UNIVERSITY
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Turku School of
Economics

Corporate Tax Reform, Investment and Capital Allocation: Evidence from Finnish Publicly Listed Firms

Accounting and Finance
Bachelor’s thesis

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24.4.2026
Turku

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Subject: Accounting and Finance
Author: Joel Junttila
Title: Corporate Tax Reform, Investment and Capital Allocation: Evidence from Finnish Publicly Listed Firms
Supervisor: Md Khaled Hossain Rafi
Number of pages: 33 pages + appendices 2 pages
Date: 24.4.2026

This thesis examines the effects of the 2012-2014 Finnish corporate income tax (CIT) reform on investment, dividend policy and debt adjustment decisions. The CIT was reduced from 26% to 20% in two steps between 2012 and 2014. A balanced panel of 70 firms continuously listed on Nasdaq Helsinki from 2007 to 2019 is used in this thesis to examine the effects of a CIT reduction for large, publicly listed and established firms.

To measure each firm's exposure to the reform, variation in pre-reform effective tax rates (ETRs) among firms, averaged over 2009–2011 and interacted with a post-2014 indicator, is used within a difference-in-differences framework that includes firm and year fixed effects. The model is estimated using OLS. An event study specification, using 2011 as the reference year, provides additional evidence on the timing of any response.

No statistically significant effects in this sample are observed. Firms with higher pre-reform ETRs do not increase investment, nor do they adjust dividend payouts or net debt relative to other firms. The estimated coefficients are small in economic effects across all the outcomes and remain stable across different robustness checks. Baseline ETR should be interpreted as an imperfect proxy for heterogeneous tax exposure rather than as a fully exogenous treatment-intensity measure.

The analysis is limited to publicly listed firms, which restricts generalisability to smaller or privately held companies. The results provide no evidence of statistically detectable differential changes in capital allocation among large, capital-accessible firms. Given the sample size of 70 firms, only relatively large effects can be detected, and smaller effects may remain undetected.

Future research could compare listed and unlisted firms to assess whether access to capital markets influences responses to corporate tax changes.

Keywords: Effective Tax Rate, Corporate Tax Reform, Investment Decisions, Capital Allocation, Difference-in-Difference, Event Study

Kanditutkielma

Oppiaine: Laskentatoimi ja rahoitus
Tekijä: Joel Junttila
Otsikko: Yritysverouudistus, Investoinnit ja Pääoman Allokointi: Aineistoa Suomalaisilta Pörssiyhtiöiltä
Ohjaaja: Md Khaled Hossain Rafi
Sivumäärä: 33 sivua + liitteet 2 sivua
Päivämäärä: 24.4.2026

Tässä kandidaatintutkielmassa tarkastellaan vuosien 2012–2014 suomalaisen yhteisöverouudistuksen vaikutuksia investointeihin, osingonjakoon ja velan muutoksiin. Yhteisöverokantaa alennettiin kahdessa vaiheessa 26 prosentista 20 prosenttiin vuosina 2012–2014. Tutkielmassa hyödynnetään tasapainoista paneeliaineistoa, joka koostuu 70 Nasdaq Helsingissä yhtäjaksoisesti vuosina 2007–2019 listattuna olleesta yrityksestä. Tavoitteena on arvioida, miten yhteisöveron alennus vaikutti suuriin, julkisesti noteerattuihin ja vakiintuneisiin yrityksiin.

Yrityskohtaisen verouudistukselle altistumisen mittarina käytetään uudistusta edeltävää efektiivistä veroastetta (ETR), joka on laskettu vuosien 2009–2011 keskiarvona. Analyysi toteutetaan difference-in-differences-asetelmalla, jossa tätä mittaria yhdistetään vuoden 2014 jälkeistä ajanjaksoa kuvaavaan indikaattorimuuttujaan. Malli sisältää yritys- ja vuosikiinteät vaikutukset, ja estimointimenetelmänä käytetään pienimmän neliösumman menetelmää (OLS). Lisäksi analyysia täydennetään event study -spesifikaatiolla, jossa vertailuvuotena toimii vuosi 2011.

Tulosten perusteella ei havaita tilastollisesti merkitseviä differentiaalisia vaikutuksia investointeihin, osingonjakoon tai velan muutoksiin. Yritykset, joilla oli korkeampi uudistusta edeltävä efektiivinen veroaste, eivät poikenneet tilastollisesti havaittavalla tavalla muista yrityksistä reformin jälkeen. Estimoidut vaikutukset ovat myös taloudelliselta merkitykseltään pieniä, ja tulokset säilyvät pääosin samansuuntaisina vaihtoehtoisissa mallispesifikaatioissa. Uudistusta edeltävää efektiivistä veroastetta tulee kuitenkin tulkita epätäydellisenä mittarina yrityskohtaiselle verouudistusaltistukselle, ei täysin eksogeenisena treatment intensity -muuttujana.

Tutkimus rajoittuu julkisesti noteerattuihin yrityksiin, minkä vuoksi tuloksia ei voida suoraan yleistää pienempiin tai listaamattomiin yrityksiin. Tulokset viittaavat siihen, ettei yhteisöveron alennus johtanut tilastollisesti havaittaviin muutoksiin pääoman kohdentumisessa suurissa yrityksissä, joilla on hyvät mahdollisuudet ulkoiseen rahoitukseen. Lisäksi 70 yrityksen otoskoko merkitsee, että tutkimusasetelmalla voidaan havaita lähinnä suhteellisen suuria vaikutuksia, jolloin pienemmät vaikutukset voivat jäädä havaitsematta.

Jatkotutkimuksessa olisi hyödyllistä vertailla listattuja ja listaamattomia yrityksiä, jotta voitaisiin arvioida, vaikuttaako pääomamarkkinoille pääsy yritysten reagointiin yhteisöveromuutoksiin.

Avainsanat: efektiivinen veroaste, yhteisöverouudistus, investointipäätökset, pääoman kohdentuminen, difference-in-differences, event study

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1 Introduction

1.1 Background and context

Changes in corporate income tax (CIT) affect investment and capital allocation by altering post-tax returns and the user cost of capital. A lower CIT rate reduces the cost of capital, making new investment projects more attractive and increasing firms' capital stocks. These mechanisms are central to policy discussions on corporate tax reforms.

Between 2012 and 2014, Finland's statutory CIT rate was reduced in two steps: The first reduction lowered the rate from 26 to 24.5 percent in 2012, and the second brought it further down to 20 percent in 2014. The reason for the reform was to support economic growth and improve Finland's national competitiveness. This setting provides an opportunity to examine, whether CIT reductions generate the investment responses predicted by economic theory.

Prior studies have found that firms respond differently to CIT reductions depending on the characteristics of the firms. Smaller firms and those with financial constraints generally show a stronger reaction to reforms when compared to larger or more established firms (Simmler, 2012; Zwick & Mahon, 2017; Arin et al., 2023). Differences in these reactions may be a result of variations in firms' effective tax rates (ETRs). ETR is a firm's total tax expense divided by pre-tax income. ETR represents the actual tax level of a firm. In addition to possible changes in investment decisions, an increase in dividends payouts or reducing net debt due to better after-tax profits are a possible outcome.

1.2 Research question and motivation

This thesis examines whether the 2012–2014 Finnish corporate income tax reform produced differential responses in investment, dividend policy, and debt adjustment among large publicly listed firms. Variation in firms' pre-reform effective tax rates is used as a measure of exposure to tax changes. The sample consists of 70 continuously listed firms on Nasdaq Helsinki between 2007 and 2019.

The empirical specification is a difference-in-differences model with firm and year fixed effects, interacting each firm's baseline ETR, averaged over 2009–2011, with a post-reform indicator equal to one from 2014 onward. The analysis is complemented by an event study specification that traces the timing of any response across the full sample period. This thesis addresses the following research questions:

1. Did firms with higher pre-reform effective tax rates increase investment more following the 2014 reform compared to firms with lower effective tax rates?

2. Did higher-ETR firms increase dividend payouts or reduce net debt more following the reform?

This thesis aims to provide evidence on whether a statutory rate reduction of the magnitude implemented in Finland was sufficient to alter capital allocation among large, capital-market-accessible firms.

1.3 Structure

The remainder of the thesis is structured as follows. Chapter 2 reviews the theoretical and empirical literature on corporate taxation, investment, and financial policy. How effective tax rates capture heterogeneous exposure to statutory rate changes is examined to validate them as a proxy for treatment intensity approach.

The data, methodology, empirical design and limitations are covered in Chapter 3. Baseline regression results for investment, dividends, and debt adjustments, alongside robustness checks are shown and discussed in Chapter 4.

Chapter 5 concludes by summarising and discussing the results, outlining potential directions for future research.

2 Literature review & theory

This chapter reviews the theoretical and empirical literature on how corporate income taxation shapes firms' investment and capital allocation decisions. Effective tax rates (ETRs) are introduced as a measure of a firm's exposure to tax reforms and their use as a proxy for treatment intensity in a difference-in-differences framework is justified.

2.1 Tax-Induced Positive Cash-Flow Shocks

A decrease in CIT affects firms through the cost of capital and after-tax earnings channels. Lower taxes reduce the user cost of capital, making new investments more appealing. Hall and Jorgenson (1967) imply that firms respond to such changes by adjusting their capital stocks. Hassett and Hubbard (2002) survey the empirical literature, and report that investment responds moderately to changes in the user cost of capital, with varying estimates.

Tax reductions increase after-tax earnings which raises internal liquidity. This is more relevant for firms that cannot tap external capital markets. Fazzari et al. (1988) document that firms that face financial constraints respond more strongly than unconstrained firms to changes in internal cash flows when making investment decisions. CIT reduction can be considered a positive cash flow shock, that relieves financing constraints and alters capital allocation behaviour.

Whether this channel matters at all depends on how easily firms can access external funding. Simmler (2012) shows that financially constrained firms respond differently to corporate tax changes than unconstrained firms. For unconstrained firms' investment is mainly affected through the user cost of capital. For constrained firms the effect works more strongly through changes in internal liquidity and cash flow. This means that financial constraints increase the importance of the cash-flow channel in firms' investment decisions.

Zwick and Mahon (2017) show that smaller firms respond much more aggressively to tax incentives such as bonus depreciation than larger firms. Large, publicly traded firms with capital market access are unlikely to be liquidity constrained and not expected to respond strongly to tax-induced cash flow variations.

2.2 Investment Channel

2.2.1 Heterogeneity in Investment Responses

A firm's response to corporate tax changes is influenced by its effective tax rate. The ETR measures the share of earnings paid in taxes although its exact definition varies across accounting and empirical contexts (Hanlon & Heitzman, 2010). The statutory tax rate is uniform across firms, but ETRs vary due to differences in tax planning, profitability, and the presence of special tax rules (Dyreng, Hanlon, & Maydew, 2008). Firms with higher pre-reform ETRs are more exposed to a statutory rate cut, which may translate into stronger responses in investment or financial policies (Ohrn, 2018).

The use of pre-reform ETRs as a continuous exposure measure is consistent with the treatment-intensity approach in applied microeconomics (Angrist & Pischke, 2009). Variation in pre-reform ETRs across firms can imply that a statutory rate cut produces differential cash flow shocks across firms, making the baseline ETR a plausible firm-level proxy for treatment intensity (Ohrn, 2018).

2.2.2 Cost of Capital Mechanism

Corporate income taxes primarily affect investment by influencing the cost of capital. The q-theory framework suggests that investment depends on the relationship between the market value of a firm's assets and their replacement cost (Hayashi, 1982). When the market value exceeds replacement cost, firms have an incentive to expand their capital stock. When it falls below, contraction is optimal.

The relationship is formalised through the use of the User Cost of Capital that represents the connection between taxation and the required return on investment for an investor. In the Hall and Jorgenson (1967) framework, reducing the corporate tax rate reduces the user cost of capital which in turn increases the potential for future investments as it makes what were once marginal projects into viable ones. The magnitude of the response is dependent on the amount of the tax change.

The cost-of-capital channel is expected to be the primary mechanism through which tax changes affect investment for large listed companies. Smaller or financially constrained firms are more likely to depend on tax-induced improvements in internal liquidity when financing new projects. Investment responses among large, financially unconstrained firms are expected to operate mainly through changes in the cost of capital rather than through cash-flow effects.

2.3 Payout and Financing Channel

2.3.1 Dividend Payout Policy

The allocation of internal funds is linked to payout decisions. Myers and Majluf (1984) show that when external financing is costly, internally generated funds become more important for financing decisions. Firms may still not choose to reinvest all available funds as Agency theory suggests that excess cash can be distributed to shareholders, especially when profitable investment opportunities are limited (Jensen, 1986).

This is particularly relevant for the large, publicly listed firms. With their established market position and broad access to external financing, these firms are less likely to face binding investment constraints. An increase in internal funds from a tax cut may be more likely to appear in payout behaviour, rather than a positive change in capital expenditure decisions.

This channel may be limited by management's decisions once a dividend level is established. Investors expect the payout level to be maintained, creating asymmetric pressure against increases that cannot be sustained in the long run (Lintner, 1956). A temporary increase in after-tax cash flow from a tax cut may not be sufficient to trigger an increase in dividend payout.

2.3.2 Capital Structure and Debt Adjustment

Tax reform may influence firms' capital structure. The Trade-off framework shows that firms weigh the tax advantages of debt against the costs of financial distress and agency conflicts (Modigliani & Miller, 1963; Kraus & Litzenberger, 1973). A reduction in corporate tax rates lowers the value of the interest tax shield. This reduction in the value of tax shield weakens the incentive to rely on debt financing. Heider and Ljungqvist (2015) show that leverage responds to tax incentives although the effect is more noticeable for tax increases than for cuts.

Firms may not act on the incentives to reduce debt, as capital structure adjustments are costly and slow processes, and firms tend to rebalance their leverage infrequently rather than continuously in response to small changes in the tax environment (Leary & Roberts, 2005). A modest statutory rate reduction may not be sufficient to trigger active deleveraging among the large, listed firms examined in this thesis.

2.4 Heterogeneity in Tax Exposure

The ETR captures the actual tax burden a firm faces after accounting for deductions, credits, and tax planning strategies and can therefore differ substantially from the statutory rate (Hanlon & Heitzman, 2010). More profitable firms tend to have higher ETRs because they cannot fully shelter their earnings, whereas firms with access to tax-planning strategies can reduce their ETR below the statutory level.

Firm size, capital intensity, and geographic allocation of income further contribute to the variation among firms (Graham, Raedy, & Shackelford, 2012). ETRs are firm-specific and relatively persistent over time (Dyreng, Hanlon, & Maydew, 2008), which makes pre-reform ETRs a plausible measure of heterogeneous exposure to a statutory rate cut (Ohrn, 2018).

2.4.1 Effective Tax Rates as Exposure Measures

Researchers have used pre-reform ETRs to measure differential tax exposure across firms in the U.S. Ohrn (2018) demonstrates that firm-level variation in effective tax rates can be used to identify heterogeneous responses in investment, payouts, and debt policy within a continuous difference-in-differences framework, which is the approach adopted in this thesis.

ETRs may also reflect factors beyond tax exposures. Profitability, firm size, and tax planning ability can all affect the measured rates. As these characteristics may independently influence investment behaviour, most empirical studies include firm fixed effects and control variables to account for tax exposure from other sources of variation (Hanlon & Heitzman, 2010). Accounting-based tax measures reflect both the underlying tax rules and financial reporting choices and ETRs should be interpreted with caution.

Dyreng et al. (2008) document that firm-level ETRs are relatively stable over time. This finding supports use of ETRs as a pre-determined measure of tax exposure to anticipated policy changes. Measuring exposure continuously preserves variation among firms and allows the estimated response to scale with exposure (Ohrn, 2018).

2.4.2 Measurement Issues in Effective Tax Rates

Measuring ETRs is challenging when a firm reports zero or negative pre-tax income, as the ETR may be an economically meaningless value that does not accurately reflect the firm's tax burden. Empirical studies can exclude firms with non-positive pre-tax income, and winsorize ETR observations to limit the influence of outliers (Dyreng, Hanlon, & Maydew, 2008). Net operating loss carryforwards may alter the timing of investment responses by reducing firms' immediate incentives to use new investments as tax shields (Xu & Zwick, 2024). This can make a firm look less exposed to a CIT cut than it is.

Prior research rarely relies on a single-year ETR measure as averaging ETRs over several pre-reform periods to obtain a benchmark is a common method used in empirical research. The averaging helps smooth out short-term fluctuations in income and tax payments. This provides a more stable picture of a firm's typical tax position prior to the reform (Dyreng, Hanlon & Maydew, 2008). These methods are used in this thesis to improve the reliability of the ETR as a proxy for treatment intensity.

2.5 Empirical Evidence

2.5.1 Investment Responses to Corporate Taxation

Hassett and Hubbard (2002) demonstrate that empirical work based on aggregate time-series data finds relatively modest investment responses to tax incentives. Studies exploiting policy changes as natural experiments have improved identification and made it possible to study heterogeneous responses across firms, compared to the time-series data based studies.

Cummins et al. (1994) show how investment becomes more responsive when tax policy affects the user's cost of capital. House and Shapiro (2008) document that investment increased following temporary bonus depreciation in the United States. Zwick and Mahon (2017) show that effects are particularly strong among smaller and financially constrained firms. These reforms affect both the intensive margin of how much firms invest and the extensive margin of whether they invest.

Edgerton (2010) demonstrated that adjustment costs and tax asymmetries can limit the impact of tax incentives as firms may face high costs in adapting to new tax conditions. Bond and Xing (2015) find that higher corporate taxation is associated with lower capital intensity across countries. The lower capital intensity is consistent with the cost-of-capital channel. The magnitude of firm-level responses varies considerably as adjustment costs imply that firms do not respond immediately to tax changes, but instead invest gradually over time (Abel, 1983; Edgerton, 2010). Anticipation effects may complicate the timing of the observed responses when reforms are implemented gradually (Abel, 1982).

More relevant to this thesis, Arin et al. (2023) suggests that the aggregate investment response to corporate tax shocks is largely driven by smaller and financially constrained firms. Larger firms, like the ones examined in this thesis, show more limited responses. This is consistent with the view that the liquidity channel matters when firms face binding financing constraints.

A concern in the literature is the statistical power. Empirical tests of corporate tax effects using difference-in-differences designs are frequently underpowered because of adjustment costs, which cause only a subset of firms to react directly to tax changes. When tax reforms are expected to be short-lived, firms adjust less aggressively and firm-specific shocks further obscure any systematic response between the treatment and control groups. Hennessy et al. (2020) formalise this problem by showing that the null of no tax effect is incorrectly retained in approximately half of the simulated natural experiments, even with decade-long tax regimes and ideal identification. They show that null results in tax reform studies could possibly reflect the inherent limitations of the empirical design, rather than an absence of corporate responses.

2.5.2 Financial Policy Responses to Tax Changes

Corporate tax reforms affect not only investment but also dividend policies and capital structure decisions. Chetty and Saez (2005) find that lower dividend tax rates led to increased shareholder payouts. Yagan (2015) suggests that the same reform has no detectable effect on real investment.

Graham (2000) shows that the tax benefits of debt can be economically significant in the long run. Estimating that the capitalized tax benefit of interest deductibility equals approximately 9.7 percent of firm value for the typical firm. This evidence shows that corporate taxation can affect financing choices through the capital structure channel in addition to investment decisions.

Ohrn (2018) shows that firm-level variation in effective tax rates can generate heterogeneous responses in investment, payout, and debt policies, suggesting that tax-induced improvements in after-tax cash flow may be reflected across several financial margins rather than exclusively in capital expenditures.

Evidence from the same Finnish reform among small firms suggests that the tax cut did not increase capital investment but did affect business activity through other channels, with additional cash resources appearing to be an important driver of these responses (Harju, Koivisto, & Matikka, 2022). It should be noted, that in their setting, the corporate tax cut was accompanied by a dividend tax increase that left the effective dividend tax rate unchanged for most small firm owners, an institutional feature that does not directly apply to the large, listed firms examined in the present thesis.

Empirical evidence indicates that the effects of CIT reform vary substantially across firms, financing conditions and institutional settings (Hassett & Hubbard, 2002; Zwick & Mahon, 2017).

2.6 Hypotheses

The empirical literature suggests that corporate tax changes can affect firms through multiple channels, including investment, payout policy, and capital structure. Responses depend on firm-specific characteristics such as tax exposure and size. Based on prior empirical research and economic theory, the following three hypotheses are formed:

H1: Firms with higher pre-reform effective tax rates exhibit a stronger increase in investment after the corporate tax reform than firms with lower effective tax rates.

H2: Firms with higher pre-reform effective tax rates increase dividend payouts more after the reform than firms with lower effective tax rates.

H3: Firms with higher pre-reform effective tax rates reduce net debt issuance more after the reform than firms with lower effective tax rates.

For each of the three hypotheses, the null hypothesis is that no such differential response is found.

3 Research Data and Design

3.1 Data and Sample

This section provides information on the sample and data used to examine how corporate investment, dividend policy, and debt adjustment responded to the 2012-2014 CIT reform. This thesis uses a balanced panel dataset of publicly listed firms on Nasdaq Helsinki from 2007 to 2019. Only firms that remained listed throughout the entire period were included, resulting in a balanced panel of 70 firms.

Corporate investment is defined as capital expenditures scaled by total assets. Using total assets as the denominator eliminates potential scaling issues when comparing CAPEX across firms of different sizes. The same is done for dividends and debt.

Table 1 presents the descriptive statistics of the main variables used in the analysis. The sample mean of CAPEX/assets is 4.5%, with a standard deviation of 3.9%. Dividend payouts average 4.2% of total assets with a standard deviation of 4.4%. The debt variable, measured as the annual change in debt scaled by assets, averages 2.2% with a standard deviation of 8.5%. This indicates that firms, on average, neither substantially increased nor decreased their net debt during the sample period. The baseline ETR averages 0.172 across the 70 firms, with a standard deviation of 21.1%, indicating substantial variation in pre-reform tax exposure across firms.

Table 1 Descriptive Statistics

Variable	Mean	SD	Min	Median	Max	N
CAPEX / Assets	0.045	0.039	0.001	0.033	0.121	910
Dividends / Assets	0.042	0.044	0.000	0.028	0.138	910
Debt / Assets	0.022	0.085	−0.109	0.001	0.191	910
Cash Flow / Assets	0.094	0.082	−0.038	0.079	0.233	910
Log (Assets)	5.989	2.380	1.497	5.558	13.460	910
Baseline ETR	0.172	0.211	−0.327	0.209	0.599	70

All variables except Baseline ETR are firm-year observations (70 firms × 13 years = 910). Baseline ETR is a firm-level variable averaged over 2009–2011 and winsorized at the 5th and 95th percentiles. CAPEX/Assets, Dividends/Assets, Debt/Assets, and Cash Flow/Assets are winsorized at the 5th and 95th percentiles

Two additional data quality checks were performed to ensure that the data were reliable. First, the listing status of each firm was confirmed to remain constant throughout the time frame. Second, firms with missing or incomplete financial reporting data were excluded. Irregular accounting values were also removed to prevent the distortion of the CAPEX calculation. All data for this thesis were obtained from the LSEG Refinitiv Workspace.

3.2 Measuring tax exposure

Each firm's exposure to tax reform is measured using a pre-reform effective tax rate.

The firm-year effective tax rate is calculated as:

$$ETR_{it} = \frac{Total\ tax\ expense_{it}}{pretax\ income_{it}}$$

To increase the reliability of this measure, the average ETR from 2009 to 2011 was used. The average over multiple years reduces the influence of short-term fluctuations in tax payments. Years in which a firm reports zero or negative pre-tax income are excluded from the average, as the ETR is undefined or unreliable in such cases.

3.2.1 Winsorization

Winsorization is applied to five variables: the baseline ETR, CAPEX/Assets, Debt/Assets, Dividends/Assets and Cash Flow/Assets. For the baseline ETR, the firm-level average is first calculated over the 2009–2011 window, and winsorization is then applied to this averaged value at the 5th and 95th percentiles across firms. For the time-varying outcome and control variables, winsorization is applied to each firm-year observation at the 5th and 95th percentiles. The same rule is applied uniformly across all model specifications, including the dividend and debt adjustment regressions and all robustness checks.

3.2.2 Validation Regression

A potential problem is that the baseline ETR may capture aspects of the firm besides exposure to tax legislation. To test for this, a regression model is estimated in which the baseline ETR is predicted with firm attributes (size, profitability, Debt/Assets, and sales growth), all of which were observed prior to the enactment of the tax legislation.

The regression specification is given by:

$$Baseline\ ETR_i = \alpha + \beta_1 Size_i + \beta_2 ROA_i + \beta_3 (\frac{Debt}{Assets})_i + \beta_4 SalesGrowth_i + \varepsilon_i$$

Here, the dependent variable is the average ETR from 2009 to 2011 for each firm, and the independent variables are the various firm attributes If firm attributes do not account for a large portion of the variation in baseline ETR, this supports its use as an imperfect proxy for heterogeneous tax exposure. At the same time, any statistically significant association between baseline ETR and firm characteristics indicates that the measure should not be interpreted as a clean exogenous treatment-intensity variable.

Table 2 shows that the explained variance (R^2) of this model is 0.143. This means 14.3% of the variation in baseline ETR can be attributed to firm size, profitability, Debt/Assets, or sales growth. Debt/Assets and sales growth are statistically significant, while firm size and profitability are not statistically significant. The low overall explanatory power suggests that the baseline ETR is not simply a proxy for firm type. Though the significant coefficients indicate that it is not entirely independent of firm characteristics either. The inclusion of firm fixed effects and controls in the main specification addresses this concern partially.

Table 2 Determinants of Baseline Effective Tax Rate

Variable	Coefficient	Robust Std. Error	P-value
Size	-0.0047	0.0091	0.608
Profitability (ROA)	-0.0317	0.0571	0.580
Debt/Assets	-0.3366*	0.1484	0.027
Sales growth	0.5687*	0.2432	0.022
Constant	0.2920***	0.0754	0.000
R^2	0.143		

The dependent variable is the firm-level baseline ETR averaged over 2009–2011. The standard errors are heteroskedasticity robust. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

3.2.3 Descriptive ETR Statistics

Table 3 displays the descriptive statistics for the average baseline ETR for different quartiles. The firms are sorted into quartiles based on the winsorized baseline ETR. The ETR is averaged over 2009-2011. The results presented in Table 3 indicate that even prior to the reform, there was considerable variance in tax exposure among the firms.

Table 3 Descriptive Statistics of the Baseline ETR by Quartile

Baseline ETR quartile	Firms	Mean baseline ETR	Mean log assets	Median ROA
Q1 (lowest)	18	-0.099	6.27	0.084
Q2	18	0.147	5.95	0.016
Q3	17	0.250	6.33	0.099
Q4 (highest)	17	0.409	5.26	0.052

Firms are sorted into quartiles based on their winsorized baseline ETR, averaged from 2009 to 2011. The mean log assets and Median ROA are calculated over the same pre-reform window. Median is used for ROA due to outliers.

Firms in the lowest quartile have a negative mean baseline ETR of −0.099. This can happen when a firm reports a negative total tax expense due to tax refunds or the use of loss carryforwards from prior years. A cut in the statutory rate may not produce the same cash flow benefit for these firms as it would for firms with positive ETRs. These firms are kept in the sample as removing them would reduce the variation among firms that the identification strategy depends on. Winsorisation at the 5th and 95th percentiles already limit the effect of extreme values. Their presence in Q1 means that the estimated relationship between the baseline ETR and post-reform outcomes may be weaker at the lower end of the distribution.

The average firm size (mean log assets) is similar for all the quartiles. It appears that firm size does not account for most of the difference in tax exposure. Average profitability measured as median return on assets varies somewhat among the quartiles but does not follow a systematic pattern, suggesting that profitability alone does not drive the variation among firms in tax exposure. To control for any association between tax exposure and firm characteristics included in the regression model, the model includes controls for size, profitability, debt adjustment and sales growth.

3.2.4 Descriptive Evidence on Changes in Effective Tax Rates

Figure 1 presents the effective tax rates over time (2007-2019) for firms grouped into quartiles based on their baseline ETR. The ranking of quartiles remains stable throughout the sample period. This indicates that the baseline ETR captures persistent differences in tax exposure across firms.

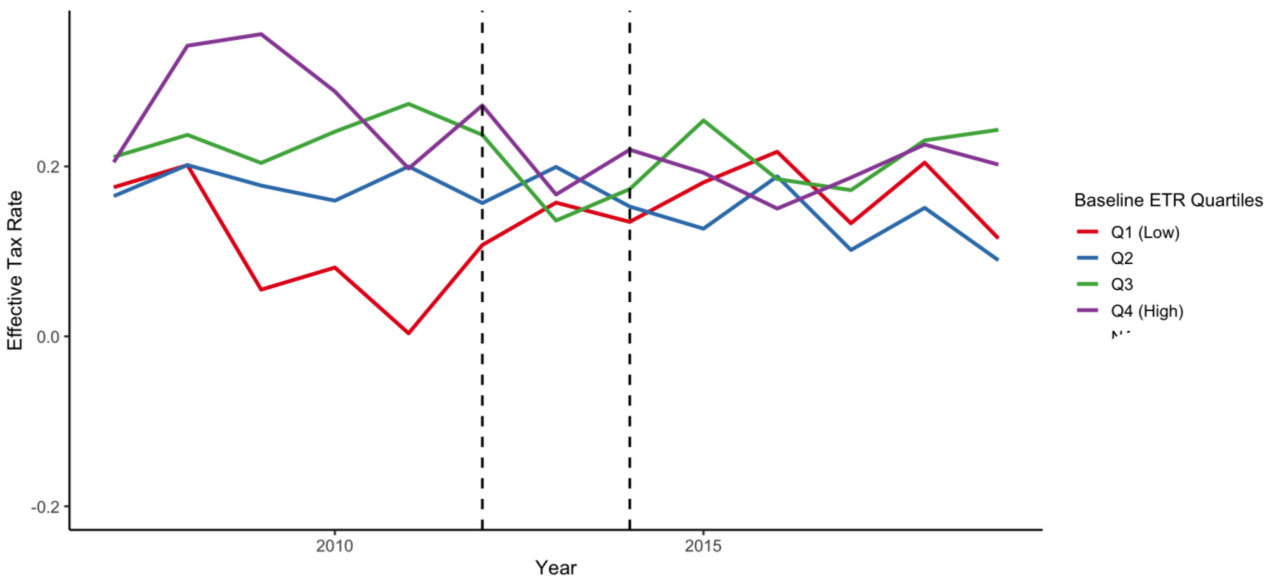

Figure 1 ETRs by Baseline ETR Quartiles Around the Corporate Tax Reform

The Figure presents the effective tax rate (ETR) by year (2007-2019) for firms grouped into quartiles based on their baseline ETR. Vertical dashed lines indicate the reform years 2012 and 2014. ETR is defined as total tax expense divided by pre-tax income.

The effective tax rate decreased from 0.189 pre reform to 0.175 after the reform. The magnitude of the change varied among the quartiles. Firms in the highest baseline ETR quartile experience a decline of 6.5 percentage points (pp). Firms in the lowest quartile show an increase of 5.2 pp. This implies a difference of 11.7 pp between high- and low-exposure firms. A similar pattern is observed when comparing firms above and below the median baseline ETR. High-ETR firms experience a decline of 3.7 pp, while low-ETR firms show almost no change.

These patterns provide descriptive evidence that firms with higher pre-reform ETRs experienced larger effective tax reductions. This evidenced support the use of baseline ETR as a proxy for treatment intensity in this thesis's empirical analysis.

3.3 Institutional background and staged tax reform

Between 2012 and 2014, the Finnish government implemented two successive reductions in the statutory corporate income tax rate. The first reduction took effect in 2012, lowering the rate from 26% to 24.5%. The second, larger reduction occurred in 2014, reducing the rate to 20%.

The primary treatment year for the analysis is 2014. This year is chosen over the 2012, because a larger reduction is more likely to produce a detectable effect. But the staged nature of the reform raises the possibility that firms began adjusting as early as 2012, either in response to the first cut or in anticipation of the second cut.

Two different methods were used to address these issues empirically. First, the event-study specification described in Section 3.6 allows the interaction between baseline tax exposure and year to vary freely across all sample years, making it possible to observe whether any adjustment began around 2012 or materialised only after 2014.

Second, an additional post-reform dummy variable is used for robustness checks in Section 4.3.3. In these checks the baseline model is re-estimated using a post-2012 indicator, to test whether the 2012 cut produced differential responses among high-ETR firms.

3.4 Empirical Strategy

To assess the influence of tax law changes on corporate investment, a panel regression model is applied using both firm and year fixed effects.

The baseline specification is written as:

$$\left(\frac{CAPEX}{Assets}\right)_{it} = \alpha + \beta(Baseline\ ETR_i \times Post_t) + \gamma_1\left(\frac{CashFlow}{Assets}\right)_{it} + \gamma_2\left(\frac{Debt}{Assets}\right)_{it} + \gamma_3 log(Assets)_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

Table 4 defines all variables used in the baseline specification with a short description of each variable.

Table 4 Regression Estimation Variables

Variable	**Explanation**
$(\frac{CAPEX}{Assets})_{it}$	Capital expenditure scaled by total assets for firm *i* in year *t*
Baseline ETR_i	The baseline effective tax rate averaged over the pre-reform period (2009–2011): Serves as a proxy for treatment intensity
$Post_t$	An indicator variable equal to one for years from 2014 onward and zero otherwise
$(\frac{CashFlow}{Assets})_{it}$	Cash flow scaled by total assets: proxy for internally generated funds
$(\frac{Debt}{Assets})_{it}$	Annual change in debt scaled by total assets: Controls for debt adjustment
$log(Assets)_{it}$	Natural logarithm of total assets: Controls for firm size
μ_i	Firm fixed effects: Absorb time-invariant firm characteristics
λ_t	Year fixed effects: Absorbs common macroeconomic shocks

Firm-specific effects capture differences across firms that do not change over time (industry, typical tax strategy). Time-specific effects represent economy-wide shocks or trends that impact all firms in a particular year. The time-varying controls of cash flow, Debt/Assets, and firm size are included to absorb variation in investment that is driven by changes in firm characteristics rather than by tax reform. Including these controls reduces the residual variation in the dependent variable which is important given the relatively small sample of 70 firms.

A potential concern is that time-varying controls, such as cash flow and debt adjustment, may be influenced by the tax reform and could therefore absorb part of the treatment effect (Angrist & Pischke, 2009). To address this, the model is estimated without time-varying controls as a sensitivity check. If the coefficient of interest changes substantially when the controls are removed, this suggests that the controls absorb part of the treatment effect.

A positive β indicates that higher-ETR firms increased investment more following the reform. A coefficient β close to zero suggests no systematic differential response. A negative β indicates that higher-ETR firms reduced investment relative to lower-ETR firms. Importantly, β captures relative differences across firms rather than the average effect of the reform. Because the Baseline ETR is time-invariant, its main effect is absorbed by firm fixed effects, and identification relies on within-firm changes in the interaction term.

3.5 Pre-Reform Validation

To confirm that the identification strategy is appropriate, the relationship between baseline tax exposure and investment in the years preceding the reform is examined. If there is evidence that firms with higher ETRs invested more or less than other firms prior to the reform, the ETR could capture some underlying firm-specific characteristics instead of just the level of exposure to the policy change. A regression analysis is performed using only data from the period preceding the reform to assess this possibility. The results are presented in Table 5.

The coefficient for the baseline ETR is small and statistically insignificant and the findings provide no evidence of a pre-existing systematic relationship between the baseline ETR and investment prior to the reform, supporting the validity of the identification strategy. Firm fixed effects are excluded from this specification, as the baseline ETR is time-invariant and would be absorbed by firm fixed effects. This regression examines whether baseline ETR predicts pre-reform investment levels in the sample, rather than testing for differential trends over time. Pre-reform trend evidence is provided by the event study in Section 3.6.

Table 5 Pre-reform relationship between baseline ETR and investment

Variable	Coefficient	Robust Std. Error	p-value

Baseline ETR	-0.0029	0.0183	0.873
Firm size (log assets)	0.0001	0.0009	0.888
Cash flow / assets	0.1015**	0.0392	0.012
Debt / assets	0.1279***	0.0332	0.000

The dependent variable is CAPEX/Assets. The sample is restricted to pre-reform years (2007–2011). The standard errors are clustered at the firm level. *** p < 0.001, ** p < 0.01, * p < 0.05. The specification includes year fixed effects.

3.6 Event study specification

The event study investigates when the tax law's impact became apparent. The event study model is specified as follows:

$$\frac{CAPEX}{Assets}_{it} = \alpha + \sum_{k \neq 0} \beta_k (Baseline\ ETR_i \times D_t^k) + \gamma_1 (\frac{CashFlow}{Assets})_{it} + \gamma_2 (\frac{Debt}{Assets})_{it}$$
$$+ \gamma_3 ln(Assets)_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

where D_t^k is an indicator variable equal to one when the calendar year t corresponds to the event year k, and zero otherwise. The summation runs over:

$$k \in \{-4, -3, -2, -1, +1, +2, +3, +4, +5, +6, +7, +8\},$$

corresponding to calendar years 2007–2010 and 2012–2019. The year $k = 0$ (2011) serves as the reference category so all coefficients β_k measure the differential investment of higher-ETR firms relative to 2011. The pre-reform coefficients ($k < 0$) serve as a test of the parallel trend's assumption: if higher- and lower-ETR firms followed similar investment trajectories before the reform, these coefficients should be statistically indistinguishable from zero.

2011 is chosen as the reference year because the first reduction took effect in 2012, meaning that 2012 and 2013 may already reflect partial treatment and are not clean pre-reform years. Using 2011 ensures a clean pre-reform baseline.

The post-reform coefficients (k > 0) capture the differential investment of higher-ETR firms in each year following the reform, allowing the timing and persistence of any response to be traced across the sample period. The coefficients for 2012 and 2013 (k = +1, +2) are included in the post-reform window and allow a direct test of whether the initial rate reduction already produced a differential response prior to the main 2014 reform. Identification relies on the interaction between the time-invariant baseline ETR and year indicators, exploiting the differential exposure across firms.

3.7 Alternative Reallocations: Debt and Dividends

In addition to investment, this thesis examines firms' dividend and debt adjustment responses to tax reform. A similar OLS regression to investment is constructed to examine the effects.

The specification is:

$$\mathrm{Y}_{it} = \alpha + \beta(\text{Baseline ETR}_i \times \text{Post}_t) + \gamma\mathrm{X}_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

Where Y_{it} represents either Dividends/Assets or Debt/Assets, and X_{it} represents the vector of controls used in the baseline investment regression.

When Dividends/Assets is the dependent variable, the control vector includes Cash Flow/Assets, Debt/Assets, and Log(Assets). When Debt/Assets is the dependent variable, Cash Flow/Assets and Log(Assets) are the control vectors.

Using a consistent empirical specification across all outcomes (investments, dividends and debt adjustment) allows for a direct comparison of coefficients and provides an opportunity for a joint interpretation of how firms allocate tax-induced changes.

3.8 Limitations and additional considerations

Baseline Effective Tax Rates do not necessarily reflect the tax burden alone; they can reflect the level of sophistication a company has regarding its tax strategy. Companies with a higher degree of tax strategy sophistication are more likely to have lower effective tax rates and will probably differ in terms of investment activity. Although observable firm characteristics (i.e. size, profitability, debt adjustment and sales growth) are controlled for and firm fixed effects are included to capture persistent firm differences, it is impossible to fully differentiate tax exposure from tax strategy sophistication.

Evidence from an earlier Finnish tax reform suggests that firms can adjust their financial behaviour in anticipation of announced changes. Finnish corporations increased dividend payouts ahead of the 2005 dividend tax increase before it took effect (Kari, Karikallio, & Pirttilä, 2008). Although this reform involved dividends rather than corporate taxation, the finding illustrates that anticipation effects are empirically relevant in the Finnish context and may complicate post-reform identification more broadly. The sample is restricted to large listed firms with broad access to capital markets, which limits generalisability to smaller or privately held companies.

This thesis focuses solely on financial reporting, and does not examine how firms respond to tax law changes through other mechanisms such as altering their employment, wage structures or by shifting profits internationally.

The validation regressions suggests that the baseline ETR is negatively correlated with pre-reform Debt/Assets. This relationship indicates that there is some degree of overlap between the measurement of ETR and a company's capital structure, it would suggest that even if ETR were measuring pure tax exposure, it could have limited ability to identify the impact of debt adjustments compared to investments and dividend payments.

A major limitation of this thesis is its statistical power due to the sample size of 70 firms. Because of the limitation of sample size, the model can only detect relatively large effects. The use of firm and year fixed effects improves identification but absorbs a portion of the variation in the dependent variable and may lead to larger standard errors.

The statistical power of the present design is assessed using the minimum detectable effect (MDE). MDE is defined as the smallest true coefficient detectable at the 5% significance level with 80% power. $MDE = 2.80 \times SE$. Based on the clustered standard errors from the baseline specifications, the MDEs for investment, dividends, and net debt adjustment are 0.029, 0.033, and 0.102. Expressed per one standard deviation increase in the baseline ETR (SD = 0.211), these correspond to differential changes of 0.60, 0.69, and 2.15 percentage points. This is equivalent to: 13%, 16%, and 98% of the respective sample means.

The strategy used to identify the treatment group also assumes that firms had sufficiently different baseline ETRs. If firms experience little variation in their tax burden or their investment behavior is affected by firm-specific events independent of the tax law change, then precision in estimating coefficients is decreased. Results that are statistically insignificant should be viewed with caution. Due to lower power, results regarding net debt adjustments are particularly suspect since MDE exceeds the sample mean for net debt adjustment.

4 Results

This section presents evidence on the extent to which publicly traded Finnish companies reacted to the corporate income tax reform. The focus is on changes in investment, dividend payments, and debt adjustment. By comparing firms with varying levels of pre-reform tax exposure, the analysis examines whether firms that experienced a larger tax-induced cash flow shock adjusted their behaviour more than others following the reform.

4.1 Regression results

The main analysis examines whether firms with higher baseline effective tax rates increased their investment following the 2014 corporate tax reform. Table 6 presents the estimates from the baseline regression model.

Table 6 Baseline Investment Response Difference-in-Differences Estimates

Variable	Coefficient	Clustered Std. Error	P-Value
Baseline ETR × Post	−0.0074	(0.0102)	0.467
Cash Flow / Assets	0.0925***	(0.0218)	0.000
Debt / Assets	0.1155***	(0.0186)	0.000
Log(assets)	0.0080**	(0.0026)	0.003
Firm fixed effects	Yes		
Year fixed effects	Yes		
Observations	910		
Within R^2	0.147		

The dependent variable is CAPEX/Assets. Post = 1 for 2014 onward, 0 otherwise. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The results provide no evidence of a statistically significant differential investment response among higher-ETR firms after the reform. The point estimate of the interaction term (−0.0074) is economically small and statistically insignificant ($p = 0.467$). This suggests no meaningful difference in investment behaviour between high and low ETR firms after the reform.

Cash flow scaled by total assets (0.0925, $p < 0.001$) and debt adjustment (0.1155, $p < 0.001$) are both positively associated with investment and statistically significant. This is consistent with the pre-reform validation results and the broader empirical literature on the determinants of investment. Firm size, measured by the log of total assets, is also statistically significant (0.0080, $p = 0.003$), indicating that larger firms tend to invest slightly more relative to their asset base.

The within R^2 of 0.147 reflects a moderate fit. The standard error on the main interaction term is 0.0102. This indicates that only large effects would reach statistical significance at the 5% level and the point estimate of -0.0074 falls below this.

The point estimate of −0.0074, combined with a baseline ETR standard deviation of 0.211, implies a differential of approximately −0.16 percentage points in CAPEX/Assets for a firm one standard deviation above the mean. This is equivalent to a relative difference of roughly 3.5% against the sample mean of 4.5%. The 95% confidence interval spans −0.028 to +0.013, showing that the data remain consistent with a meaningful negative response and only a modest positive response.

The absence of an effective statistical difference in results is supported by the theoretical framework discussed in Section 2. Large listed firms generally have access to external financing. They would be less dependent of tax-induced changes in internally generated cash flows when making investment decisions. (Simmler, 2012; Zwick & Mahon, 2017).

The evidence does not support H1. The null hypothesis of no differential investment response cannot be rejected.

4.2 Investment Response - Event study

Figure 2 presents the estimated year-by-year interaction coefficients from the event study specification. The pre-reform coefficients (2007–2010) are statistically indistinguishable from zero. This supports the parallel trends assumption, indicating that higher- and lower-ETR firms followed similar investment trajectories before the reform, suggesting that the baseline ETR does not capture pre-existing differences in investment behaviour. The coefficients for 2012 and 2013 are similarly close to zero and statistically insignificant. This suggests that the initial rate reduction in 2012 did not produce a differential investment response among higher-ETR firms. In the post-reform years (2014–2019) the coefficients remain close to zero and statistically insignificant, consistent with the baseline regression's null results.

The absence of a systematic post-reform separation between high- and low-ETR firms is consistent with the baseline DiD estimate of −0.0074 ($p = 0.467$) and confirms that the null result is not due to the chosen post-reform window. The wide confidence intervals in both the pre- and post-reform periods reflect the limited statistical power available in a sample of 70 firms, suggesting that modest effects would not be detectable even if they existed.

In Figure 2, each dot represents the point estimate and the vertical bars the 95% confidence interval. None of the pre- or post-reform coefficients is statistically distinguishable from zero.

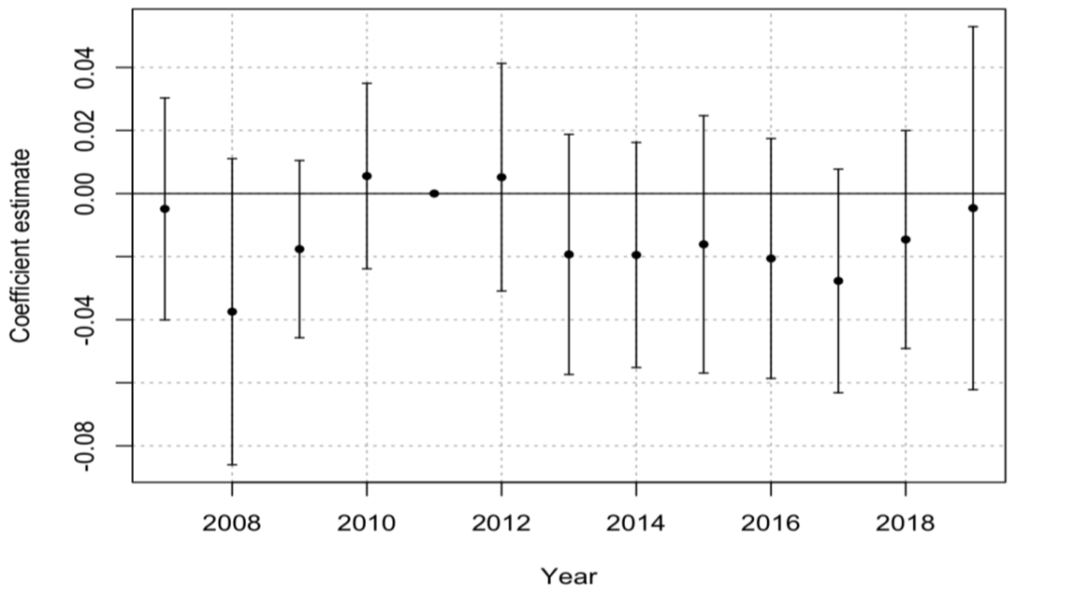

Figure 2 Event Study for Investment Response

The Figure presents the estimated coefficients β_k from the event study specification. Each coefficient measures the differential CAPEX/Assets for a one-unit increase in the baseline ETR relative to the reference year 2011 (omitted). Vertical bars represent 95% confidence intervals based on standard errors clustered at the firm level. The pre-reform coefficients (2007–2010) support the parallel trends assumption.

4.3 Robustness Checks

To assess the robustness of the main finding, four additional checks are conducted to examine the sensitivity to the choice of the baseline ETR window, binary classification of tax exposure, definition of the post-reform period, and the inclusion of time-varying controls. Across all specifications, the interaction coefficient remains small and statistically insignificant, supporting the conclusion that the null result is not driven by any single modelling choice.

4.3.1 Model 1: Alternative Baseline ETR Window

A potential concern is that the baseline ETR, averaged from 2009 to 2011, may be sensitive to the choice of the period. The years 2009 and 2010 coincide with the recovery from the global financial crisis, which may have temporarily affected firms' tax expenses. To test this, the main model is re-estimated using a longer averaging window of 2007-2011 for the baseline ETR. This approach provides a more stable measure of each firm's typical tax exposure before the reform.

The results are presented in Table 7. The interaction term remains negative (−0.0150) and statistically insignificant (p = 0.239). The control variables behave similarly to the baseline model, confirming that the main finding does not depend on the specific years used to construct the baseline ETR. The observation count drops slightly from 910 to 897 due to some firms having no positive pre-tax income in the extended window, making their ETR undefined.

Table 7 Robustness Check 1: Baseline ETR Window

Variable	**Coefficient**	**Std. Error**	**p-value**
Baseline ETR (alt) × Post	−0.0150	(0.0126)	0.239
Cash flow / Assets	0.0878***	(0.0216)	0.000
Debt / Assets	0.1166***	(0.0187)	0.000
Log (Assets)	0.0081**	(0.0025)	0.002
Firm fixed effects	Yes		
Year fixed effects	Yes		
Observations	897		
Within R^2	0.1502		

The dependent variable is CAPEX/Assets. Baseline ETR (alt) is averaged over 2007–2011 instead of 2009–2011. Post = 1 for years 2014 onward. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

4.3.2 Model 2: High vs. Low Tax Exposure

To test whether the results differ between firms with relatively high and low pre-reform tax exposures. A binary indicator is constructed that equals one if a firm’s baseline ETR is above the sample median and zero otherwise. The empirical specification is otherwise identical to the baseline model, including firm and year fixed effects and control variables.

The coefficient of interest is the interaction term between the high-ETR indicator and the post-reform period variable, capturing whether firms with above-median tax exposure adjusted their behaviour differently compared to firms below the median.

The results presented in Table 8 show no statistically significant differences between high- and low-ETR firms. The interaction coefficients for investment (−0.0037), dividends (−0.0036), and debt adjustment (−0.0061) are all small and insignificant. These findings are consistent with the baseline results of this thesis.

Table 8 Robustness Check 2: High vs. Low Tax Exposure

Variable	CAPEX	Dividends	Debt Adjustment
High ETR × Post	-0.0037	-0.0036	-0.0061
	(0.0044)	(0.0051)	(0.0099)
CF/Assets	0.0915***	0.1096**	-0.1881***
	(0.0219)	(0.0325)	(0.0543)
Debt/Assets	0.1155***	0.0478***	
	(0.0187)	(0.0115)	
Log (Assets)	0.0079**	-0.0011	0.0038
	(0.0025)	(0.0058)	(0.0094)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	910	910	910
Within R^2	0.1472	0.0767	0.0225

High ETR is a binary indicator equal to 1 if the firm's baseline ETR is above the sample median, 0 otherwise. Post = 1 for years 2014 onward. Dependent variables are CAPEX/Assets, Dividends/Assets, and Debt/Assets respectively. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

4.3.3 Model 3: Post 2012 Indicator

The baseline model is re-estimated using an alternative post-reform indicator that equals one from 2012 onward, capturing the first and smaller statutory rate reduction from 26% to 24.5%. This approach tests whether the earlier cut produced a detectable investment response that the main specification, which focuses on the larger 2014 reduction, might have obscured.

The results are reported alongside the main specifications in Table 9. The interaction coefficient is −0.0038 ($p = 0.732$), which is smaller in magnitude and statistically insignificant. Both the 2012 and 2014 specifications produce qualitatively identical conclusions, supporting the choice of 2014 as the primary treatment year for analysis.

The event study provides additional evidence on this distinction. The coefficients for 2012 and 2013 are both statistically insignificant and close to zero. This indicates that the initial 2012 rate reduction did not produce a differential investment response prior to the main 2014 reform.

Table 9 Robustness Check 3: Post 2012 Specification

Variable	Robustness (Post 2012)	Main (Post 2014)
Baseline ETR × Post 2014		−0.0074 (0.0102)
Baseline ETR × Post 2012	−0.0038 (0.0111)	
Cash Flow / Assets	0.0925*** (0.0218)	0.0925*** (0.0218)
Debt / Assets	0.1158*** (0.0186)	0.1155*** (0.0186)
Log(Assets)	0.0080** (0.0027)	0.0080** (0.0026)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	910	910
Within R^2	0.146	0.147

The dependent variable is CAPEX/Assets. Column 1: Post 2012 = 1 for years 2012 onward. Column 2: Post 2014 = 1 for years 2014 onward. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and presented in the parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

4.3.4 Model 4: No Time-Varying Controls

The baseline model is re-estimated without time-varying controls to address the concern that post-reform changes in cash flow or debt adjustment could partially absorb the treatment effects. The coefficient of interest is virtually unchanged at −0.0074 ($p = 0.481$), suggesting that the inclusion of controls does not affect this result. The within R^2 drops substantially from 0.147 to 0.001 in the uncontrolled specification, confirming that the controls explain meaningful variation in investment but do not affect the estimated treatment effect.

Table 10 Robustness Check 4: No Controls Specification

Variable	With Controls	Without Controls
Baseline ETR × Post	−0.0074 (0.0102)	−0.0074 (0.0104)
Cash Flow / Assets	0.0925*** (0.0218)	—
Debt / Assets	0.1155*** (0.0186)	—
Log(Assets)	0.0080** (0.0026)	—
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	910	910
Within R^2	0.147	0.001

The dependent variable is CAPEX/Assets. Column 1 includes firm and year fixed effects and time-varying controls for Cash Flow/Assets, Debt/Assets, and log(Assets). Column 2 excludes time-varying controls to assess whether their inclusion affects the coefficient of interest. Standard errors are clustered at the firm level and reported in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Taken together, the robustness checks presented in this section provide consistent support for the main finding. The results from different robustness checks confirm that no detectable differential investment response is found among higher-ETR firms.

4.4 Additional Outcomes: Dividend Payments

4.4.1 Dividend Baseline Regression

Table 11 presents the baseline regression results for dividends scaled by total assets.

The estimated coefficient for the interaction term, $Baseline\ ETR \times Post$, is 0.0033. This coefficient is not statistically different from zero (p = 0.776). Suggesting that there were no systematic differences in the dividend policies of Finnish publicly traded companies following the corporate tax reform, as with investment decisions.

Table 11 Baseline Dividends Response DiD Estimates

Variable	**Coefficient**	**Std. Error**	**p-value**
Baseline ETR × Post	0.0033	(0.0116)	0.776
Cash flow / Assets	0.1108***	(0.0321)	0.000
Debt / Assets	0.0482***	(0.0114)	0.000
Log (Assets)	-0.0012	(0.0059)	0.836
Firm fixed effects	Yes		
Year fixed effects	Yes		
Observations	910		
Within R^2	0.0753		

The dependent variable is Dividends/Assets. Post = 1 for years 2014 onward. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The additional funds provided by reduced tax burdens are not generally paid out to shareholders as dividends. The absence of a dividend response coupled with the lack of investment growth, provides an interesting perspective on the agency theory discussed in Chapter 2.

The positive relationship between cash flow divided by total assets and dividends indicates that firms with more internal funding tend to pay out more dividends. The positive relationship between debt and dividends suggests that firms with greater debt adjustment are more likely to provide a portion of profits to shareholders through dividend payments. Firm size measured as the natural logarithm of total assets is not statistically significant, suggesting that larger firms may not have systematically higher dividend payouts per unit of firm size than smaller firms do.

The point estimate is also economically small. One standard deviation increase in baseline ETR is associated with a differential change of only +0.07 percentage points in Dividends/Assets relative to a sample mean of 4.2%.

The evidence does not support H2. The null hypothesis of no differential dividend payout response cannot be rejected.

4.4.2 Event Study of Dividend Payments

Figure 3 illustrates the estimated coefficients from the event study model. The year preceding the first reform (2011) is used as the base period and the evidence from the event study reveals that prior to the reform, there were no systematic differences in dividend payout levels among firms with high tax burdens, relative to firms with lower tax burdens.

The pre-reform coefficients (2007–2010) are very close to zero and lack statistical significance, implying that the two groups followed similar patterns of dividend payments. The coefficients for 2012 and 2013 are similarly insignificant, suggesting that the initial rate reduction did not produce a differential dividend response. In the post-reform years (2014-2019) the coefficients remain small and statistically insignificant across all periods. This is consistent with the null result from the baseline regression.

In Figure 3, each dot represents the point estimate and the vertical bars the 95% confidence interval. None of the pre- or post-reform coefficients is statistically distinguishable from zero.

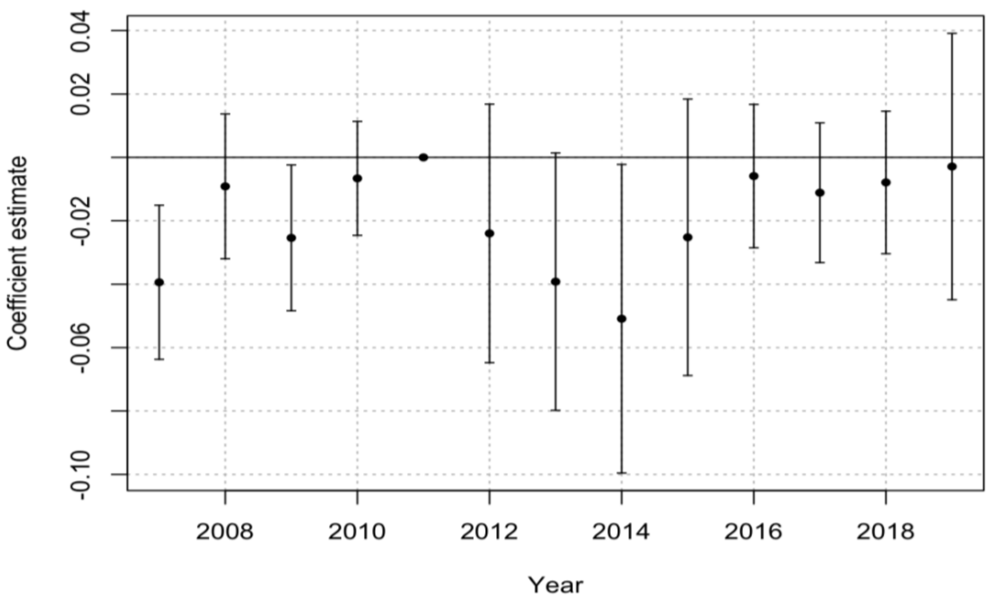

Figure 3 Event Study for Dividend Response

The figure presents the estimated coefficients β_k from the event study specification. Each coefficient measures the differential Dividends/Assets for a one-unit increase in baseline ETR relative to the reference year 2011 (omitted). Vertical bars represent 95% confidence intervals based on standard errors clustered at the firm level. Pre-reform coefficients (2007–2010) support the parallel trends assumption.

4.5 Additional Outcomes: Corporate Debt Adjustment

4.5.1 Debt Baseline Regression

Table 12 presents the baseline regression results for corporate net debt adjustments, measured as the annual change in debt scaled by total assets.

The estimated coefficient for the interaction term, $Baseline\ ETR\ \times\ Post$ is 0.0265. The p-value for the interaction term was 0.470. The evidence does not support a statistically significant differential debt adjustment response among higher-ETR firms after the tax reform.

Table 12 Baseline Corporate Net Debt Adjustments Response DiD Estimates

Variable	**Coefficient**	**Std. Error**	**p-value**
Baseline ETR × Post	0.0265	(0.0364)	0.470
Cash flow / Assets	-0.1842***	(0.0546)	0.001
Log (Assets)	0.0038	(0.0093)	0.681
Firm fixed effects	Yes		
Year fixed effects	Yes		
Observations	910		
Within R^2	0.0224		

The dependent variable is Debt/Assets. Post = 1 for years 2014 onward. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level and reported in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Of the control variables included in the model, only one was statistically significant. Cash flow scaled by total assets is negatively associated with debt adjustment (−0.1842, p = 0.001). This suggests that firms with better internal cash flows may rely less on external debt financing and that firm size is not statistically significant, which shows that debt adjustment decisions are not systematically related to firm size.

The point estimate of 0.0265 implies a differential change of +0.56 pp in Debt/Assets per one standard deviation increase in the baseline ETR. A statistically insignificant and modest change relative to the variation among firms in debt adjustment.

Given that the confidence interval spans nearly two percentage points in either direction, the debt adjustment result is best treated as inconclusive rather than as evidence against H3.

4.5.2 Corporate Debt Adjustment – Event Study

Figure 4 presents the estimated interaction coefficients from the event study specification for corporate debt adjustments.

In Figure 4, each dot represents the point estimate and the vertical bars the 95% confidence interval. The pre-reform coefficients (2007–2010) are statistically indistinguishable from zero, and they tend to sit in negative territory, suggesting some pre-existing differences in debt adjustment trajectories across the ETR distribution. This warrants some caution in interpreting the parallel trends assumption for debt adjustment. The coefficients for 2012 and 2013 are similarly insignificant, consistent with the view that the initial rate reduction did not produce a differential debt adjustment response.

In the post-reform years (2014–2019), the coefficients drift upward toward zero relative to the pre-reform period. This drift could indicate a modest convergence in debt adjustment between higher- and lower-ETR firms. None of the post-reform coefficients are statistically significant, and the wide confidence intervals preclude firm conclusions. The baseline estimates of 0.0265 (p = 0.470) remains consistent with this pattern.

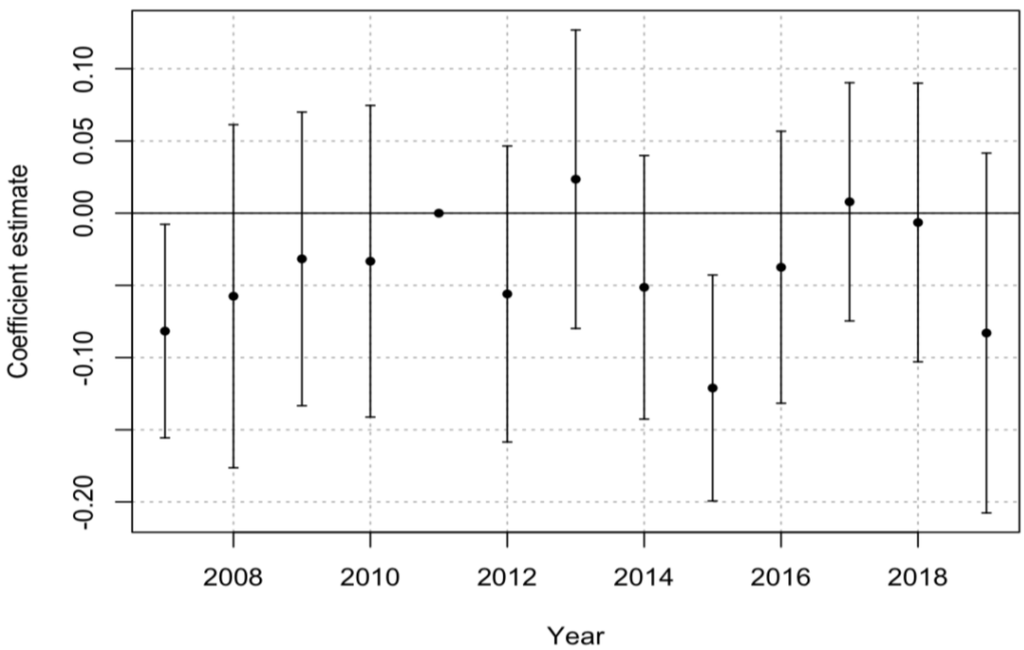

Figure 4 Event Study for Debt Adjustments Responses

The Figure presents estimated coefficients β_k from the event study specification. Each coefficient measures the differential Debt/Assets for a one-unit increase in baseline ETR relative to the reference year 2011 (omitted). Vertical bars represent 95% confidence intervals based on standard errors clustered at the firm level. Pre-reform coefficients (2007–2010) are statistically insignificant, though they tend to sit in negative territory, warranting caution in interpreting the parallel trends assumption.

Compared to the investment and dividend results where pre-reform trends are flat and support the parallel trends assumption, the debt adjustment event study provides weaker identification support. The null result should be interpreted with more caution than the results for investment and dividends.

4.6 Joint Interpretation of the results: Capital Allocation Across All Three Channels

The null hypothesis of no differential response cannot be rejected for any of the outcomes. The results do not support H1, H2 and H3. This evidence is stronger for investment and dividends, where narrow confidence intervals exclude large positive effects. For debt adjustment, the wide confidence interval prevents conclusions in either direction.

No statistically significant differential effects are detected for investment ($\beta = -0.0074$, $p = 0.467$), dividends ($\beta = 0.0033$, $p = 0.776$), or debt adjustment ($\beta = 0.0265$, $p = 0.470$). The estimated effects are also economically small. A one standard deviation increase in baseline ETR is associated with a differential change of approximately −0.16 percentage points in CAPEX/Assets, +0.07 percentage points in Dividends/Assets, and +0.56 percentage points in Debt/Assets.

The absence of statistically significant movement across all three outcomes is consistent with no observable reallocation. Though the analysis cannot rule out modest effects below the detection threshold. Large publicly listed Finnish firms may have absorbed the tax-induced cash flow increase into internal liquidity or retained earnings without reallocating it across observable financial margins. This is consistent with evidence that mature, capital-market-accessible firms are less sensitive to tax-induced cash flow shocks than smaller or financially constrained firms (Zwick & Mahon, 2017; Simmler, 2012).

The anticipation effects associated with the 2012–2014 reform may have led firms to adjust prior to the main treatment window. If firms responded to the announced rate reduction before 2014, the post-reform period captures only residual adjustment. The difference-in-differences design, which relies on a clean before-after comparison, may also understate the true effect. This makes post-reform changes inherently difficult to detect. The relatively modest magnitude of the 2014 rate reduction, from 24.5% to 20%, may have been insufficient to induce observable reallocation among firms with access to external financing.

Two interpretations of the null results remain plausible. One possibility is that the reform did not generate meaningful differential responses across capital allocation among large publicly listed firms. The second possibility is that the effect exists but is too small to detect given that the sample of 70 firms limits the ability to detect anything but relatively large effects. These interpretations cannot be separated based on the current analysis alone and represent a natural avenue for future research with larger sample sizes.

5 Conclusions

This thesis examined whether the 2012–2014 Finnish corporate income tax reform produced differential responses in investment, dividend policy, and debt adjustment among large publicly listed firms. Firms' pre-reform effective tax rates were used as a continuous measure of treatment intensity among firms. The study used a balanced panel of 70 continuously listed firms on Nasdaq Helsinki from 2007 to 2019. The regression was estimated using OLS, in a difference-in-differences framework, with firm and year fixed effects. The analysis was complemented with event study design with pre-reform year 2011 as a reference year.

No statistically significant differential responses are detected across any of the three outcomes. The findings are robust across alternative specifications for the investment and dividends, and the event study confirms parallel pre-reform trends, supporting the validity of the identification strategy. The debt adjustment result requires more caution, as the pre-reform coefficients in the debt adjustment event study tend to sit in negative territory. This provides weaker identification support for debt adjustments.

The null results across investment, dividends, and debt adjustment indicate that the tax-induced increase in after-tax cash flow was not associated with statistically detectable reallocation through these observable financial channels. A firm responding to a tax cut could invest more, pay out more dividends, or reduce debt because of decreased user cost of capital and increased after-tax earnings. The fact that none of these margins shows a statistically detectable response suggests either that no economically meaningful reallocation occurred, or that any real effect falls below what the current design can detect. The debt adjustment result is less informative given the imprecision of the estimate and should not be weighted equally with the investment and dividend findings.

The findings are consistent with the theoretical framework discussed in Section 2. The liquidity channel, through which tax cuts affect investment is more relevant for financially constrained firms, which do not characterise the large, listed firms with established capital market access analysed in this thesis (Simmler, 2012; Zwick & Mahon, 2017). This is also consistent with Harju et al. (2022), who examine the same Finnish reform among small firms and find no increase in capital investment, but do find responses through other channels. Smaller firms responded differently to the same reform, which strengthens the interpretation that firm size and financing access can be moderators of how corporate tax policy translates into real behaviour.

The absence of a dividend payout response does not support simple agency theory prediction that mature firms with excess cash distribute it to shareholders (Jensen, 1986). One interpretation is that firms preferred to retain the additional liquidity rather than increase payouts to shareholders. This is consistent with the dividend smoothing argument of Lintner (1956). Lintner suggests that temporary cash flow improvements are unlikely to trigger sustained payout increases. The absence of a debt adjustment response is similarly consistent with the view that capital structure adjustments are infrequent and costly (Leary & Roberts, 2005).

The staged nature of the reform may impact the findings ,as both the first and second cuts may allow firms to adjust ahead of the main reform year. If firms did adjust prior to the main reform year, then the pre-reform years are no longer a clean baseline; which challenges the basic before-after approach used by DiD designs and potentially leads to a bias towards an estimate of zero. The results from the debt adjustment event study provide some evidence of this phenomenon, where the pre-reform estimates sit in negative numbers, then gradually drift back toward zero after the reforms were implemented. A modest reduction in the corporate income tax rate from 24.5% to 20%, could have been too small to cause behaviour shifts among the large listed firms.

These considerations make it difficult to distinguish between two interpretations of the null results. The reform either produced no economically meaningful effects with respect to capital allocation decisions made by the examined firms; or there existed real effects, but those effects fell beneath the detection level of this study given its sample size of 70 firms and therefore unable to identify any other than very large effects.

Using only continuously listed firms in this sample includes survivorship bias and reduces the generalizability to smaller or private company firms. For example, the initial ETRs may represent nothing more than how sophisticated these firms are about tax planning rather than their true exposure to taxes. Although firm-fixed-effects and control variables help to address each of these issues in part; however, they do not eliminate them completely.

From a policy perspective the results suggest that moderate statutory rate reductions may have limited effects on the observable capital allocation of established firms. Corporate taxation is still relevant for larger firms, but the margin of response may lie outside standard financial statement data. It can show up in outcomes such as employment, wages, and international profit-shifting.

Future research could involve comparing listed versus unlisted firms directly. Alternatively, an additional area for future research could involve investigating whether smaller changes in tax rates result in measurable responses once the statistical power has been enhanced by increasing sample sizes and extending time horizons.

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Appendices

Appendix 1: Declaration of the Use of Artificial Intelligence (AI)

I used Generative AI as a resource during the preparation of this thesis. The use of these resources has been cautious and made in compliance with the policies of University. I accept full liability for all content included here.

1. Tool Anthropic Claude (Claude.AI, Claude Sonnet)

Stage of Use: Composition/Editing

- **Purpose of Use**: Claude was used to fix grammatical errors and to improve sentence clarity. Claude was also used to identify and reduce unintentional repetition across sections.
- **Example prompts:** *"Proofread this for grammatical errors and repetition without changing substance."*
- **Verification**: Every suggestion was reviewed before applying to the final text and I rejected suggestions that altered the substance or added unsupported claims. No AI-generated text was inserted into the thesis without my review and approval.

Stage of Use: Literature Review and Synthesis

- **Purpose of Use**: I used Claude to assist with the formatting of in-text citations and reference list. I also consulted the tool to identify potential gaps in my literature coverage and to generate search term suggestions for sources related to this thesis.
- **Example prompts:** *"I am writing a literature review on corporate investment responses to tax reforms using a difference-in-differences design. What related topics or research strands might I be missing?"*
- **Verification**: All citations were cross-checked against the original sources. Suggested literature leads were evaluated by me against the actual publications before inclusion. No source was cited based on AI output alone.

Stage of Use: Ideation and Research Planning

- **Purpose of Use**: I used Claude to obtain critical feedback on the overall structure and argumentation of the thesis. I requested an assessment of weaknesses in the methodology and contribution sections in order to identify areas for revision.
- **Example prompts:** *"Here is the methodology section of my bachelor's thesis. Give me honest critical feedback of its weaknesses from the perspective of an academic examiner." "I am trying to formulate three testable hypotheses for my DiD study on investment, dividends, and* debt adjustment. *Does the structure of the hypotheses follow logically?"*
- **Verification**: I verified that structural changes were consistent with the theoretical framework and empirical design of the thesis. No argument, conclusion, or interpretation was generated by AI.

2. Tool OpenAI ChatGPT (5.4)

Stage of Use: Methodology and Data Analysis

- **Purpose of Use**: I used ChatGPT to assist with writing and debugging R code for the empirical analysis. Tasks for which I consulted the tool included structuring panel data regressions, implementing fixed effects, constructing the event study specifications and resolving syntax errors..
- **Example prompts:**
  - *"I am running a difference-in-differences regression in R using panel data. My treatment variable is a continuous baseline ETR interacted with a post-reform indicator. How do I implement two-way fixed effects with clustered standard errors using the felm function?"*

  - *"I am getting the following error when running my event study in R: [error message]. What is causing this and how do I fix it?"*
- **Verification**: I reviewed all generated code line by line before executing it. I verified that the specifications matched my intended econometric design and cross-checked the output against expected results. I can independently explain the function of every line of code used in the final analysis. No part of the interpretation of results was delegated to or generated by AI.

3. Google Notbeook.LM

Stage of Use: Literature Review

- **Purpose of Use**: I used Notebook.LM to assist with reviewing and navigating academic sources included in the literature review. The tool was used to identify relevant passages within uploaded papers. All substantive synthesis, evaluation, and argumentation remained my own work.
- **Example prompts:**
  - *"What does this paper say about the relationship between effective tax rates and investment responses?"*
- **Verification:** All outputs were verified against the original source documents. Notebook.LM was used as a reading and navigation aid. No claims or arguments were included in the thesis based on AI-generated summaries alone.