# Tax Reforms and Corporate Behavior in Sweden: An Empirical Corporate Finance Analysis

Nikoleta Boziou

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**Skattereformer och företagsbeteende i Sverige: En empirisk analys inom företagsfinansiering**

Nikoleta Boziou



| Godkänt | Examinator | Handledare |
|---|---|---|
| 2026-Sep-28 | Jannis Angelis | Christian Thomann |



# *Sammanfattning*

Företagsskatter påverkar hur företag lånar och investerar. En anledning till detta är att räntekostnader på skulder är avdragsgilla, vilket gör skuldfinansiering billigare än eget kapital. Detta skapar det som forskare kallar en skuldbias. Sverige genomförde tre reformer 2009, 2013 och 2019 som gradvis begränsade rätten till ränteavdrag, samtidigt som bolagsskattesatsen sänktes. Dessa två förändringar skedde parallellt, vilket gör den svenska kontexten användbar för att studera hur företag reagerar på olika typer av skattepolitik. De flesta befintliga studier fokuserar på Tyskland eller USA, och få undersöker hur företag anpassar både sin upplåning och sina investeringar samtidigt. Denna avhandling försöker fylla det gapet. Den använder företagsspecifika redovisningsdata från Serrano-databasen för perioden 2005 till 2023. Metoden är en difference-in-differences-design som jämför högt belånade företag med hög exponering mot varje reform med liknande högt belånade företag med lägre exponering. Resultaten gäller därför den exponerade och högt belånade behandlingsgruppen, inte hela populationen svenska företag. Det tydligaste finansieringsresultatet framkommer efter 2013 års reform. Exponerade företag minskade sin internkoncernskuld med cirka 7,5 procentenheter men ökade sin bankskuld med cirka 4,8 procentenheter. Detta tyder på att företagen skiftade mellan skuldtyper snarare än att de lånade mindre totalt sett. 2019 års reform, som införde ett brett tak baserat på rörelseresultatet, är mer försiktigt kopplad till lägre investeringar. Exponerade företag minskade materiella investeringar relativt mindre exponerade företag, men denna tolkning bör läsas med hänsyn till problematiska pre-trender och att Covid-19-perioden överlappar den tidiga efterreformperioden. Resultaten visar att regelns utformning spelar roll. Snäva regler förändrar skuldens sammansättning. Bredare regler kan även påverka verkliga affärsbeslut som investeringar.

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## *Abstract*

Corporate taxes shape how firms borrow and invest. One reason for this is that interest payments on debt are tax-deductible, which makes debt financing cheaper than equity. This creates what researchers call a debt bias. Sweden introduced three reforms in 2009, 2013, and 2019 that gradually restricted this interest deduction benefit while also cutting the corporate tax rate. These two changes happened at the same time, which makes the Swedish setting useful for studying how firms react to different types of tax policy. Most existing studies focus on Germany or the United States, and few look at how firms adjust both their borrowing and their investment together. This thesis tries to fill that gap. It uses firm-level accounting data from the Serrano database, covering the period 2005 to 2023. The method is a difference-in-differences design, which compares highly leveraged firms that were more exposed to each reform against similar highly leveraged firms that were less exposed. The estimated effects therefore apply to the exposed high-leverage treatment population, not to the universe of Swedish firms. The clearest financing result appears after the 2013 reform. Exposed firms reduced their intra-group debt by around 7.5 percentage points but increased their bank debt by around 4.8 percentage points. This suggests that firms shifted between types of debt rather than simply borrowing less overall. The 2019 reform, which introduced a broad cap based on earnings, is more cautiously associated with lower tangible investment. This result is interpreted with particular care because the early post-reform period overlaps with Covid-19 and because the pre-trend diagnostics indicate longer-term divergence. Overall, the findings show that rule design matters. Narrow rules change the structure of debt. Broader rules may also affect real business decisions such as investment.

# Foreword

*I would like to express my deepest gratitude to my examiner, Christian Thomann, for his invaluable guidance, constructive feedback, and continuous support throughout the development of this master's thesis at KTH Royal Institute of Technology. His expertise and insights were instrumental in shaping the direction of this research.*

*I also want to express my heartfelt gratitude to Sweden. Moving here from Greece was a big, life-changing step. This beautiful country welcomed me with open arms. It gave me a chance to grow both as a student and as a person. I will always deeply cherish the opportunities I was given here.*

*Finally, my biggest and warmest thank you goes to my family and friends. They believed in me. Their endless patience and warm words kept me going. Their love gave me strength during the longest, most demanding days of this academic journey.*

*Nikoleta Boziou*

*Stockholm, June 2026*

# Nomenclature

*Here are the Notations and Abbreviations that are used in this Master's thesis.*

## *Notations*

| Symbol | Description |
|---|---|
| $\gamma t$ | Year fixed effect absorbing common time shocks |
| $\varepsilon i,t$ | Idiosyncratic error term |
| EXPOSEDir | Time-invariant pre-reform indicator capturing exposure to the reform-specific interest limitation |
| IBD_RATIO | Interest-bearing debt to assets (non-current and current liabilities to credit institutions / total assets) |
| INT_DEBT_RATIO | Intra-group debt to assets (non-current and current liabilities to group and associated companies / total assets) |
| INVEST | Tangible investment proxy (change in tangible fixed assets plus depreciation, scaled by lagged total assets) |
| LEV | Total leverage (total non-current and current liabilities to total assets) |
| LOG_ASSETS | Firm size (natural log of total assets) |
| POSTtr | Indicator equal to 1 for years at and after reform year r, 0 otherwise |
| ROA | Profitability (operating profit/loss divided by total assets) |
| SALES_GROWTH | Sales growth (year-to-year change in the natural log of net sales) |
| TANGIBILITY | Asset tangibility (tangible fixed assets divided by total assets) |
| Xi,t | Vector of time-varying controls |
| Yi,t | Outcome variable for firm i in calendar year t |
| | |

## *Abbreviations*

| Abbreviation | Full term |
|---|---|
| ATAD | Anti-Tax Avoidance Directive (EU Council Directive 2016/1164) |
| BEPS | Base Erosion and Profit Shifting |

| | |
|---|---|
| CFC | Controlled Foreign Company |
| DiD | Difference-in-Differences |
| EBITDA | Earnings Before Interest, Tax, Depreciation and Amortisation |
| EU | European Union |
| FE | Fixed Effects |
| GAAR | General Anti-Abuse Rule |
| IMF | International Monetary Fund |
| MNE | Multinational Enterprise |
| OECD | Organisation for Economic Co-operation and Development |
| SEK | Swedish Krona |
| SNI07 | Swedish Standard Industrial Classification 2007 |
| TCJA | Tax Cuts and Jobs Act (U.S., 2017) |
| TWFE | Two-Way Fixed Effects |

## *Serrano database variable codes*

| **Code** | **Description** |
|---|---|
| TILLGSU | Total assets |
| LSKSU | Total non-current liabilities |
| KSKSU | Total current liabilities |
| MATANLSU | Total tangible fixed assets |
| RORRESUL | Operating profit/loss |
| AVSKRIV | Depreciation and amortisation |
| RTEKOKNC | Interest expenses to group companies |
| RTEKOEXT | External interest expenses |
| RTEINKNC | Interest income from group companies |
| RTEINEXT | External interest income |
| LSKKRIN | Non-current liabilities to credit institutions |
| KSKKRIN | Current liabilities to credit institutions |

| | |
|---|---|
| LSKKNC | Non-current liabilities to group and associated companies |
| KSKKNC | Current liabilities to group and associated companies |
| KNC_KONCERN | Group affiliation indicator |
| KNC_ORGNRK | Parent organisation number |

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# 1 Introduction

## 1.1 Background

Corporate taxes are one of the most powerful forces shaping how firms finance themselves and where they direct investment. Since interest payments on tax are deductible, corporate capital structures tend towards leverage. Still, tax incentives seem not to have a consistent effect on corporate capital structure. That happens even if the governments restrict increasingly interest deductibility, so they can avoid overleverage and financial distress (Hanlon and Heitzman, 2022). This highlights the need for additional analysis that can focus on the effects of specific tax reforms on firm behavior.

Tax deductibility of interest payments creates a debt tax shield. This means that each dollar of interest expense reduces taxable income, making debt financing cheaper than equity on an after-tax basis (Kolari and Velez-Pareja, 2010a). Given that there are no countervailing frictions, such as bankruptcy costs, the optimal capital structure would consist almost entirely of debt (Brennan and Schwartz, 1978). Empirically, the debt bias is economically significant. An IMF study that uses data from a large-firm dataset shows that tax-included preference for debt accounts for up to one quarter of corporate leverage (Dallari et al., 2020). OECD also confirms a positive relationship between the generosity of average leverage and debt tax shields (Castillo et al., 2017).

Moreover, multinational enterprises use interest deductibility to shift profits across borders through intra-group lending. That happens while equity is channeled into a low-tax affiliate, which then lends to related entities in high-tax jurisdictions, reducing taxable income where rates are higher. Sweden, where the generous interest deduction rules existed are highest.

The distortion extends beyond domestic financing decisions. Multinational enterprises exploit interest deductibility to shift profits across borders through intra-group lending: equity is channeled into a low-tax affiliate, which then lends to related entities in high-tax jurisdictions, reducing taxable income where rates are highest. Sweden, with its historically generous interest deduction rules, was particularly exposed to these structures. Thomann (2014) mentions that Sweden's tax system led to systematic differences in the effective taxes paid by different sectors, with debt-intensive industries paying substantially less (Thomann, 2014). Lodin (2014) emphasized the need to stop the ongoing erosion of the Swedish company tax base by international profit shifting, using interest payments to lenders in low-tax countries. The analysis from the Swedish tax agency showed that companies were still trying to establish intra-group debts with interest deductions in Sweden, even after the first round of restrictions.

These concerns led to a sequence of increasingly broad reforms. Sweden’s first interest deductibility restrictions entered into force on January, 2009, targeting intercompany loans related to share acquisitions within corporate groups. That was called räntesnurror (interest-spin) structures, and the goal was to decrease the tax avoidance that was happening through the groups. These rules were insufficient, and tax avoidance continued. Therefore, extra limitations were applied in 2013 to protect the tax base in Sweden and cover all the loans that were going through internally, regardless of the purpose (Nydahl, 2013). The framework changed again in 2019. It aligned with the EU Anti-Tax Avoidance Directive (ATAD) and the OECD's BEPS Action Plan 4, which specifically targets base erosion through excessive interest deductions (Nilsson, 2019). In parallel, each reform was accompanied by a decrease in the statutory tax rate, thereby creating a natural experiment. Two distinct policy channels operated simultaneously: one constrained leverage, while the other potentially stimulated investment.

From international studies, these restrictions seem to alter the behavior of the firm. Alberternst and Sureth (2015), exploiting Germany's 2008 interest barrier as a quasi-experiment, find that affected firms reduced their leverage by 4.7 percentage points more than unaffected firms (Alberternst and

Sureth, 2015). De Mooij and Hebous (2017) show that rules targeted only at related-party borrowing have no significant impact on overall debt bias, while rules applying to all debt reduce the debt-to-asset ratio by about 5 percentage points and lower the probability of financial distress by 5 percent (De Mooij and Hebous, 2018). Whether similar magnitudes also appear in Sweden, given its specific institutions, firms, and reform design, is still an open question.

## 1.2 Purpose

The purpose of the thesis is to examine how these three tax reforms affected the financial behaviour of Swedish firms. Specifically, the thesis tests two related but distinct hypotheses. First, it tests whether the restrictions on interest deductibility led highly leveraged firms to reduce their debt ratios. If the debt tax shield is a meaningful driver of capital structure decisions, then limiting the tax benefit of interest payments should disproportionately affect firms that rely on debt financing, prompting them to deleverage or shift toward equity. Second, the thesis will examine whether the reductions in the statutory tax rate increased investment activity. A lower tax rate increases the after-tax return on investment. This encourages capital expenditure and reduces the value of the debt tax shield, gaining the potential of neglecting the effect of the interest deductibility restrictions.

Thus, this thesis analyzes both financing and investment decisions. Examining both outcomes is important since these two channels are related to each other. Specifically, if firms reduce leverage in response to deductibility restrictions, their cost of capital and capacity to fund new investment also change. Studying only one channel would miss the interaction.

Highly leveraged firms are the main focus of this thesis. This occurs because, first, the debt tax shield is most valuable to firms with the highest interest expenses, so any policy that limits deductibility will be most restrictive for these firms. From empirical perspective, this heterogeneity in exposure is what makes the difference-in-differences design possible. The highly leveraged firms serve as the treatment group, as they are the most affected by the reforms. This approach follows the logic of Alberternst and Sureth (2015), who use a similar design to study Germany's interest barrier and find that affected firms reduced leverage by 4.7 percentage points more than unaffected firms (Alberternst and Sureth, 2015). The Swedish setting extends this research by offering three successive reforms of different scope. This provides a space for a more nuanced analysis of how firms adapt over time and in response to various types of tax policy changes.

## 1.3 Research Question

This thesis addresses the following main research question:

**How have Swedish corporate tax reforms restricting interest deductibility and reducing the statutory tax rate influenced the debt ratios and investment behavior of highly leveraged firms?**

To operationalize this, the thesis investigates two sub-questions:

i. Did highly leveraged firms reduce their reliance on debt financing following the interest deductibility restrictions introduced in 2009, broadened in 2013, and further tightened in 2019?
ii. Did the concurrent corporate tax rate reductions increase investment activity, and did this effect differ based on direct exposure to deductibility restrictions?

The main findings can be summarized as follows. The strongest evidence concerns the 2013 reform, where more exposed highly leveraged firms reduced intra-group debt and increased bank debt relative to less exposed highly leveraged firms. The 2019 reform is associated with lower tangible investment, but this result is interpreted cautiously because the early post-reform period

overlaps with Covid-19 and because the pre-trend diagnostics show longer-term divergence. The 2009 reform provides weaker evidence of broad adjustment. Overall, the thesis studies treated exposed high-leverage firms, not the universe of Swedish firms.

## 1.4 Scope

This thesis focuses on Swedish corporate tax reforms. It specifically examines their impact on highly leveraged firms. The thematic scope includes two main policy changes. First, it looks at restrictions on interest deductibility. Second, it examines concurrent reductions in the statutory corporate tax rate. The study evaluates these policies across three distinct reform episodes. These specific episodes occurred in the years 2009, 2013, and 2019.

The temporal scope covers an extended timeframe. The empirical data window spans from 2005 to 2023. The study also restricts its evaluation of corporate behavior to specific accounting variables. Capital structure is analyzed through three distinct metrics. These are total leverage, intra-group debt ratios, and external interest-bearing bank debt. Additionally, real economic activity is evaluated. This is proxied exclusively through tangible fixed asset investments.

The target population is strictly defined. The research is confined to Swedish limited liability companies. The study also requires these firms to be economically active. The data relies solely on individual legal-entity financial statements. It explicitly avoids using consolidated corporate group accounts. Furthermore, financial and insurance institutions are excluded from the dataset. They have unique capital structures that differ from standard non-financial firms.

The primary sample intentionally isolates highly leveraged firms. These firms rely heavily on the debt tax shield. Because of this, they are the most exposed to the new policy limitations. Methodologically, the study is strictly quantitative. It utilizes a Difference-in-Differences (DiD) empirical design. This approach incorporates both firm and year fixed effects. Finally, the analysis is limited to a specific comparison. It compares more exposed firms against less exposed firms. This comparison occurs entirely within the highly leveraged population; hence, the estimates are treatment-population effects rather than estimates for all Swedish firms.

## 1.5 Structure

This thesis is organized as follows:

Chapter 2 gives the background and rules needed to understand the tax reforms studied in this work. It explains how Sweden taxes corporate income and how interest deductibility encourages firms to use more debt. It then places the Swedish reforms in an international setting, discussing profit shifting through intra-group debt, the OECD's BEPS Action Plan 4, and the EU Anti-Tax Avoidance Directive (ATAD). The chapter describes what changed in the three reform rounds in 2009, 2013, and 2019, and ends by showing how these reforms may affect firms' leverage, cost of capital, and investment.

Chapter 3 reviews what earlier studies have said and found. It first explains the basic trade-off theory of capital structure and what it predicts about taxes and debt. It then looks at evidence on whether firms actually react to tax incentives. The chapter also discusses profit shifting and how it relates to real investment, the role of tax enforcement and available information, and new evidence on whether BEPS and ATAD interest limitation rules change how much firms borrow or invest. It ends by pointing out the main research gap of this thesis: we still lack causal evidence on how Swedish firms react, and we need to study borrowing and investment together.

Chapter 4 outlines the empirical approach, research design, and data utilized in the study. It introduces the dataset and details the sample construction process alongside the definitions for key variables, including leverage, investment, reform exposure, and firm controls. The chapter

describes the quantitative analytical strategy, which features a before-and-after analysis of highly leveraged firms around each reform episode using a difference-in-differences design. Finally, it outlines the primary econometric assumptions and details the robustness checks employed, such as alternative leverage cutoffs, varying time periods, and placebo tests.

Chapter 5 explains the data and methods used in the study. It introduces the Serrano dataset, how the sample is chosen, and how the main variables are defined, such as leverage, investment, reform exposure, and firm controls. The chapter then describes a two-step approach: a before-and-after analysis of highly leveraged firms around each reform, and a difference-in-differences design that uses differences in reform exposure among highly leveraged firms. It also explains the main assumptions, especially parallel trends, and briefly describes robustness checks using different leverage cutoffs, time periods, and placebo tests.

Chapter 6 interprets the empirical results. The chapter specifically analyzes how the Swedish corporate tax reforms and individual reform episodes influenced corporate financial behavior. It also provides a critical review of the study's limitations and discusses the broader implications of the findings for both academia and industry.

Finally, Chapter 7 directly answers the primary and secondary research questions based on the synthesized evidence. It concludes the thesis by offering actionable policy recommendations and identifying promising avenues for future research.

# 2 Institutional Background and Policy Framework

*This chapter describes the institutional background on which Swedish corporate tax reforms are based. Emphasis is placed more on the taxation of corporate financing and the policy response to profit shifting due to interests. It begins with the Swedish corporate income tax base and the deductibility of interest expenses under the Swedish Income Tax Act. This feature creates a structural preference for debt over equity. The chapter also mentions how MNEs utilize cross-border debt and interest deductions to erode tax bases (BEPS). Additionally, it highlights how OECD BEPS rules, the EU ATAD, and Sweden's reforms establish coordinated interest limitation standards that influence leverage, the cost of capital, and investment.*

## 2.1 Swedish Corporate Tax System

Corporate income tax applies to Swedish entities and to foreign companies that operate through a permanent establishment in Sweden. That defines the main set of corporate taxpayers covered by the Swedish corporate tax system. A firm in Sweden is taxed on its worldwide income, meaning that corporate residence can bring income from a foreign source into the Swedish tax base ("CORPORATE INCOME TAX IN SWEDEN,"2023).

The statutory corporate income tax rate has been reduced over time and was 21.4% from 1 January 2019. Further reduction was applied in 2021, where the tax rate reached 20.6%. The statutory tax is important since the value of any deductible cost item, including the interest, scales with the corporate tax rate at which that cost reduces the taxable income (Dallari et al., 2020).

The Swedish tax system has traditionally allowed broad deductions for interest, and restrictions were long seen as unnecessary. The main policy reason given is that interest deductibility helps companies finance their operations through borrowing by improving their financing conditions and access to credit. Over time, Sweden has added interest limitation rules to prevent tax planning and base erosion. In particular, interest on internal debts to low-tax jurisdictions can be entirely non-deductible, and interest is also denied if a debt exists mainly to obtain a significant tax benefit for the group, acting as an anti-abuse safeguard against tax-driven debt structures. (Regeringen, 2018).

A great change happened in 2019 when Sweden applied a general interest deduction rule. With this rule, negative interest was deductible only up to 30% of EBITDA, with a simplification rule allowing up to SEK 5,000,000 (Björkeson, 2023). Due to this, excess non-deductible interest expense could be carried forward and used within six years. This preserved some value of disallowed deduction but delayed their use and could reduce their present value relative to immediate deductibility (Björkeson, 2023). A notable contrast is that until 2019, there were no restrictions on the deductibility of interest on external debt. That underscored how the post-2019 general rule broadened the potential set of affected financing costs beyond the earlier focus on intra-group arrangements.

In most corporate income tax systems, interest expenses are deductible, and returns on equity are not. This creates an asymmetric tax treatment of financing sources that favours debt over equity in the corporate sector (De Mooij and Hebous, 2018). In other words, companies can usually deduct interest payments on debt from their taxes, but they rarely get to deduct dividend payments. This makes borrowing with debt artificially cheaper than raising money with equity, and it breaks tax neutrality between different ways of financing (Dallari et al., 2020). This pushes firms to use debt over equity to finance investment more than they would. As a result, the way that the tax system is designed encourages higher corporate debt and can increase the financial risk (De Mooij and Hebous, 2018). Swedish-focused discussion similarly highlights that treating borrowed capital differently from venture capital also creates distortions, because financing with borrowed capital is tax-favoured due to the deductibility of interest expenses (Sparrfelt, 2011).

## 2.2 International Tax Avoidance

In international tax policy, profit shifting under BEPS refers to tax planning that exploits gaps in tax rules to move profits to low- or no-tax locations with little or no economic activity. Different tax rates incentivize reallocating taxable income and deductions across jurisdictions, and when one channel is restricted, firms shift to other, less restricted profit-shifting strategies (Nicolay et al., 2017).

Profit shifting can be divided into income shifting (moving revenues) and deduction shifting (moving costs), both of which change where profits are taxed within a multinational group (Alfandia, 2024). Income shifting often uses transfer pricing, where prices for transactions within the group are set so that more income appears in low-tax countries and less in high-tax ones (Alfandia, 2024). Deduction shifting usually relies on debt. Groups can shift debt and adjust interest on intra-group loans so that interest deductions reduce taxable income in high-tax affiliates while interest income is recorded in low-tax affiliates (OECD, 2016). They may even create intra-group interest deductions that surpass the group's actual third-party interest expense (OECD, 2016). Hybrid instruments and mismatches can generate interest deductions without matching taxable income and can also fund exempt or deferred income, further weakening the link between taxable profits and real activity (Vleggeert, 2015).

Debt is useful for cross-border tax avoidance because interest is often deductible for the payer but taxed for the recipient, so firms place deductions in high-tax countries and interest income in low-tax countries (Vleggeert, 2015). MNEs may thinly capitalize affiliates in high-tax countries and fund a low-tax affiliate with equity, which then lends to high-tax affiliates so they can deduct interest and reduce their tax base. A 10% cut in the tax rate of the lowest-tax affiliate is linked to about a 7% rise in internal borrowing elsewhere, showing how sensitive internal leverage is to tax-rate differences (Institute et al., n.d.).

The Swedish experience with "räntesnurror" shows how debt-based tax planning works in practice. Companies claimed large interest deductions in Sweden, while the corresponding interest income was taxed at low rates abroad or not at all. Typically, a Swedish subsidiary shifted profits to another through group contributions. The second subsidiary then borrowed from a foreign company in a low-tax country and paid out its profits as interest, which was deductible in Sweden, while gains on participations were tax-exempt. The Swedish Tax Agency estimated that these structures cost up to SEK 7 billion in lost tax revenue, highlighting their fiscal significance.

To illustrate the cross-border dimension of corporate expansion, Figures 1 and 2 report the first appearance of new subsidiaries abroad and in Sweden between 2007 and 2021. Although these figures are descriptive rather than causal, they provide useful background for the international tax-avoidance discussion by showing where Swedish parent firms expanded and how foreign expansion evolved.

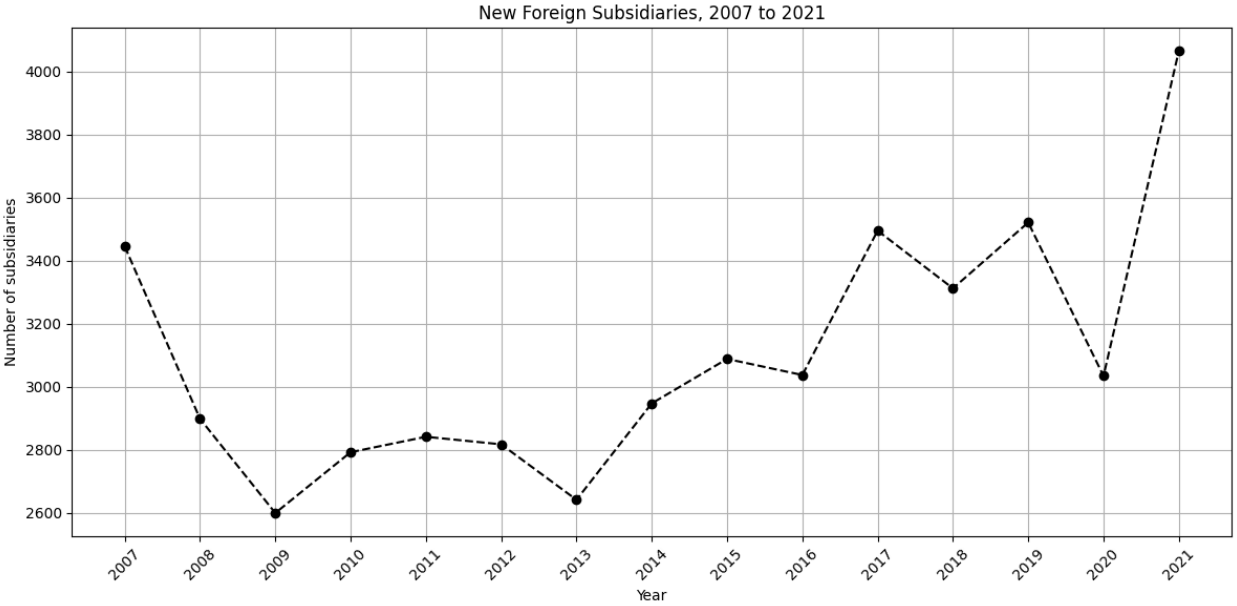


*Figure 2.1: New foreign subsidiaries by first appearance, 2007-2021. Source: Calculations based on Serrano data.*

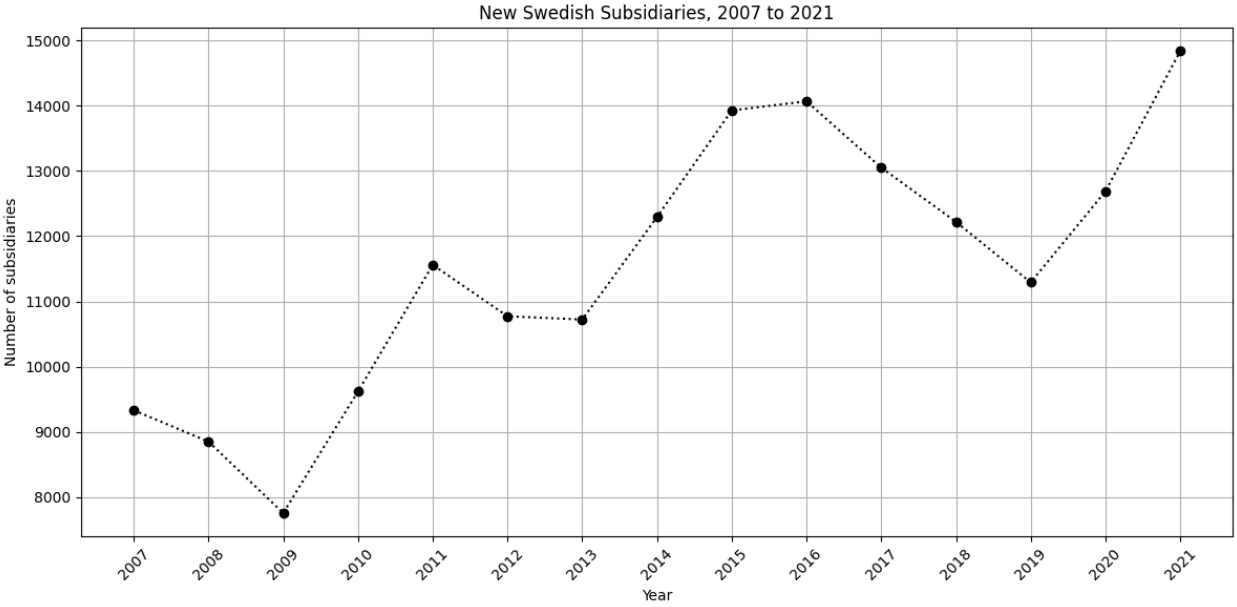


*Figure 2.2: New subsidiaries in Sweden by first appearance, 2007-2021. Source: Calculations based on Serrano data.*

Figures 2.1 and 2.2 also show a descriptive divergence between new foreign and domestic subsidiaries. The decline in new foreign subsidiaries coincides with the first reform, and foreign subsidiaries also fall in 2013 around the second reform, while domestic subsidiaries do not show a similar drop around the second reform. This pattern is descriptive rather than causal, but it supports the relevance of the cross-border and group-financing channel discussed in this chapter.

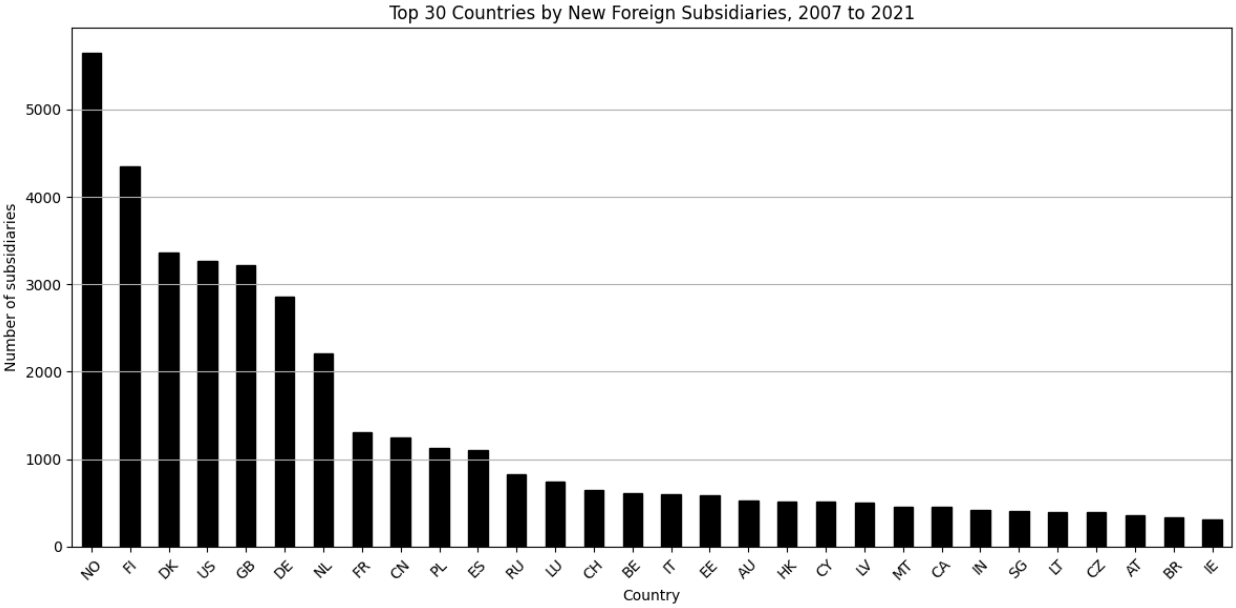


*Figure 2.3: Top 30 countries by new foreign subsidiaries, 2007-2021. Source: Calculations based on Serrano data.*

Figure 3 further shows that foreign subsidiary creation is concentrated mainly in nearby and economically integrated markets, such as Norway, Finland, Denmark, the United States, and the United Kingdom. Some jurisdictions often associated with tax planning, including Ireland, Luxembourg, and Switzerland, are present but not among the dominant destinations. This shows that havens may matter for specific structures, such as intra-group financing, but they do not appear to be the main destination pattern in the broader subsidiary-creation data.

For governments, the core fiscal problem is base erosion. When interest is deducted in high-tax jurisdictions, taxable profits and tax revenues fall because deductible interest reduces the corporate tax base (Institute et al., n.d.). Exploiting BEPS opportunities reduces revenues and harms tax-system integrity, while lawful planning can still undermine fairness and distort competition. These concerns were a direct motivation for the OECD/G20 BEPS project and, in particular, for Action 4 on limiting base erosion involving interest deductions and other financial payments (OECD, 2016).

## 2.3 OECD BEPS Initiative

The OECD/G20 BEPS project was created because gaps in international tax rules let companies shift profits and avoid taxes. Its goal was to fix these rules so profits are taxed where business activities and value creation actually happen (OECD, 2016). In OECD usage, BEPS refers to tax planning strategies that use gaps in tax rules to artificially shift profits to low- or no-tax locations where there is little or no economic activity (Tell, 2017). A key goal of the BEPS Action Plan is that companies report their profits in the countries where they actually do business and create value. The OECD expects that, once the measures apply, profits will be reported where the economic activities that generate them become applicable (Ibrahim and Sari, 2023).

BEPS became a key global policy issue after the OECD and G20 launched the BEPS project in February 2013 and adopted a 15-point Action Plan in September 2013 (Tell, 2017). The policy was also driven by the belief that profit shifting by MNEs was a serious current problem. It urged countries to review how they tax intragroup financial transactions, especially interest and other financial payments (OECD, 2016).

If one tax-avoidance method is restricted, firms often switch to other methods. This allows them to keep shifting taxable income across countries, even when some rules become stricter (OECD,

2016). To address these risks, Action 4 proposes rules that limit base erosion resulting from interest and similar financial payments. International rules are necessary because limits set by a single country often fail to work effectively, as businesses can easily transfer money through various means (Vleggeert, 2015). Action 4 focuses on the use of third-party, related-party, and intragroup debt to secure excessive interest deductions or to fund income that is exempt or deferred. The OECD found that interest limitation rules are needed as part of the BEPS response (Ibrahim and Sari, 2023). On 5 October 2015, the OECD issued BEPS Action 4 recommendations on interest deduction limits, which later informed domestic implementation debates (Regeringen, 2018).

The recommended architecture includes three elements: a fixed ratio rule based on a benchmark net interest-to-EBITDA ratio, a group ratio rule that can allow higher deductions based on the worldwide group's position, and targeted rules to address specific risks and circumvention strategies (OECD, 2016). Under this rule, the OECD suggests that a company's net interest expenses should be limited to a set percentage of its EBITDA (earnings before interest, tax, depreciation and amortization). It recommends choosing this percentage between 10% and 30% so that the rule is strict enough but still flexible for different countries (OECD, 2016). A key design feature is that the fixed ratio rule limits net interest deductions. Since a fixed ratio can be too rough for high-leveraged groups, the OECD also allows a worldwide group ratio rule. This lets a company deduct more interest than a country's fixed ratio, as long as it stays within its group's overall net interest-to-EBITDA ratio (OECD, 2016).

Action 4 changes how firms behave by reducing the extra tax benefit they get from adding more debt to high-tax entities once those entities hit their fixed (or group) limit on interest deductions. This directly lowers the reward from using thin capitalization and earnings stripping strategies that depend on very large net interest deductions (OECD, 2016). For this to be effective, countries must coordinate so the rules are applied in a similar way everywhere and companies cannot shift their profits to other countries just because national rules are different (Alfandia, 2024).

## 2.4 EU Anti-Tax Avoidance Directive

The EU Anti-Tax Avoidance Directive is Council Directive (EU) 2016/1164, adopted on 12 July 2016, and framed as laying down rules against tax avoidance practices. These practices directly affect the functioning of the internal market (Council of the European Union, 2016). In EU policy, priorities in international taxation include ensuring that taxes are paid where profits and value are generated. This priority is presented as a motivation for adopting ATAD as an internal market measure (Stejskalová et al., 2020).

ATAD is a minimum directive that sets an absolute minimum standard of anti-tax avoidance rules in the EU that Member States must implement (European Commission, 2020). ATAD can be the defined as the minimum directive that must as a main rule be adopted by Member States as of 1 January 2019, creating a coordinated timeline for EU-wide adoption (Tell, 2017). Although ATAD is differs from the OECD BEPS project in institutional terms, it is designed to implement BEPS proposals and to harmonize the adoption of anti-BEPS measures across EU Member States (KPMG, 2024). ATAD also offers a minimum level of protection for the internal market and the integration of OECD BEPS measures, which is particularly relevant for interest limitation rules linked to BEPS Action 4.

A basic element of ATAD finance is the interest limitation rule in Article 4. Article 4 provides that "exceeding borrowing costs" shall be deductible only up to 30% of the taxpayer's EBITDA, embedding an earnings-stripping rule as an EU-wide minimum standard (Council of the European Union, 2016). The directive says that "exceeding borrowing costs" are the borrowing costs you can deduct minus the taxable interest and similar taxable income you receive. In other words, it looks at the net financing cost, not just the total interest costs (Council of the European Union, 2016). It also specifies how EBITDA is calculated. It starts from the income that is subject to

corporate tax and then adds back tax-adjusted exceeding borrowing costs and depreciation or amortisation, while leaving out tax-exempt income. This produces a standardized earnings measure that the cap is based on (Council of the European Union, 2016).

Analytically, Article 4 implements a fixed-ratio rule that can be summarized as:

$$\text{Deductible exceeding borrowing costs} \leq 0.30 \times \text{EBITDA}$$
$$\text{(Council of the European Union, 2016)}$$

If net financing costs exceed the fixed ratio, the entity cannot deduct these exceeding financing costs in the current period.

ATAD embeds options and safe harbours for Member States. The directive lets taxpayers fully deduct up to EUR 3,000,000 of extra borrowing costs, regardless of the EBITDA limit, which eases admin work for smaller taxpayers (Council of the European Union, 2016). It also allows full deduction for standalone entities, reflecting the view that entities outside group structures have less risk of aggressive financing-based tax planning (Council of the European Union, 2016). ATAD also provides measures in five distinct areas that Member States are required to implement: the interest limitation rule, exit taxation, a general anti-abuse rule (GAAR), controlled foreign company (CFC) rules, and hybrid mismatch rules (European Commission, 2020). Sweden applies a general interest limitation rule (Article 4) and hybrid mismatch rules (Article 9), reinforcing the centrality of financing structures and hybrid arrangements in the EU anti-avoidance agenda (Regeringen, 2018). An amending directive broadened the hybrid mismatch rules to also cover third countries, showing that EU anti-hybrid policy now extends beyond EU-only arrangements (Regeringen, 2018). ATAD is important for EU countries because it turns BEPS-based anti-tax-avoidance rules into binding EU law. This helps reduce the ability of countries to play one set of rules against another within the EU and creates a common basic standard for all countries in the internal market. ATAD is important because Member States must develop base-protection rules compatible with EU law, balancing this with protecting an attractive business environment, a core challenge for nations policymaker.

## 2.5 Tax Reforms 2009, 2013 and 2019

Sweden's current corporate tax policy can be described as a combination of reductions in the statutory tax rate and restrictions on interest deductions. Authorities often reacted when they saw companies use interest deductions to move profits out of Sweden through "interest turbines" and similar setups, motivating statutory limitation rules.

In 2009, the Swedish government removed the possibility of making interest deductions in cases of internally financed acquisitions of ownership rights. This reflected a specific focus on acquisition-related intra-group debt structures. At the same time, two exemptions were introduced. Interest remained deductible if it would be taxed at a rate of at least 10% in the recipient country, or if the acquisition and the loan were considered commercially justified. Since the main issue was that companies used interest deductions to shift untaxed profits abroad, the Supreme Administrative Court found that Skatteflyktslagen did not apply in such cases. Therefore, Sweden introduced interest limitation rules in 2009.

The 2013 reform was a substantial broadening of the earlier approach. Further limitations on interest deductibility were introduced, and the legislation limited deductibility for all forms of loans. 2013 restrictions were applicable to corporate group loans no matter what kind of loan would be used for. That created an expansion beyond acquisition- linked constraints toward a general intercompany-loan scope. The 2013 reform also included a major statutory tax rate cut from 26,3 to 22%. Sweden faced a competitive disadvantage because its corporate tax rate was higher than both the EU and OECD average rates, and the reduction was expected to strengthen the investment climate and growth in Sweden, which frames the reform as combining

competitiveness concerns with base-protection measures (Zeldin, 2012). The rate cut was partly funded by stricter limits on deducting interest on internal loans, as part of a plan to broaden the tax base (Zeldin, 2012).

The 2019 reform introduced the general EBITDA-based interest deduction limitation. That meant that negative net interest can only be deducted up to 30% of EBITDA or under a SEK 5,000,000 simplification rule, and that excess non-deductible interest expense can be carried forward for six years (KPMG, 2020). Those changes were highly related to international commitments and coordinated anti-avoidance initiatives. Sweden introduced general rules limiting interest deductions, in line with the EU Anti-Tax Avoidance Directive, and these rules were based on the OECD BEPS project from 2015 (Deloitte, 2025). The 2019 package was introduced along with a cut in the corporate tax rate. According to a country profile, the rate was 21.4% from 1 January 2019 and fell to 20.6%.

*Figure 2.4: Statutory corporate tax rates in Sweden, the EU average, and the OECD average, 2007-2024. Source: OECD Tax Database and European Commission, Taxation Trends in the European Union.*

Figure 4 shows that Sweden entered the reform period with a statutory corporate tax rate above both the EU and OECD averages. The 2013 rate cut from 26.3% to 22% narrowed this gap substantially, while the 2019 and 2021 reductions brought Sweden much closer to the international averages. The figure is therefore useful for understanding the second policy channel in this thesis: alongside tighter interest-deduction rules, Sweden also reduced the statutory tax rate in a way that could improve the post-tax return to investment.

## 2.6 Economic Channels

Since companies can deduct interest but not equity returns from the taxes, companies prefer to use more debt, creating what is known as debt bias (Alberternst and Sureth, 2015). High debt makes firms less resilient, and tax rules that favor debt can encourage companies to borrow, affecting their financial stability (Luca and Tieman, 2016). Policy changes that limit interest deductions work through connected channels. Firms change their debt levels, which alters financing costs and the cost of capital, and this can affect real investment (Sanati and Beyhaghi, 2025)

With corporate taxes and deductible interest, the Modigliani–Miller framework predicts that firm value is maximized with all-debt financing because the interest tax shield makes borrowing “cheap money” and its value rises with leverage (Farrar and Selwyn, 1967). Modern trade-off theory

argues instead that tax benefits are balanced by increasing expected bankruptcy and distress costs, so firms typically choose an interior optimal leverage level, especially since the cost of equity rises more than proportionally with leverage, reducing the effective value of the tax shield at high debt ratios (Kolari and Velez-Pareja, 2010).

Brennan's option-pricing model formalizes this by noting that interest tax savings stop at bankruptcy: more debt increases tax savings conditional on survival but also raises bankruptcy risk, yielding an interior optimum, with higher risk lowering the optimal leverage because bankruptcy becomes more likely (Fleckenstein et al., 2020).

Empirical evidence generally shows that higher corporate taxes raise leverage. A 1% increase in the marginal corporate tax rate raises leverage by 0.15%, with leverage changes following tax changes and smaller, financially constrained firms adjusting more slowly (Longstaff and Strebulaev, 2014). Quasi-experimental evidence on interest deductibility limits supports that Germany's "interest barrier" causes affected firms to cut leverage by 3–4.7 percentage points more than unaffected firms, with unconstrained firms reducing leverage by up to 6 points, indicating that tighter deductibility rules induce deleveraging (Alberternst and Sureth, 2015).

Thin capitalization evidence shows that narrow rules are less important than broad rules. Rules targeting only related-party or intracompany debt have no clear impact on external debt (De Mooij and Hebous, 2018). Thin capitalization rules that cover all corporate debt reduce average debt ratios by about 5 percentage points and cut the bankruptcy risk for manufacturing firms by a similar amount, showing that broad leverage limits can improve financial stability (De Mooij and Hebous, 2018).

The cost-of-capital channel stems from the tax treatment of financing costs. Because interest is typically tax-deductible, debt is favored over equity and the tax shield benefits more highly levered firms, lowering their cost of capital and affecting value and financing incentives (Alberternst and Sureth, 2015). In standard theory with corporate taxes, higher leverage reduces the cost of capital, encouraging firms to maximize debt when the tax shield is available (Fliers et al., 2024). Interest limitation rules partially reverse this channel by increasing the after-tax cost of debt for affected firms, thereby discouraging high debt ratios and shifting financing toward equity or retained earnings where possible (De Mooij and Hebous, 2018).

Firms invest until the marginal revenue product of capital equals the user cost, with greater tax deductibility lowering this user cost and the investment hurdle rate (Lester and Olbert, 2025). Restricting interest deductibility reduces the deductibility of a key financing cost, raising effective capital costs and potentially depressing real investment (OECD, 2016). Limiting interest deductions can reduce investment and hiring, as these limits decrease leverage but also raise the cost of external finance, leading treated firms to reduce investment by 7.3% and hiring by 18.4% (Sanati and Beyhaghi, 2025). Higher corporate income taxes reduce after-tax returns and discourage investment. Industry estimates suggest that a 5% increase in the effective marginal tax rate leads to a 1.6% long-run and 0.8% short-run drop in investment. Large and highly profitable firms have become less sensitive to taxes since the global financial crisis (Hanappi et al., 2023). OECD evidence from Germany shows that thin capitalization and interest barrier rules do not significantly reduce investment. This suggests that institutional details, tax avoidance options, and changes in firms' capital structure can offset real investment effects even when taxable income changes (OECD, 2016).

Sweden's shift to broader limits on interest deductions, especially the 30% EBITDA cap introduced in 2019, should discourage firms from using as much debt once the cap applies. This matches evidence that stricter thin capitalization rules make firms reduce their internal or total debt-to-capital ratios (Björkeson, 2023). However, research also shows that highly leveraged firms are more directly affected by interest stripping rules, while large multinationals can often soften the impact by replacing debt with equity, leading to different effects depending on leverage and multinational status (Kaserer, 2008). Further evidence indicates that debt bias is stronger for larger

firms and those with more tangible assets, and weaker for firms with volatile or high revenue. So, different industries and firms with different balance sheets are likely to react differently to the Swedish reforms in terms of leverage and investment (Dallari et al., 2020).

Sweden's corporate tax system, with broad interest deductibility under IL 16:1 and falling corporate tax rates, created a domestic debt bias and scope for cross-border interest-based tax planning, including tax avoidance schemes (De Mooij and Hebous, 2018). Motivated by this and the OECD's BEPS diagnosis of gaps in tax rules, interest limitation rules were adopted internationally, notably OECD BEPS Action 4 and the EU ATAD EBITDA-based cap on net borrowing costs. Sweden's reforms provide a setting to study how interest deductibility rules affect leverage, the cost of capital, and investment, and whether these effects differ across firms more or less likely to be constrained by the rules.

# 3 Literature Review

*This chapter reviews empirical evidence on how corporate taxes affect capital structure and how restricting interest deductibility affects real investment.*

## 3.1 Capital Structure and Taxation

Long-run U.S. evidence indicates that corporate leverage moves with corporate tax rates. Longstaff and Strebulaev find that changes in corporate leverage are directly related to changes in corporate tax rates for all but the smallest firms (Longstaff and Strebulaev, 2014). They show that leverage adjustment to tax changes is slower for smaller firms facing financial constraints, pointing to gradual balance-sheet re-optimization rather than instantaneous rebalancing (Longstaff and Strebulaev, 2014). U.S. evidence shows that a 1% rise in the marginal corporate tax rate is associated with a 0.15% increase in corporate leverage. This positive link between taxes and leverage remains even after controlling for distress costs, liquidity, and macroeconomic conditions (Fleckenstein et al., 2020).

Difference-in-differences evidence from U.S. state tax changes is consistent with these long-run patterns. Heider and Ljungqvist find that firms increase debt in their capital structure after tax increases relative to control firms operating in the same industry and time period but located in states without tax changes (Heider and Ljungqvist, 2012). They estimate that a tax rise increases long-term leverage by 104 basis points on average, and they interpret unchanged total assets as indicating substitution from equity into debt rather than balance-sheet expansion (Heider and Ljungqvist, 2012). Their estimates are asymmetric. Firms borrow more after tax rises, but tax cuts do not produce a corresponding reduction in leverage (Heider and Ljungqvist, 2012).

Microdata from Germany yields similar magnitudes in proportional terms. Dwenger and Steiner estimate that for profit-making firms, a 1% increase in the marginal tax rate increases financial leverage by approximately 0.7% on average (Dwenger and Steiner, 2014). Meta-analytic evidence aggregates across many contexts and also supports a positive tax–leverage relationship. Feld concludes that the debt-to-asset ratio rises by 2.7% when the simulated marginal tax rate increases by 10%.

Tax incentives can produce economically meaningful changes in leverage. Regression estimates suggest that debt bias may explain over 5 percentage points of firms' leverage, relative to a median leverage of 20% of assets. This implies that tax treatment accounts for a meaningful share of observed leverage. The effect is not uniform: debt bias is stronger for firms with more tangible assets and weaker for firms with volatile or high revenue (Dallari et al., 2020). Related work also shows that tax avoidance shapes how leverage responds to tax rates. Brok finds that a standard deviation increase in the tax rate is associated with 2.8% higher leverage for firms with low tax avoidance, but with lower leverage for firms with high tax avoidance, suggesting that tax avoidance can substitute for debt-based tax benefits (Brok, 2026).

Limiting the tax value of interest can reduce leverage for treated firms. For Germany's interest barrier, affected companies reduce leverage by 3% more than unaffected companies, with larger reductions among affected firms without liquidity constraints (4.9%).[6] A closely related study finds a reduction of 4.7% for affected firms relative to non-affected firms, and a 6% reduction for firms with no liquidity constraint (Alberternst and Sureth, 2015). U.S. evidence from Carrizosa, Gaertner, and Lynch finds that affected firms decrease leverage by 2.9% of assets relative to unaffected firms, corresponding to a 5.8% debt decrease relative to the pre-TCJA affected firms' average debt-to-asset ratio (Carrizosa et al., 2019)

Rule design matters for how firms adjust their balance sheets. De Mooij and Hebous show that rules aimed only at related-party borrowing do not significantly affect debt bias (which depends

on third-party borrowing). In contrast, rules that apply to all debt lower the debt-to-asset ratio by about 5 percentage points and cut the probability of financial distress by about 5% (De Mooij and Hebous, 2018). At the multinational affiliate level, leverage restrictions also change how debt is structured. Blouin et al. find that a total leverage cap reduces an affiliate's overall leverage by 1.9% on average, while rules limiting parent borrowing reduce internal leverage by 6.3% on average (Blouin et al., 2014). Buettner and Overesch also find that thin-capitalization rules increase the influence of local taxes on financial structure and make investment more sensitive to tax when debt finance is constrained (Buettner et al., 2006; Heckemeyer and Overesch, 2013).

## 3.2 Profit Shifting and Investment

Meta-analytic evidence implies that reported profits are responsive to international tax differentials: Heckemeyer and Overesch predict a tax semi-elasticity of pre-tax profit of about 0.8 in absolute value, implying that reported profits decrease by about 0.8% when the exploitable international tax differential increases by 1% (Heckemeyer and Overesch, 2013). Evidence on functional form suggests that constant-elasticity models may be misleading. Dowd, Landefeld, and Moore show that log-linear specifications can greatly understate profit sensitivity in low-tax jurisdictions and overstate it in high-tax jurisdictions (Dowd et al., 2016).

Recent quasi-experimental estimates can be smaller than earlier consensus values. Merlo and Wamser estimate a semi-tax elasticity of pre-tax profits of about 0.22 and show that profits are relocated to the "next-best" low-tax alternative within the MNE when a preferred location is blocked by a CFC rule (Merlo and Wamser, 2024). At a more aggregate level for Europe, Huizinga and Laeven estimate a macro semi-elasticity of reported profits with respect to the top statutory tax rate of 1.43 and estimate shifting costs of 1.6% of the tax base, implying substantial redistribution of tax bases across countries (Huizinga et al., 2007).

The same literature suggests interactions between profit shifting and real investment. Overesch finds that German subsidiaries invest more when their foreign parent companies face lower tax rates in their home countries, consistent with cross-border tax planning affecting the effective tax wedge on investment in high-tax host countries (Heckemeyer and Overesch, 2013). Buettner further shows that thin-capitalization rules affect financial structure and that investment becomes more sensitive to taxes when debt finance is restricted (Buettner et al., 2006).

Transparency and enforcement can also induce reallocations even when aggregate investment does not change. Evidence using regression discontinuity and difference-in-differences designs finds no changes in consolidated human or physical investment but documents reallocation toward tax-favorable European jurisdictions and away from tax havens (Lester and Olbert, 2025). Consistent with this, country-by-country reporting is described as increasing tax authorities' ability to detect profit shifting, and De Simone and Olbert are summarized as finding that affected multinationals reallocated real resources across European countries in response (De Simone and Olbert, 2021)

Direct evidence on earnings-stripping rules shows negative real effects for affected firms. Sanati finds that treated firms reduce their investment rate by 7.3% and their hiring rate by 18.4% relative to a control group, with stronger effects among cash-constrained firms, consistent with a cost-of-capital channel through external finance (Sanati and Beyhaghi, 2025). These effects can spread through supply chains. Shevlin, Venkat, and Yoo find that suppliers whose customers are subject to the TCJA limitation report 11.2%–14.9% higher accounts receivable relative to their pre-TCJA average, indicating working-capital spillovers from interest limitation exposure (Shevlin et al., 2026)

Finally, evidence on broader corporate tax reductions points to positive investment responses. Crawford and Markarian find that investment increases for U.S. firms relative to Canadian firms after U.S. tax cuts under the TCJA (Crawford and Markarian, 2024). Overall, this evidence implies that policies restricting interest deductibility and profit shifting can affect both reported profits and

real activity. But the sign and magnitude for investment depends on enforcement, firm constraints, and how reforms bundle base-broadening with other tax changes (Sanati and Beyhaghi, 2025).

## 3.3 Research Gap

The first gap is geographic. There is little evidence that focuses specifically on Sweden's recent corporate tax reforms. Most existing studies instead examine large economies or cross-country patterns, and therefore do not speak directly to Sweden's institutional setting. The second gap is analytical: relatively few studies provide an integrated analysis of financing and investment behavior under a single, unified tax change, even though both margins may respond jointly when interest deductibility is restricted. The third gap is design-based: Sweden expanded its interest-deduction limitation rules in 2013 to prevent erosion of the corporate tax base and revised them again in 2019 due to international commitments, creating sequential reform episodes that are well-suited for comparative analysis across policy regimes.

This thesis addresses these gaps by focusing on Sweden and by jointly studying financing and investment outcomes, drawing on insights from both corporate finance and public economics. It exploits Sweden's sequence of reforms as policy variation and uses a difference-in-differences design to compare more leveraged firms with less leveraged firms across the three reform episodes.

# 4 Method

*This chapter explains how the study measures the impact of Swedish corporate tax reforms that restrict interest deductibility and lower the corporate tax rate. It analyzes effects on firms' debt ratios and investment, focusing on highly leveraged firms and comparing those with higher and lower exposure to each reform. It also summarizes the reform timeline, data, variables, econometric methods, assumptions, and robustness checks.*

## 4.1 Research Design

The thesis examines how Swedish corporate tax reforms affect highly leveraged firms. The reforms restrict interest deductibility and reduce the statutory corporate tax rate. The study focuses on four outcomes: total leverage, interest-bearing bank debt, intra-group debt, and tangible investment. The design asks whether firms with stronger pre-reform exposure changed these outcomes more than comparable firms with lower exposure.

The empirical analysis is organised around two operational questions. First, it examines whether highly leveraged firms reduced their reliance on debt financing after the 2009, 2013, and 2019 interest-deduction restrictions. Second, it examines whether investment changed around the same reforms, and whether this differed by exposure to the deductibility restrictions.

The identifying idea is that firms were differentially exposed to the reforms based on pre-reform characteristics such as leverage and reform-specific interest exposure, allowing for a difference-in-differences design. The comparison is not between highly leveraged firms and all other Swedish firms. It is between more- and less-exposed firms within the highly leveraged eligible population. The estimated effects should therefore be read as effects for the treated exposed/high-leverage firms rather than for the universe of Swedish firms. Each reform-specific design includes firm fixed effects and year fixed effects. Firm fixed effects absorb stable firm characteristics. Year fixed effects absorb shocks that are common to all firms in the same year, including the general macroeconomic environment.

### 4.1.1 Baseline DiD specification

$r \in \{2009,2013,2019\} Y_{i,t}$For each reform year and each outcome, the baseline (fixed-effects-only) DiD regression is specified as:

$$Y_{i,t} = \alpha_i + \gamma_t + \beta \cdot \left(\text{POST}_t^{(r)} \times \text{EXPOSED}_i^{(r)}\right) + \varepsilon_{i,t}.$$

$Y_{i,t} it \alpha_i \text{EXPOSED}_i^{(r)} \gamma_t \text{POST}_t^{(r)} r \text{EXPOSED}_i^{(r)} \varepsilon_{i,t} \text{POST}_t^{(r)} \gamma_t \text{EXPOSED}_i^{(r)} \alpha_i$where is the outcome for firm in calendar year; is a firm fixed effect absorbing time-invariant firm heterogeneity (including the main effect of); is a year fixed effect absorbing common time shocks (including the common component of the reform-year policy package); is an indicator equal to 1 for years at and after the reform year and 0 otherwise; is a time-invariant pre-reform indicator capturing exposure to the reform-specific interest limitation; and is an idiosyncratic error term (Zwick and Mahon, 2017). In this design, is collinear with the year fixed effects within the reform-specific estimation window, and is collinear with the firm fixed effects, so the interaction term is the only treatment regressor included in the baseline specification (Zwick and Mahon, 2017).

### 4.1.2 Control-rich robustness specification

A second, control-rich specification is estimated as a robustness exercise to assess sensitivity to including time-varying firm characteristics in the right-hand side, even though these covariates can be affected by the reform package and are therefore treated cautiously. The robustness specification is:

$$Y_{i,t} = \alpha_i + \gamma_t + \beta \cdot \left(\text{POST}_t^{(r)} \times \text{EXPOSED}_i^{(r)}\right) + \text{X}_{i,t}'\delta + \varepsilon_{i,t}.$$

$\mathbf{X}_{i,t}$where is a vector of time-varying controls. These include firm size, profitability, asset tangibility, sales growth, and group affiliation where the variable is available and not collinear with the estimation sample. The controls are based on Serrano accounting variables: total assets (TILLGSU), operating profit/loss (RORRESUL), tangible fixed assets (MATANLSU), net sales (NTOMS), and group relationship information (KNC_KONCERN) (Dun & Bradstreet, 2023).

### 4.1.3 Reform timing

Each reform is treated as a distinct policy episode because the reforms differ in scope and therefore imply different mechanisms for exposure within the highly leveraged population. The first reform enters into force in January 2009 and targets intercompany loans related to share acquisitions within corporate groups, reflecting concerns about "räntesnurror" (interest-spin) structures used for tax avoidance through intra-group financing. The second reform enters into force in January 2013 and expands the rules to all intercompany loans regardless of purpose, newly bringing additional intra-group debt structures into scope while still sparing firms financed primarily through external debt. The third reform is proposed to enter into force on 1 January 2019 and introduces a general EBITDA-based cap on all interest expenses (internal and external), making a firm's interest-to-EBITDA ratio the key determinant of whether deductions are restricted (Regeringen, 2018).

### 4.1.4 Reform-specific exposure

Exposure is defined separately for each reform. It is measured using pre-reform information only. This avoids classifying firms based on financing choices that may already reflect the reform. The updated design also uses net internal-interest exposure for the 2009 and 2013 reforms. This is more conservative than using any gross intra-group interest expense, because it focuses on the net internal financing burden rather than a small accounting entry.

Serrano financial statement items can follow different sign conventions across firms. Interest expenses and depreciation are therefore converted into non-negative magnitudes before the exposure variables are constructed. Interest income is also converted into a positive magnitude. This makes the variables reflect economic exposure rather than bookkeeping signs (Dun & Bradstreet, 2023).

Using these magnitudes, the auxiliary exposure components are defined as:

$$\text{TOT_INT_EXP} = \text{KNC_INT_EXP_MAG} + \text{EXT_INT_EXP_MAG},$$

$$\text{TOT_INT_INC} = \text{KNC_INT_INC_MAG} + \text{EXT_INT_INC_MAG},$$

$$\text{NET_INT_EXP} = \text{TOT_INT_EXP} - \text{TOT_INT_INC},$$

$$\text{EBITDA} = \text{RORRESUL} + \text{DEPR_MAG},$$

$$\text{NET_INT_EBITDA} = \frac{\text{NET_INT_EXP}}{\text{EBITDA}} \quad \text{defined only if EBITDA} > 0,$$

$$\text{INT_INT_SHARE} = \frac{\text{KNC_INT_EXP_MAG}}{\text{TOT_INT_EXP}} \quad \text{defined only if TOT_INT_EXP} > 0.$$

These constructions correspond to the reform logic that exposure depends on whether firms have intra-group interest payments (2009), rely more heavily on intra-group relative to external interest (2013), and exceed a net-interest-to-EBITDA cap and a de minimis net interest threshold (2019).

Exposure indicators are then defined as follows.

First, for the 2009 reform, exposure is defined as:

$$\text{EXPOSED}_i^{(2009)} = \mathbf{1}\left[\text{KNC_INT_EXP_MAG}_{i,2008} > 0\right]$$

This definition avoids treating very small or mechanical intra-group interest entries as economically meaningful exposure. It also keeps the 2009 treatment assignment fixed before the reform starts.

$m_{2012}$INT_INT_SHARE$_{i,2012}$INT_INT_SHARE$_{i,2012} > 0$Second, for the 2013 reform, exposure is based on the net internal-interest share in 2012. The median is calculated only among firms with positive net internal-interest exposure. This prevents the control group from being defined against a large mass of firms with no internal interest exposure. Let denote the median of computed only across firms with. Exposure is then defined as:

$$\text{EXPOSED}_i^{(2013)} = 1\left[\text{INT_INT_SHARE}_{i,2012} \geq m_{2012}\right].$$

This definition is aligned with the 2013 broadening of the rules to intercompany loans more generally. It separates firms that relied more strongly on net intra-group interest from high-leverage firms with lower internal-interest exposure.

Third, for the 2019 reform, exposure is based on the general earnings-stripping rule. A firm is classified as exposed when its net-interest-to-EBITDA ratio exceeds 30 percent and its net interest expense exceeds the SEK 5,000,000 simplification threshold in 2018. Serrano monetary values are implemented in thousands SEK, so the threshold is applied as 5,000 KSEK.

Exposure is defined as:

$$\text{EXPOSED}_i^{(2019)} = 1\left[\text{NET_INT_EBITDA}_{i,2018} > 0.30 \text{ AND NET_INT_EXP}_{i,2018} > 5{,}000\right].$$

This definition operationalizes the idea that the general earnings-stripping rule binds primarily when net interest expenses are both large in absolute terms and large relative to EBITDA.

## 4.2 Data

### 4.2.1 Source

The empirical analysis uses the Serrano database produced by the Swedish House of Finance. Serrano provides firm-level financial statement data converted into calendar-year observations. This is useful for reform analysis because each reform can be linked to a common calendar year. The thesis uses legal-entity accounts rather than consolidated group accounts, since the interest limitation rules apply at the entity level (Dun & Bradstreet, 2023).

### 4.2.2 Pipeline

The analysis is implemented in two steps. First, the Python cleaning file prepares a thesis-ready Serrano panel from the raw extract. It keeps the necessary accounting, industry, group, and employee variables. It also records audit files and data-lineage outputs. Second, the Jupyter notebook builds the reform specific DiD samples and estimates the models. The cleaning file does not define treatment and control groups. It prepares the variables needed to define them in the notebook.

### 4.2.3 Sample construction

The sample construction follows a specific logic. The purpose is to keep firm-year observations comparable over time and to avoid using imputed accounting values in the main analysis. The cleaning file first keeps observations with a valid organisation number and year. The retained period is 2005 to 2023. This broad window is needed because the reform-specific event windows use pre- and post-reform years around 2009, 2013, and 2019.

The sample is restricted to Swedish limited liability companies where the legal-form variable is available. In the Serrano extract this is implemented with SER_JURFORM equal to 49. The

cleaning script also keeps active firms when the activity indicator is available. This is done before the regression sample is built, so inactive or invalid accounting records do not drive the results.

The analysis keeps full 12-month financial-statement observations using BSLOV_MONTHS equal to 12. It also excludes observations flagged as imputed using BSLOV_IMPUT. These filters matter because broken or imputed fiscal-year observations can blur the timing of a reform. The reforms are annual policy events, so the accounting year must map as cleanly as possible to the calendar year (Dun & Bradstreet, 2023).

The sample also requires positive total assets. Firms in finance and insurance are excluded using SNI07 industry codes 64 to 66 where the industry variable is available. These firms have balance sheets and leverage rules that are not comparable to ordinary non-financial firms. Duplicate firm-year observations are removed, keeping the last available record for the firm-year.

After these cleaning filters, firms must have at least four consecutive annual observations. This supports the fixed-effects design and gives enough within-firm variation around reform windows. Treatment and control groups are then defined in the notebook, not in the cleaning file. For each reform, the notebook first takes a four-year window before and after the reform. It then applies the eligible comparison-pool rule in the pre-reform year: firms must be group members and belong to a group with at least 50 observed group-level employees. High-leverage firms are then defined inside this eligible pool as firms at or above the 75th percentile of leverage in the pre-reform year. Treated firms are the high-leverage firms that meet the reform-specific exposure rule. Control firms are high-leverage firms in the same eligible pool that do not meet the exposure rule. This directly addresses the concern that the old control group was too broad and included many very small Swedish firms.

### 4.2.4 Group structure

Group structure is measured using Serrano KNC information. GROUP_ID is based on the observed parent organisation number (KNC_ORGNRK) when it exists, otherwise the firm's own organisation number is used. GROUP_MEMBER is a broad group-membership indicator based on KNC_KONCERN or the presence of an observed parent. GROUP_EMPLOYEES is the sum of observed employees within the same group and year. This variable is used to form a more comparable control pool. The threshold is applied at group level, not firm level, because holding companies and financing entities may have few direct employees while still belonging to larger corporate groups. GROUP_TOTAL_ASSETS, GROUP_SALES, and GROUP_N_FIRMS are also constructed for diagnostics. These variables are based on the firms retained in the cleaned Serrano sample and should not be read as consolidated group accounts.

## 4.3 Variable Construction

The empirical analysis examines financing decisions through changes in debt ratios. It examines investment decisions through changes in capital expenditure or fixed asset growth. Serrano is based on legal-entity financial statements. As a result, the leverage and investment measures are book-value measures. This is consistent with applied work that defines leverage as a ratio of debt (liabilities) to total assets using financial statement data. (Alberternst and Sureth, 2015; Fossen and Simmler, 2012).

### 4.3.1 Balance sheet inputs

Total assets are measured using Serrano's balance sheet total (TILLGSU), documented as the total amount of fixed and current assets and equal to total equity and liabilities by the balance sheet identity (Dun & Bradstreet, 2023). Total non-current liabilities are measured using Serrano's non-current liabilities aggregate (LSKSU), documented as the sum of non-current liabilities to credit institutions, non-current liabilities to group and associated companies, and other non-current liabilities (Dun & Bradstreet, 2023). Total current liabilities are measured using Serrano's current

liabilities aggregate (KSKSU), documented as the sum of accounts payable, current liabilities to credit institutions, current liabilities to group and associated companies, and other current liabilities (Dun & Bradstreet, 2023).

Tangible fixed assets are measured using Serrano's total tangible fixed assets item (MATANLSU), defined as the sum of buildings and land, machinery and equipment, and other tangible fixed assets (Dun & Bradstreet, 2023). This item supports both a tangibility proxy and the investment proxy based on year-to-year changes in tangible fixed assets (Dun & Bradstreet, 2023).

### 4.3.2 Income statements inputs

Operating profit/loss is measured using Serrano's operating profit/loss item (RORRESUL), documented as net sales plus other operating income plus/minus items affecting comparability minus personnel expenses minus depreciation and amortisation minus other operating expenses (Dun & Bradstreet, 2023). Depreciation and amortisation are measured using Serrano's depreciation item (AVSKRIV), recognised in the financial statements as depreciation and amortisation, and transformed into a magnitude variable for construction of investment and EBITDA-related measures (Dun & Bradstreet, 2023).

Interest expense variables are constructed from Serrano's decomposition of financial expenses into interest expenses to group companies (RTEKOKNC) and external interest expenses paid to banks and similar credit institutions (RTEKOEXT) (Dun & Bradstreet, 2023). Interest income variables are constructed from Serrano's decomposition of financial income into interest income from group companies (RTEINKNC) and external interest income (RTEINEXT) (Dun & Bradstreet, 2023).

### 4.3.3 Outcome measures

The primary leverage outcome follows the accounting-based convention that the debt ratio is calculated as the ratio of total liabilities to total book assets and that leverage (LEV) is defined as the ratio between debt and total assets (Alberternst and Sureth, 2015; Fossen and Simmler, 2012). In this thesis, total liabilities are operationalised as total non-current liabilities plus total current liabilities, and total assets are measured using Serrano's balance sheet total (Dun & Bradstreet, 2023).

Leverage is measured as:

$$\mathrm{LEV}_{i,t} = \frac{\mathrm{LSKSU}_{i,t} + \mathrm{KSKSU}_{i,t}}{\mathrm{TILLGSU}_{i,t}},$$

$\mathrm{LSKSU}_{i,t}\mathrm{KSKSU}_{i,t}\mathrm{TILLGSU}_{i,t}it$where is total non-current liabilities, is total current liabilities, and is total assets for firm in year (Dun & Bradstreet, 2023).

To isolate debt margins more directly relevant to interest deductibility, two additional ratios are constructed using Serrano's decomposition of liabilities into liabilities to credit institutions and liabilities to group and associated companies (Dun & Bradstreet, 2023).

Interest-bearing debt to assets is measured as:

$$\mathrm{IBD_RATIO}_{i,t} = \frac{\mathrm{LSKKRIN}_{i,t} + \mathrm{KSKKRIN}_{i,t}}{\mathrm{TILLGSU}_{i,t}},$$

$\mathrm{LSKKRIN}_{i,t}\mathrm{KSKKRIN}_{i,t}$where is non-current liabilities to credit institutions and is current liabilities to credit institutions (Dun & Bradstreet, 2023).

Intra-group debt to assets is measured as:

$$\mathrm{INT_DEBT_RATIO}_{i,t} = \frac{\mathrm{LSKKNC}_{i,t} + \mathrm{KSKKNC}_{i,t}}{\mathrm{TILLGSU}_{i,t}},$$

$\text{LSKKNC}_{i,t}$$\text{KSKKNC}_{i,t}$where is non-current liabilities to group and associated companies and is current liabilities to group and associated companies (Dun & Bradstreet, 2023).

Investment is proxied using changes in tangible fixed assets together with depreciation magnitude to approximate gross investment in tangible capital, consistent with the thesis's definition of investment as changes in capital expenditure or fixed asset growth and with Serrano's definitions of tangible fixed assets and depreciation (Dun & Bradstreet, 2023).

Investment is measured as:

$$\text{INVEST}_{i,t} = \frac{\text{MATANLSU}_{i,t} - \text{MATANLSU}_{i,t-1} + \text{DEPR_MAG}_{i,t}}{\text{TILLGSU}_{i,t-1}},$$

$\text{MATANLSU}_{i,t}t\text{DEPR_MAG}_{i,t}\text{TILLGSU}_{i,t-1}$where is total tangible fixed assets in year, is depreciation magnitude constructed from depreciation and amortisation (AVSKRIV), and the ratio is scaled by lagged total assets (Dun & Bradstreet, 2023).

### 4.3.4 Winsorisation

To limit sensitivity to extreme outliers in accounting ratios, the outcome variables and time-varying control variables are winsorised at the 1st and 99th percentiles before estimation. Exposure assignment is not based on winsorised values. It uses raw pre-reform exposure variables. This preserves the policy-relevant classification while reducing the influence of extreme values in the regressions.

### 4.3.5 High leverage definition

The empirical analysis focuses on highly leveraged firms. The debt tax shield is most relevant for firms that rely heavily on debt. High-leverage status is defined separately for each reform. It is measured in the pre-reform year and only inside the eligible comparison pool. This means that the 75th percentile leverage cutoff is not calculated from all Swedish firms. It is calculated after the group-member and group-level employee restrictions have been applied. As a result, treated and control firms are compared within a more similar population of large group firms. This enhances the credibility of the control group and keeps the analysis aligned with the reform mechanisms (Dreßler and Scheuering, 2012).

## 4.4 Identification Strategy

### 4.4.1 Assumption

The key identifying assumption for the DiD design is parallel trends: absent the reform, more exposed and less exposed firms within the highly leveraged population follow similar trajectories in the outcomes of interest. This assumption is central because the thesis's identifying logic relies on comparing more exposed to less exposed firms around each reform to isolate the relation between deductibility restrictions and outcomes from broader macroeconomic forces influencing all leveraged firms.

### 4.4.2 Event-study diagnostics

Parallel trends are assessed using an event-study extension that estimates event-time-specific differential changes for exposed firms relative to less exposed firms, consistent with applied DiD practice that begins by graphing pre-reform trends in the outcomes of interest (Guceri and Albinowski, 2021). The baseline event-study model is:

$$Y_{i,t} = \alpha_i + \gamma_t + \sum_{k=-4,\, k\neq -1}^{+4} \beta_k \cdot \left(1[t - r = k] \times \text{EXPOSED}_i^{(r)}\right) + \varepsilon_{i,t}.$$

$kr1[t - r = k]kk = -1$where indexes event time relative to reform year, indicates being years from the reform, and is omitted as the reference period, with remaining terms defined as in the baseline model (Guceri and Albinowski, 2021; Zwick and Mahon, 2017). Pre-reform coefficients close to zero are interpreted as support for parallel trends, consistent with applied work emphasising graphical inspection of pretrends as a key diagnostic and the interpretation of smooth pretrends as supportive evidence (Guceri and Albinowski, 2021; Zwick and Mahon, 2017)

A control-rich event study is also estimated as robustness by adding the time-varying controls described in Section 4.1 to the event-study regression, mirroring the structure of the control-rich DiD specification.

### 4.4.3 Threats

Anticipation effects are a key threat because firms can adjust debt structures in advance of reforms, and applied work tests for anticipation by re-estimating the main regression around treatment thresholds and related eligibility rules (Alberternst and Sureth, 2015; Fossen and Simmler, 2012). The empirical analysis therefore treats pre-reform event-study coefficients and robustness checks using alternative time periods as diagnostics for potential anticipation, consistent with the thesis's plan to use robustness checks across different time periods (Guceri and Albinowski, 2021).

Bundled policy changes are another threat because interest limitation rules are introduced alongside statutory corporate tax rate reductions, and the thesis explicitly asks whether concurrent rate cuts are associated with higher investment and whether investment responses differ by exposure to deductibility restrictions. The design mitigates this concern by identifying differential changes from exposure variation within the highly leveraged population rather than relying on aggregate before-after comparisons that are confounded by broad-based policy changes and macroeconomic shocks affecting all firms.

Data comparability is a further threat because Serrano's calendar-year conversion manages broken accounting periods and can involve imputation for missing periods, which can smooth annualised values if imputed observations are retained (Dun & Bradstreet, 2023). This concern is addressed by relying on Serrano's calendar-year framework while excluding observations flagged as imputed, consistent with Serrano's documentation of imputation and its imputation indicator field (Dun & Bradstreet, 2023).

### 4.4.4 Inference

$\alpha_i \gamma_t$Inference is based on firm-clustered standard errors to allow for serial correlation within firms over time, consistent with empirical practice reporting firm-level clustered standard errors (Zwick and Mahon, 2017). All specifications include firm fixed effects and year fixed effects, consistent with applied difference-in-differences practice (Zwick and Mahon, 2017).

## 4.5 Robustness checks

Robustness checks are motivated by the thesis's plan to use different leverage cutoffs, different time periods, and placebo tests as sensitivity analyses. Placebo tests are emphasised because applied DiD work interprets a placebo coefficient that is not significantly different from zero as reassuring evidence that there is no differential time trend between the treatment and control groups (Fossen and Simmler, 2012; Zwick and Mahon, 2017).

Robustness analysis therefore examines sensitivity across alternative leverage cutoffs and alternative event windows, consistent with applied work defining high leverage using a high pre-reform debt-to-assets quantile and focusing on years immediately prior to and after the reform before assessing sensitivity to alternative windows (Dreßler and Scheuering, 2012; Fossen and Simmler, 2012). Because longer windows increase the risk that treated and control firms diverge for reasons unrelated to the reform, the interpretation gives more weight to shorter-window

evidence when the event-study diagnostics suggest problematic pre-trends. Wider-window regressions are retained as additional descriptive evidence rather than as the only basis for inference. Robustness diagnostics also assess sensitivity to accounting period heterogeneity by excluding financial statements with reporting dates that differ from 31 December, consistent with applied work using such exclusions to test robustness to reaction-period heterogeneity and reporting-date heterogeneity (Alberternst and Sureth, 2015).

The robustness checks are organised as follows.

a. Control-rich specification. The baseline fixed-effects-only DiD model is re-estimated with time-varying controls as described in Section 4.1 to assess sensitivity to conditioning on firm characteristics that may be endogenous to the reform package.
b. Alternative HIGH_LEV cutoffs. High-leverage status is varied using alternative pre-reform leverage quantiles to assess whether estimated associations are concentrated among the most highly levered firms.
c. Alternative outcomes. The full set of outcomes (LEV, IBD_RATIO, INT_DEBT_RATIO, INVEST) is reported to distinguish changes in overall liabilities, bank/interest-bearing debt, intra-group debt, and investment activity.(Fossen and Simmler, 2012; Zwick and Mahon, 2017)
d. Excluding real estate for the 2019 reform. The sensitivity of 2019 estimates to sectoral composition is assessed by excluding real estate firms using Serrano's SNI section field, leveraging the same SNI classification infrastructure used for excluding financial and insurance firms (Dun & Bradstreet, 2023; Fossen and Simmler, 2012).
e. Placebo reforms. False reform years are assigned to assess whether treatment and control groups exhibit differential changes when no reform occurs, extending the logic of placebo tests used in applied work
f. Lagged exposure. Exposure is re-defined using earlier pre-reform years to mitigate concerns that exposure classification might reflect short-run pre-reform adjustments rather than longer-run financing structure.
g. Differential pre-trends robustness. Reform-outcome combinations with evidence of differential pre-trends in the event study are examined as a sensitivity check, consistent with the emphasis on assessing parallel trends through pre-reform trajectories (Guceri and Albinowski, 2021; Zwick and Mahon, 2017).
h. Optional active-only robustness. The sensitivity of results to alternative activity restrictions is examined as a robustness check alongside the baseline reliance on Serrano's balance sheet total to identify active firms (Dun & Bradstreet, 2023).
i. Alternative event-study windows. Event windows are varied to assess whether results are sensitive to the chosen pre- and post-reform horizon, consistent with applied work emphasising alternative windows as a robustness diagnostic (Fossen and Simmler, 2012).

The empirical design produces estimates for multiple outcomes capturing financing and real adjustment such as total leverage, interest-bearing debt, intra-group debt, and tangible investment. These were identified through within-high-leverage comparisons of exposed versus less exposed firms around each reform episode and supported by event-study diagnostics and placebo tests. For transparency and replication, the analysis pipeline produces a sample-loss audit, a data-lineage record, a baseline balance table with standardised mean differences and an accompanying love plot, an event-study grid with pre-trend tests, and a final analysis dataset used for estimation (Dun & Bradstreet, 2023; Guceri and Albinowski, 2021).

# 5 Results

This chapter presents and interprets the empirical results obtained using the methodology described in Chapter 4. The analysis uses the cleaned Serrano company-year dataset rather than the raw Bokslut extract to ensure consistency across accounting periods. The 2009, 2013, and 2019 reform episodes are not pooled into a single staggered two-way fixed effects estimator. Instead, each reform is estimated separately so that the treatment definition matches the changing regulatory scope.

The section first evaluates the validity of the parallel-trends assumption. Since the pre-trend diagnostics are problematic and suggest longer-term divergence between treated and control firms, the interpretation gives greater weight to shorter-window and conservative specifications. Wider-window regressions are retained as additional descriptive evidence.

To provide context for the broader economic environment during these reform periods, Figure 5.1 illustrates the macroeconomic trends over the sample window.

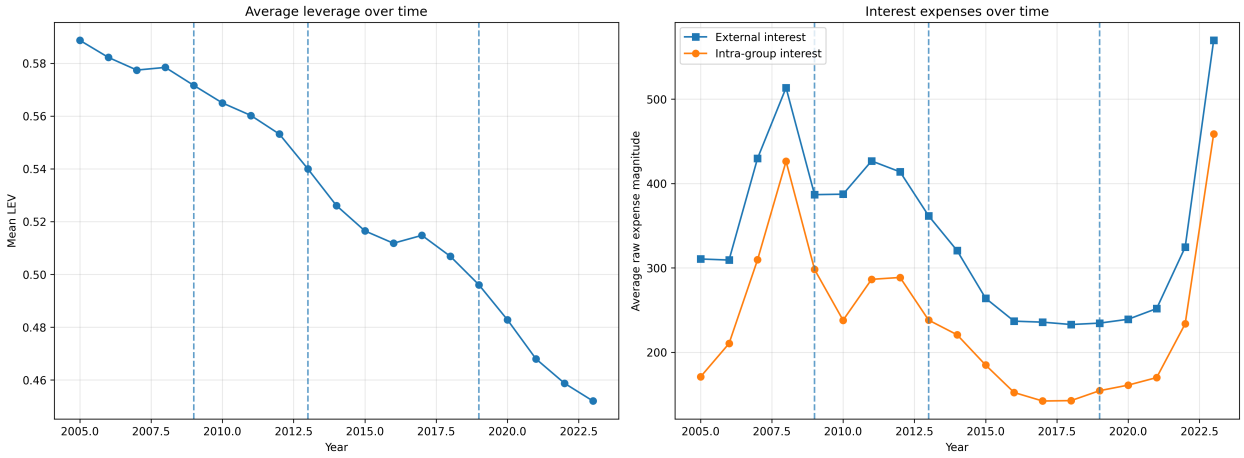


*Figure 5.1 Macroeconomic trends across the analysis period.*

## 5.1 Sample Diagnostics and Baseline Balance

Table 5.1 reports how the sample changes after the main cleaning and comparison-pool restrictions. The cleaned Serrano file contains 5.87 million firm-year observations and 473,115 firms. The consecutive-year filter removes only two observations and one firm. The main reduction happens later, when the analysis restricts each reform window to group firms with at least 50 observed group-level employees and then keeps only the high-leverage firms.

*Table 5.1 Methodology and Sample Diagnostics*

| Step | Reform | Outcome | Rows | Firms |
|---|---|---|---|---|
| Loaded cleaned Serrano CSV | | | 5,870,149 | 473,115 |
| After minimum consecutive-year filter | | | 5,870,147 | 473,114 |
| Eligible comparison pool | 2009 | | 167,818 | 21,173 |
| High-leverage DiD sample | 2009 | | 40,726 | 5,294 |
| Eligible comparison pool | 2013 | | 190,411 | 23,584 |
| High-leverage DiD sample | 2013 | | 46,347 | 5,896 |

| | | | | |
|---|---|---|---|---|
| Eligible comparison pool | 2019 | | 240,416 | 28,906 |
| High-leverage DiD sample | 2019 | | 59,619 | 7,227 |

*Note: The eligible comparison pool is restricted to group firms with at least 50 observed group-level employees before high-leverage and exposure assignment.*

Table 5.2 gives the reform-specific design. In each reform, the pre-reform comparison pool is first restricted. The high-leverage sample is then defined using the top quartile of leverage in the base year. Treatment is assigned only inside that high-leverage sample. This keeps the empirical comparison closer to the firms that were economically relevant for interest limitation rules.

*Table 5.2 Reform-specific sample design and treatment assignment.*

| Reform | Base | Window | Eligible firms | High-lev. firms | Treated | Control | Treated share | Lev. cutoff | Exposure rule |
|---|---|---|---|---|---|---|---|---|---|
| 2009 | 2008 | 2005-2013 | 21,173 | 5,294 | 791 | 4,503 | 14.9% | 0.867 | Internal interest expense |
| 2013 | 2012 | 2009-2017 | 23,584 | 5,896 | 1,047 | 4,849 | 17.8% | 0.873 | Internal interest share |
| 2019 | 2018 | 2015-2023 | 28,906 | 7,227 | 196 | 7,031 | 2.7% | 0.869 | 30% EBITDA and SEK 5m |

*Note: The 2009 and 2013 exposure rules capture internal-interest exposure. The 2019 rule captures the EBITDA-based interest limitation channel.*

Figure 5.2 shows the number of treated and control firms by event time. The control group remains larger than the treated group in all reform windows. This is expected because exposure to the relevant interest limitation channel is narrower than eligibility for the comparison pool itself. The treated share is particularly small in 2019.

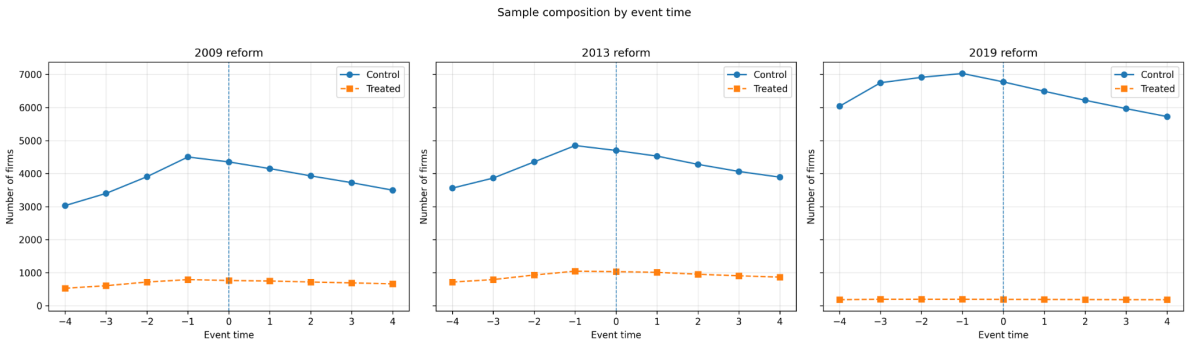


*Figure 5.2 Sample composition by event time.*

Table 5.3 reports descriptive statistics for the main outcomes and controls. Leverage is high in the analysis sample by design, but the full cleaned sample also shows wide variation in leverage, investment, profitability, and group scale. Investment has a higher missing share because it uses lagged tangible assets in its construction.

*Table 5.3 Baseline Covariate Balance (2019 Reform vs. 2018 Control)*

| Variable | N | Mean | SD | Median | 1% | 99% | Missing |
|---|---|---|---|---|---|---|---|
| LEV | 5,870,147 | 0.522 | 0.398 | 0.489 | 0.000 | 2.628 | 0.0% |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| IBD_RATIO | 5,870,147 | 0.100 | 0.209 | 0.000 | 0.000 | 0.901 | 0.0% |
| INT_DEBT_RATIO | 5,870,147 | 0.061 | 0.178 | 0.000 | 0.000 | 0.930 | 0.0% |
| INVEST | 5,397,033 | 0.037 | 0.130 | 0.000 | -0.253 | 0.860 | 8.1% |
| LOG_ASSETS | 5,870,147 | 7.748 | 1.780 | 7.586 | 4.043 | 13.050 | 0.0% |
| ROA | 5,870,147 | 0.063 | 0.252 | 0.045 | -1.129 | 0.760 | 0.0% |
| TANGIBILITY | 5,870,147 | 0.182 | 0.284 | 0.024 | 0.000 | 0.984 | 0.0% |
| SALES_GROWTH | 4,630,867 | -0.017 | 0.591 | 0.018 | -2.548 | 2.072 | 21.1% |
| GROUP_EMPLOYEES | 5,870,147 | 90.401 | 853.897 | 2.000 | 0.000 | 1,824 | 0.0% |
| GROUP_TOTAL_ASSETS | 5,870,147 | 1,301,690 | 13,151,633 | 2,545 | 59.000 | 35,897,731 | 0.0% |

*Note: Variables are based on the cleaned Serrano firm-year panel. Investment uses lagged assets and therefore has fewer usable observations.*

The baseline balance plot and Table 5.4 confirm that the new comparison pool improves the design, but it does not make treated and control firms identical. Some variables are close to zero standardized difference. Others remain imbalanced. Size and group scale are the clearest examples. This is not surprising because firms exposed to interest limitation rules are usually larger and more group-intensive.

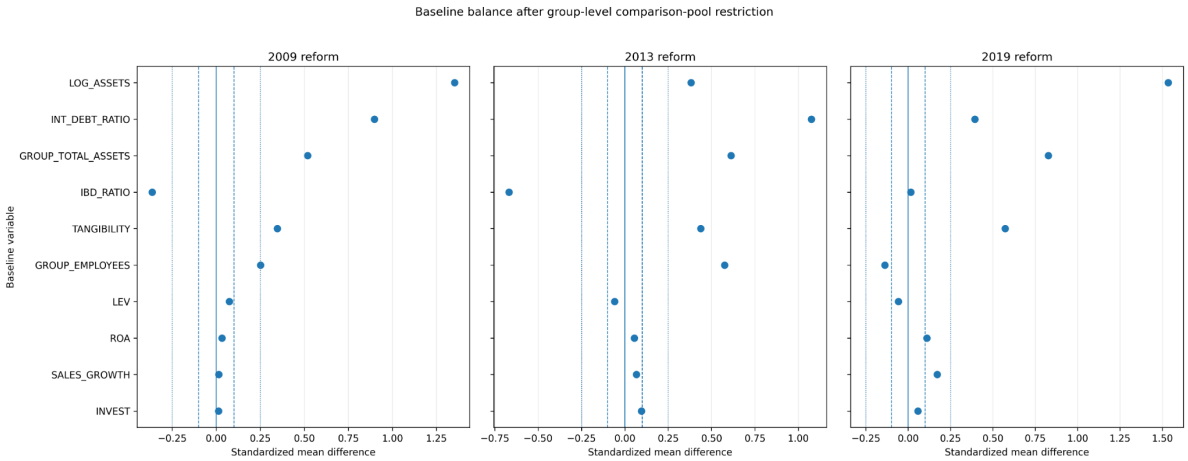


*Figure 5.3 Love plot demonstrating baseline covariate balance.*

*Table 5.4 Baseline balance by reform window: standardized mean differences.*

| **Outcome** | **2009** | **2013** | **2019** |
|---|---|---|---|
| LEV | 0.0089 | -0.0174** | 0.0075 |
| | (0.0098) | (0.0078) | (0.0139) |
| IBD_RATIO | -0.0094 | 0.0481*** | 0.0153 |
| | (0.0082) | (0.0066) | (0.0178) |
| INT_DEBT_RATIO | -0.0178 | -0.0754*** | -0.0193 |
| | (0.0112) | (0.0100) | (0.0218) |
| INVEST | -0.0116* | -0.0098* | -0.0378*** |

| | | | |
|---|---|---|---|
| | (0.0070) | (0.0057) | (0.0131) |
| Observations | 35,432-40,726 | 44,326-46,347 | 58,027-59,619 |
| Firms | 5,293-5,294 | 5,896 | 7,227 |
| Treated firms | 791 | 1,047 | 196 |
| Control firms | 4,502-4,503 | 4,849 | 7,031 |

*Note: Values are standardized mean differences between treated and control firms in the base year. Larger absolute values indicate weaker baseline balance.*

## 5.2 Event Study Diagnostics and Parallel Trends

The parallel-trends assumption is the key diagnostic condition for the DiD design. The raw and rebased trend plots show that treated and control firms often move in the same broad direction, but not always with the same slope. This is especially visible in the debt-composition variables.

For total leverage, treated and control firms show a common rise before the 2009 and 2013 reforms and a decline afterwards. For the 2019 reform, both groups peak before the reform, but the control group deleverages more strongly after 2019. This explains why the 2019 leverage coefficient is not negative.

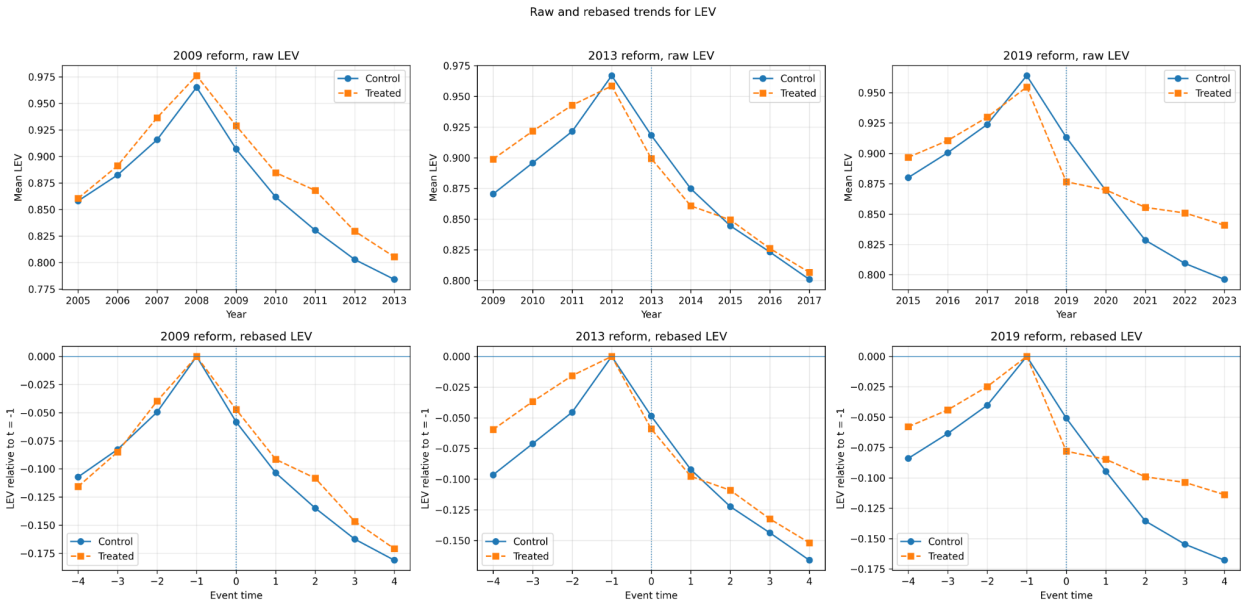


*Figure 5.4 Raw and rebased trends for total leverage*

The intra-group debt trends are closer to the policy mechanism. Treated firms have higher intra-group debt ratios before each reform. After the reforms, the treated group shows stronger reductions, especially around 2013 and 2019. This is consistent with a reduced tax advantage from internal borrowing.

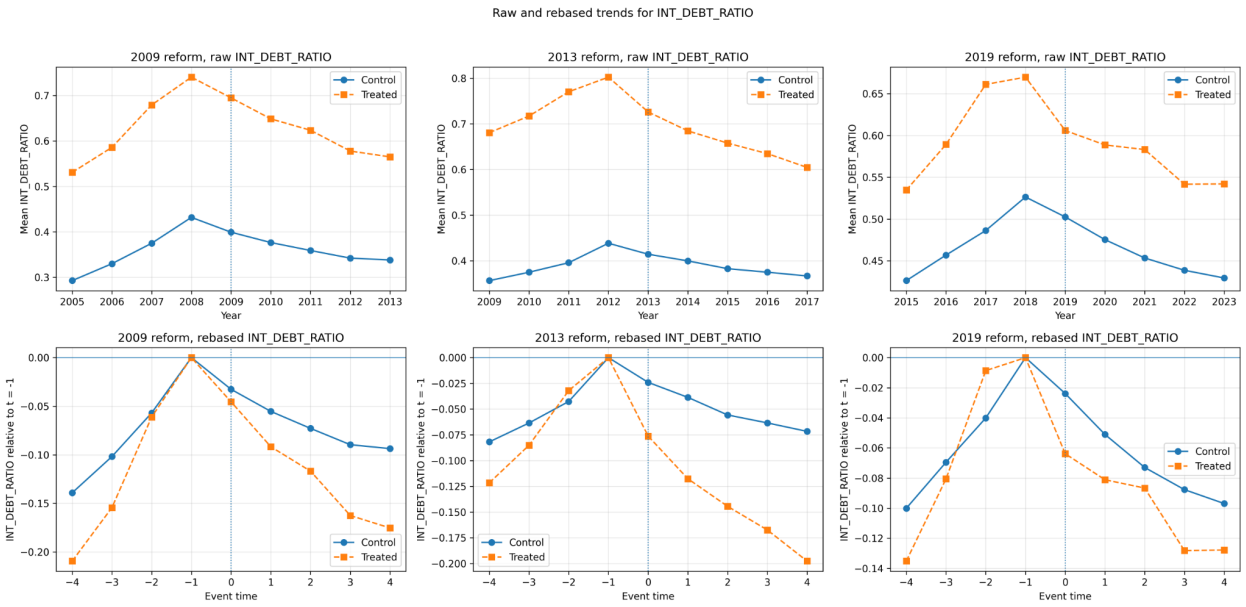


*Figure 5.5 Raw and rebased trends for intra-group debt ratio*

The interest-bearing debt ratio follows a different adjustment pattern. In 2013, treated firms increase their bank-debt ratio while control firms decline. This suggests a possible shift from internal borrowing toward external credit. The interpretation is descriptive, but it supports the debt-composition mechanism.

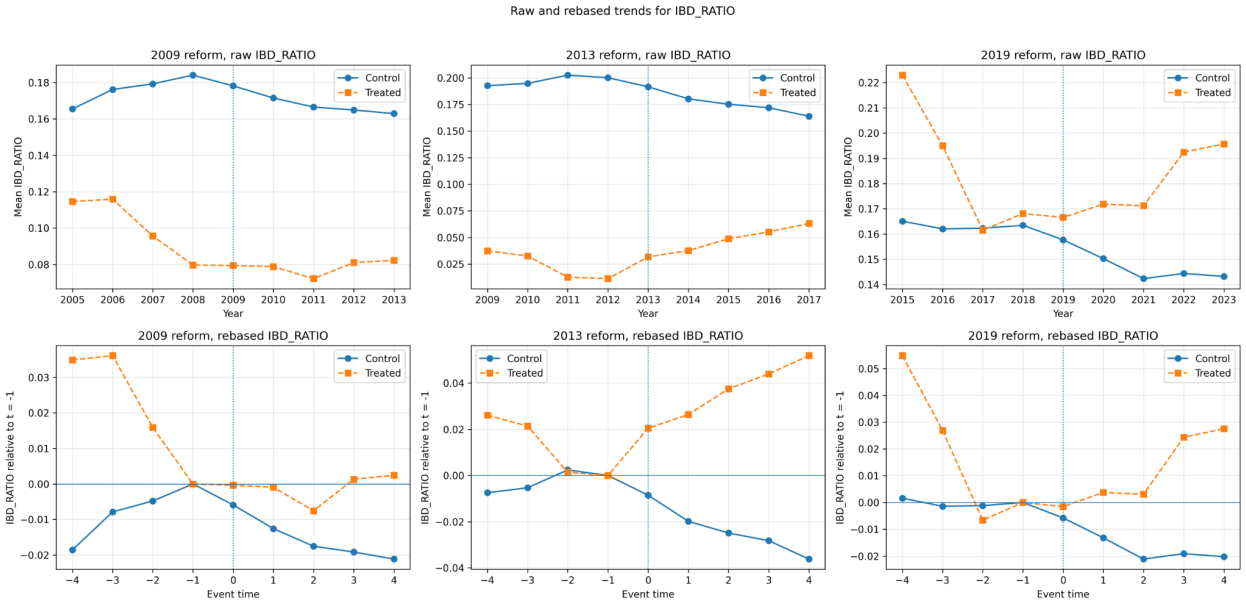


*Figure 5.6 Raw and rebased trends for interest-bearing debt ratio*

Investment falls around the reform years for both groups. The decline is strongest in the 2019 window, but this evidence must be interpreted carefully because the post-reform period overlaps with Covid-19. The treated group starts from a higher investment level before the reform and then converges downward after the EBITDA-based limitation.

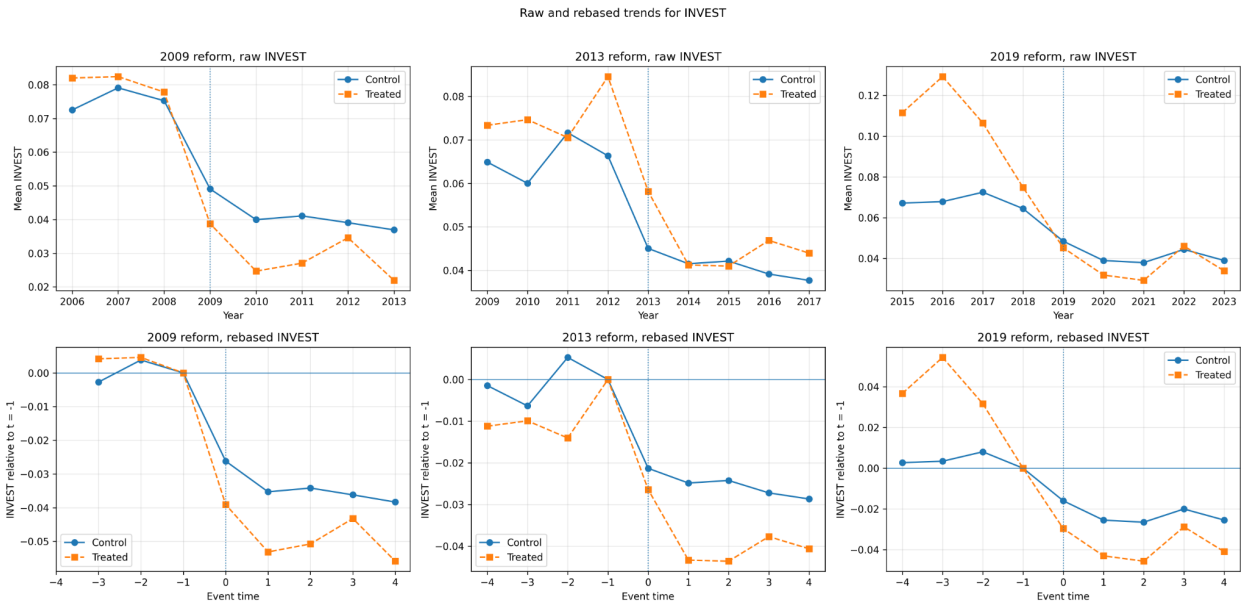


*Figure 5.7 Raw and rebased trends for tangible investment*

The formal event-study tests in Table 5.5 reject the null of no differential pre-trends in all reform-outcome combinations. The p-values are below 0.001. This is an important limitation, but it also motivates the empirical presentation. The longer the window, the more likely it is that exposed and less exposed firms diverge for reasons not fully attributable to the reform. The main interpretation therefore focuses on shorter-term and conservative evidence, while wider-window estimates are treated as additional descriptive evidence rather than unrestricted causal estimates.

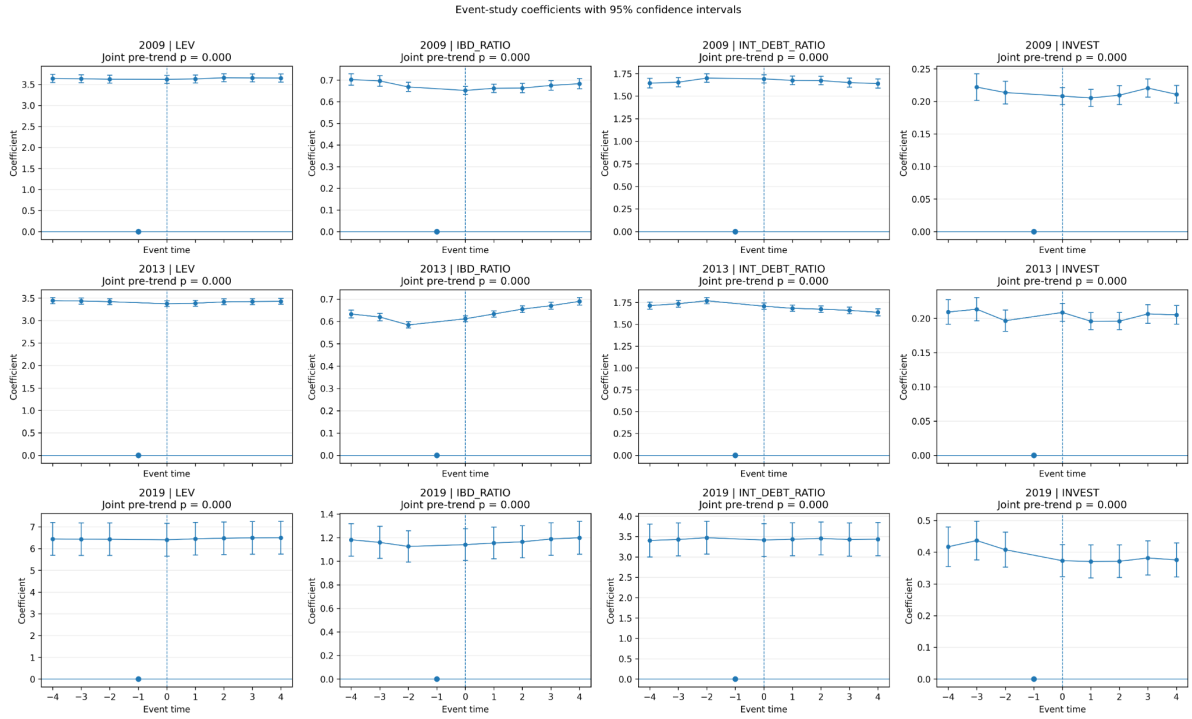


*Figure 5.8 Event-study coefficients with 95 percent confidence intervals*

*Table 5.5 Joint pre-trend tests from the baseline event-study specification*

| Reform | Outcome | Chi-square | DF | p-value | N | Firms | Conclusion |
|---|---|---|---|---|---|---|---|
| 2009 | IBD ratio | 4,219 | 3 | <0.001 | 40,726 | 5,294 | Rejected |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 2009 | Intra-group debt | 5,037 | 3 | <0.001 | 40,726 | 5,294 | Rejected |
| 2009 | Investment | 779.6 | 2 | <0.001 | 35,432 | 5,293 | Rejected |
| 2009 | Leverage | 5,834 | 3 | <0.001 | 40,726 | 5,294 | Rejected |
| 2013 | IBD ratio | 7,786 | 3 | <0.001 | 46,347 | 5,896 | Rejected |
| 2013 | Intra-group debt | 9,334 | 3 | <0.001 | 46,347 | 5,896 | Rejected |
| 2013 | Investment | 1,262 | 3 | <0.001 | 44,326 | 5,896 | Rejected |
| 2013 | Leverage | 10,838 | 3 | <0.001 | 46,347 | 5,896 | Rejected |
| 2019 | IBD ratio | 283.1 | 3 | <0.001 | 59,619 | 7,227 | Rejected |
| 2019 | Intra-group debt | 294.7 | 3 | <0.001 | 59,619 | 7,227 | Rejected |
| 2019 | Investment | 236.4 | 3 | <0.001 | 58,027 | 7,227 | Rejected |
| 2019 | Leverage | 284.8 | 3 | <0.001 | 59,619 | 7,227 | Rejected |

Note: The omitted event time is t = -1. All tests reject parallel pre-trends at conventional levels, so the following estimates are interpreted as structured conditional associations rather than definitive causal effects.

## 5.3 Preferred Main Results and Additional Descriptive Evidence

Table 5.6 reports the FE-only estimates with firm fixed effects and year fixed effects. The coefficient is the interaction between the post-reform period and the exposure indicator. Given the event-study evidence, these wider-window estimates are used as additional descriptive evidence. The substantive interpretation focuses on the most stable patterns: the strong 2013 debt-composition response and the more cautious short-term evidence for 2019 investment.

*Table 5.6 Additional descriptive evidence from the FE-only specification*

| **Outcome** | **2009** | **2013** | **2019** |
|---|---|---|---|
| LEV | 0.0089 | -0.0174** | 0.0075 |
| | (0.0098) | (0.0078) | (0.0139) |
| IBD_RATIO | -0.0094 | 0.0481*** | 0.0153 |
| | (0.0082) | (0.0066) | (0.0178) |
| INT_DEBT_RATIO | -0.0178 | -0.0754*** | -0.0193 |
| | (0.0112) | (0.0100) | (0.0218) |
| INVEST | -0.0116* | -0.0098* | -0.0378*** |
| | (0.0070) | (0.0057) | (0.0131) |
| Observations | 35,432-40,726 | 44,326-46,347 | 58,027-59,619 |
| Firms | 5,293-5,294 | 5,896 | 7,227 |

| | | | |
|---|---|---|---|
| Treated firms | 791 | 1,047 | 196 |
| Control firms | 4,502-4,503 | 4,849 | 7,031 |

*Note: Standard errors are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. Firm and year fixed effects are included. Observation and firm counts are shown as ranges where the usable sample differs across outcomes.*

For 2009, the estimates are small and mostly statistically imprecise. Investment is negative at the 10 percent level, but the debt-ratio estimates are not statistically distinct from zero. This suggests that the first reform did not generate a clear average financing response in the restricted sample.

For 2013, the results are stronger. Intra-group debt falls by 7.54 percentage points relative to the control group. Interest-bearing debt rises by 4.81 percentage points. Leverage falls by 1.74 percentage points. This supports a debt-composition interpretation. Firms appear to reduce internal borrowing while increasing external credit.

For 2019, investment falls by 3.78 percentage points in the wider-window FE-only specification and is statistically significant. This should be stated cautiously. The leverage, bank-debt, and intra-group debt estimates are not statistically precise, and the early post-reform years include the Covid-19 period. The result is therefore best interpreted as suggestive evidence of lower investment among exposed high-leverage firms, not as a definitive economy-wide effect of the 2019 reform.

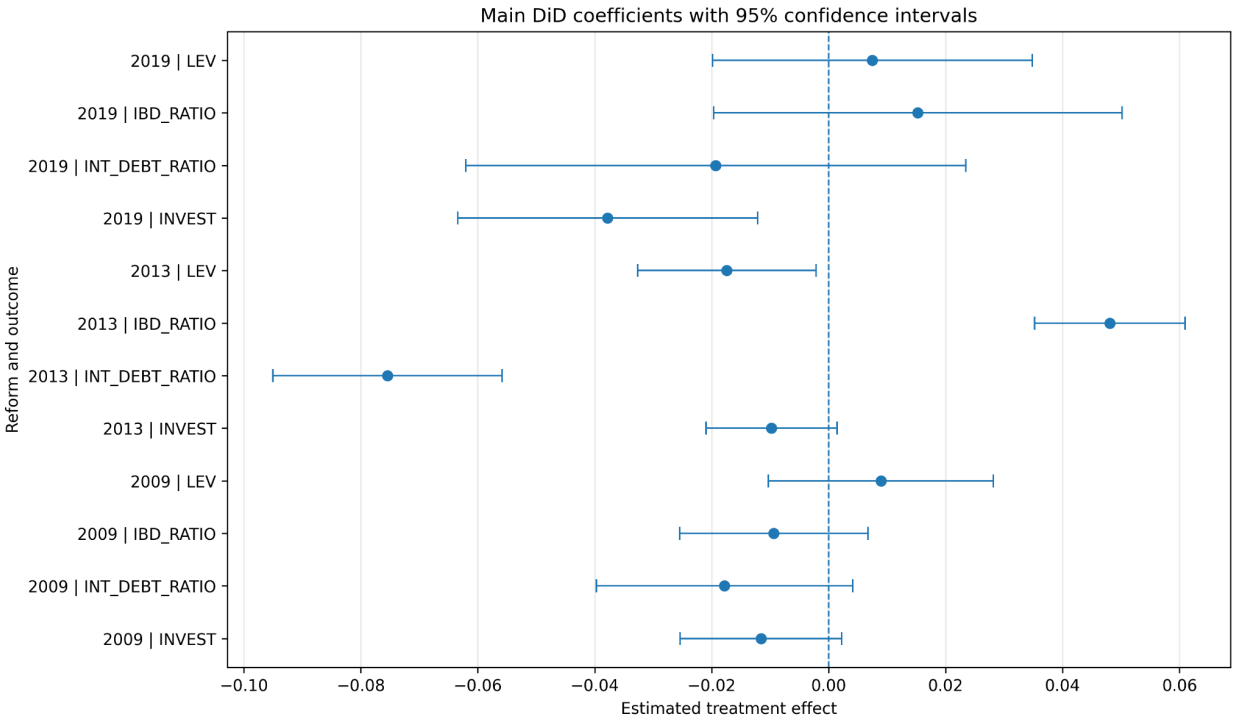


*Figure 5.9 Main DiD coefficients with 95 percent confidence intervals*

## 5.4 Additional Descriptive Evidence and Robustness Checks

Table 5.7 adds time-varying controls to the main specification. The controls are firm size, profitability, tangibility, sales growth, and group membership. These controls reduce the usable sample because some variables have missing observations. The results therefore test sensitivity rather than replacing the main specification.

*Table 5.6 DiD results with time-varying controls*

| Reform | Outcome | Treated | Control | N | Beta (SE) | p-value |
|---|---|---|---|---|---|---|
| 2009 | Leverage | 695 | 4,105 | 30,078 | 0.0057 (0.0080) | 0.474 |
| 2009 | IBD ratio | 695 | 4,105 | 30,078 | 0.0036 (0.0095) | 0.707 |
| 2009 | Intra-group debt | 695 | 4,105 | 30,078 | -0.0264** (0.0122) | 0.030 |
| 2009 | Investment | 695 | 4,105 | 30,078 | -0.0025 (0.0066) | 0.703 |
| 2013 | Leverage | 908 | 4,345 | 36,807 | -0.0103 (0.0068) | 0.129 |
| 2013 | IBD ratio | 908 | 4,345 | 36,807 | 0.0542*** (0.0080) | <0.001 |
| 2013 | Intra-group debt | 908 | 4,345 | 36,807 | -0.0858*** (0.0108) | <0.001 |
| 2013 | Investment | 908 | 4,345 | 36,807 | -0.0011 (0.0058) | 0.849 |
| 2019 | Leverage | 191 | 6,106 | 47,183 | 0.0014 (0.0121) | 0.911 |
| 2019 | IBD ratio | 191 | 6,106 | 47,183 | 0.0178 (0.0180) | 0.324 |
| 2019 | Intra-group debt | 191 | 6,106 | 47,183 | -0.0164 (0.0213) | 0.440 |
| 2019 | Investment | 191 | 6,106 | 47,183 | -0.0278** (0.0110) | 0.012 |

*Note: Controls are LOG_ASSETS, ROA, TANGIBILITY, SALES_GROWTH, and GROUP_MEMBER. Firm and year fixed effects are included.*

The control-rich model preserves the main 2013 debt-composition result. The positive estimate for bank debt and the negative estimate for intra-group debt both remain statistically significant. The 2019 investment result also remains negative and statistically significant. The winsorisation audit supports that these estimates are not driven by the most extreme accounting ratios, although the pre-trend limitation remains.

Table 5.8 reports selected additional evidence and robustness checks for the most relevant findings. The shorter two-year window for 2019 investment remains negative, but the estimate is smaller and only marginally significant. This supports a more cautious interpretation of the 2019 result. The 2013 debt-composition result remains the strongest empirical pattern because the positive estimate for bank debt and the negative estimate for intra-group debt remain statistically significant after adding controls.

*Table 5.8 Additional descriptive evidence and robustness checks for key findings*

| Check | Reform | Outcome | Estimate (SE) | p-value | N | Firms |
|---|---|---|---|---|---|---|
| Controls | 2009 | Intra-group debt | -0.0264** (0.0122) | 0.030 | 30,078 | 4,800 |
| Controls | 2013 | IBD ratio | 0.0542*** (0.008) | <0.001 | 36,807 | 5,253 |
| Controls | 2013 | Intra-group debt | -0.0858*** (0.0108) | <0.001 | 36,807 | 5,253 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Alt. high-leverage cutoff (q=0.50) | 2019 | Investment | -0.0245*** (0.0082) | 0.003 | 118,670 | 14,453 |
| Alt. high-leverage cutoff (q=0.75) | 2019 | Investment | -0.0378*** (0.0131) | 0.004 | 58,027 | 7,227 |
| Alt. high-leverage cutoff (q=0.90) | 2019 | Investment | -0.0768*** (0.0267) | 0.004 | 22,443 | 2,891 |
| Exclude LEV > 2 | 2019 | Investment | -0.0378*** (0.0131) | 0.004 | 57,664 | 7,196 |
| Exclude positive building/land | 2019 | Investment | -0.0064 (0.0152) | 0.673 | 31,260 | 4,319 |
| ±2 window | 2019 | Investment | -0.0256* (0.014) | 0.068 | 34,199 | 7,227 |
| Controls | 2019 | Investment | -0.0278** (0.011) | 0.012 | 47,183 | 6,297 |

Note: The table focuses on the results most relevant to the thesis interpretation: the 2013 debt-composition pattern and the more cautious 2019 investment pattern.

## 5.5 Summary of Findings

The updated results change the thesis interpretation in a specific way. The new comparison-pool restriction improves the design because it removes very small firms before treatment and control assignment. However, it does not eliminate all imbalance. Treated firms remain larger and more group-intensive in several reform windows. In addition, the pre-trend tests indicate longer-term divergence, so the results are framed as conditional associations from a structured panel design.

The clearest financing result appears in 2013. Treated firms reduce intra-group debt and increase interest-bearing bank debt relative to control firms. This supports the idea that the reform changed debt composition rather than only total borrowing.

The 2019 investment evidence is negative but should be presented more cautiously than the 2013 financing result. The wider-window and control-rich specifications point to lower tangible investment among exposed firms, while the shorter-window estimate is smaller and only marginally significant. In addition, Covid-19 in 2020 overlaps with the early post-reform period, which may affect interpretation.

The main limitation is the rejection of parallel pre-trends. This qualification is carried into the discussion and conclusion. The results are therefore best presented as credible descriptive and conditional evidence from a structured panel design, rather than as definitive causal estimates.

# 6 Discussion

## 6.1 Swedish corporate tax reforms and financial behaviour

The results show that Swedish corporate tax reforms were associated with selective financial adjustment among the exposed high-leverage treatment population, rather than a uniform response by all Swedish firms. This is important because the theoretical expectation from the debt tax shield is straightforward: when interest deductibility becomes less generous, debt should become less attractive, and firms should reduce leverage. In practice, the Swedish evidence is more nuanced. Firms did not simply deleverage across all reform episodes. Instead, the clearest response was a change in the type of debt firms used.

The strongest financing result appears after the 2013 reform. Exposed firms reduced their intra-group debt ratio by 7.54 percentage points relative to the control group, while their interest-bearing bank debt ratio increased by 4.81 percentage points. This pattern suggests substitution from internal group loans toward external credit. The reform, therefore, appears to have changed debt composition more clearly than total debt levels. This is consistent with the design of the 2013 reform, which broadened the restrictions on intra-group interest deductions but did not yet introduce a general limitation on all interest expenses.

This interpretation fits well with the literature on internal debt and profit shifting. Wamser (2014) argues that thin-capitalisation rules can reduce internal debt but may leave firms able to use external borrowing. Blouin et al. (2014) similarly show that restrictions on related-party debt are most visible in internal leverage, while their effect on total leverage depends on the scope of the rule. De Mooij and Hebous (2018) make the same distinction more broadly: rules aimed only at related-party borrowing do not necessarily eliminate the overall debt bias, while rules covering all debt are more likely to reduce total leverage. The Swedish 2013 result follows this logic. The reform restricted the internal-debt channel, and firms responded by reducing internal debt.

The results also speak directly to the Swedish institutional context described by Thomann (2014). Swedish corporate taxation created incentives for firms to use debt-intensive structures, and these incentives were especially relevant for groups able to organize financing internally. The 2013 reform appears to have weakened this internal financing advantage. Yet the rise in bank debt indicates that firms did not necessarily reduce their need for debt finance. They changed the financing channel. This means the reform was effective in reducing a tax-sensitive internal debt margin, but less effective in producing broad deleveraging.

The weaker results for 2009 support the same interpretation. The 2009 reform was narrower and targeted intra-group loans connected to share acquisitions. In the baseline results, the estimated effects on total leverage, interest-bearing debt, and intra-group debt are small and mostly statistically imprecise. This does not mean that the reform was irrelevant. Rather, it suggests that a narrow anti-avoidance rule should not be expected to reshape the balance sheets of the full high-leverage group. It targeted a specific structure, not all internal financing.

The 2019 reform also does not show a clear short-run reduction in leverage. The leverage, bank-debt, and intra-group debt coefficients are not statistically precise. This is surprising only if one expects broad interest limitation rules to produce immediate deleveraging. A more realistic interpretation is that leverage adjusts slowly. Existing debt contracts, refinancing costs, group-level financing policies, and bank relationships may prevent firms from reducing debt immediately. Firms may instead adjust through new investment, retained earnings, or refinancing margins. This is why the 2019 result appears more clearly in investment than in total leverage, although the Covid-19 shock in 2020 makes the early post-reform investment evidence harder to interpret cleanly.

This interpretation is consistent with the broader capital-structure literature, including Sanati and Beyhaghi (2025). Their evidence shows that removing the tax benefits of debt can lead firms to reduce leverage and real activity. The Swedish evidence supports the same underlying mechanism, but it shows a more gradual and composition-based adjustment. Swedish firms did not immediately deleverage in a uniform way. Instead, they first adjusted the most exposed financing margins. This adds nuance to the Sanati and Beyhaghi perspective: the removal of debt tax benefits matters, but the observed response depends on institutional design and available substitution channels.

Overall, the results suggest that Swedish firms responded to tax reforms in a targeted way. Narrow rules changed narrow financing margins. Broader rules created pressure on debt-financed activity, but did not generate immediate broad deleveraging. The main financial behaviour observed in this thesis is therefore not a simple fall in debt. It is a restructuring of debt toward less restricted channels.

## 6.2 Effect of specific reform episodes

The three reform episodes had different effects because they targeted different parts of corporate financing. This is why the thesis estimates each reform separately instead of pooling them into a single staggered design. The 2009, 2013, and 2019 reforms were not equivalent treatments. They differed in scope, mechanism, and likely firm exposure. A reform-specific Difference-in-Differences design is therefore more appropriate because it allows the treatment definition to match the institutional content of each reform.

The 2009 reform was the narrowest policy episode. It targeted interest deductions on internal loans connected to intra-group share acquisitions, especially the *räntesnurror* structures that were central to Swedish anti-avoidance debates. These structures allowed interest deductions in Sweden while the corresponding income could be taxed lightly elsewhere. This is the type of mechanism discussed by Vleggeert (2015), who emphasizes the role of interest deductions in cross-border base erosion. However, because the 2009 rule targeted a specific type of internal loan, many high-leverage firms may not have been directly constrained. The weak 2009 estimates are therefore consistent with the narrow legal design of the reform.

The 2013 reform produced the clearest financial response because it broadened the restrictions to intra-group loans more generally. This increased the number of internal financing structures exposed to limitation. The empirical result is exactly what this design predicts: intra-group debt falls significantly, while bank debt rises. The 2013 reform therefore appears to have reduced the tax advantage of internal debt without fully reducing firms' demand for debt financing. Firms substituted. They moved away from the restricted channel and toward a less restricted channel.

This finding also aligns with the logic of the BEPS literature. Tell (2017) argues that the post-BEPS tax environment moved from narrow anti-avoidance rules toward more systematic limits on interest deductions. The 2013 Swedish reform sits between these two regimes. It was broader than the 2009 rule, but it was still focused on internal debt. It therefore reduced one profit-shifting channel but did not fully neutralise the debt tax shield. This explains why the reform changed debt composition rather than producing a large fall in total leverage.

The 2019 reform represented a different type of policy. It introduced a general EBITDA-based limitation and aligned Sweden with the EU Anti-Tax Avoidance Directive and OECD BEPS Action 4. Under this rule, exposure depends on net interest expenses relative to EBITDA and on the SEK 5 million threshold. The rule therefore affects the cost of debt more broadly, including external borrowing. This is why the 2019 reform is conceptually closer to the setting studied by Sanati and Beyhaghi (2025), where removing the tax benefits of debt affects both financing and real outcomes.

The Swedish 2019 evidence is partly consistent with Sanati and Beyhaghi, but not identical. Sanati and Beyhaghi find that restricting the tax benefits of debt leads to deleveraging and investment contraction. In this thesis, the clearest 2019 pattern is investment contraction, not statistically precise deleveraging. Exposed firms reduced investment in the wider-window and control-rich specifications, while the shorter-window estimate remains negative but weaker. This suggests that Swedish firms may have responded to the broader limitation by reducing tangible investment rather than immediately reducing book leverage. However, the 2019 interpretation is deliberately cautious because the early post-reform years overlap with Covid-19 and because the event-study diagnostics point to pre-existing divergence. The difference may also reflect Sweden's institutional details, including the simplification threshold and carry-forward rules, which can soften the immediate balance-sheet impact of the reform.

Stejskalová et al. (2020) provide a useful interpretation of this result. They argue that ATAD-style interest limitation rules affect investment decisions because deductibility changes the net present value of debt-financed projects. The Swedish 2019 investment result is consistent with this mechanism. If interest deductions become less valuable, debt-financed investment becomes less attractive. Firms may therefore postpone or reduce tangible investment even if they do not immediately reduce debt stocks.

The comparison across reforms therefore shows a clear policy pattern. The 2009 reform was too narrow to produce broad adjustment. The 2013 reform changed internal financing behaviour and provides the strongest result in the thesis. The 2019 reform may have created pressure on investment because it affected debt-financed activity more generally, but the evidence is more sensitive to window choice, sectoral composition, and the Covid-19 period. The effect of each reform follows its legal scope.

## 6.3 Critical review

The main strength of the empirical design is that it matches the institutional structure of the reforms. Treating each reform separately is not only a methodological choice. It is necessary for interpretation. The reforms had different treatment mechanisms. A single pooled model would risk treating different legal and economic shocks as if they were the same. The reform-specific design therefore improves the credibility of the estimates.

The use of firm fixed effects also strengthens the analysis. Treated firms are not randomly selected. They are larger, more group-intensive, and more exposed to tax-sensitive financing structures. Firm fixed effects absorb stable differences between firms, such as ownership structure, business model, persistent financing preferences, and sector-specific balance-sheet characteristics. This means the estimates rely on within-firm changes over time rather than simple differences between treated and control firms.

Year fixed effects are equally important. The reform windows include macroeconomic shocks, changes in credit conditions, and statutory corporate tax rate reductions. Year fixed effects absorb shocks common to all firms in a given year. This is especially important because the Swedish reforms combined interest limitation rules with reductions in the corporate tax rate. The model therefore compares whether exposed firms changed differently from less exposed firms in the same period, rather than simply comparing the economy before and after reform.

The revised sample restrictions further improve the analysis. The comparison pool is restricted to group-member firms with at least 50 observed group-level employees before treatment assignment. This is important because the relevant reforms, especially in 2009 and 2013, primarily concerned group financing. Including very small standalone firms would create an inappropriate control group. The group-level employee restriction is also more defensible than a firm-level employee restriction because holding companies and financing entities may have few direct

employees while belonging to large groups. The design therefore compares firms within a more relevant economic population.

Treatment assignment based on pre-reform exposure is another important strength. It avoids classifying firms using post-reform behaviour, which could already reflect adjustment to the reform. The use of net internal-interest exposure for 2009 and 2013 is also an improvement. Gross internal-interest expense may classify firms as exposed even when internal interest income offsets the economic burden. Net exposure is more conservative and better aligned with the reform mechanism. This makes the treatment group smaller but more meaningful.

However, the analysis also has important limitations. The event-study diagnostics reject parallel pre-trends across the reform-outcome combinations. This means the results should not be interpreted as fully causal estimates. Treated and control firms were already moving differently before the reforms. This is not surprising because the treated firms are larger, more group-intensive, and more financially complex. These characteristics may generate different trends even in the absence of reform. This limitation motivates the emphasis on shorter-window evidence and the classification of wider-window regressions as additional descriptive evidence.

The correct interpretation is therefore careful. The findings provide structured and policy-relevant evidence of differential adjustment around Swedish tax reforms. They are stronger than descriptive before-and-after comparisons because they use fixed effects, reform-specific exposure, and a restricted comparison pool. But they are not perfect causal estimates because the parallel-trends assumption is not fully supported. This limitation should be stated clearly.

Another limitation is that the reforms were bundled with corporate tax rate reductions. Lower corporate tax rates reduce the value of interest deductions, but they may also increase the after-tax investment return. These two channels move in different directions. The DiD design mitigates this concern by comparing more exposed and less exposed firms within the same reform window. Still, it cannot fully separate every component of the reform package. The estimates should therefore be interpreted as differential responses to the reform environment, with interest limitation exposure as the main source of heterogeneity.

Finally, the 2019 investment result appears sensitive to sectoral composition. When firms with positive building and land assets are excluded, the investment effect weakens. This suggests that capital-intensive and real estate-related firms may drive part of the result. This is not a flaw, but it is important for interpretation. EBITDA-based interest limitation rules are likely to matter most for firms with high tangible assets and debt-financed investment. The result may therefore be concentrated in sectors where debt is part of the normal investment model.

In sum, the empirical strategy is credible, but the interpretation must remain cautious. The results are best understood as evidence of structured adjustment around reform episodes, not as definitive proof of causal effects.

## 6.4 Implications for academia and industry

The findings contribute to the academic literature by showing that the effect of interest limitation rules depends strongly on policy design. The Swedish sequence provides a useful setting because it moves from narrow internal-debt restrictions to a broad EBITDA-based rule. This allows the thesis to show that firms do not respond to all interest limitation rules in the same way. The 2013 reform changed debt composition. The 2019 reform was more clearly associated with investment reductions. This distinction is central for corporate finance and tax research.

For the literature on debt bias, the results show that restricting interest deductibility does not automatically produce immediate total deleveraging. This qualifies the standard tax-shield prediction. Firms may first substitute between financing channels. Internal debt can fall while external debt rises. This supports Wamser (2014), Blouin et al. (2014), and De Mooij and Hebous

(2018), who all emphasize that related-party debt rules affect the structure of borrowing more clearly than total debt. The Swedish evidence therefore adds a firm-level contribution from a Nordic tax setting.

For the BEPS and ATAD literature, the results show the importance of moving from targeted rules to broader rules. Vleggeert (2015) and Tell (2017) emphasize that interest deductions are central to international profit shifting and that post-BEPS rules aim to limit excessive deductions more systematically. The Swedish results support this regulatory logic. The 2013 reform reduced internal-debt exposure, while the 2019 reform affected real investment behaviour. This suggests that broader rules may be more powerful, but also more likely to affect real business decisions.

The results also speak to Sanati and Beyhaghi's argument that removing the tax benefits of debt can affect both leverage and investment. The Swedish evidence confirms the investment channel but shows weaker immediate deleveraging. This difference is important. It suggests that firms may respond to interest limitation rules through investment before they respond through balance-sheet restructuring. It also suggests that institutional features, such as thresholds and carry-forward rules, can soften the leverage response while still influencing investment decisions.

For policymakers, the findings show a clear trade-off. Narrow related-party rules can reduce tax-motivated internal borrowing, but they may encourage substitution into external debt. Broad EBITDA-based rules can reduce the value of debt tax shields more generally, but they may also reduce investment among highly leveraged firms. This means policy design should not focus only on protecting the tax base. It should also monitor financing substitution and real investment effects.

For industry, the results highlight the importance of financing structure. Firms with high internal debt, high net interest expenses, or weak EBITDA coverage are more exposed to interest limitation rules. Corporate managers should therefore treat tax deductibility as a strategic financing constraint, not only as a tax-compliance issue. Debt composition, group financing arrangements, and investment planning are all affected.

The broader implication is that tax policy changes the margins on which firms compete and invest. The Swedish reforms did not eliminate debt financing. They changed its relative attractiveness. They made some financing channels less useful and some investment projects less attractive. This is exactly why interest limitation rules matter for corporate finance.

Overall, this thesis contributes by showing that Swedish corporate tax reforms produced differentiated firm responses within the exposed high-leverage population. The evidence is strongest for debt-composition changes after the 2013 reform. The 2019 investment pattern remains relevant but should be read more cautiously because of pre-trend limitations, sectoral sensitivity, and the Covid-19 period. The findings should therefore be read as structured conditional evidence, not as unrestricted causal proof.

# 7 Conclusion

This thesis examined how Swedish corporate tax reforms affected the debt ratios and investment behaviour of highly leveraged firms. The estimates apply to treated exposed/high-leverage firms, not to the universe of Swedish firms. The analysis focused on the 2009, 2013, and 2019 reform episodes. Each reform was studied separately because the rules differed in scope and targeted different forms of interest deductibility.

The results show that the reforms were not associated with one uniform corporate response. Instead, the evidence differs across reform episodes and outcomes. The clearest financing result appears after the 2013 reform. Firms more exposed to the reform reduced their intra-group debt ratio and increased their interest-bearing bank debt ratio. This suggests a change in debt composition rather than a broad reduction in total leverage. The 2019 reform is associated with lower investment among exposed firms, but this result is stated cautiously because the early post-reform period overlaps with Covid-19 and because the event-study diagnostics show pre-existing divergence. For the 2009 reform, the evidence is weaker and does not show a clear average adjustment in leverage or investment.

These findings should be interpreted with caution. The event-study diagnostics show evidence of differential pre-trends in several reform-outcome combinations. Therefore, the estimates should not be presented as definitive causal effects. Instead, they provide structured empirical evidence on how exposed highly leveraged Swedish firms changed their financing and investment behaviour around major tax reforms. Wider-window regressions are best read as additional descriptive evidence, while the main interpretation emphasizes the most stable short-term and conservative patterns.

## 7.1 Answering the questions

The main research question asked how Swedish corporate tax reforms restricting interest deductibility and reducing the statutory corporate tax rate influenced debt ratios and investment behaviour among highly leveraged firms.

The answer is that the reforms were associated with selective changes in corporate behaviour. The results do not show a general and consistent reduction in total leverage across all reforms. Instead, the strongest evidence concerns the structure of debt. After the 2013 reform, exposed firms appear to move away from intra-group debt and toward external interest-bearing bank debt. This is consistent with the reform's focus on intra-group financing arrangements. However, it does not mean that firms simply deleveraged. Rather, they seem to have adjusted the type of debt they used.

For the first sub-question, the evidence only partly supports the expectation that highly leveraged firms reduced their reliance on debt financing. The 2009 reform does not show a clear or robust reduction in debt ratios. The 2013 reform shows a strong reduction in intra-group debt, but this is accompanied by an increase in bank debt. The 2019 reform does not show a statistically precise short-term reduction in leverage, bank debt, or intra-group debt. Therefore, the results suggest that interest limitation rules affected debt composition more clearly than overall leverage.

For the second sub-question, the findings do not support a broad positive investment effect from the corporate tax rate reductions. Instead, the clearest investment pattern is negative after the 2019 reform. Exposed firms reduced tangible investment relative to less exposed firms. This suggests that the broader EBITDA-based interest limitation may have constrained investment for some highly leveraged firms. However, this finding should be interpreted carefully because the result is sensitive to sectoral composition, the shorter-window estimate is weaker, and Covid-19 overlaps with the early post-reform period.

Overall, the thesis finds that Swedish tax reforms were associated with meaningful but uneven changes in corporate behaviour among exposed high-leverage firms. The 2013 reform is most clearly linked to financing adjustment through debt reclassification. The 2019 reform is more cautiously linked to lower tangible investment among exposed firms. The 2009 reform provides weaker evidence of behavioural change.

## 7.2 Recommendations

The findings suggest that policymakers should evaluate interest limitation rules not only by their ability to protect the tax base, but also by their effect on corporate financing and investment decisions. Rules that target intra-group debt may reduce tax-motivated internal borrowing, but firms may respond by increasing other forms of debt. This means that policy design should consider possible substitution between internal and external financing.

The results also suggest that broad interest limitation rules may have real economic effects. The negative investment association after the 2019 reform indicates that some highly leveraged firms may reduce tangible investment when interest deductibility becomes more restricted. This does not mean that the rule should be removed. However, it suggests that policymakers should monitor whether the rule creates unintended pressure on firms with high capital needs.

A practical recommendation is to review the thresholds and safe-harbour rules used in broad interest limitation frameworks. Higher thresholds or more targeted exemptions could be considered for firms or sectors where high debt is linked to real investment rather than aggressive tax planning. However, any exemption should be designed carefully. Too many exceptions could weaken the anti-avoidance purpose of the rules.

For firms, the results highlight the importance of financing structure. Companies that rely heavily on intra-group debt or high interest expenses relative to earnings may face greater regulatory exposure. Firms should therefore assess their debt composition, their interest-to-EBITDA position, and their ability to finance investment under stricter deductibility rules.

## 7.3 Future Research

Future research could extend this thesis in several ways. First, later studies could examine industry-specific effects. The results indicate that sectoral composition matters, especially for the 2019 investment result. Separating real estate, manufacturing, infrastructure, and service firms could show whether the effects are concentrated in capital-intensive sectors.

Second, future research could use methods that better address differential pre-trends. Since the event-study diagnostics show that treated and control firms were not always moving in parallel before the reforms, later studies could apply alternative identification strategies. These could include matching methods, synthetic control approaches, or more refined comparison groups.

Third, future work could study the longer-term effects of the 2019 reform. The available post-reform period may be too short to fully capture how firms adjust their financing structures and investment plans. A longer time horizon would help determine whether the observed investment decline was temporary or persistent.

Finally, future research could examine whether firms substituted between financing channels after the reforms. The 2013 results suggest a movement from intra-group debt toward bank debt. A deeper analysis of this substitution would improve understanding of whether interest limitation rules reduce overall tax-motivated borrowing or simply redirect firms toward other financing sources.

In conclusion, this thesis shows that Swedish corporate tax reforms were associated with important but differentiated changes in firm behaviour among exposed high-leverage firms. The evidence is

strongest for debt-composition changes after the 2013 reform. The 2019 investment pattern remains relevant but should be interpreted more cautiously because of pre-trend limitations, sectoral sensitivity, and the Covid-19 period. The thesis therefore contributes empirical evidence on Swedish tax reform, while also showing the need for cautious policy interpretation and further research.

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