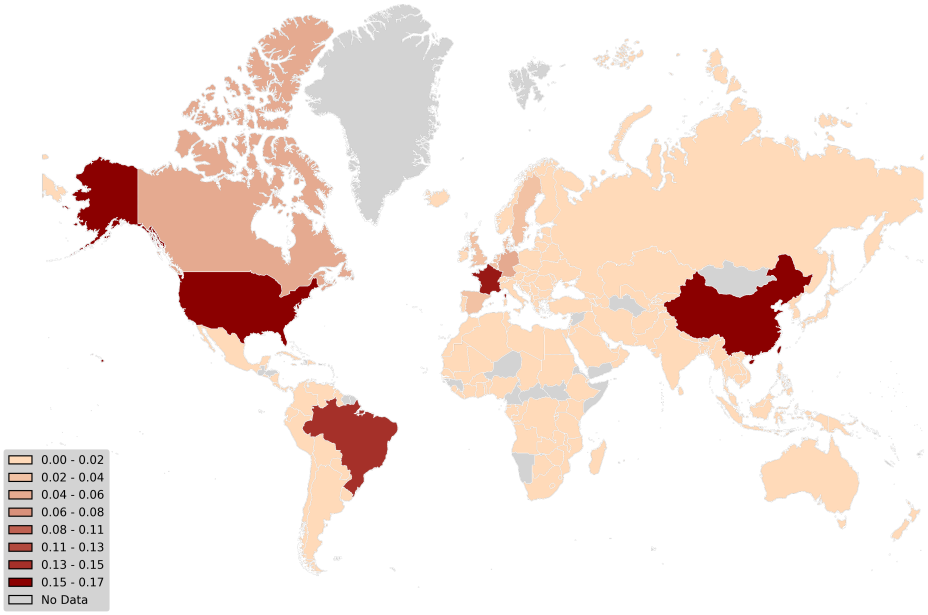
(C) **CONTROL MNES ONLY**



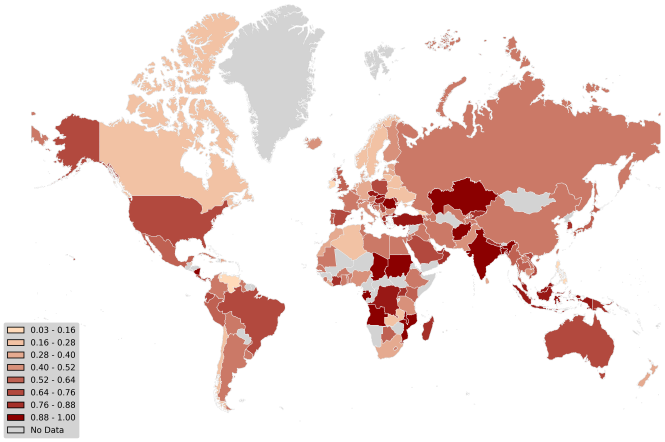
Notes: This figure plots the geographical distribution of the sample of MNE groups available in the data. I use the information on the headquarter jurisdiction of the MNE group for this analysis. Panel (A) plots the jurisdiction-wise share of MNE groups in the entire sample. Panels (B) and (C) plot the jurisdiction-wise share of treated groups and control groups, respectively, in the entire sample of MNE groups within that specific jurisdiction.

FIGURE F.III: **GEOGRAPHICAL COVERAGE OF INCORPORATIONS**

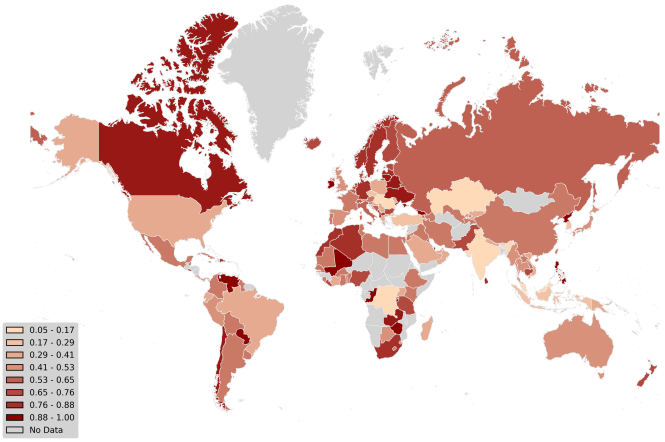
(A) **ALL MNES**



(B) **TREATED MNES ONLY**



(C) **CONTROL MNES ONLY**

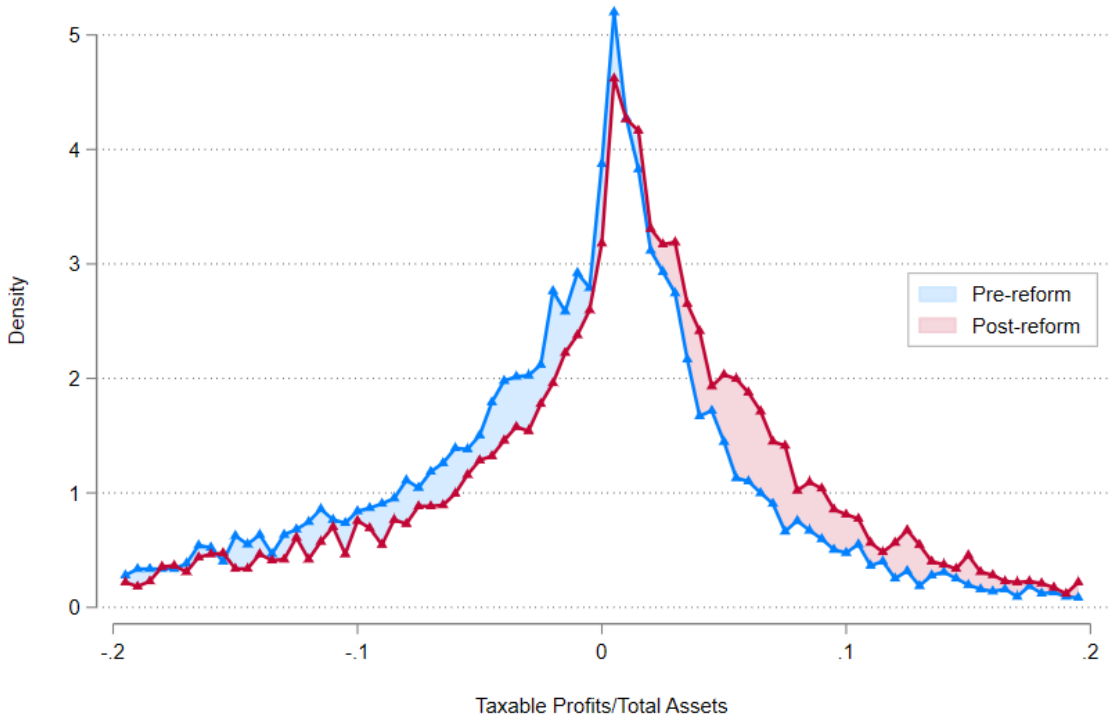


Notes: This figure plots the geographical distribution of the new group-level incorporations data used in this analysis. Panel (A) plots the share of jurisdiction-specific incorporations within the entire sample of worldwide MNE incorporations. Panels (B) and (C) plot the jurisdiction-wide share of incorporations made by the treated and control group, respectively, against the total number of jurisdiction-wide incorporations by both of the groups.

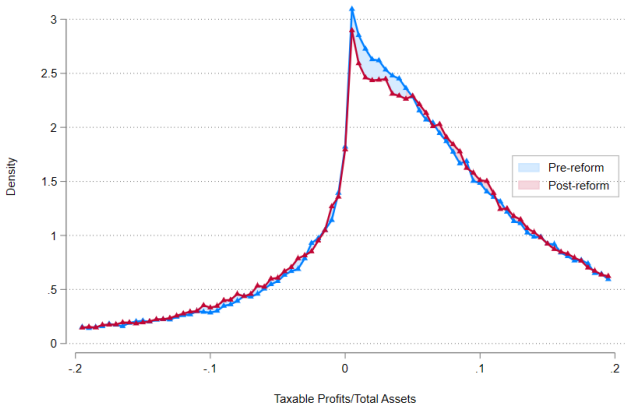
F.2 Additional Stylized Facts

FIGURE F.I: **PROFIT SHIFTING**

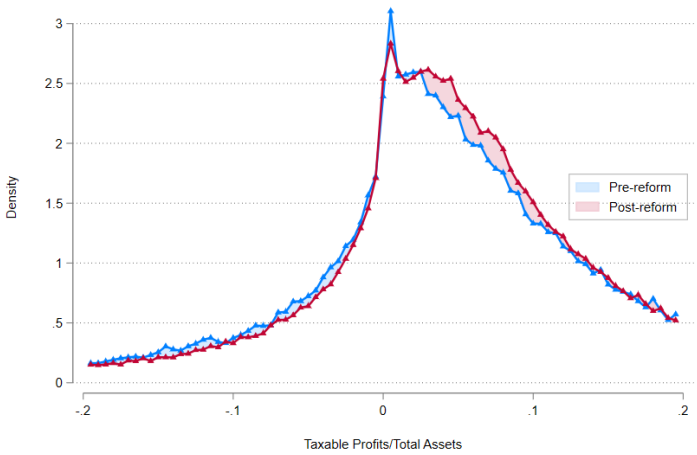
(A) **TREATED AFFILIATES**



(B) **CONTROL AFFILIATES**

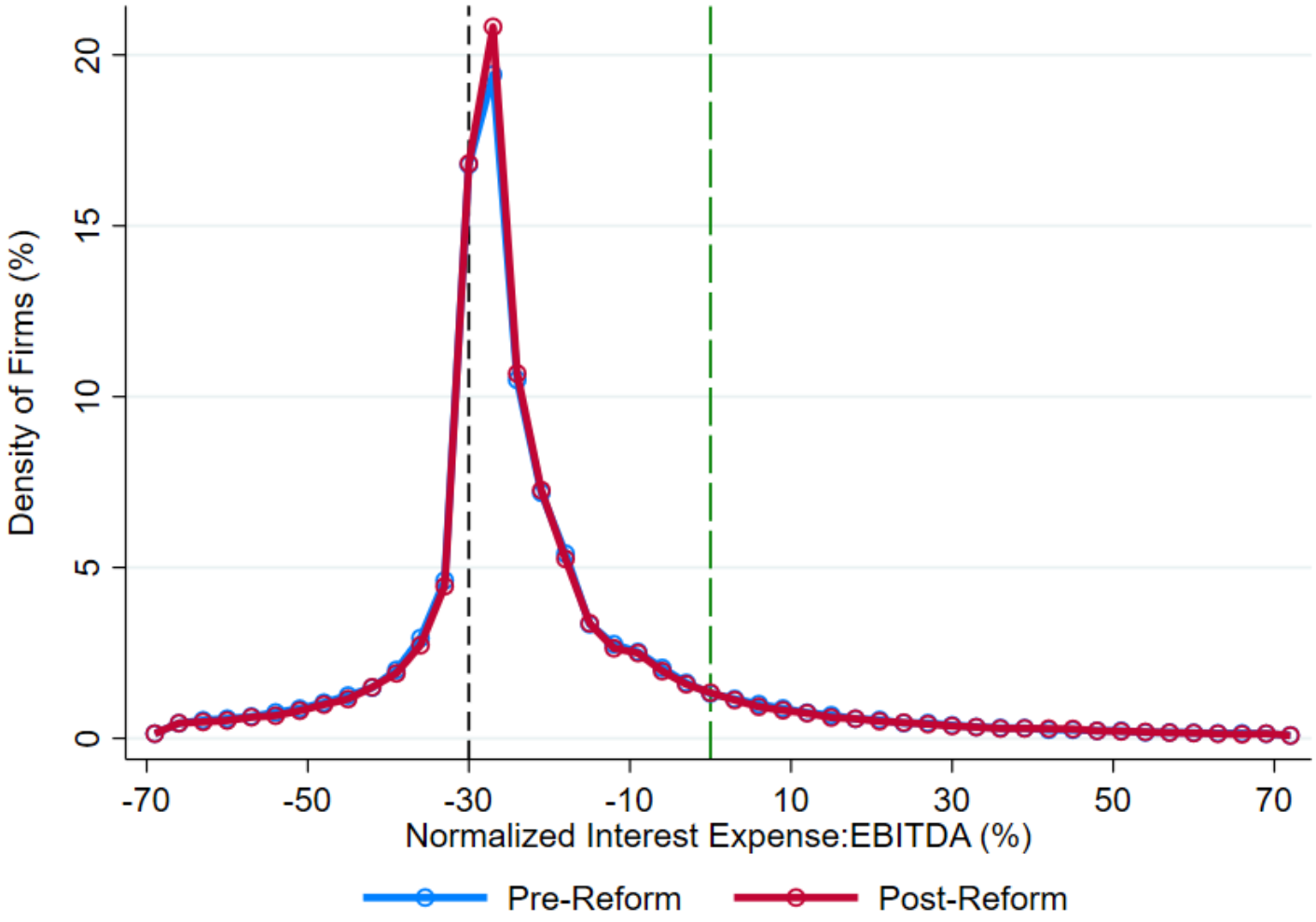


(C) **SISTER AFFILIATES**



Notes: This figure plots the pre-reform and post-reform pooled distribution of the ratio of taxabale profits to total assets for treated affiliates, control affiliates and sister affiliates in Panels (A), (B), and (C), respectively. The data used in the analysis spans 4 pre-treatment periods and 5 post-treatment periods. The ratio is trimmed at -0.2 and 0.2 and is divided into bins of size 0.005

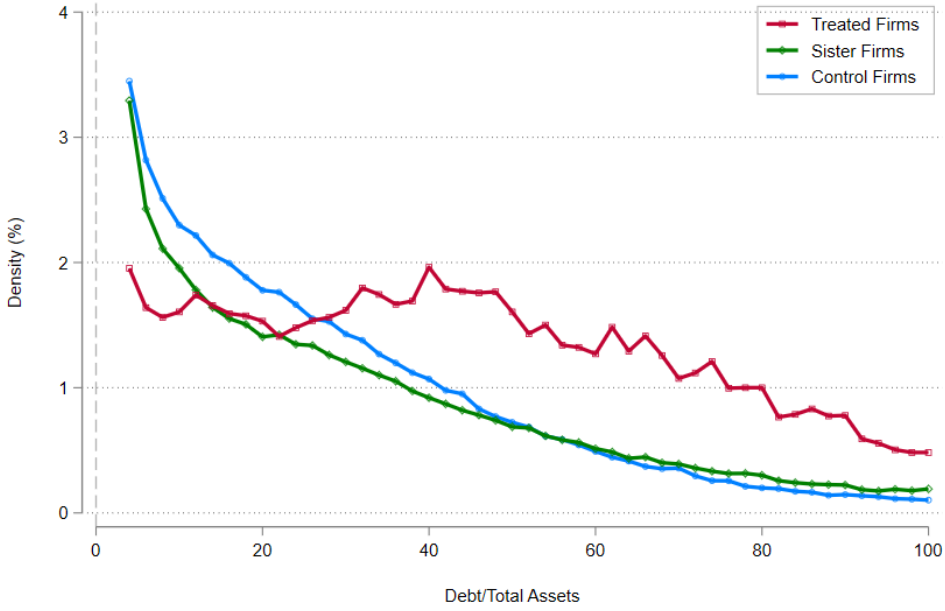
FIGURE F.II: **BUNCHING - PLACEBO**



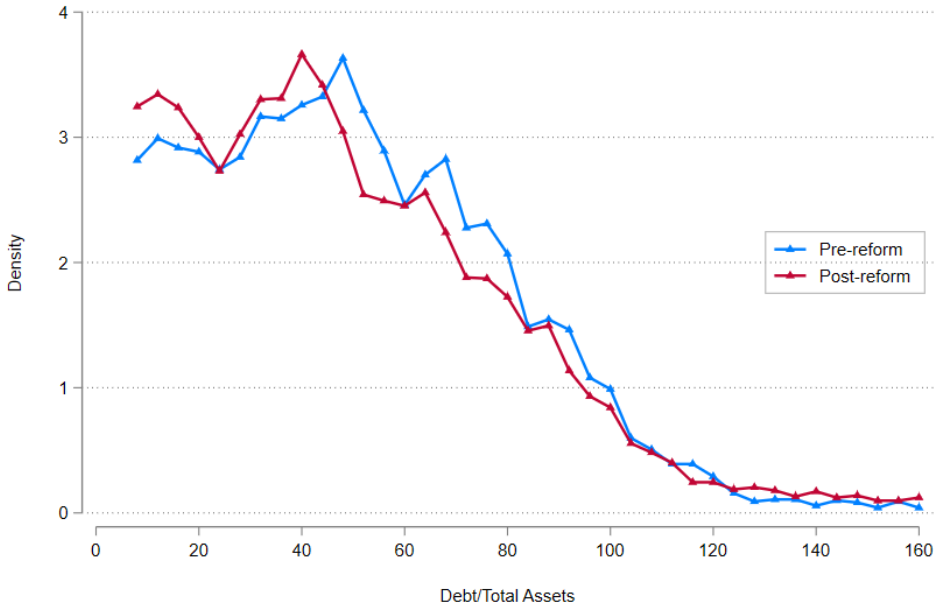
Notes: This figure displays the pre-treatment and post-treatment pooled distribution of the normalized interest expense-to-EBITDA ratio at the affiliate level. The data spans 4 pre-treatment periods and 5 post-treatment periods. Of the 36 rule-enforcing jurisdictions in the sample, 22 feature a net interest expense threshold of 30% of EBITDA. Consequently, once I normalize the exemption threshold at 0 for these jurisdictions, I get a spike at -30% (or 0% in non-normalized setting). The bin size is 0.03, and the range of the ratio is between -0.7 and 0.7.

FIGURE F.III: **PROPORTION OF ASSETS FINANCED THROUGH DEBT**

(A) **ALL AFFILIATES**



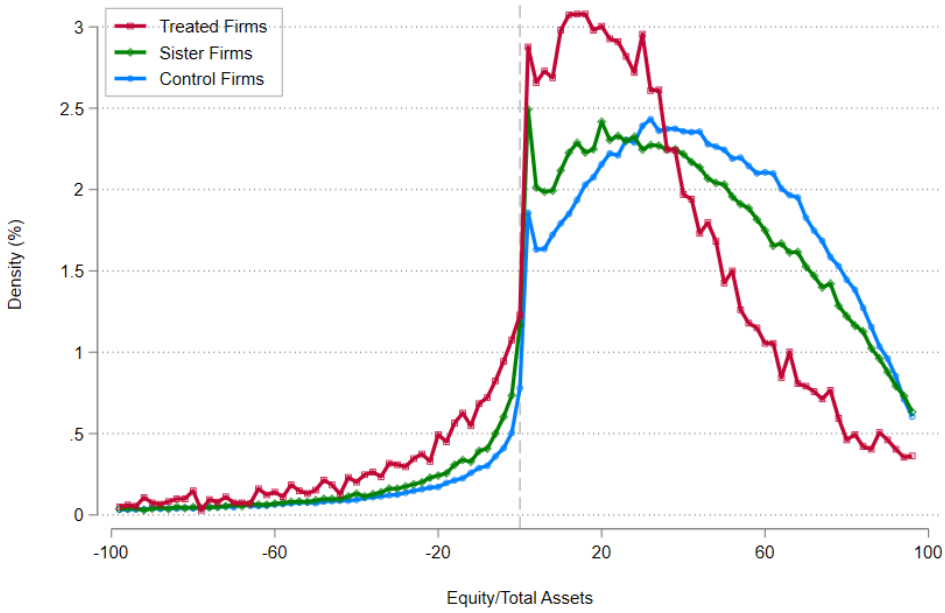
(B) **TREATED AFFILIATES ONLY**



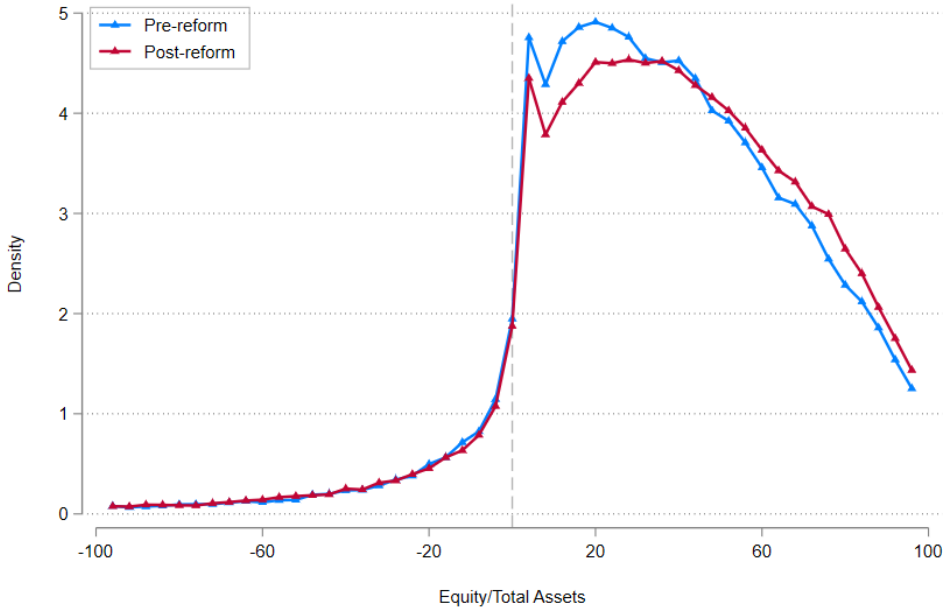
Notes: This figure plots the debt ratio, i.e., percentage of total assets financed through debt, at the affiliate level. The data used in the analysis spans 4 pre-treatment periods and 5 post-treatment periods. Panel (A) plots the pooled distribution of this ratio for each of the group of affiliates, i.e., treated, sister, and control, separately. The ratio is trimmed at 0 and 100 and the bin size is of 0.2. Panel (B) plots this ratio for treated affiliates only by pooling all the pre-treatment years and post-treatment years, separately. The ratio is trimmed at 0 and 160 and the bin size is of 0.4.

FIGURE F.IV: **PROPORTION OF ASSETS FINANCED THROUGH EQUITY**

(A) **ALL AFFILIATES**



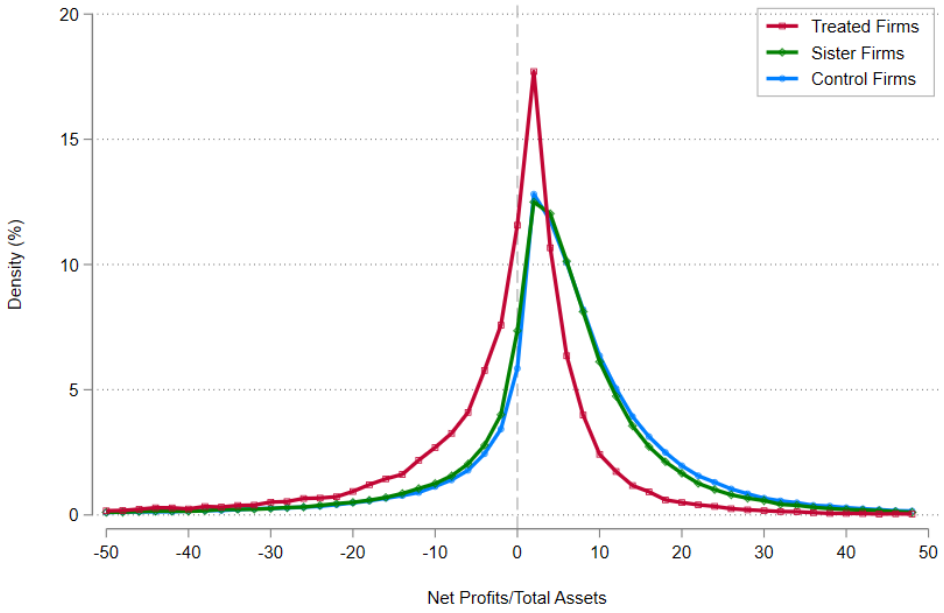
(B) **TREATED AFFILIATES ONLY**



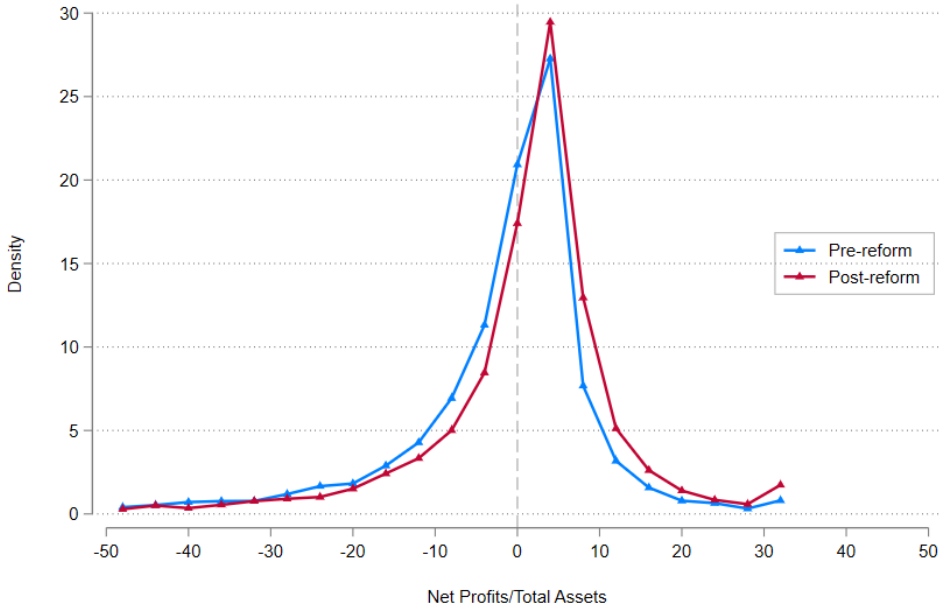
Notes: This figure plots the equity ratio, i.e., percentage of total assets financed through equity, at the affiliate level. The data used in the analysis spans 4 pre-treatment periods and 5 post-treatment periods. Panel (A) plots the pooled distribution of this ratio for each of the group of affiliates, i.e., treated, sister, and control, separately. The ratio is trimmed at -100 and 100 and the bin size is of 0.2. Panel (B) plots this ratio for treated affiliates only by pooling all the pre-treatment years and post-treatment years, separately. The ratio is trimmed at -100 and 100 and the bin size is of 0.4.

FIGURE F.V: **PROPORTION OF ASSETS FINANCED THROUGH RETAINED EARNINGS**

(A) **ALL FIRMS**



(B) **TREATED FIRMS ONLY**

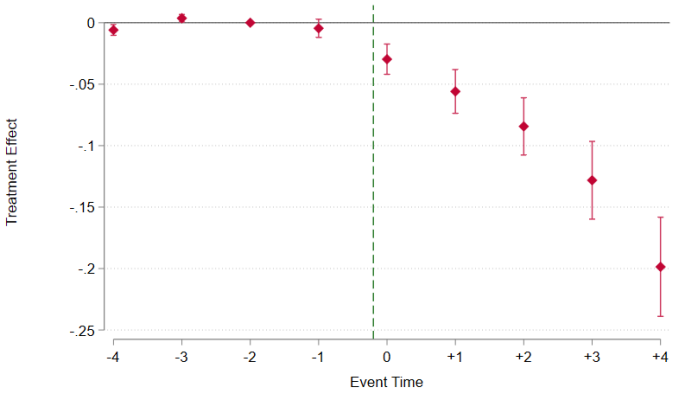


Notes:This figure plots the retained earnings ratio, i.e., percentage of total assets financed through retained earning, at the affiliate level. The data used in the analysis spans 4 pre-treatment periods and 5 post-treatment periods. Panel (A) plots the pooled distribution of this ratio for each of the group of affiliates, i.e., treated, sister, and control, separately. The ratio is trimmed at -50 and 50 and the bin size is of 0.2. Panel (B) plots this ratio for treated affiliates only by pooling all the pre-treatment years and post-treatment years, separately. The ratio is trimmed at -50 and 30 and the bin size is of 0.4.

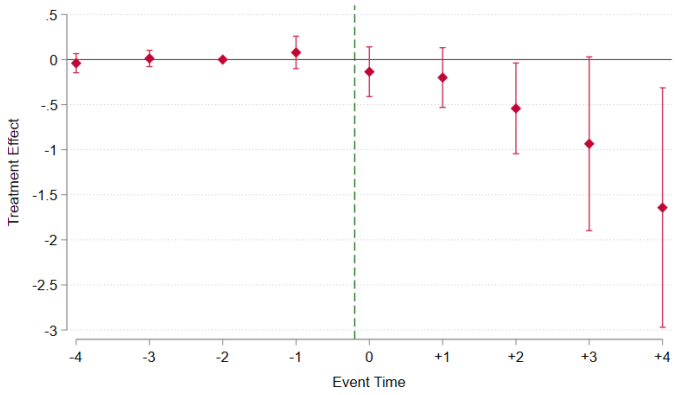
F.3 Additional Baseline Graphs

FIGURE F.I: **FINANCIAL RATIOS - TREATED AFFILIATES**

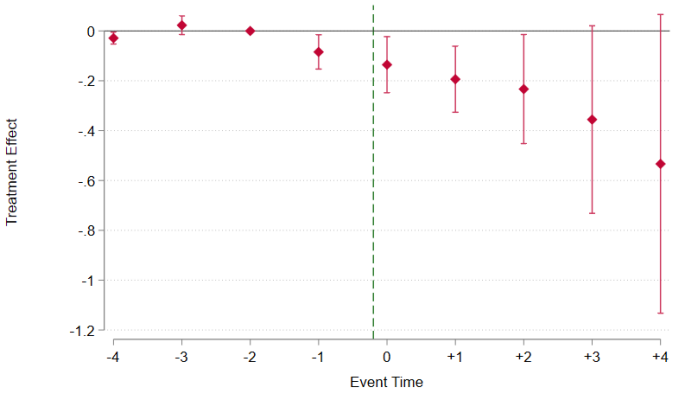
(A) **DEBT:ASSETS**



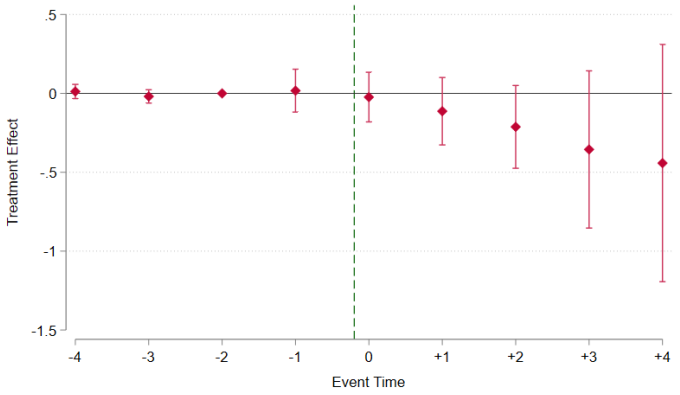
(B) **DEBT:EQUITY**



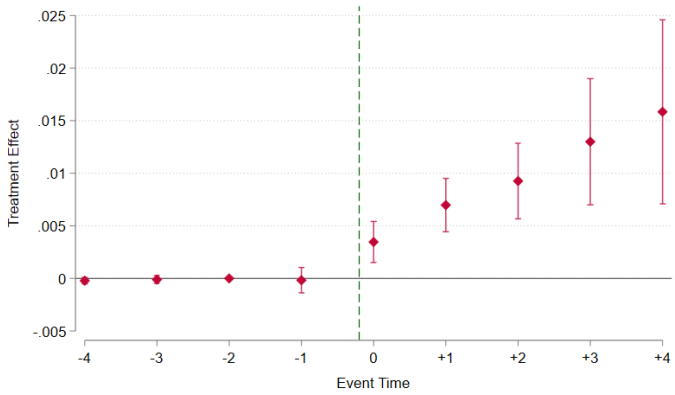
(C) **INTEREST EXPENSE:EBITDA**



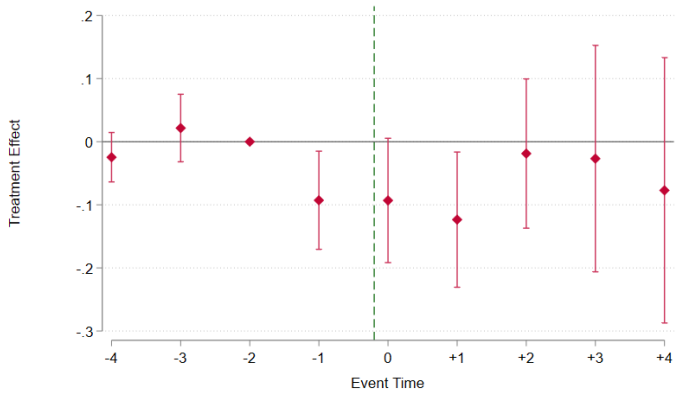
(D) **NET INTEREST:EBITDA**



(E) **TAXATION:ASSETS**



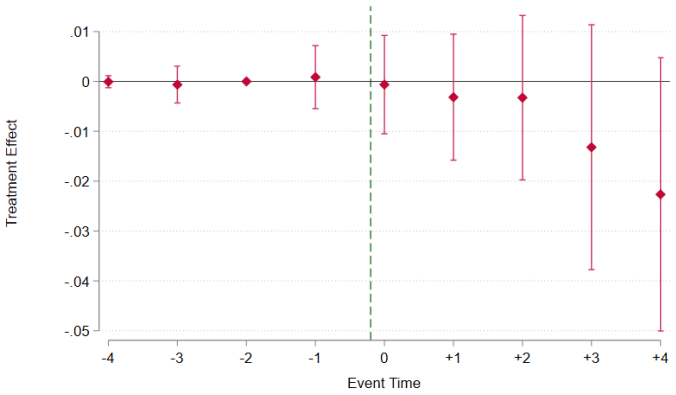
(F) **INVESTMENT RATE**



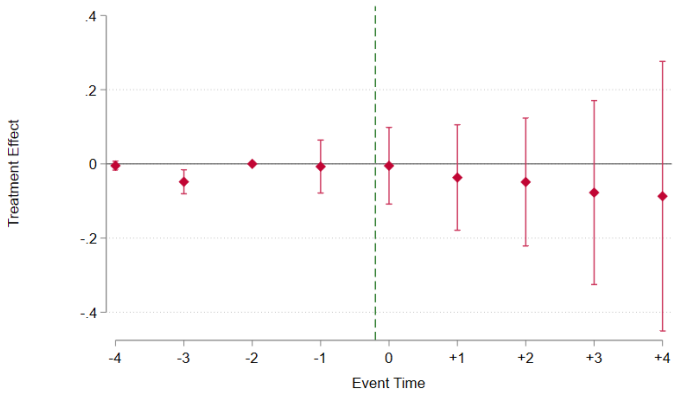
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of treated affiliates are compared with that of control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate 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.II: **FINANCIAL RATIOS - GROUP LEVEL**

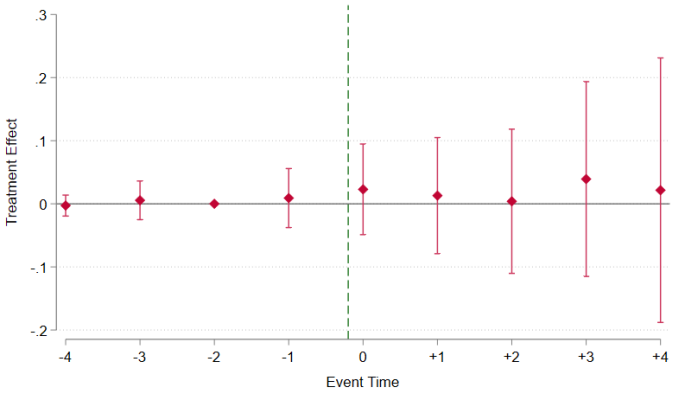
(A) **DEBT:ASSETS**



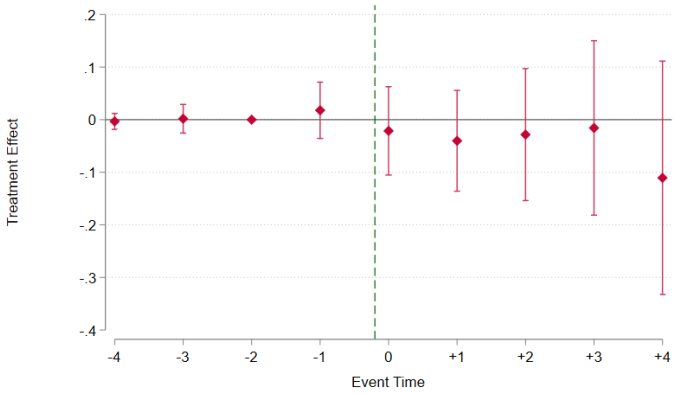
(B) **DEBT:EQUITY**



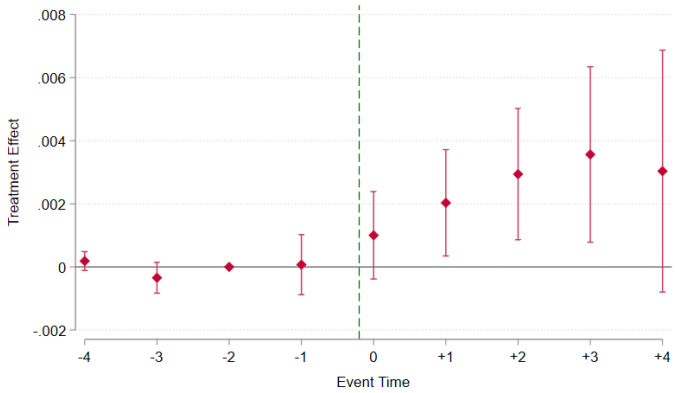
(C) **INTEREST EXPENSE:EBITDA**



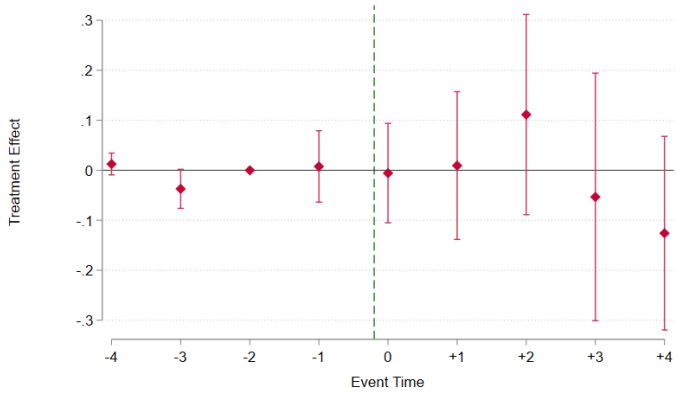
(D) **NET INTEREST:EBITDA**



(E) **TAXATION:ASSETS**



(F) **INVESTMENT RATE**

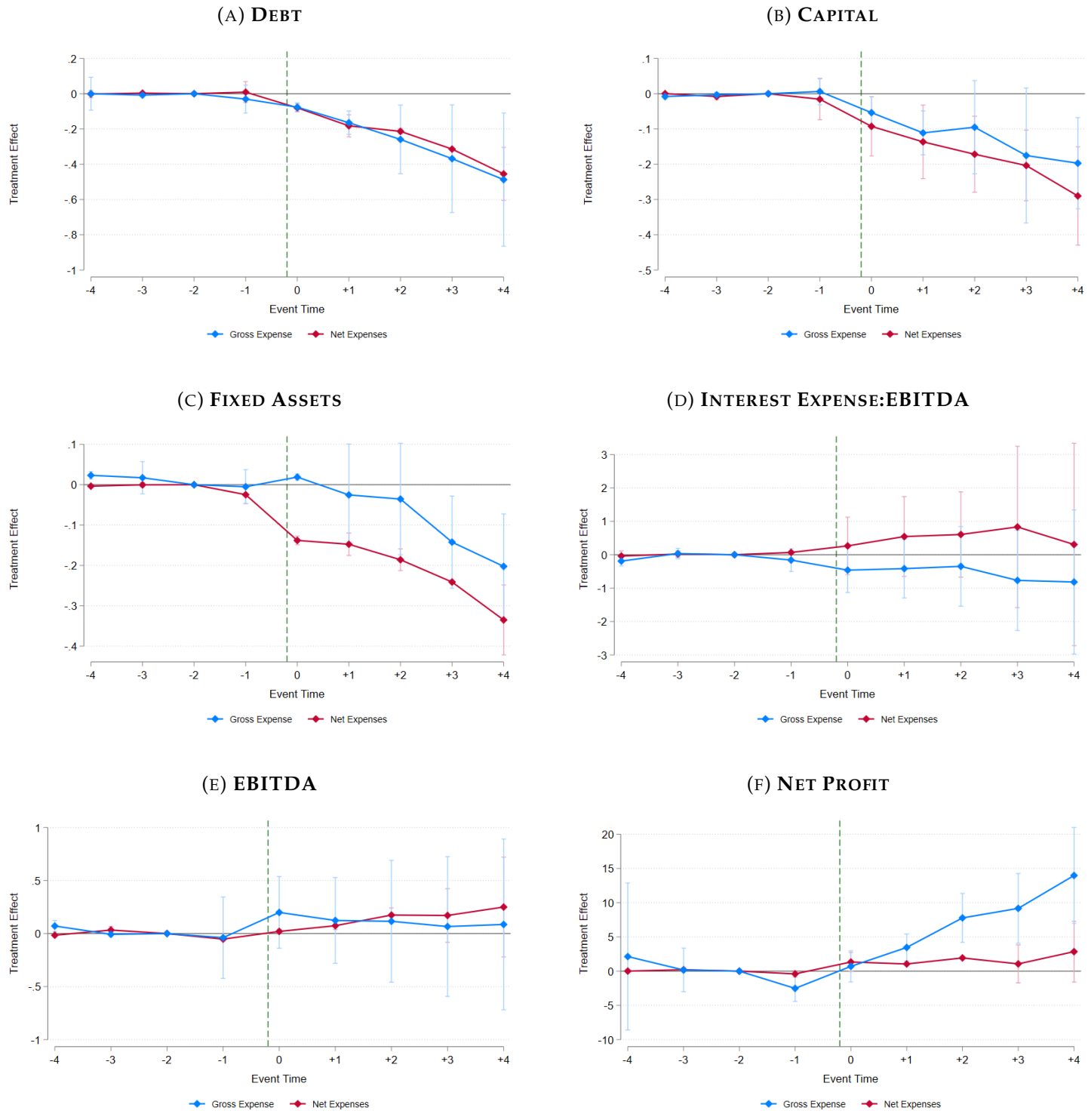


Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the MNE group level. The outcomes of treated groups are compared with that of control group. The reference year is $t^* - 2$ and I include group and year fixed effects, group-specific linear trends, and time-varying jurisdiction-level controls. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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.

G Heterogeneity Analysis

G.1 Deductions Type

FIGURE G.I: **CAPITAL & INVESTMENTS - TREATED AFFILIATES**

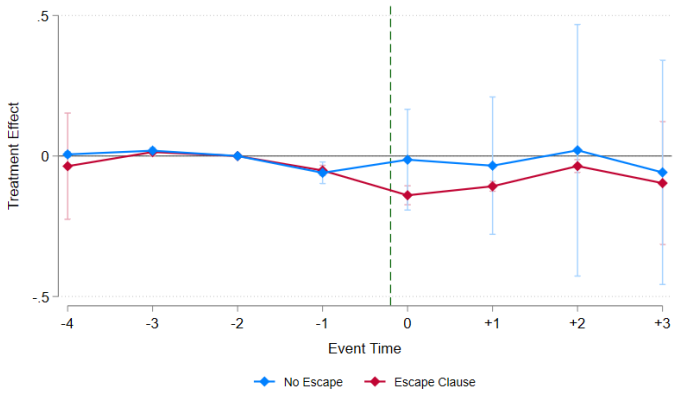


Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of treated affiliates in jurisdictions with net interest expense rule are compared with the control group and 2) the outcomes of treated affiliates in jurisdictions with gross interest expense rule are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

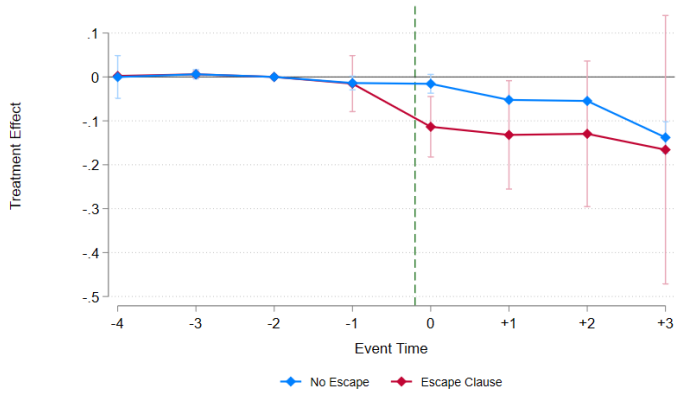
G.2 Group Escape Provisions

FIGURE G.I: **CAPITAL & INVESTMENTS - TREATED AFFILIATES**

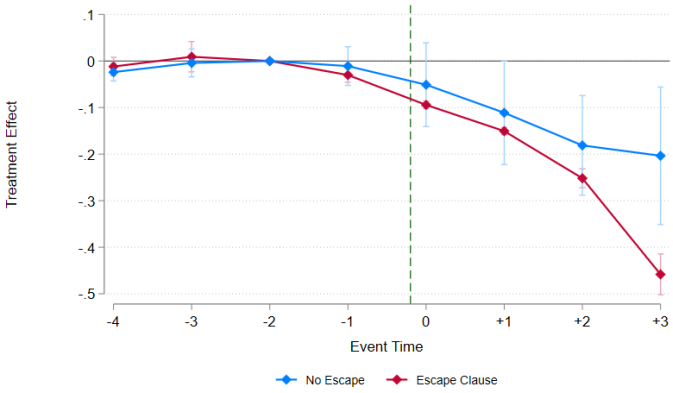
(A) **EQUITY**



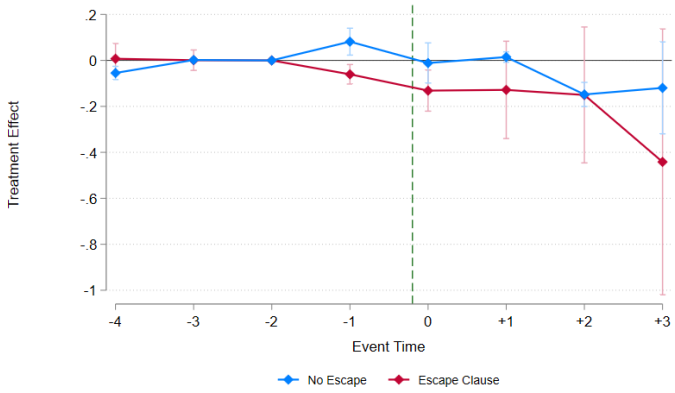
(B) **TOTAL ASSETS**



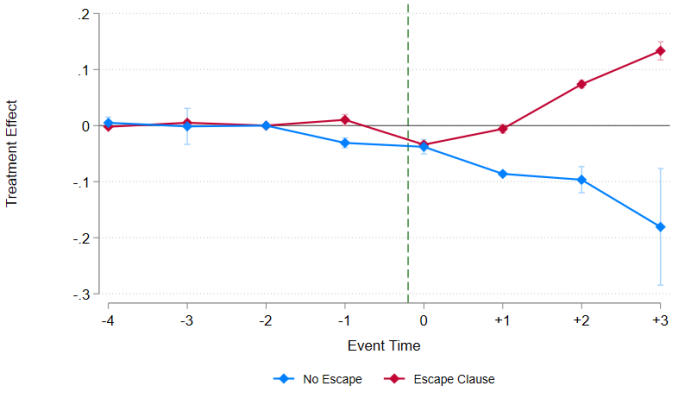
(C) **DEBT:ASSETS**



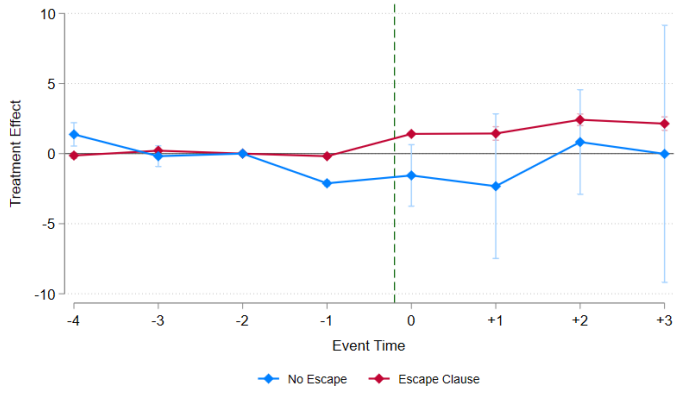
(D) **DEBT:EQUITY**



(E) **REVENUE**



(F) **NET PROFIT**

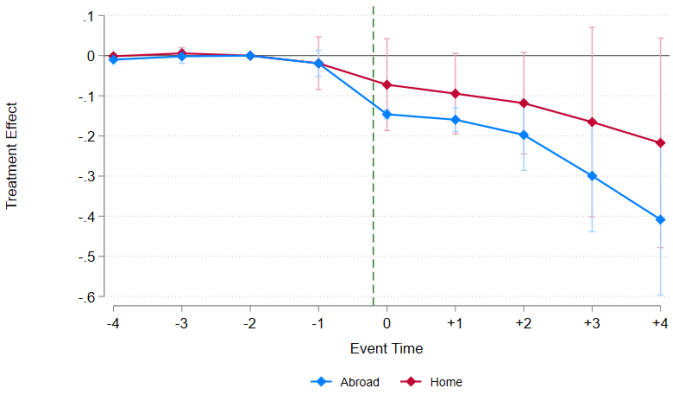


Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of treated affiliates in jurisdictions with group escape provisions are compared with the control group and 2) the outcomes of treated affiliates in jurisdictions without these provisions are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

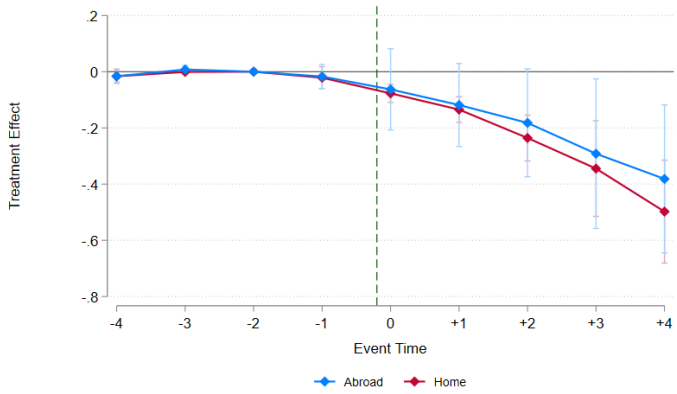
G.3 Domestic vs Foreign Affiliates

FIGURE G.I: **CAPITAL & INVESTMENTS - TREATED AFFILIATES**

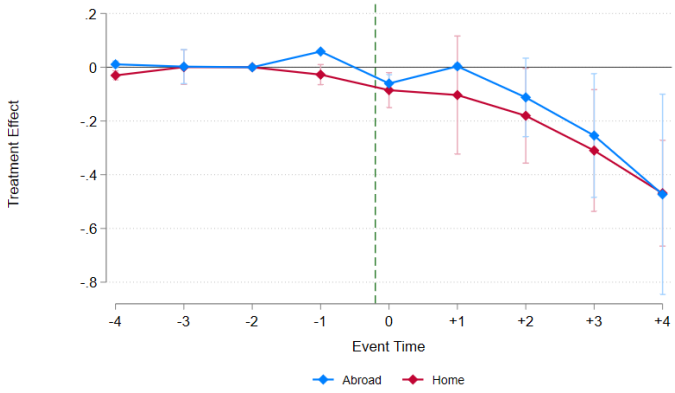
(A) **FIXED ASSETS**



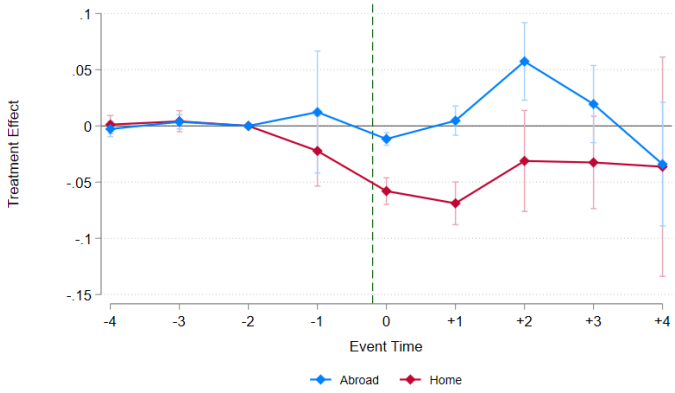
(B) **DEBT:ASSETS**



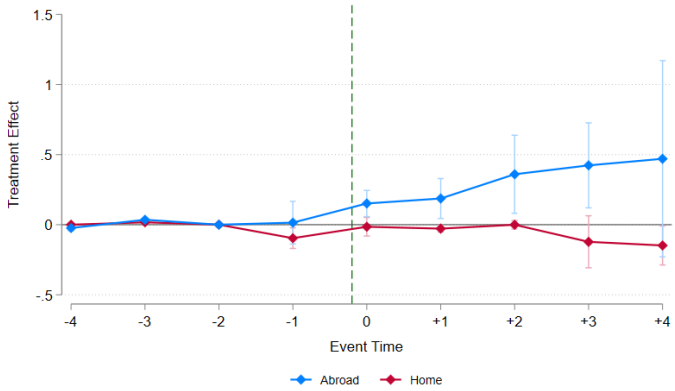
(C) **DEBT:EQUITY**



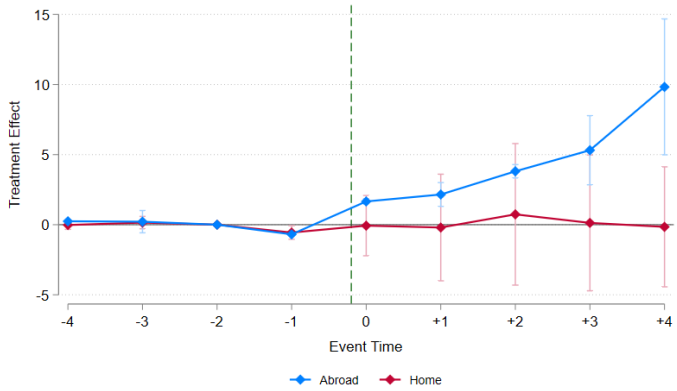
(D) **REVENUE**



(E) **EBITDA**



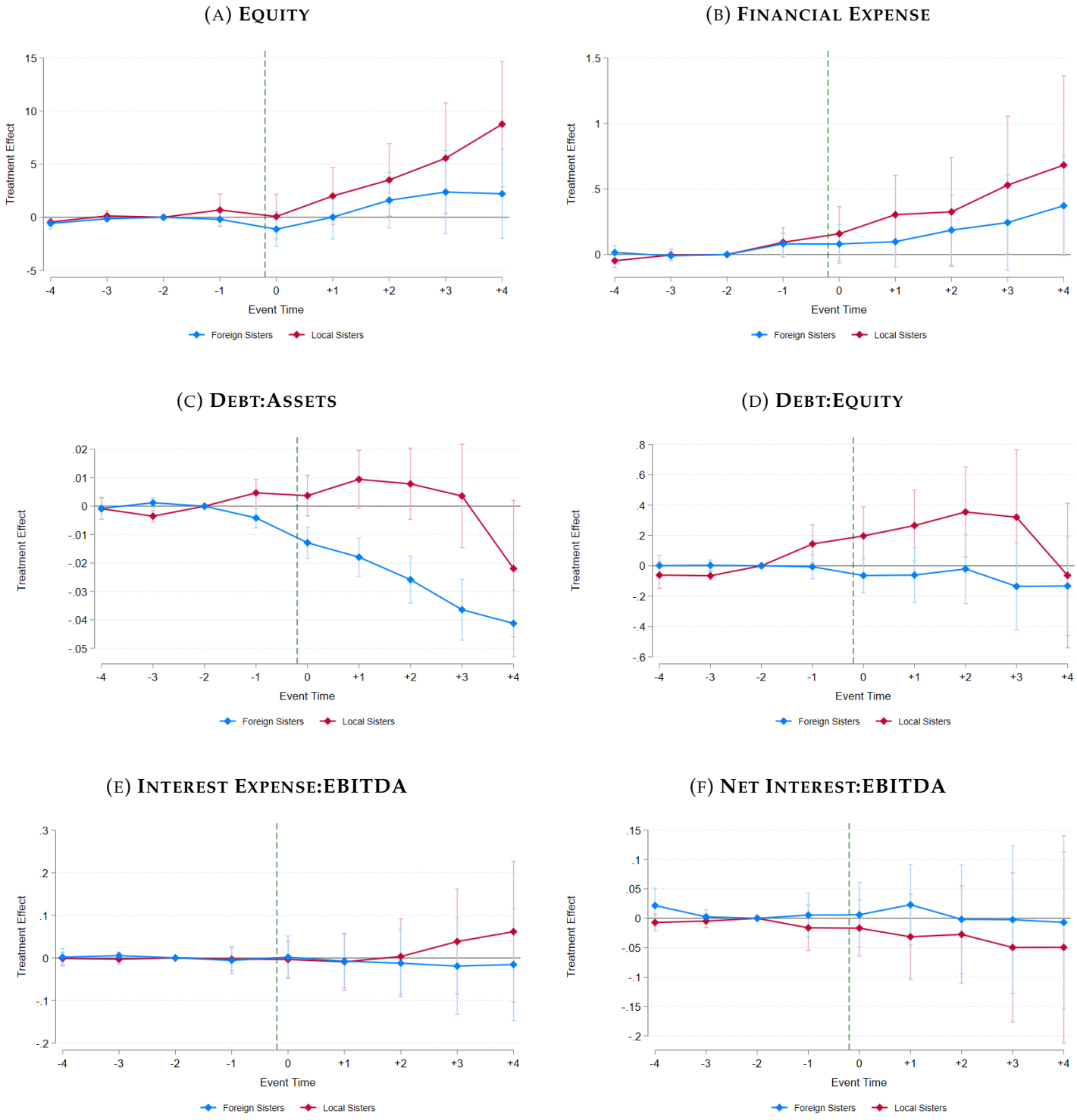
(F) **NET PROFIT**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of treated affiliates in home jurisdictions are compared with the control group and 2) the outcomes of treated affiliates in foreign jurisdictions are compared with the control group; sister affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

G.4 Reallocation to Local & Foreign Sisters

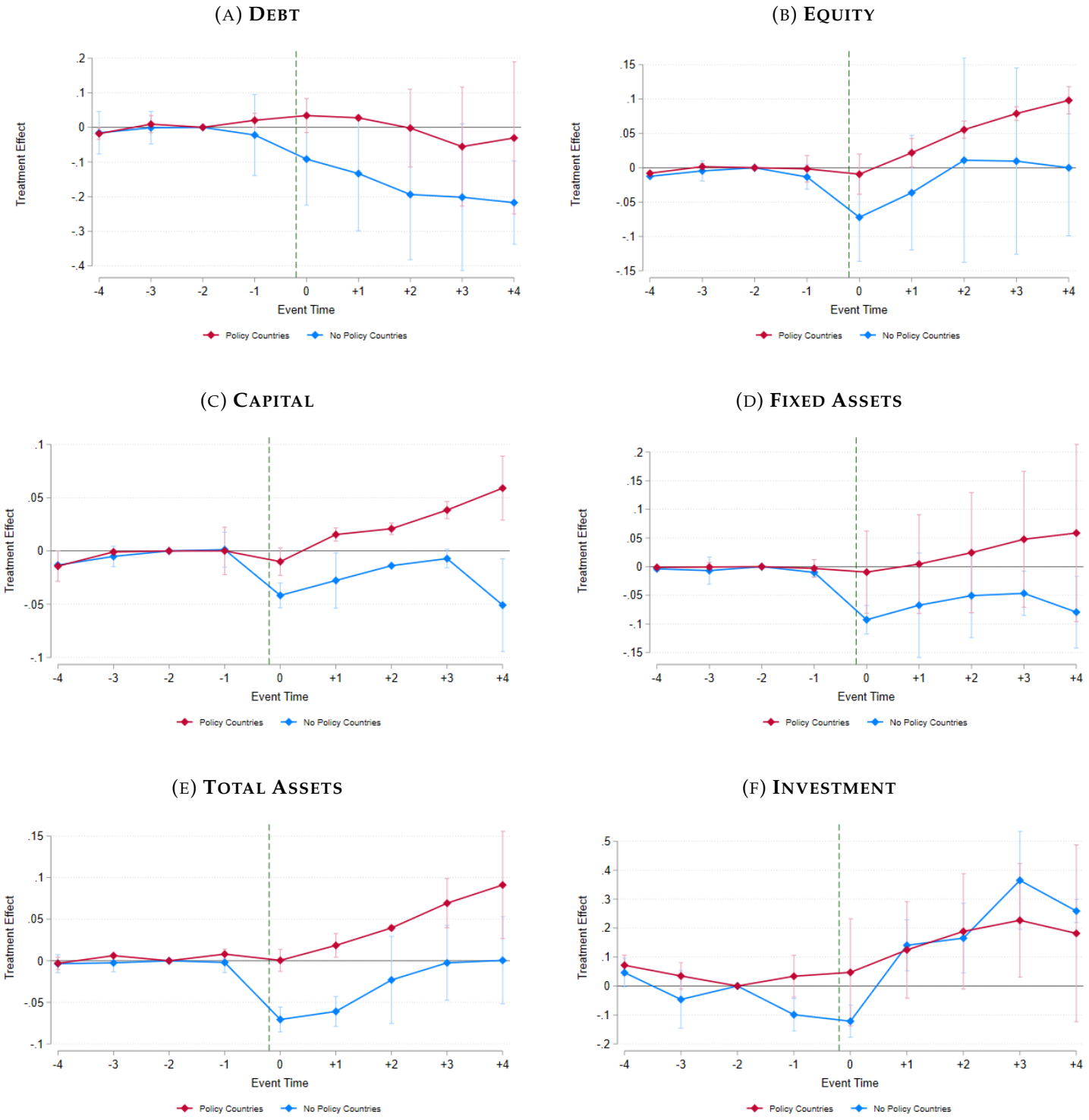
FIGURE G.I: **CAPITAL & INVESTMENTS - SISTER AFFILIATES**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of local sister affiliates are compared with the control group and 2) the outcomes of foreign sister affiliates are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

G.5 Reallocation to Foreign Sisters in ESR & non-ESR Jurisdictions

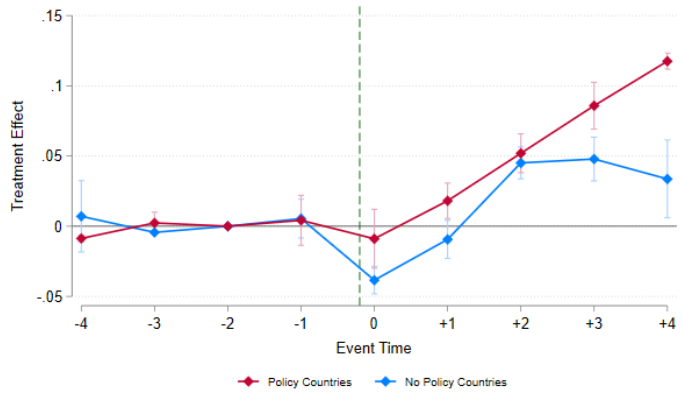
FIGURE G.I: **CAPITAL & INVESTMENTS - SISTER AFFILIATES**



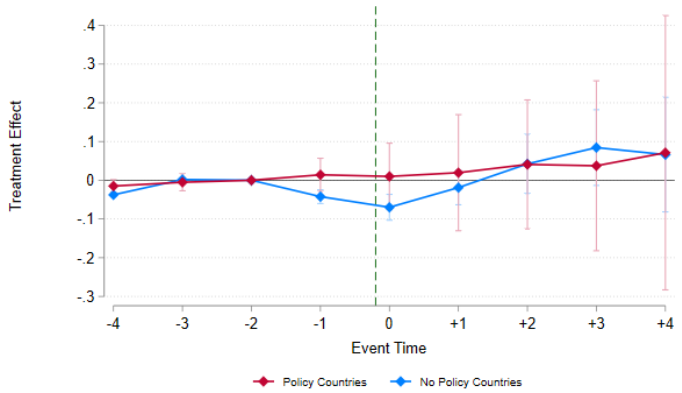
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of foreign sister affiliates in ESR enforcing jurisdictions are compared with the control group and 2) the outcomes of foreign sister affiliates in other jurisdictions are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE G.II: **OPERATIONS - SISTER AFFILIATES**

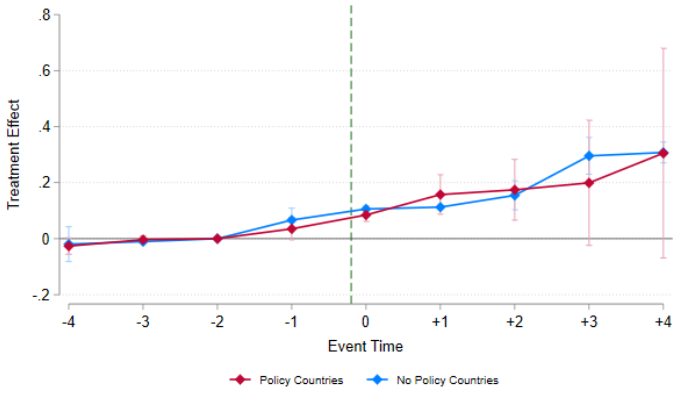
(A) **REVENUE**



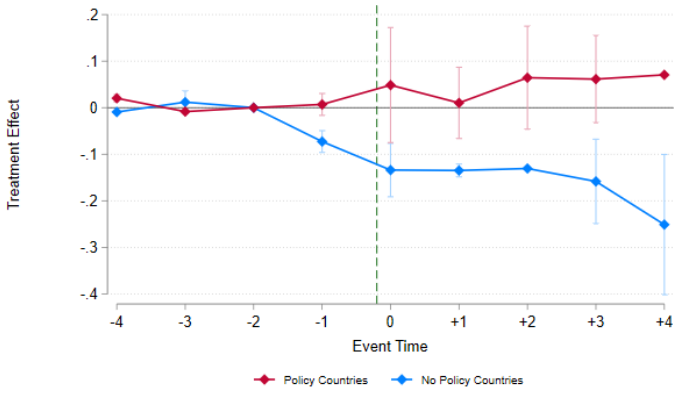
(B) **EBITDA**



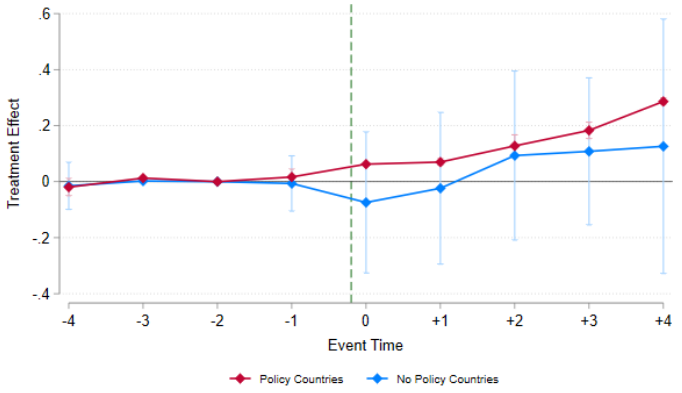
(C) **FINANCIAL EXPENSE**



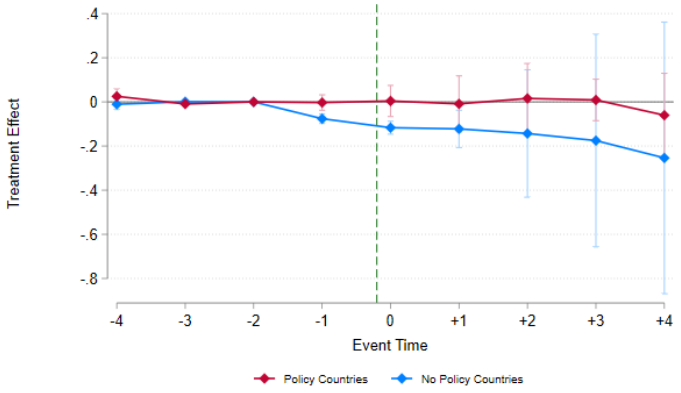
(D) **TAX BASE**



(E) **TAXATION**



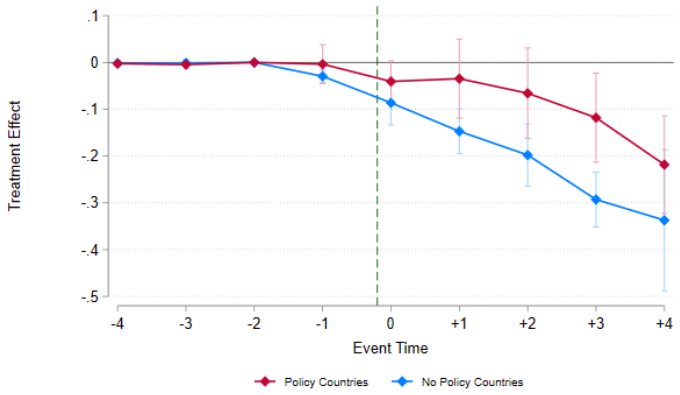
(F) **NET PROFIT**



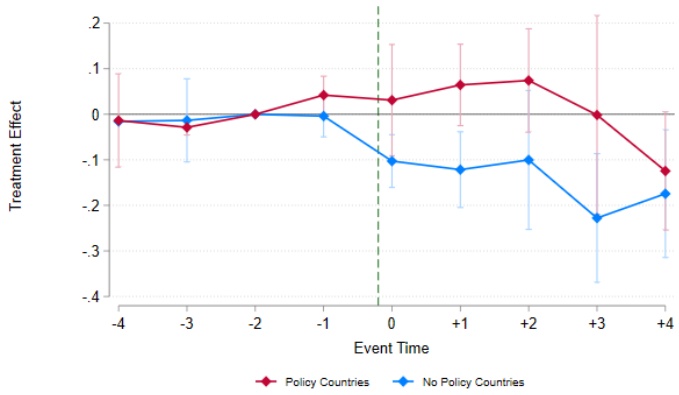
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of foreign sister affiliates in ESR enforcing jurisdictions are compared with the control group and 2) the outcomes of foreign sister affiliates in other jurisdictions are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE G.III: **FINANCIAL RATIOS - SISTER AFFILIATES**

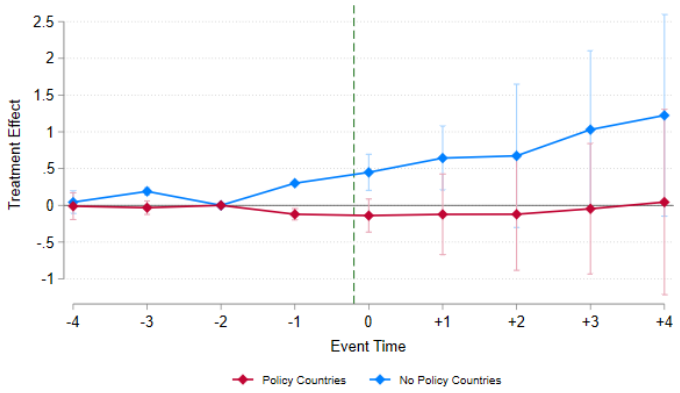
(A) **DEBT:ASSETS**



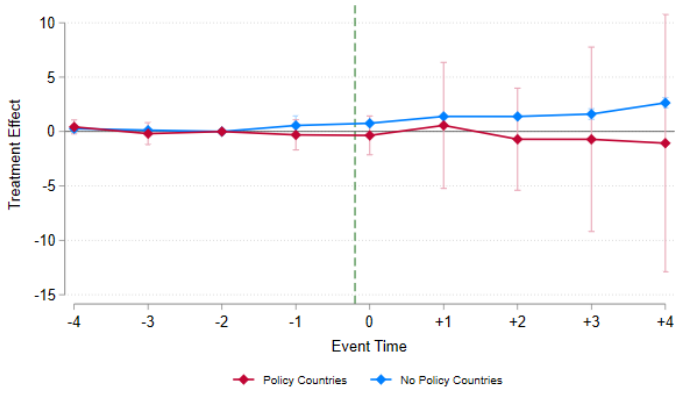
(B) **DEBT:EQUITY**



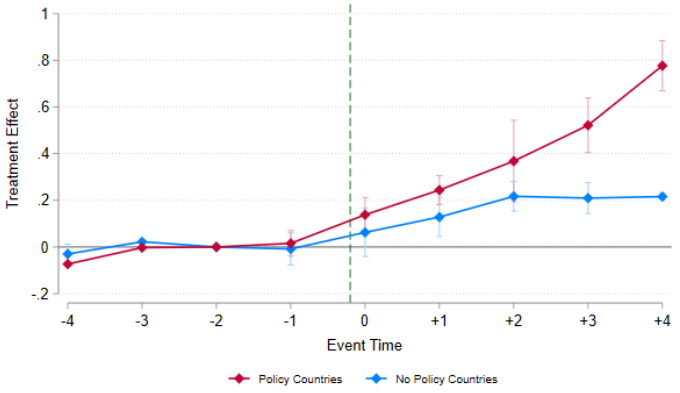
(C) **INTEREST EXPENSE:EBITDA**



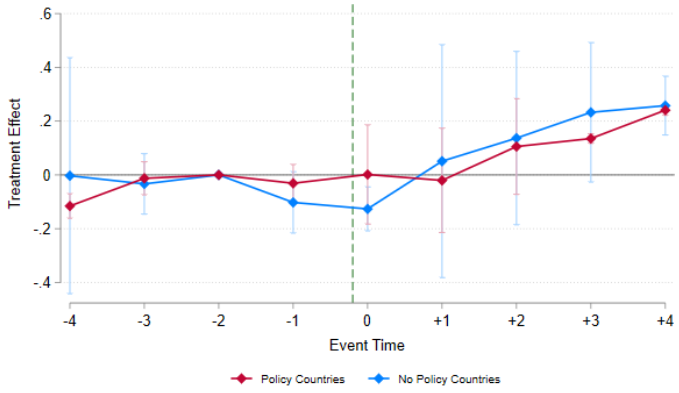
(D) **NET INTEREST:EBITDA**



(E) **TAXATION:ASSETS**



(F) **INVESTMENT RATE**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The estimation equation is run two times: 1) the outcomes of foreign sister affiliates in ESR enforcing jurisdictions are compared with the control group and 2) the outcomes of foreign sister affiliates in other jurisdictions are compared with the control group; treated affiliates are dropped from the analysis. The reference year is $t^* - 2$ and I include firm and year fixed effects, firm-specific linear trends, 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 and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

H Robustness - Different Diff-in-Diff 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 first estimate the effects without unit-specific linear trends with respect to $t^* - 1$ and plot the raw estimates. In this first stage of estimation, 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. Thereafter, I non-parametrically fit a linear trend on pre-baseline coefficients, i.e., $t^* - 4$, $t^* - 3$, and $t^* - 2$, extrapolate the trend to baseline and post-treatment periods and difference out the first stage coefficients (Dobkin *et al.*, 2018).

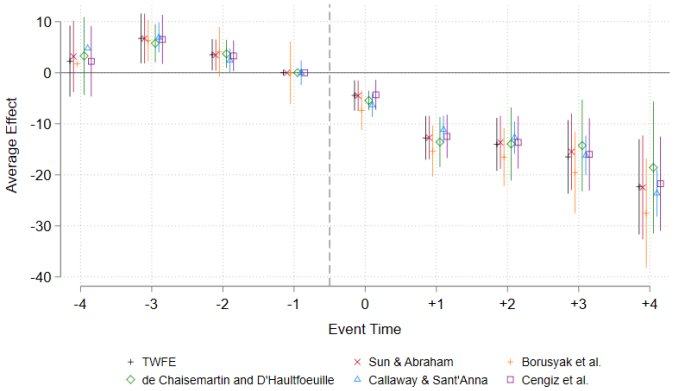
The first alternative estimation is the standard two-way fixed effects (TWFE) estimator with three leads and five lags. Next I run the Sun (2022) STATA estimation as proposed by Sun & Abraham (2021). Thereafter, I estimate Borusyak (2023) coefficients in STATA based on Borusyak *et al.* (2024) with the conditions of *autosample* set *TRUE* and with a tolerance of 0.005. 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 & DHaultfoeuille (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 & Abraham (2021), Callaway & SantAnna (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, if the coefficients from these three settings closely match that of the baseline coefficients based on de Chaisemartin & DHaultfoeuille (2023) then the results are unbiased in my baseline setting as well.

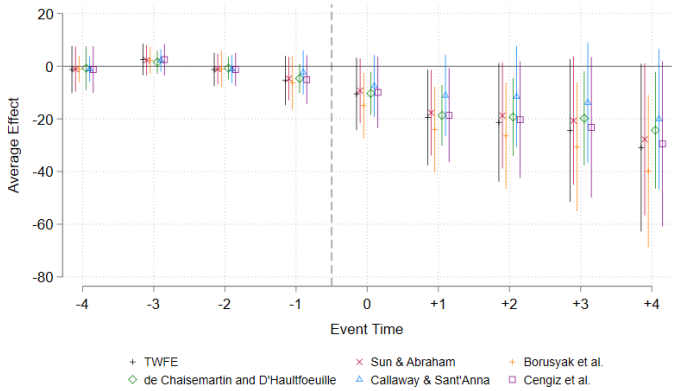
Importantly, unlike the other estimators, the estimates by Callaway & SantAnna (2021) and Borusyak *et al.* (2024) estimate the placebo and treatment coefficients in an asymmetric way (Roth, 2024). Therefore, I scale the placebo coefficients of these two estimators to equal to 0 in the baseline year and de-trend the coefficients in the second stage based on these newly scaled coefficients.

FIGURE H.II: **ROBUSTNESS TO OTHER ESTIMATORS - TREATED AFFILIATES**

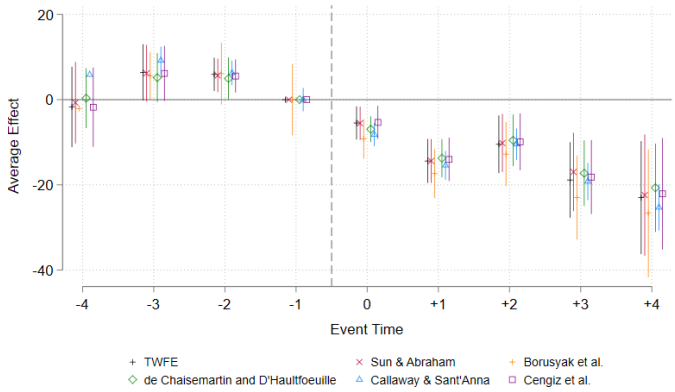
(A) **FIXED ASSETS (RAW ESTIMATES)**



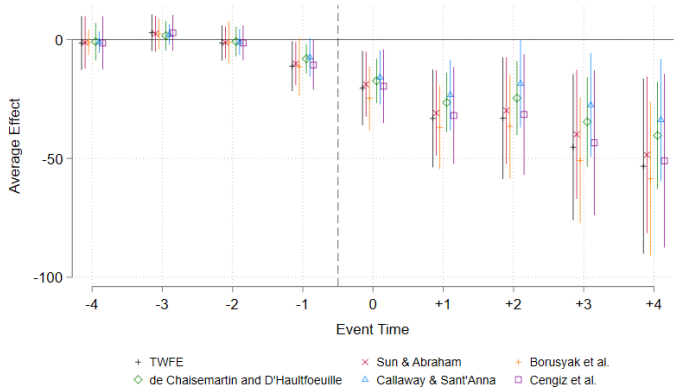
(B) **FIXED ASSETS (DE-TRENDED)**



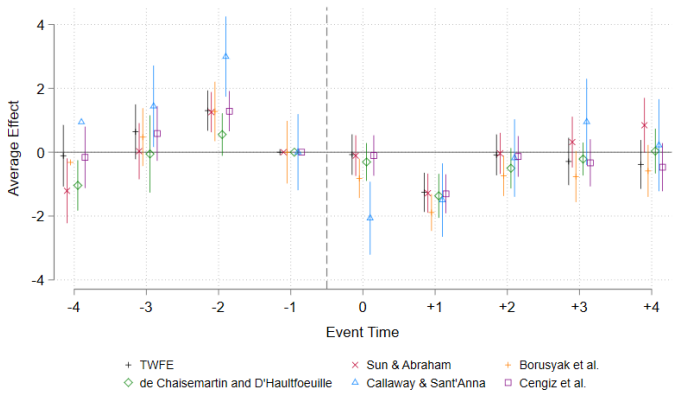
(C) **TOTAL ASSETS (RAW ESTIMATES)**



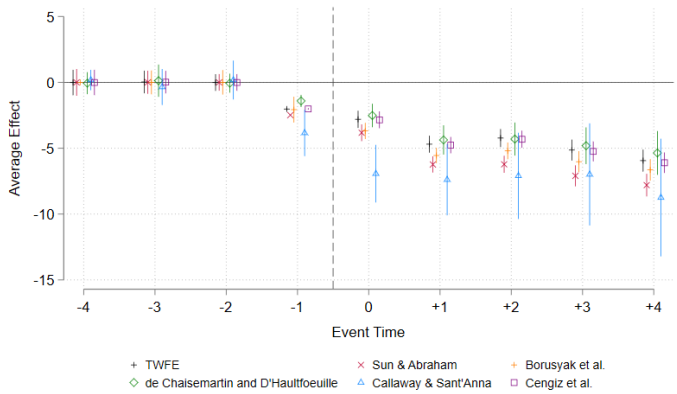
(D) **TOTAL ASSETS (DE-TRENDED)**



(E) **INVESTMENT (RAW ESTIMATES)**



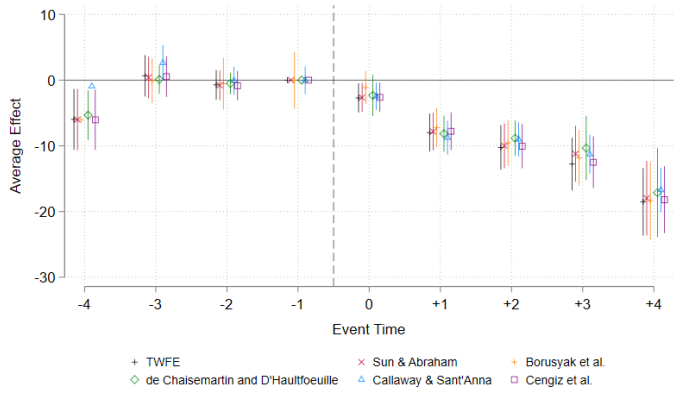
(F) **INVESTMENT (DE-TRENDED)**



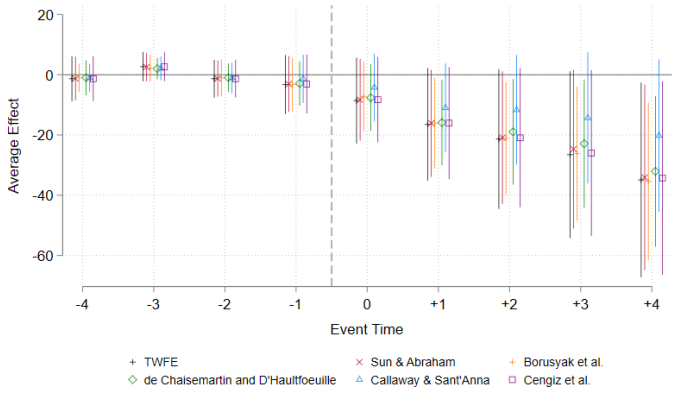
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the treated affiliates are compared with that of control affiliates; sister affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.I: **ROBUSTNESS TO OTHER ESTIMATORS - TREATED AFFILIATES**

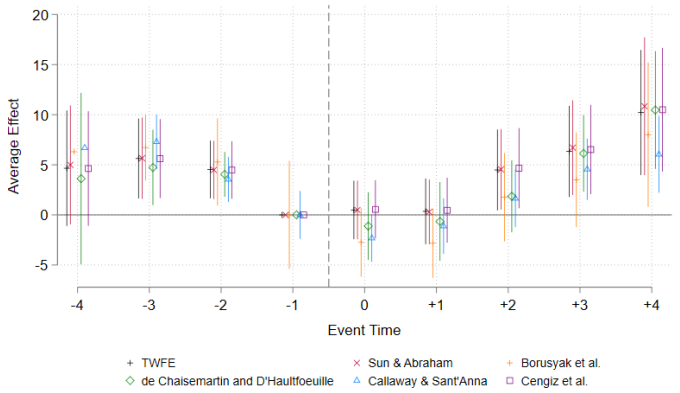
(A) **DEBT (RAW ESTIMATES)**



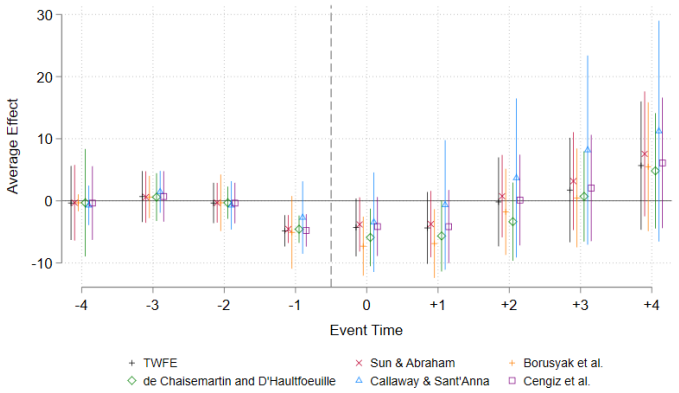
(B) **DEBT (DE-TRENDED)**



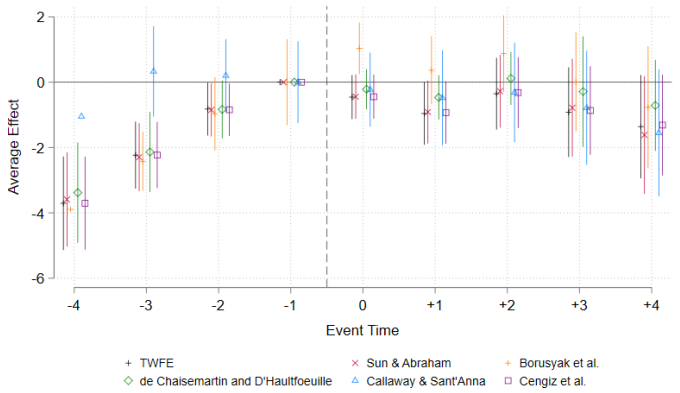
(C) **EQUITY (RAW ESTIMATES)**



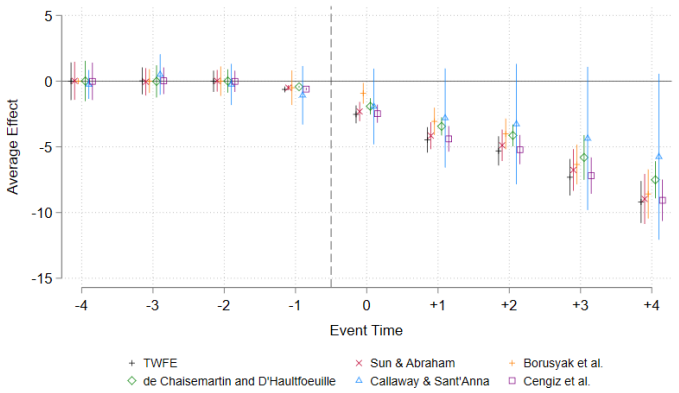
(D) **EQUITY (DE-TRENDED)**



(E) **CAPITAL (RAW ESTIMATES)**



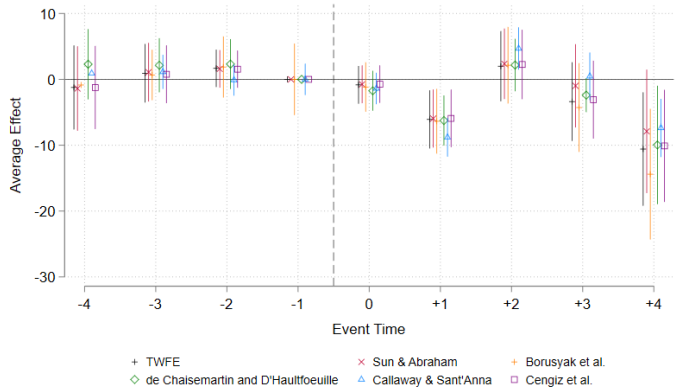
(F) **CAPITAL (DE-TRENDED)**



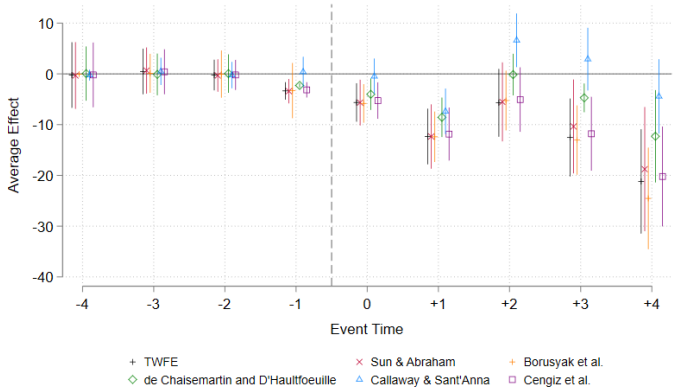
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the treated affiliates are compared with that of control affiliates; sister affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.III: **ROBUSTNESS TO OTHER ESTIMATORS - TREATED AFFILIATES**

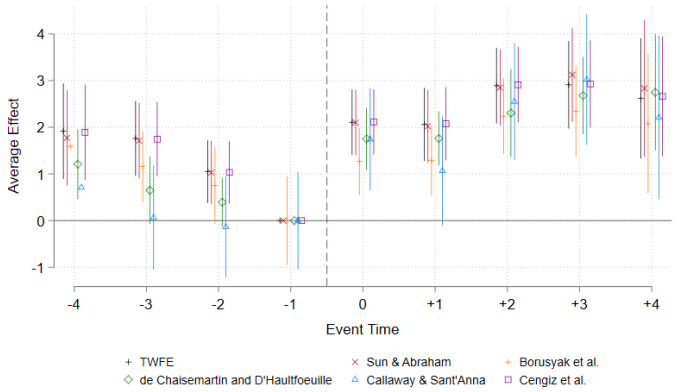
(A) **REVENUE (RAW ESTIMATES)**



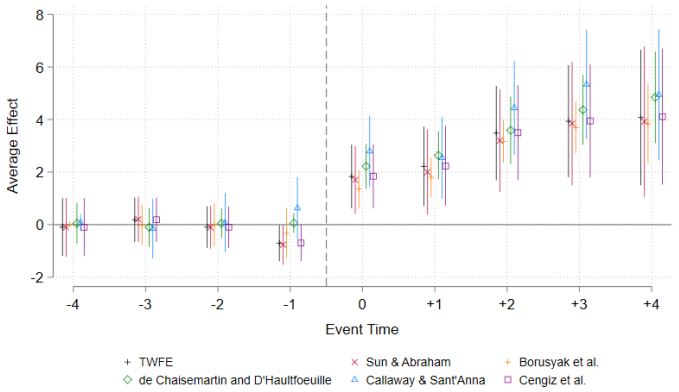
(B) **REVENUE (DE-TRENDED)**



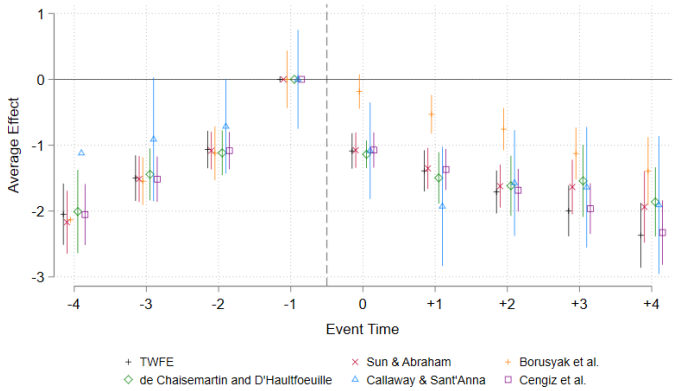
(C) **EBITDA (RAW ESTIMATES)**



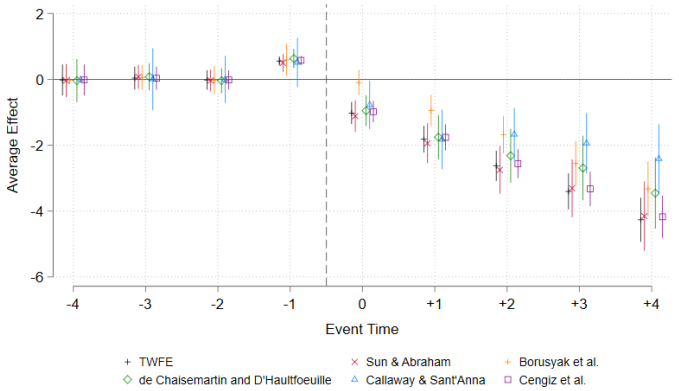
(D) **EBITDA (DE-TRENDED)**



(E) **FINANCIAL EXPENSE (RAW ESTIMATES)**



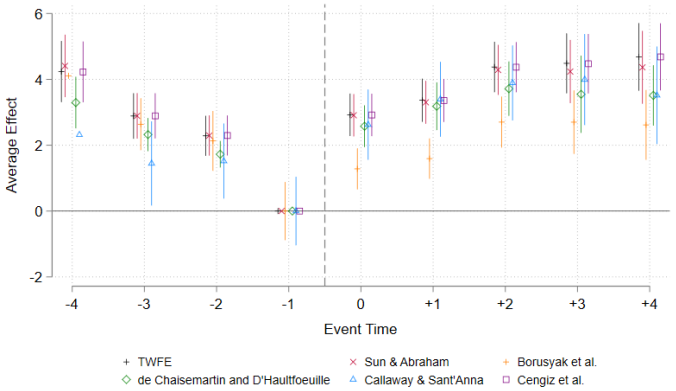
(F) **FINANCIAL EXPENSE (DE-TRENDED)**



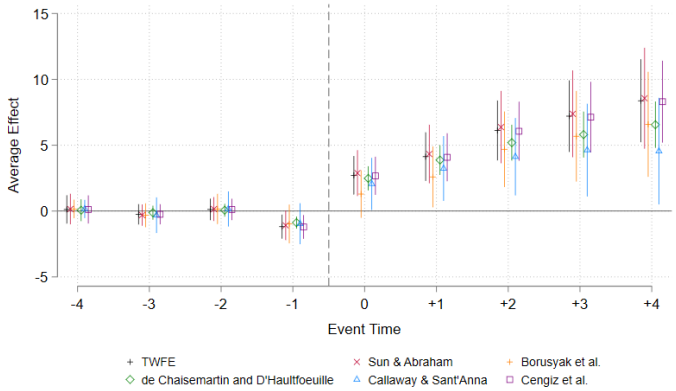
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the treated affiliates are compared with that of control affiliates; sister affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.IV: **ROBUSTNESS TO OTHER ESTIMATORS - TREATED AFFILIATES**

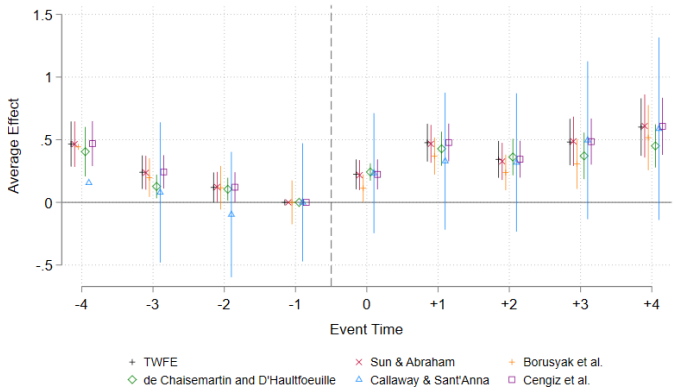
(A) **TAX BASE (RAW ESTIMATES)**



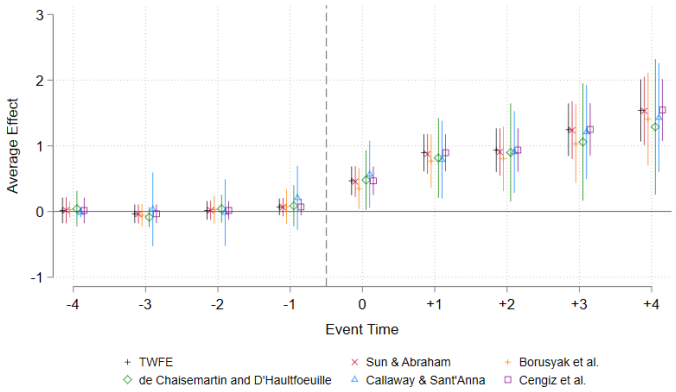
(B) **TAX BASE (DE-TRENDED)**



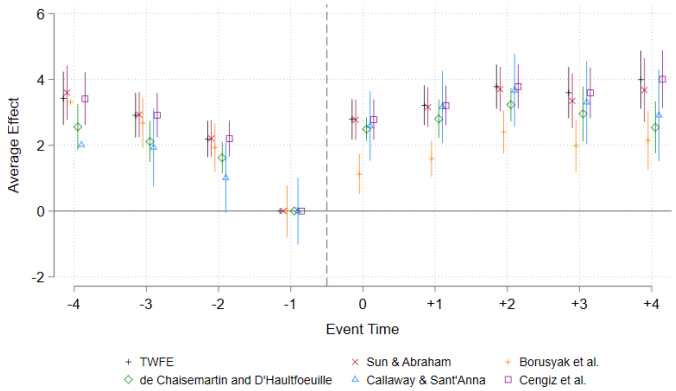
(C) **TAXATION (RAW ESTIMATES)**



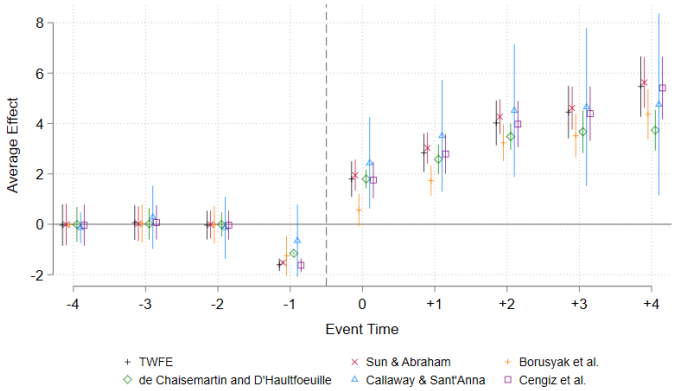
(D) **TAXATION (DE-TRENDED)**



(E) **NET PROFIT (RAW ESTIMATES)**



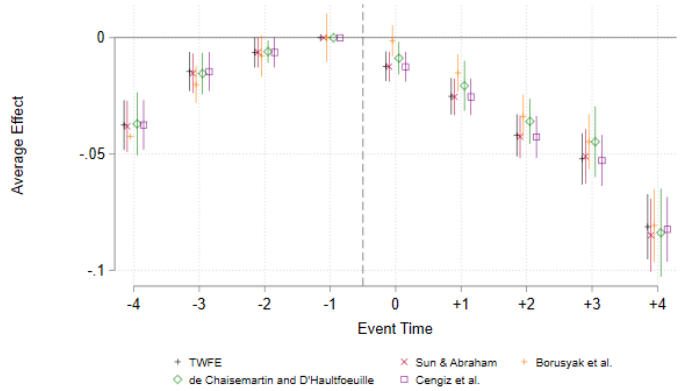
(F) **NET PROFIT (DE-TRENDED)**



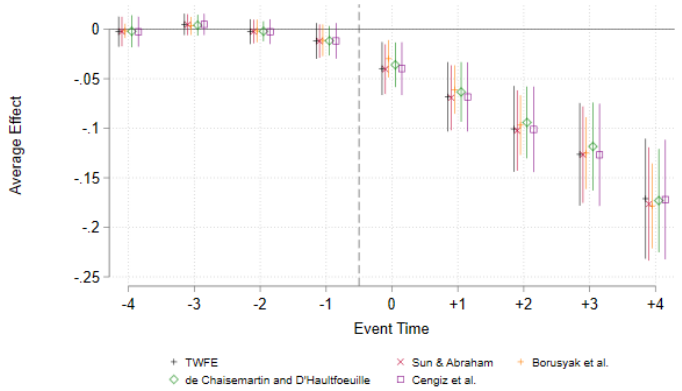
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the treated affiliates are compared with that of control affiliates; sister affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.V: **ROBUSTNESS TO OTHER ESTIMATORS - TREATED AFFILIATES**

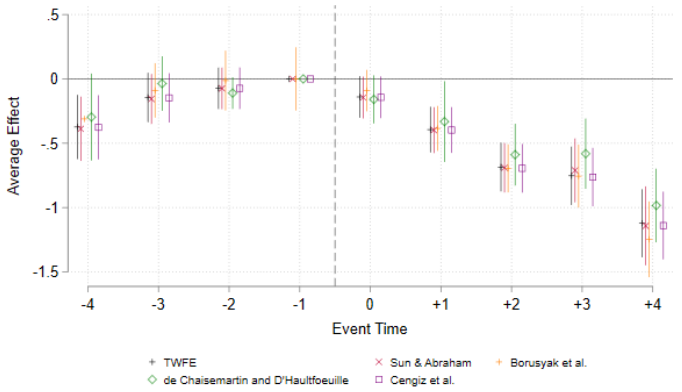
(A) **DEBT:ASSETS (RAW ESTIMATES)**



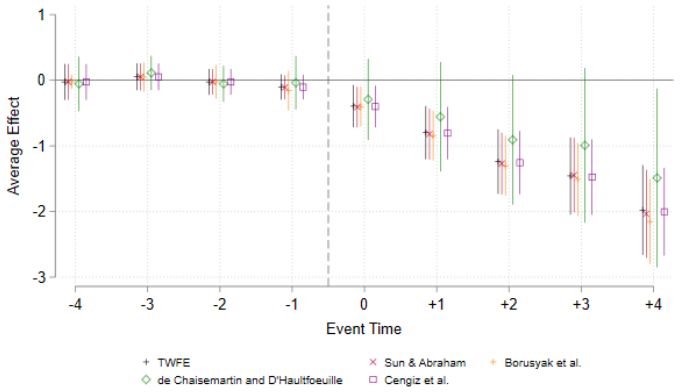
(B) **DEBT:ASSETS (DE-TRENDED)**



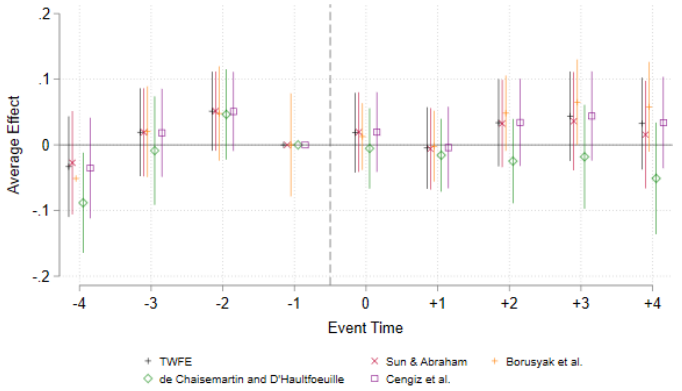
(C) **DEBT:EQUITY (RAW ESTIMATES)**



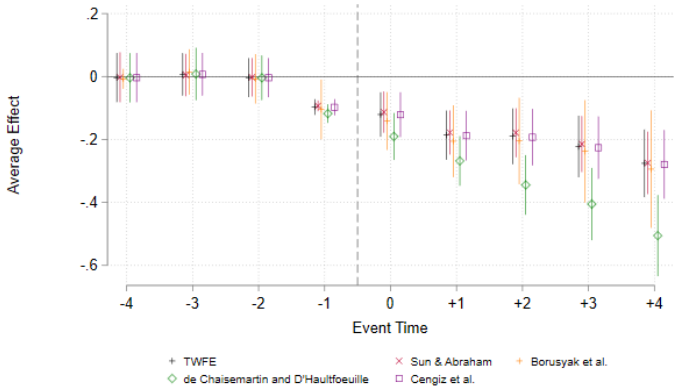
(D) **DEBT:EQUITY (DE-TRENDED)**



(E) **INTEREST:EBITDA (RAW ESTIMATES)**



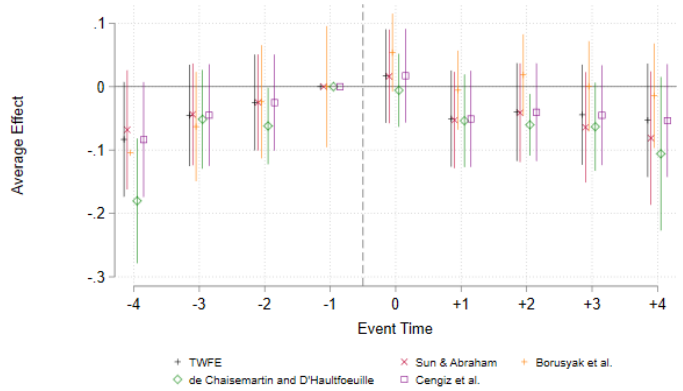
(F) **INTEREST:EBITDA (DE-TRENDED)**



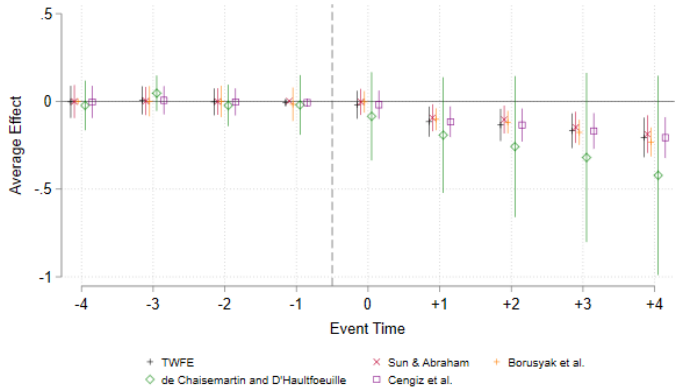
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the treated affiliates are compared with that of control affiliates; sister affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.VI: **ROBUSTNESS TO OTHER ESTIMATORS - TREATED AFFILIATES**

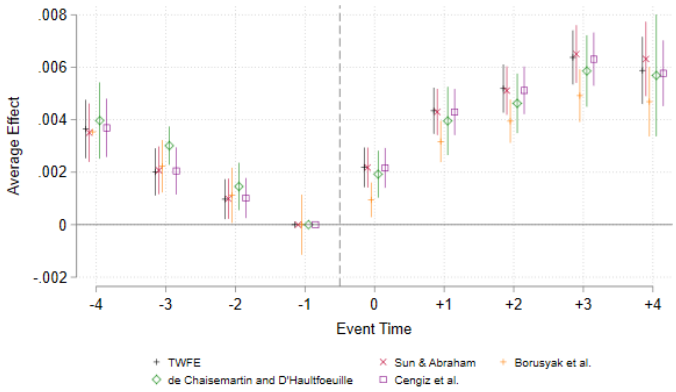
(A) **NET INTEREST:EBITDA (RAW ESTIMATES)**



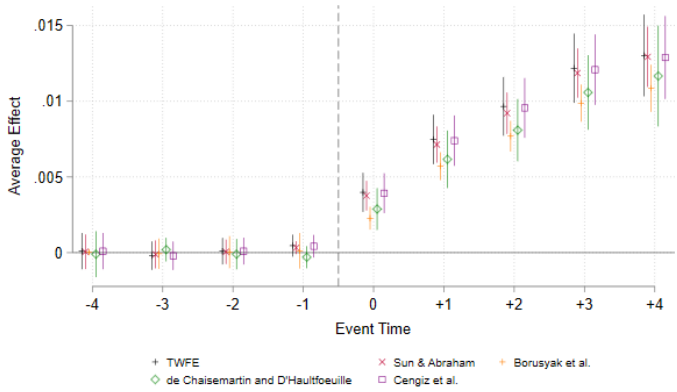
(B) **NET INTEREST:EBITDA (DE-TRENDED)**



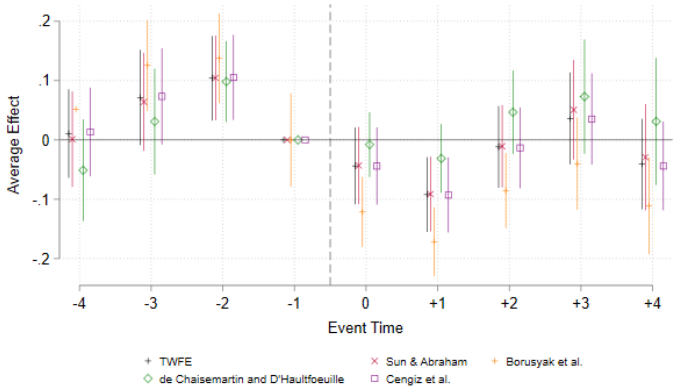
(C) **TAXATION:ASSETS (RAW ESTIMATES)**



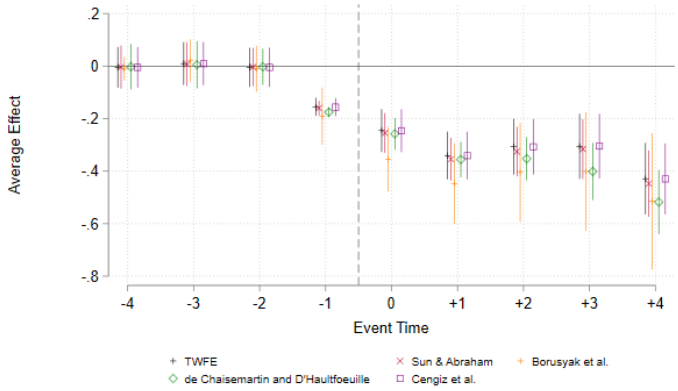
(D) **TAXATION:ASSETS (DE-TRENDED)**



(E) **INVESTMENT RATE (RAW ESTIMATES)**



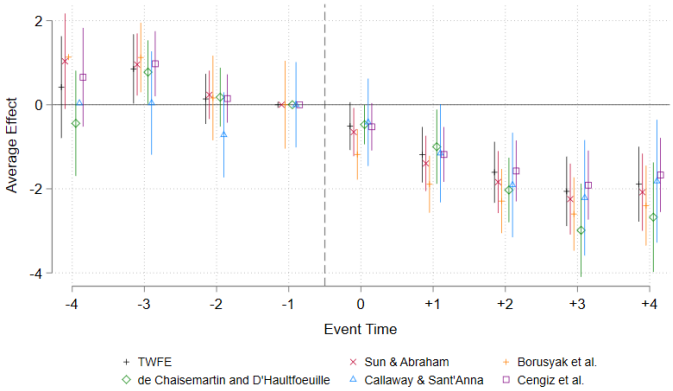
(F) **NET PROFIT (DE-TRENDED)**



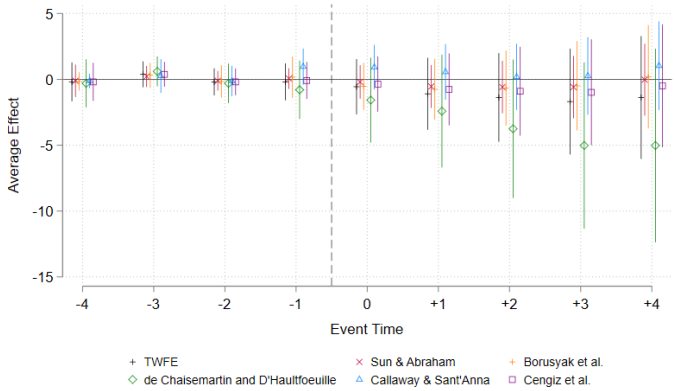
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the treated affiliates are compared with that of control affiliates; sister affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.VII: **ROBUSTNESS TO OTHER ESTIMATORS - SISTER AFFILIATES**

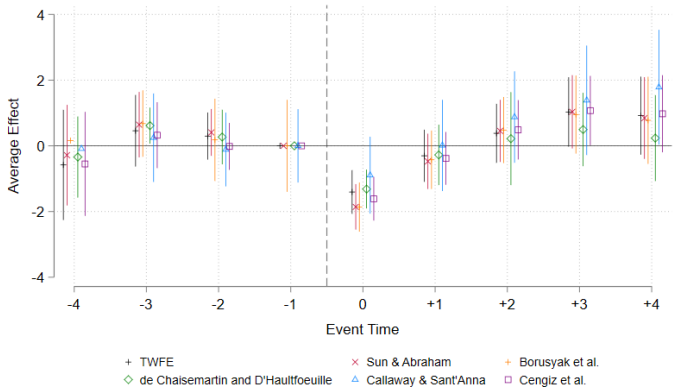
(A) **DEBT (RAW ESTIMATES)**



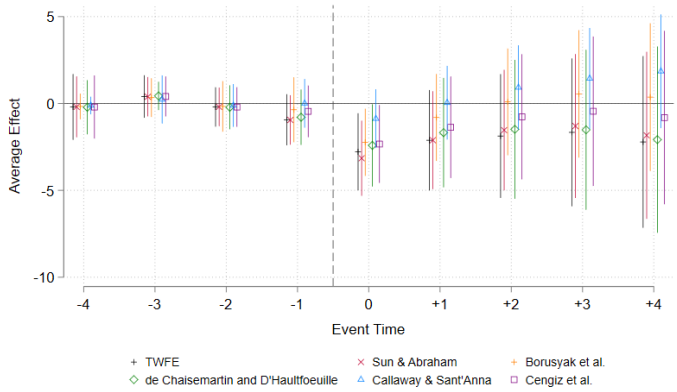
(B) **DEBT (DE-TRENDED)**



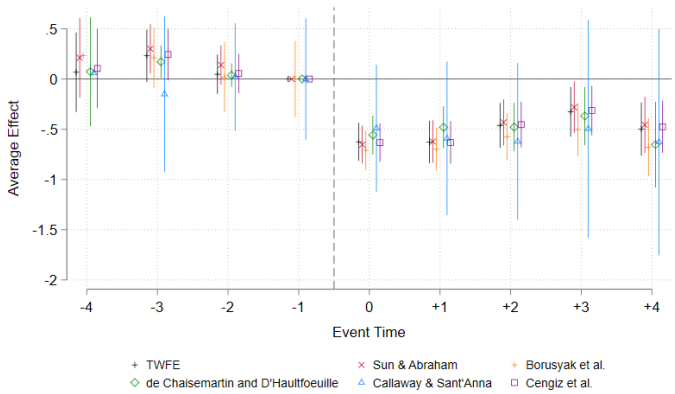
(C) **EQUITY (RAW ESTIMATES)**



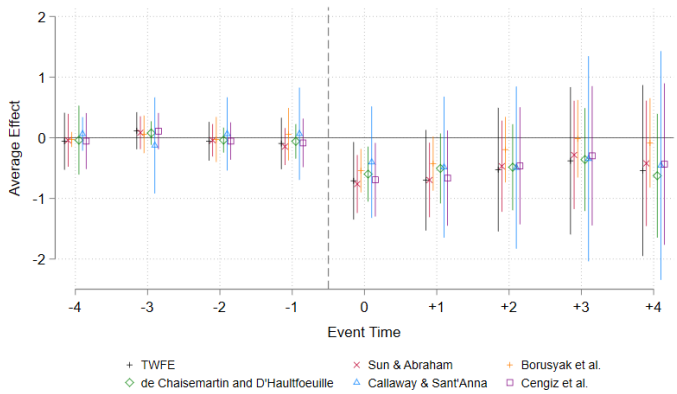
(D) **EQUITY (DE-TRENDED)**



(E) **CAPITAL (RAW ESTIMATES)**



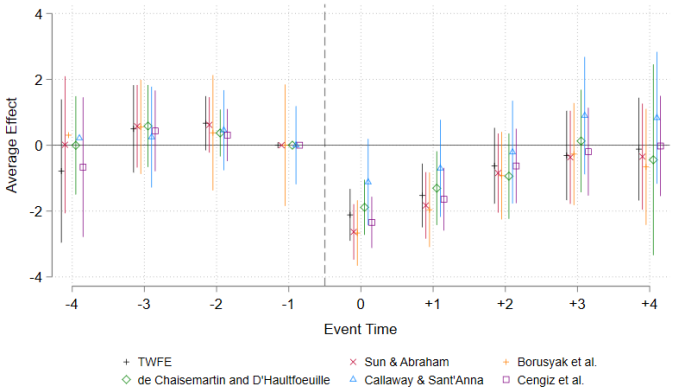
(F) **CAPITAL (DE-TRENDED)**



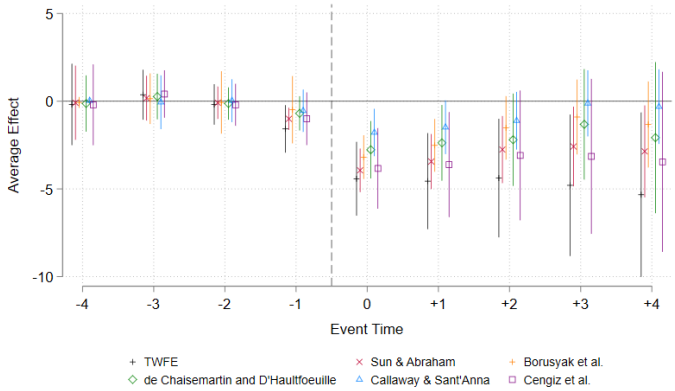
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the sister affiliates are compared with that of control affiliates; treated affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.VIII: **ROBUSTNESS TO OTHER ESTIMATORS - SISTER AFFILIATES**

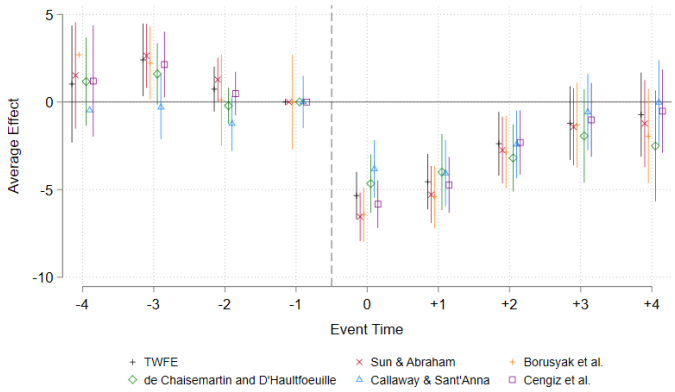
(A) **FIXED ASSETS (RAW ESTIMATES)**



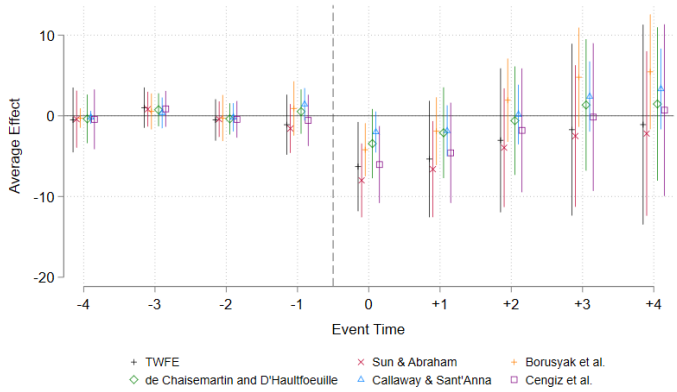
(B) **FIXED ASSETS (DE-TRENDED)**



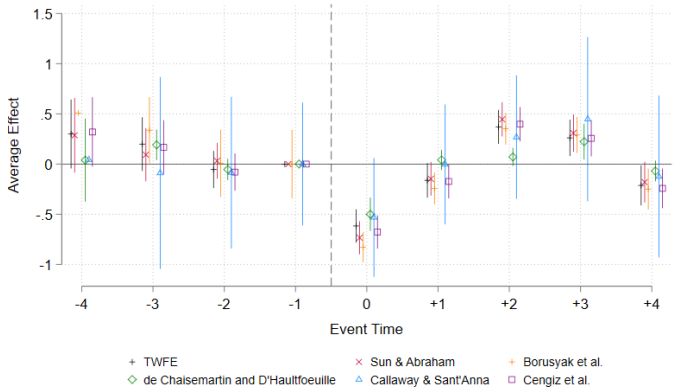
(C) **TOTAL ASSETS (RAW ESTIMATES)**



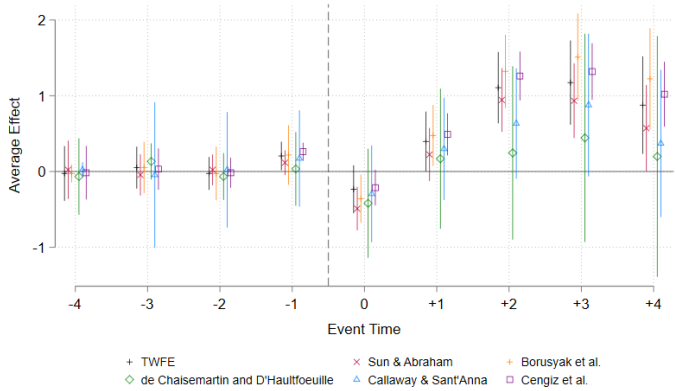
(D) **TOTAL ASSETS (DE-TRENDED)**



(E) **INVESTMENT (RAW ESTIMATES)**



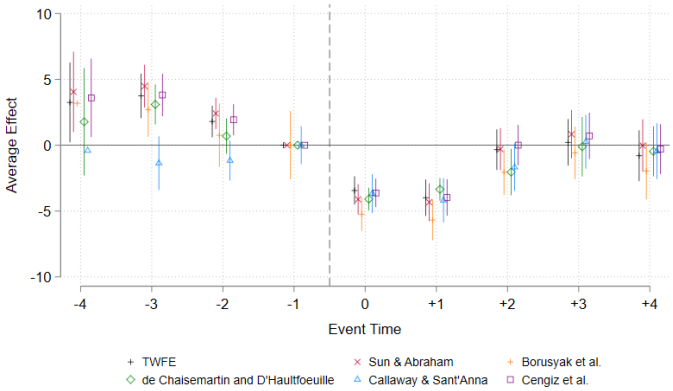
(F) **INVESTMENT (DE-TRENDED)**



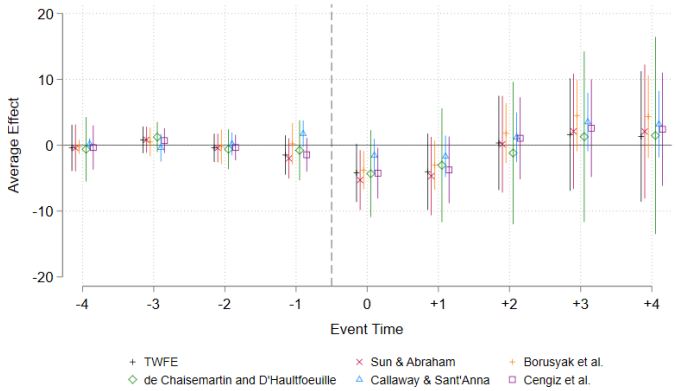
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the sister affiliates are compared with that of control affiliates; treated affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.IX: **ROBUSTNESS TO OTHER ESTIMATORS - SISTER AFFILIATES**

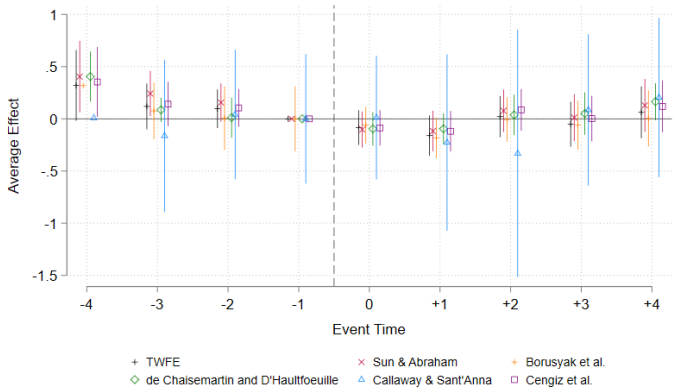
(A) **REVENUE (RAW ESTIMATES)**



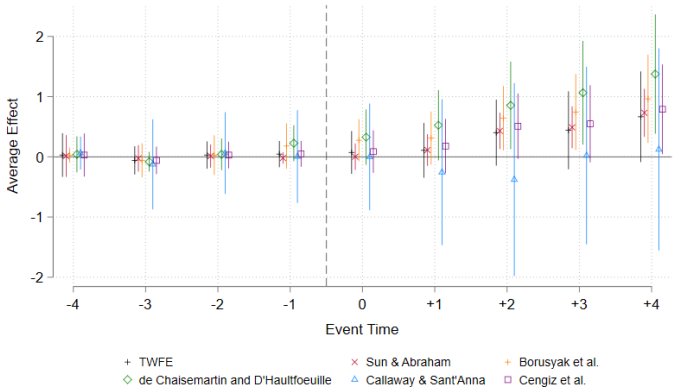
(B) **REVENUE (DE-TRENDED)**



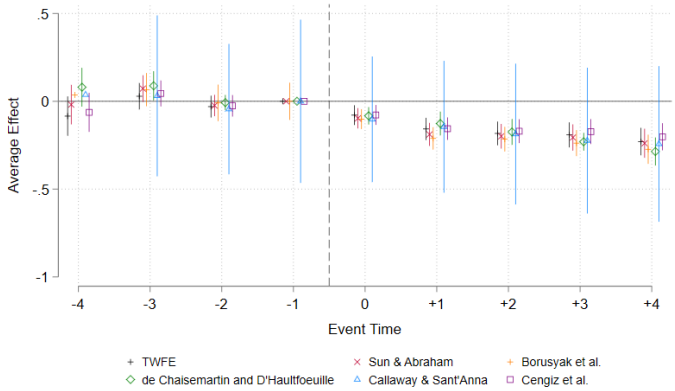
(C) **EBITDA (RAW ESTIMATES)**



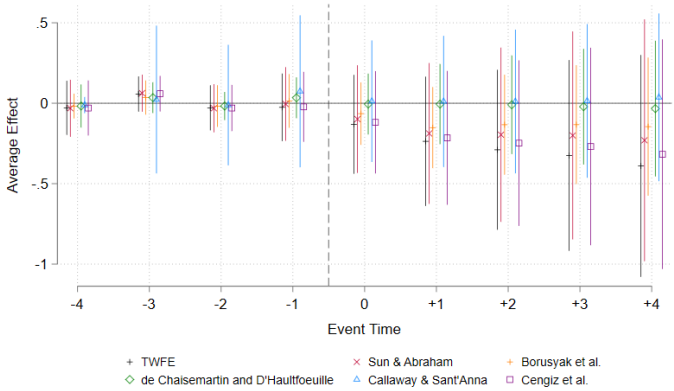
(D) **EBITDA (DE-TRENDED)**



(E) **FINANCIAL EXPENSE (RAW ESTIMATES)**



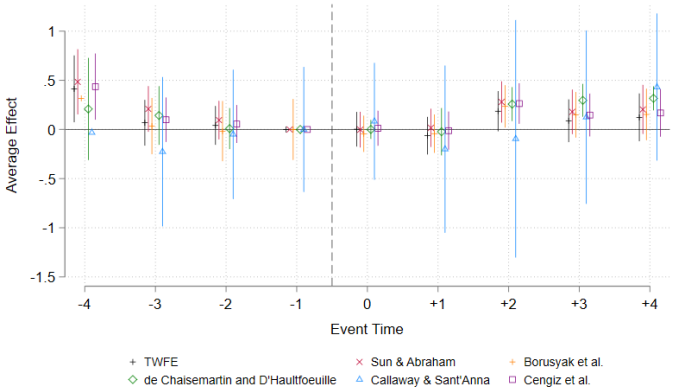
(F) **FINANCIAL EXPENSE (DE-TRENDED)**



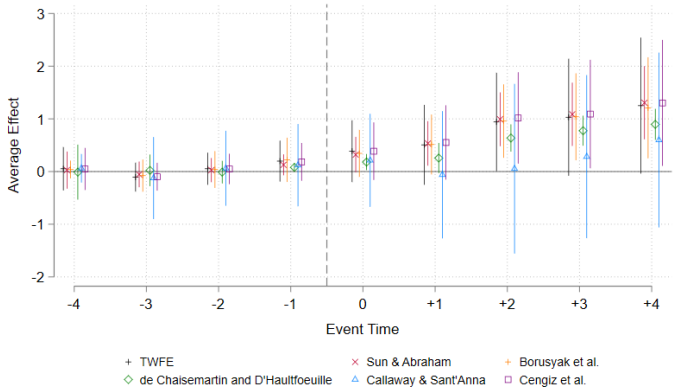
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the sister affiliates are compared with that of control affiliates; treated affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.X: **ROBUSTNESS TO OTHER ESTIMATORS - SISTER AFFILIATES**

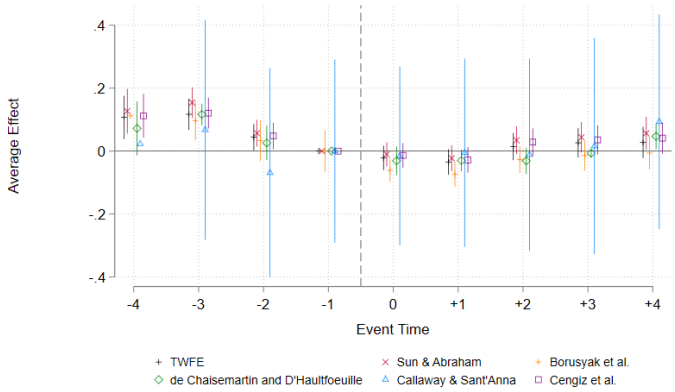
(A) **TAX BASE (RAW ESTIMATES)**



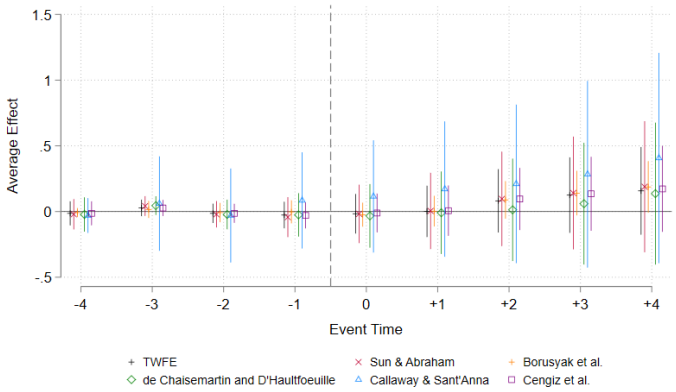
(B) **TAX BASE (DE-TRENDED)**



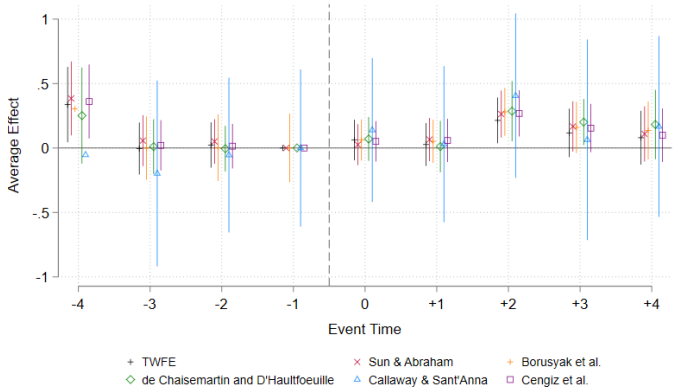
(C) **TAXATION (RAW ESTIMATES)**



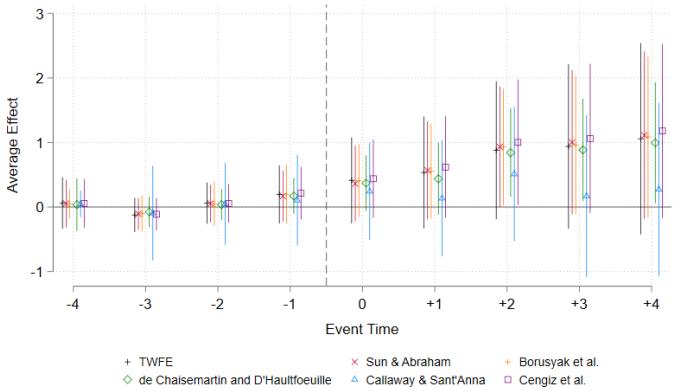
(D) **TAXATION (DE-TRENDED)**



(E) **NET PROFIT (RAW ESTIMATES)**



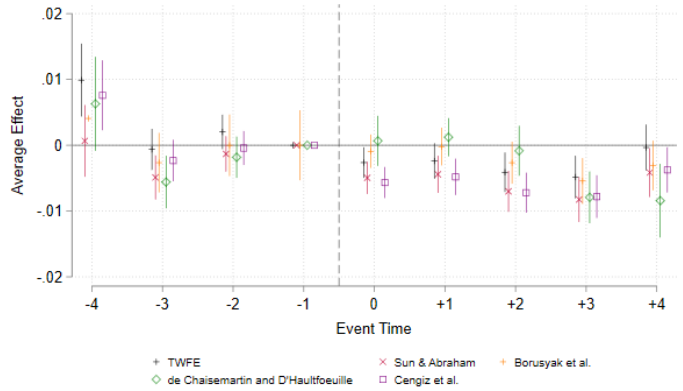
(F) **NET PROFIT (DE-TRENDED)**



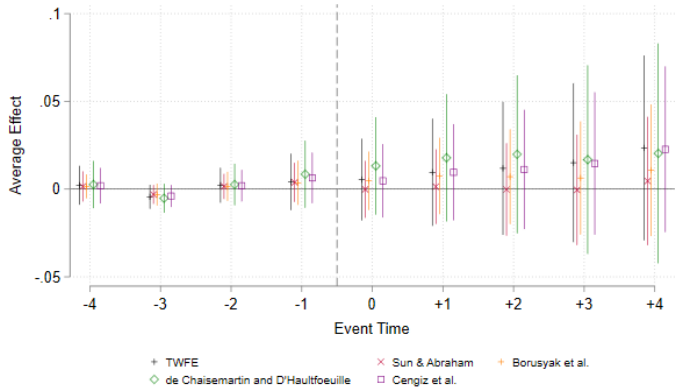
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the sister affiliates are compared with that of control affiliates; treated affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.XI: **ROBUSTNESS TO OTHER ESTIMATORS - SISTER AFFILIATES**

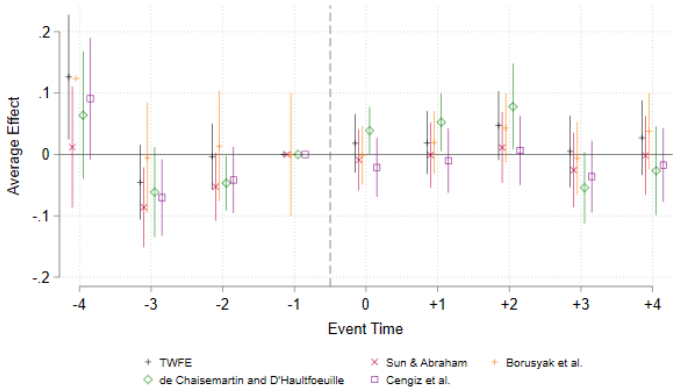
(A) **DEBT:ASSETS (RAW ESTIMATES)**



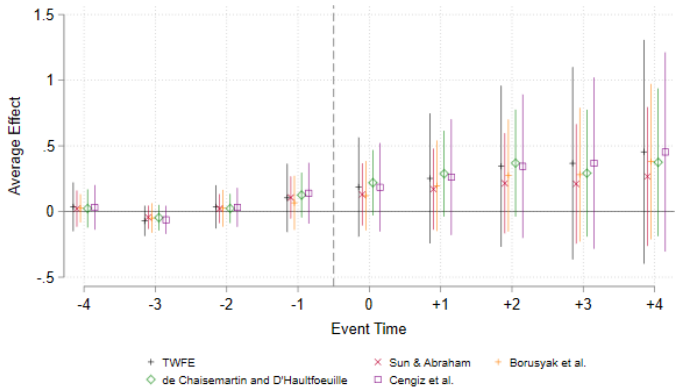
(B) **DEBT:ASSETS (DE-TRENDED)**



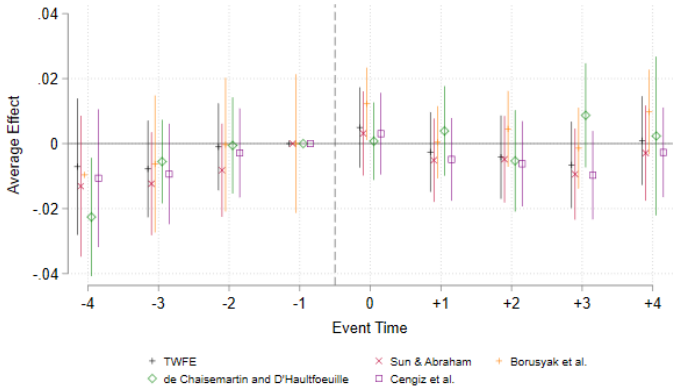
(C) **DEBT:EQUITY (RAW ESTIMATES)**



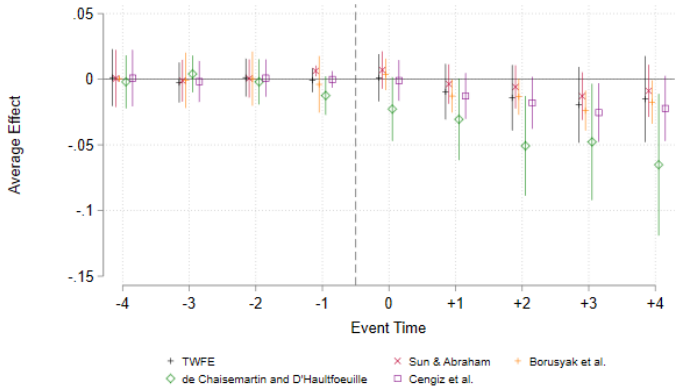
(D) **DEBT:EQUITY (DE-TRENDED)**



(E) **INTEREST:EBITDA (RAW ESTIMATES)**



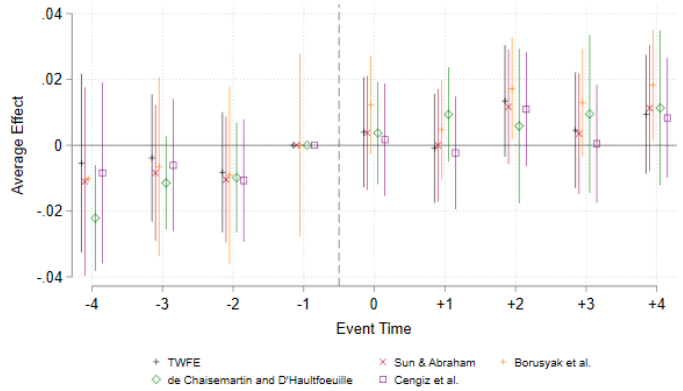
(F) **INTEREST:EBITDA (DE-TRENDED)**



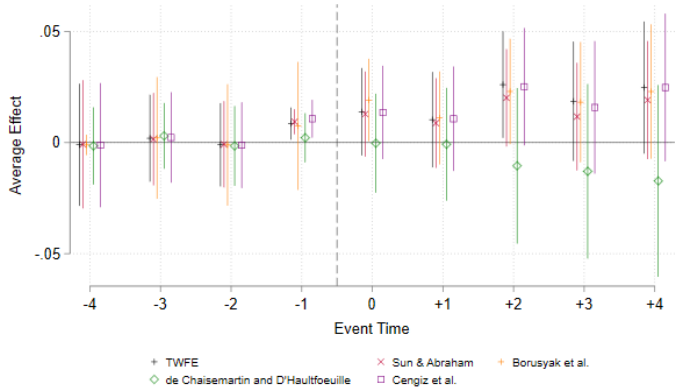
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the sister affiliates are compared with that of control affiliates; treated affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.XII: **ROBUSTNESS TO OTHER ESTIMATORS - SISTER AFFILIATES**

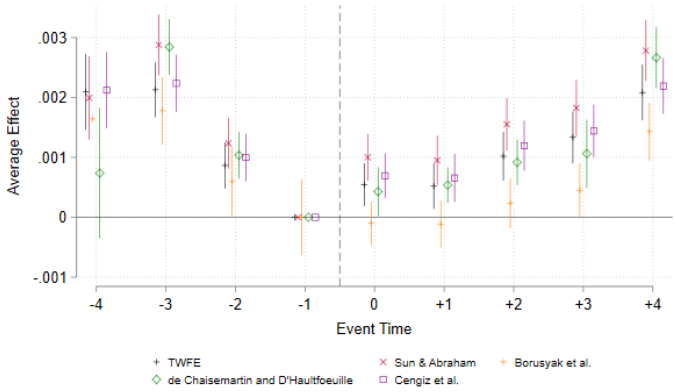
(A) **NET INTEREST:EBITDA (RAW ESTIMATES)**



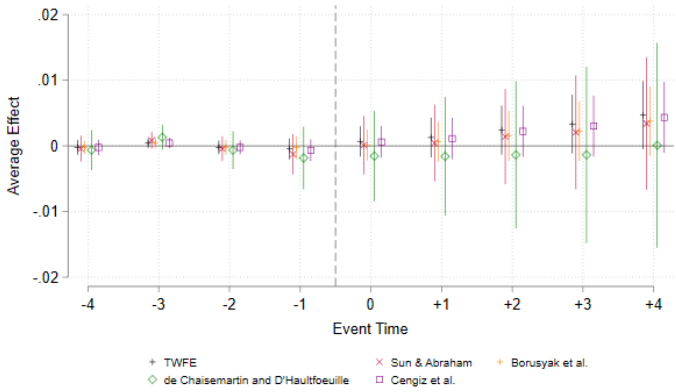
(B) **NET INTEREST:EBITDA (DE-TRENDED)**



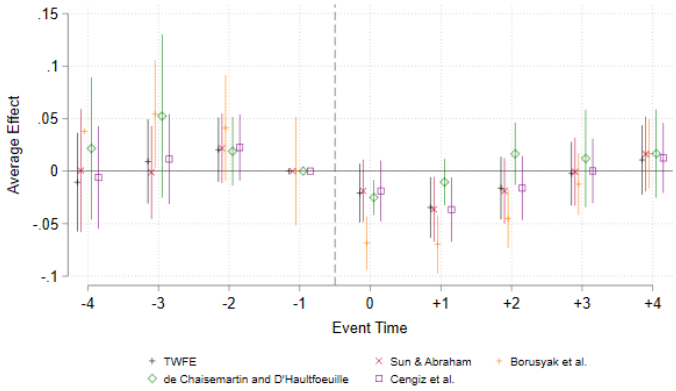
(C) **TAXATION:ASSETS (RAW ESTIMATES)**



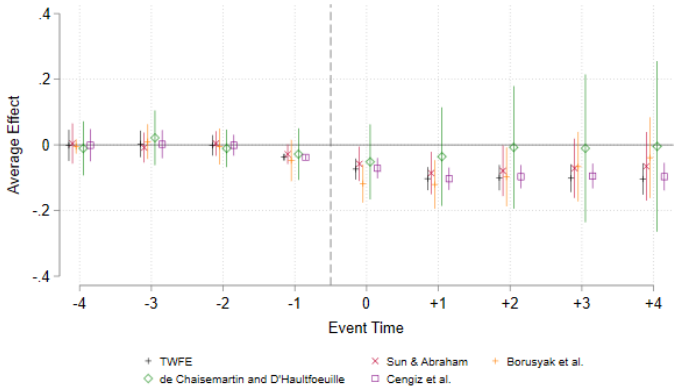
(D) **TAXATION:ASSETS (DE-TRENDED)**



(E) **INVESTMENT RATE (RAW ESTIMATES)**



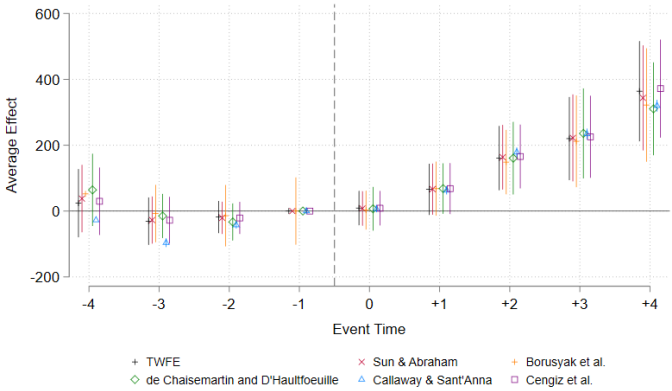
(F) **NET PROFIT (DE-TRENDED)**



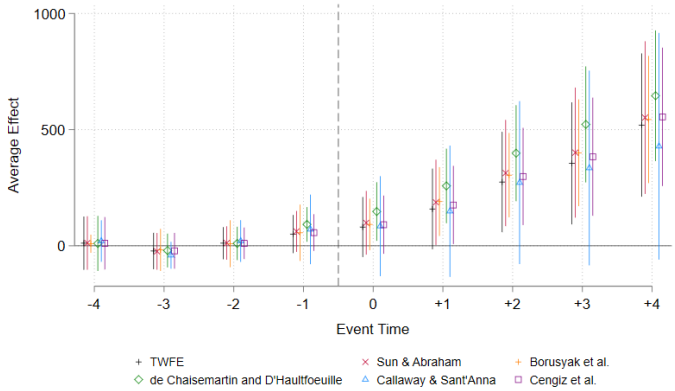
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the affiliate level. The outcomes of the sister affiliates are compared with that of control affiliates; treated affiliates are dropped from the analysis. The reference year is $t^* - 1$ and I include firm and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for firm-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust and clustered at the affiliate level. The event time at the x-axis is measured in years and the green dashed line depicts the year of policy introduction.

FIGURE H.XIII: **ROBUSTNESS TO OTHER ESTIMATORS - GROUP LEVEL**

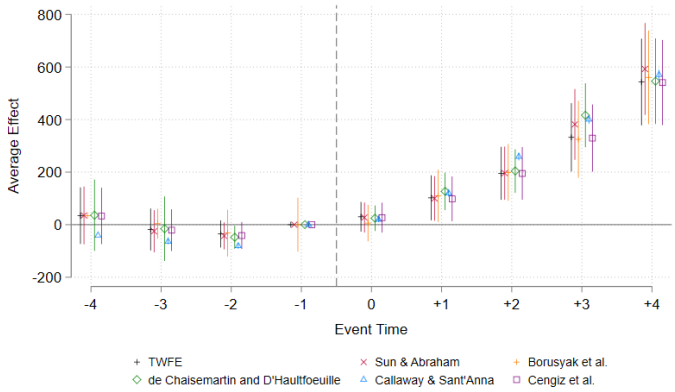
(A) **DEBT (RAW ESTIMATES)**



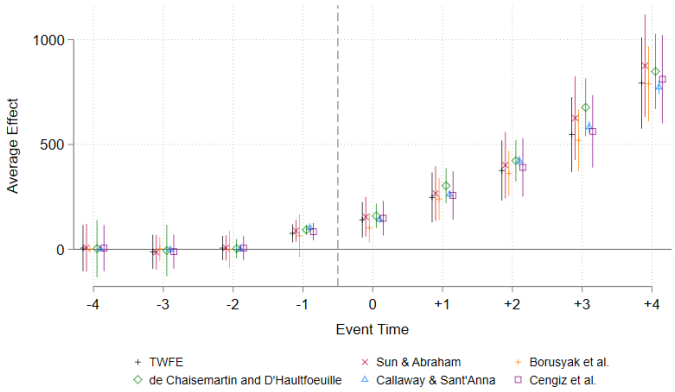
(B) **DEBT (DE-TRENDED)**



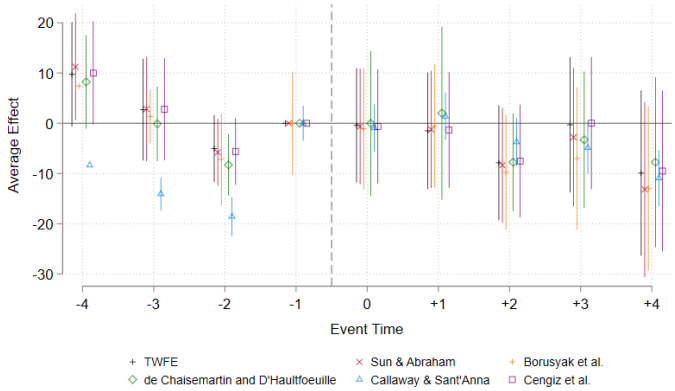
(C) **EQUITY (RAW ESTIMATES)**



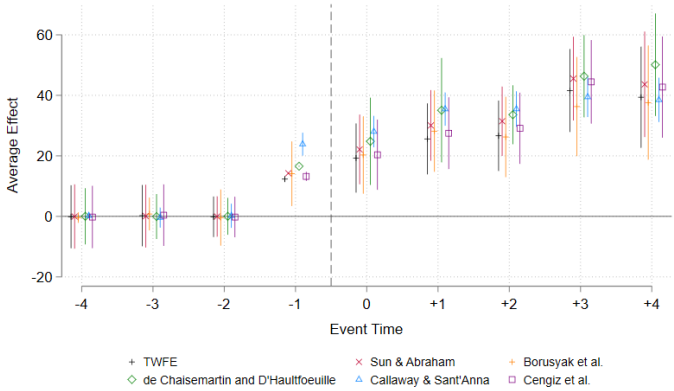
(D) **EQUITY (DE-TRENDED)**



(E) **CAPITAL (RAW ESTIMATES)**



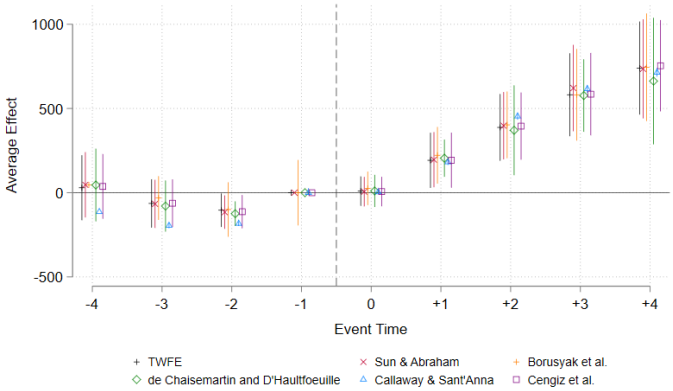
(F) **CAPITAL (DE-TRENDED)**



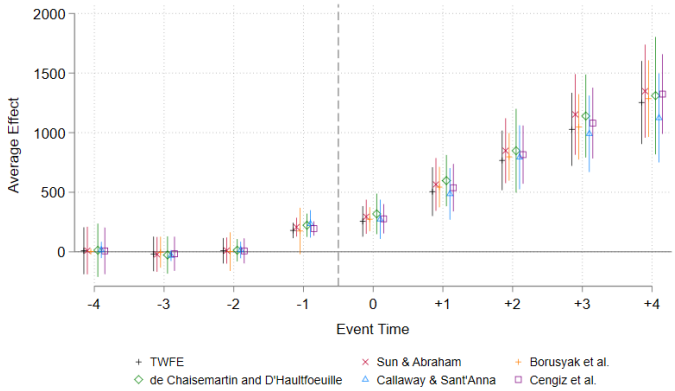
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the group level. The outcomes of the treated groups are compared with that of control groups. The reference year is $t^* - 1$ and I include group and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for group-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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 H.XIV: **ROBUSTNESS TO OTHER ESTIMATORS - GROUP LEVEL**

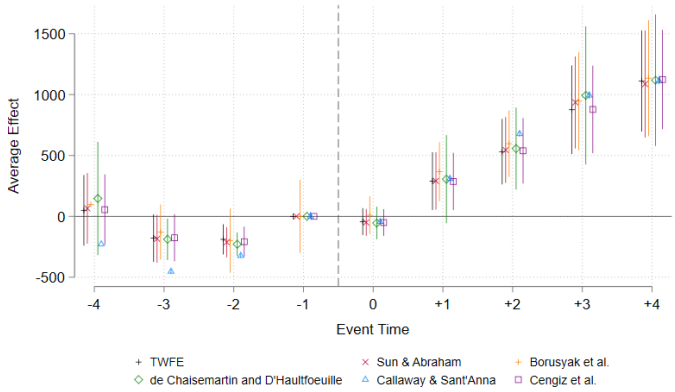
(A) **FIXED ASSETS (RAW ESTIMATES)**



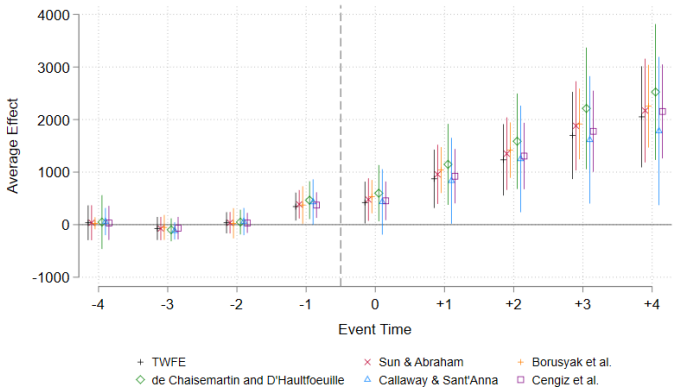
(B) **FIXED ASSETS (DE-TRENDED)**



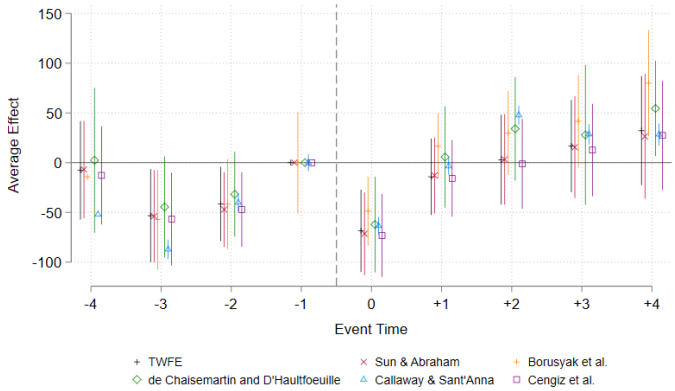
(C) **TOTAL ASSETS (RAW ESTIMATES)**



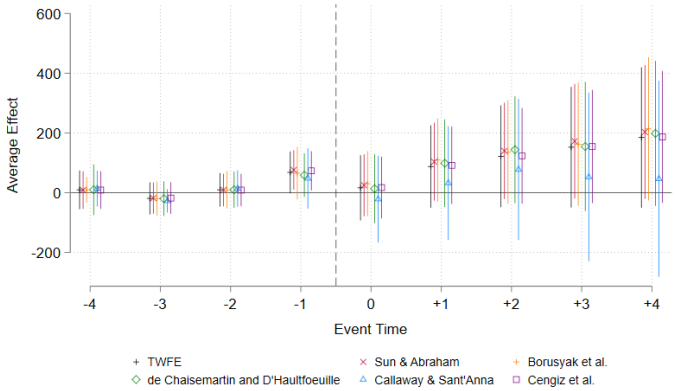
(D) **TOTAL ASSETS (DE-TRENDED)**



(E) **INVESTMENT (RAW ESTIMATES)**



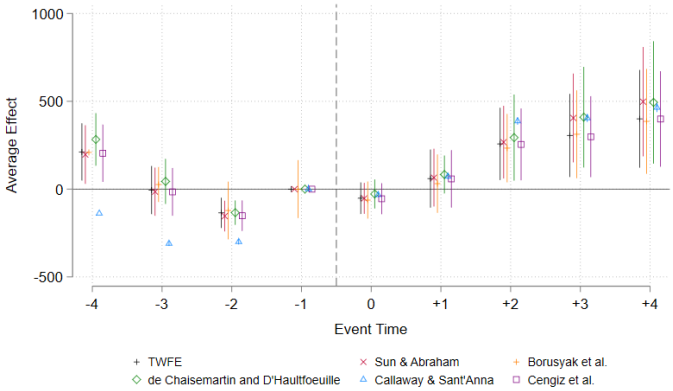
(F) **INVESTMENT (DE-TRENDED)**



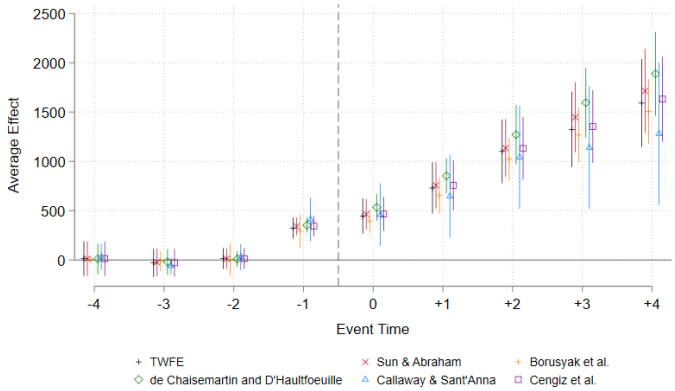
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the group level. The outcomes of the treated groups are compared with that of control groups. The reference year is $t^* - 1$ and I include group and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for group-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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 H.XV: **ROBUSTNESS TO OTHER ESTIMATORS - GROUP LEVEL**

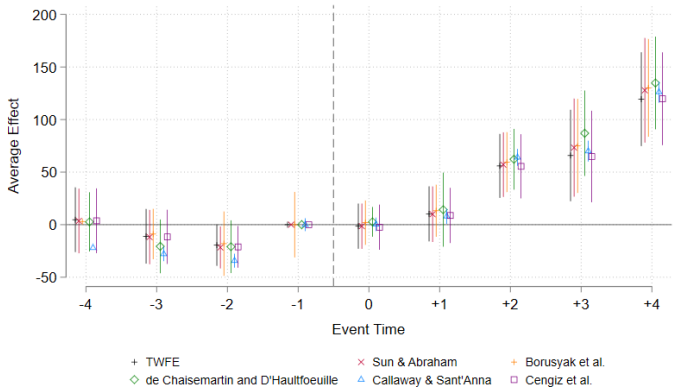
(A) **REVENUE (RAW ESTIMATES)**



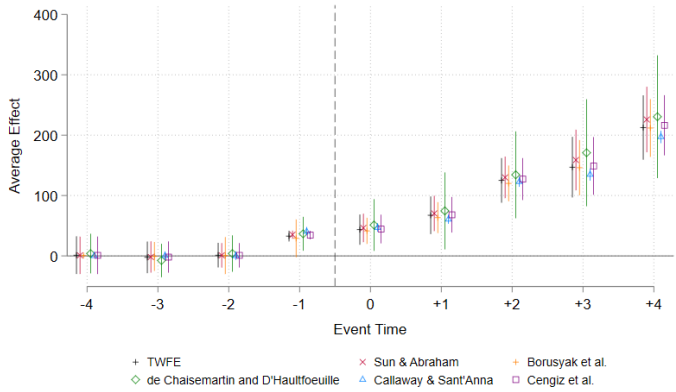
(B) **REVENUE (DE-TRENDED)**



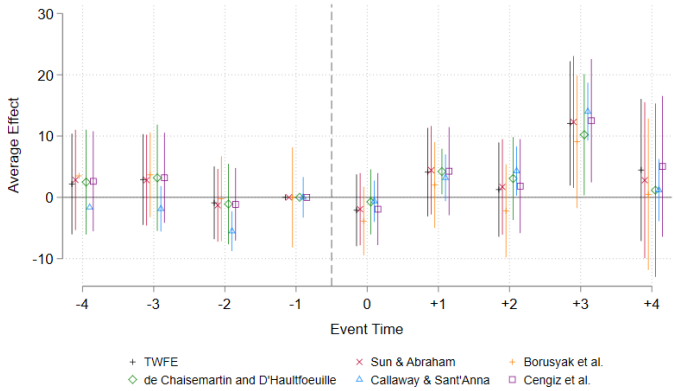
(C) **EBITDA (RAW ESTIMATES)**



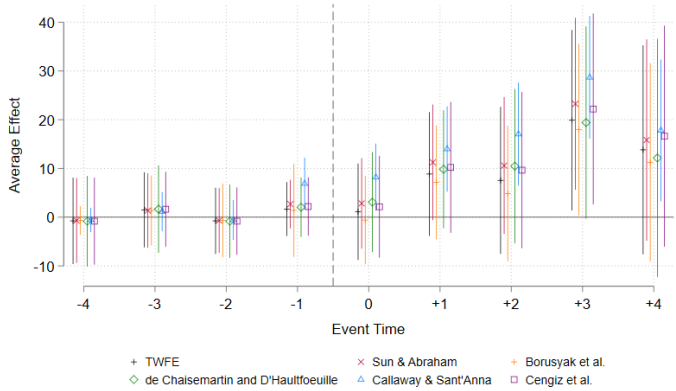
(D) **EBITDA (DE-TRENDED)**



(E) **FINANCIAL EXPENSE (RAW ESTIMATES)**



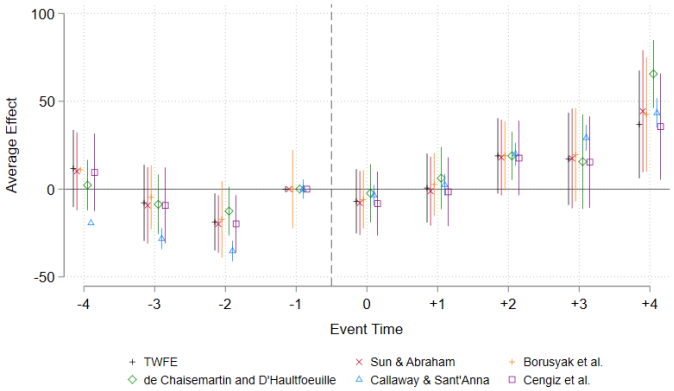
(F) **FINANCIAL EXPENSE (DE-TRENDED)**



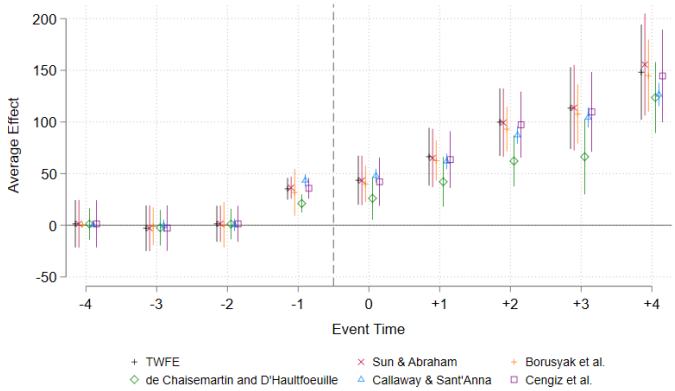
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the group level. The outcomes of the treated groups are compared with that of control groups. The reference year is $t^* - 1$ and I include group and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for group-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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 H.XVI: **ROBUSTNESS TO OTHER ESTIMATORS - GROUP LEVEL**

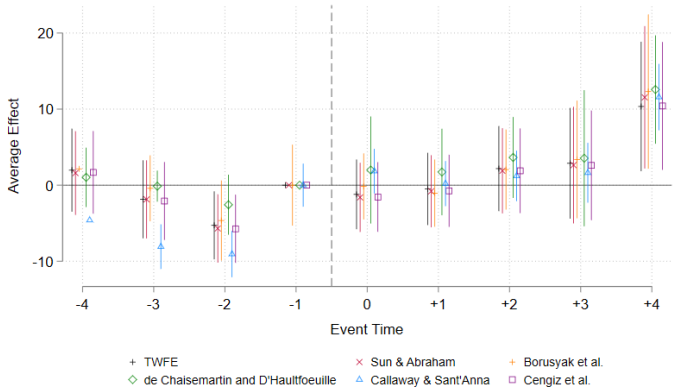
(A) **TAX BASE (RAW ESTIMATES)**



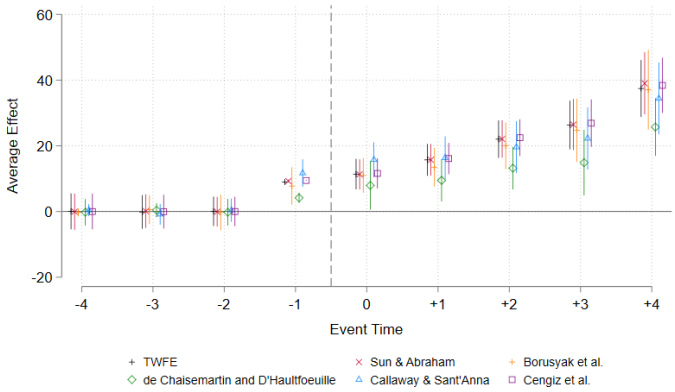
(B) **TAX BASE (DE-TRENDED)**



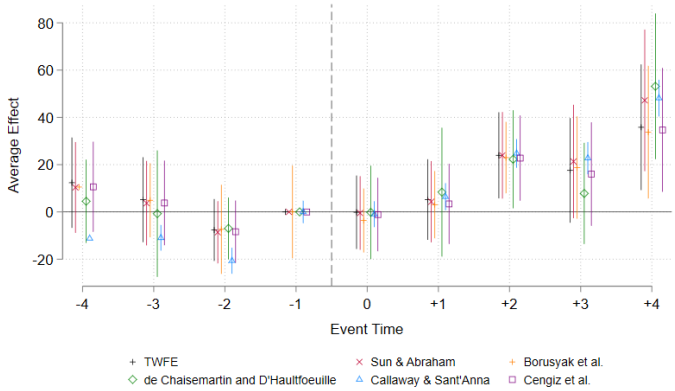
(C) **TAXATION (RAW ESTIMATES)**



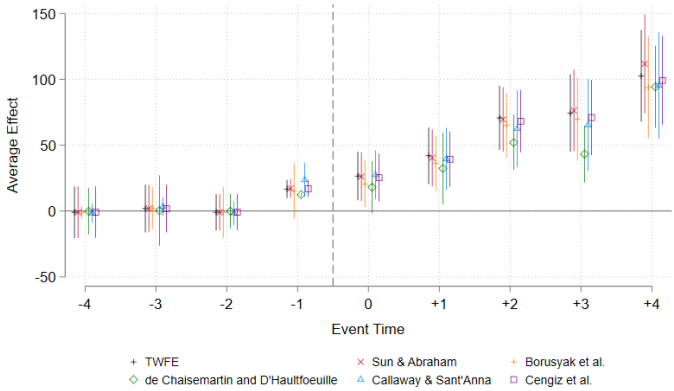
(D) **TAXATION (DE-TRENDED)**



(E) **NET PROFIT (RAW ESTIMATES)**



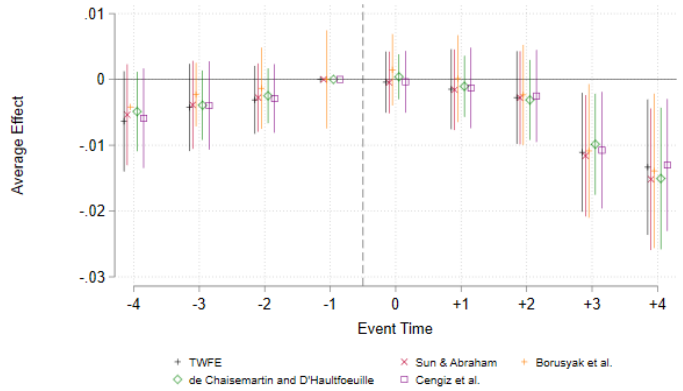
(F) **NET PROFIT (DE-TRENDED)**



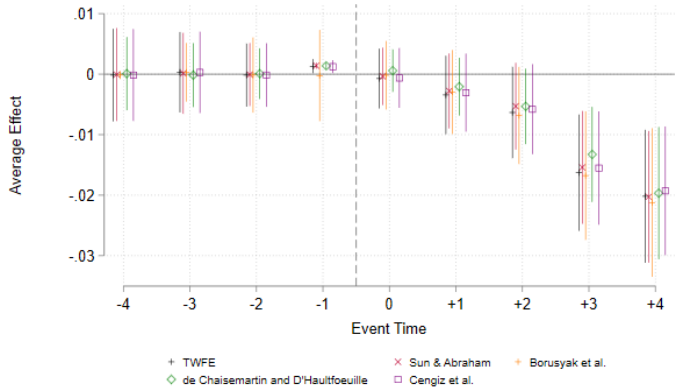
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the group level. The outcomes of the treated groups are compared with that of control groups. The reference year is $t^* - 1$ and I include group and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for group-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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 H.XVII: **ROBUSTNESS TO OTHER ESTIMATORS - GROUP LEVEL**

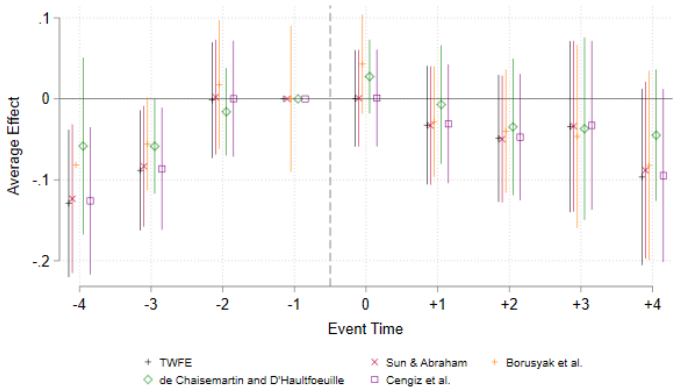
(A) **DEBT:ASSETS (RAW ESTIMATES)**



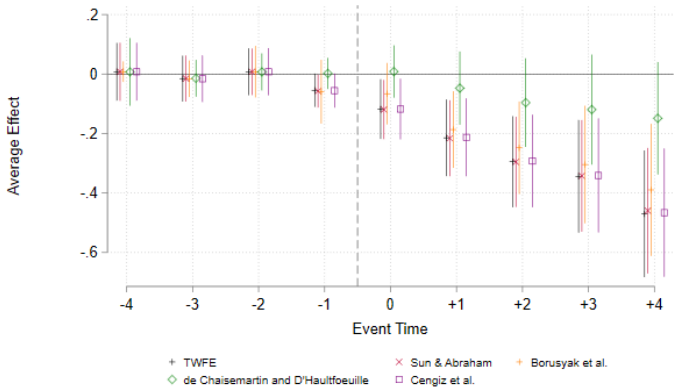
(B) **DEBT:ASSETS (DE-TRENDED)**



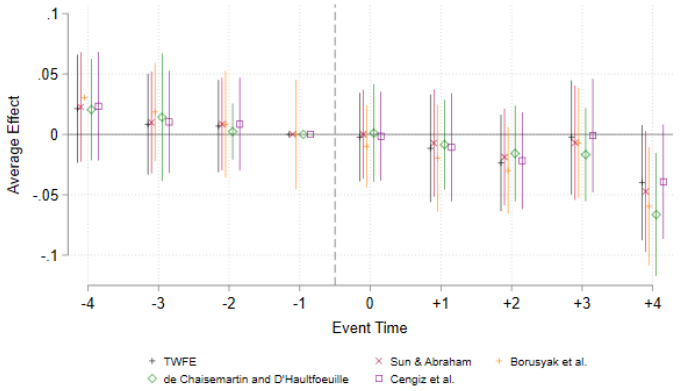
(C) **DEBT:EQUITY (RAW ESTIMATES)**



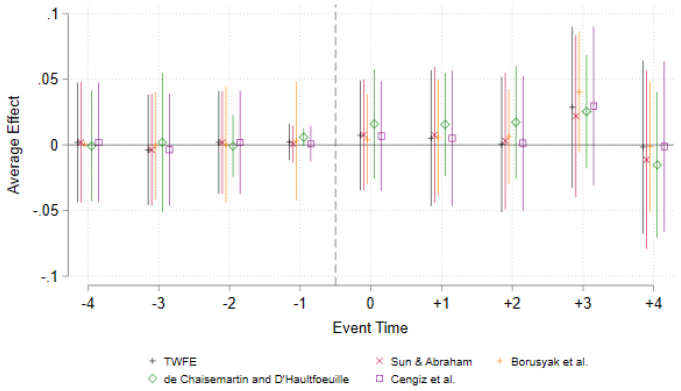
(D) **DEBT:EQUITY (DE-TRENDED)**



(E) **INTEREST:EBITDA (RAW ESTIMATES)**



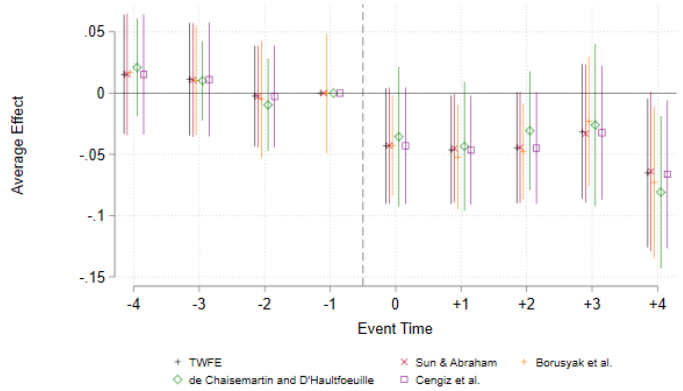
(F) **INTEREST:EBITDA (DE-TRENDED)**



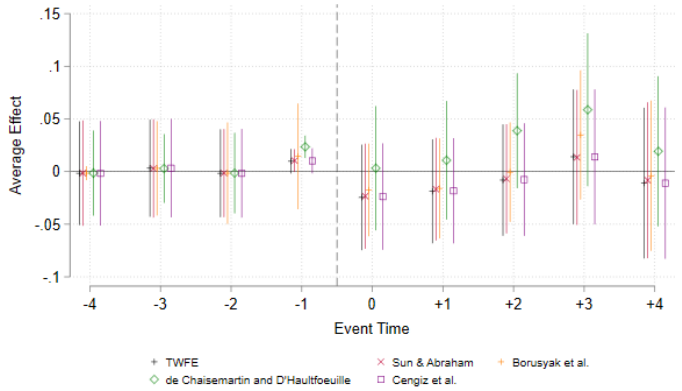
Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the group level. The outcomes of the treated groups are compared with that of control groups. The reference year is $t^* - 1$ and I include group and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for group-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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 H.XVIII: **ROBUSTNESS TO OTHER ESTIMATORS - GROUP LEVEL**

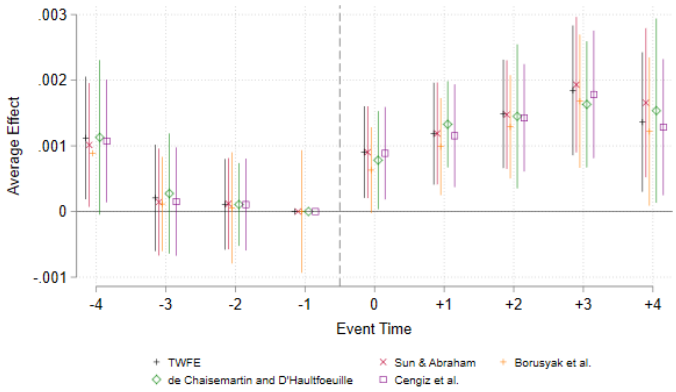
(A) **NET INTEREST:EBITDA (RAW ESTIMATES)**



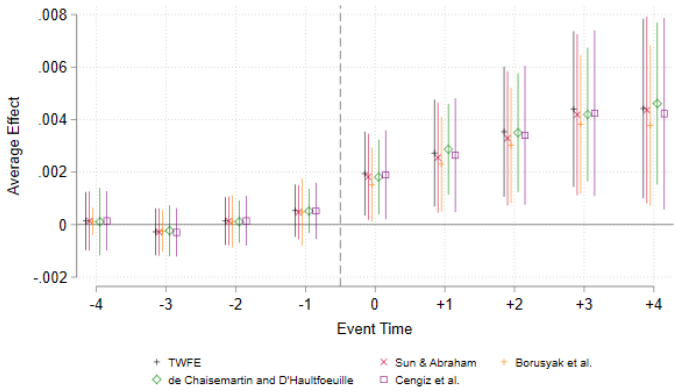
(B) **NET INTEREST:EBITDA (DE-TRENDED)**



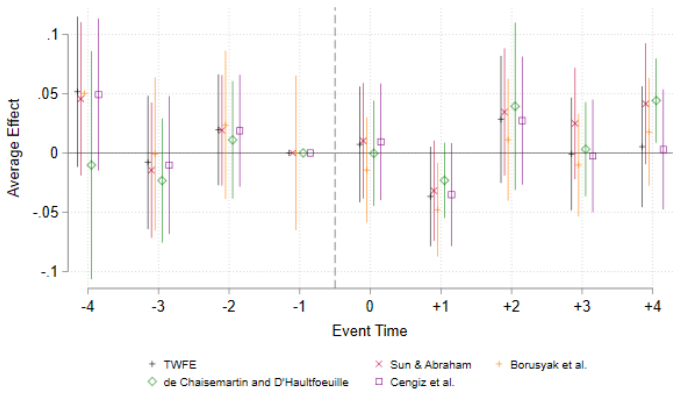
(C) **TAXATION:ASSETS (RAW ESTIMATES)**



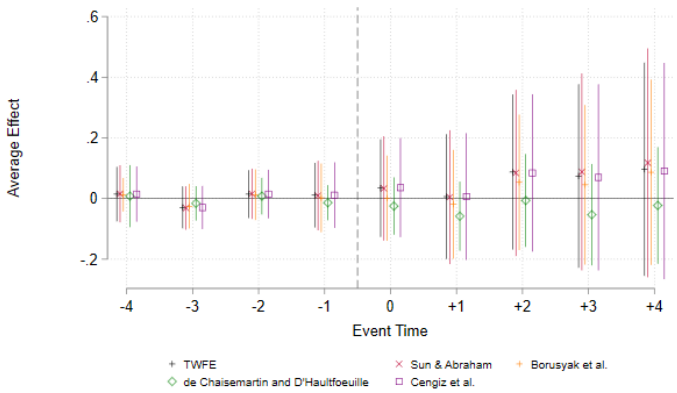
(D) **TAXATION:ASSETS (DE-TRENDED)**



(E) **INVESTMENT RATE (RAW ESTIMATES)**



(F) **NET PROFIT (DE-TRENDED)**



Notes: This graph plots the dynamic average treatment effects of ESR in USD millions at the group level. The outcomes of the treated groups are compared with that of control groups. The reference year is $t^* - 1$ and I include group and year fixed effects, and time-varying jurisdiction-level controls. In all the right-side panels of the figure, I also control for group-specific linear trends. The data is trimmed at 1% and 99% for all the graphs. The diamond symbols represent the point estimates while the bars depict confidence intervals at 95%. All the standard errors are robust 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.