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Capital Structure Determinants Across Industries: Evidence from the Swedish Market

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ABSTRACT

Using a panel dataset of 6,000 Swedish firm from 2018-2023 and a fixed effects and random effects models with interaction terms to capture industry differences, this paper investigates whether traditional capital structure determinants such as size, profitability, asset tangibility, liquidity, and non-debt tax shields affect firms differently across sectors in Sweden. Little is known about how the determinants influence varies by industry, while they are widely studied from a holistic perspective. Unlike prior studies focusing on single sectors, or the market as a whole, our comparative approach highlights cross-sectoral differences. We find somewhat limited differences between industries, differing from previous literature. We contribute by offering evidence from the Swedish market that the impact of these determinants is not uniform but partly shaped by industry-specific characteristics.

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1. Introduction

A long-standing concern in the field of corporate finance has been understanding how companies finance their operations and investments through a mix of debt and equity. The capital structure puzzle has not yet been solved despite several decades of theoretical developments and empirical research. As Frank and Goyal (2009) remarked: "*The factors that drive this decision remain elusive despite a vast theoretical literature and decades of empirical tests*.". This ongoing uncertainty underscores the importance of continuing to explore the factors that shape corporate financing decisions.

Despite Modigliani and Miller (1958)'s foundational proposition that capital structure is irrelevant under perfect capital markets, the real-world importance of capital structure remains one of the most debated and empirically studied topics in corporate finance. Frank and Goyal (2024) emphasize that capital structure decisions made by firms are not done randomly, but as a response to conditions, costs, and opportunities. Financing decisions are interconnected with corporate decisions, such as investments, and to properly understand these corporate decisions, an understanding of firm financing is required. Moreover, governments greatly affect the costs and benefits of leverage through regulations, taxes, and other policies. As these policies affect how firms will respond and react, it is crucial to understand capital structure to be able to evaluate the policies.

Stemming from the work of Modigliani and Miller (1958), a vast array of papers further adding pieces to the capital structure puzzle has been written and tested. One such paper is Daskalakis et al. (2023), which uses interaction effects to investigate whether firm-specific determinants differ between industries. Their method deviated from previous influential papers, opting for a panel data regression with interaction terms between the determinants and industry dummies (Daskalakis et al., 2023). They found that profitability, asset tangibility and liquidity negatively affect leverage across most industries, size generally increase leverage, while non-debt tax shield effects vary by industry. This supports established theories such as the pecking order and trade-off theories and highlights industry-specific differences in determinants.

This thesis builds upon the findings and methodology laid out by Daskalakis et al. (2023) by examining the same determinants across industries in Sweden. Using a balanced panel dataset of 6,000 Swedish firms from four structurally important industries being, Manufacturing, Construction, Information technology and retail, over the period 2018-2023, we estimate fixed effects and random effects models. To capture slope differences across industries, we include interaction terms between capital structure

determinants and industry dummies. We contribute to the field by examining Swedish companies and industries, further exploring how determinants affect leverage across industries, two areas that have not been heavily studied before. By using a balanced panel dataset and using both random- and fixed effects models, we argue that our approach improves upon the methodology of Daskalakis et al. (2023), offering more reliable findings.

To fully understand both the findings and the reasoning for the methodology chosen in this paper, a basic understanding of the underlying theories and prior influential papers is essential. While no universal theory of capital structure exists, a number of conditional theories have been developed to explain capital structure decisions under varying circumstances. Most of them are built on Modigliani and Miller (1958), who proposed two propositions relying on perfect capital markets and the absence of taxes, bankruptcy costs, agency costs, and asymmetric information. Succeeding theories build on the Modigliani-Miller theorem by relaxing these assumptions. Modigliani and Miller (1963) extended their work to include corporate income taxes, finding that debt could increase firm value due to the interest tax shield.

The trade-off theory builds on the insight that debt creates a tax shield but also introduces financial distress. The theory states that a firm will find its optimal debt ratio by balancing the tax benefits of debt against the costs of financial distress (Kraus and Litzenberger, 1973; Myers, 2001). An advantage of debt is that the interest rate is a tax-deductible expense, lowering taxes through a tax shield. However, additional leverage increases the present value of distress costs, as investors price in the associated risks. A firm therefore aims to reach a point where the marginal value from tax shields is balanced by the present value of distress costs (Myers, 2001). Firms with substantial non-debt tax shields, such as depreciation, may use less debt since their need for tax benefits is already mitigated (DeAngelo and Masulis, 1980).

According to the trade-off theory, debt ratios will vary between firms depending on the characteristics of a firm, and since these characteristics differ between industries, the trade-off theory suggests an explanation for the difference between industries' capital structure. If a firm has a lot of tangible assets and income to shield, a higher ratio is predicted, this would suggest that capital intensive industries should exhibit higher leverage and industries with a lot of intangible assets and low profitability should have a lower target debt ratio (Brealey et al., 2011).

The pecking order theory, introduced by Myers and Majluf (1984), suggests that firms follow a pecking order where using internal funds are preferred over external financing.

If external financing is deemed necessary, debt is preferred over equity financing. The theory rests on asymmetric information, where managers possess more knowledge than investors. Issuing equity might signal overvaluation, leading to stock declines. To avoid this, firms prefer internal financing if possible, otherwise to issue debt. Thus, profitable and liquid firms rely more on internal funds and accumulate less debt. Unlike trade-off theory, which predicts a target debt ratio, pecking order theory implies that observed debt ratios reflect cumulative financing needs (Myers, 2001).

When debt capacity is exhausted, equity may still be viable as high-growth firms in industries such as tech with low tangible assets and high costs of financial distress have good reasons for equity issuing and will be received better by investors (Brealey et al., 2011). Smaller firms also often deviate from pecking order behavior (Frank and Goyal, 2003).

While the pecking order theory emphasizes asymmetric information, the agency theory by Meckling and Jensen (1976) addresses conflicts of interest when there is a separation of ownership and control. Managers, i.e. agents, may not act in the best interest of the shareholders, i.e. principals, if their interests are not aligned. This misalignment of interest might cause agency costs from damaging acts by the manager. Jensen (1986) presents the free cash flow theory which argues that debt can serve as a disciplinary mechanism for managers to mitigate these agency costs, as debt limits free cash flow and thereby prevents managers from frivolous spending.

Agency problems also arise between shareholders and debt holders. An issuance of debt creates an agency problem because shareholders have an incentive to take on risky projects with a large potential gain, since shareholders benefit from the upside and have a limited downside due to limited liability. The debtholders, on the other hand, do not gain more than their repayments and bear the downside. Due to this risk, debt gets more expensive, as debt holders are aware of this risk. This problem is called the asset substitution effect and creates an agency cost of financing with debt. In industries where the asset substitution problem is less present, we should see higher levels of leverage (Harris and Raviv, 1991).

Several empirical studies have tested these theories. Auerbach (1985) applied OLS regressions on 200 firms from 1958 to 1977, finding leverage positively linked to earnings growth, variance, and depreciation, somewhat conflicting with agency theory. Titman and Wessels (1988) used LISREL and found that size negatively relates to short-term debt. Rajan and Zingales (1995) applied Tobit and first-difference OLS models, finding size and tangibility positively affect leverage, while profitability and market-to-book do not.

Song (2005) conducted a study on Swedish firms using a fixed effects model, a common approach in capital structure research (Lemmon et al., 2008; Frank and Goyal, 2009). The study found that profitability negatively and tangibility positively affected leverage, though tangibility was negatively related to short-term debt. The author also found that Swedish firms also showed greater reliance on short-term debt and followed the matching-maturity principle where long-term debt are used for fixed assets and short-term debt for non-fixed assets (Bevan and Danbolt, 2000; Song, 2005).

While many of these papers have shaped the capital structure field, they mostly assume homogeneous effects across industries. Previous literature has shown that leverage differs across industries and the reason why has been looked at in many papers. (MacKay and Phillips, 2005; Hall et al., 2000; Lemmon et al., 2008; Frank and Goyal, 2009). Scott (1977) differentiated between regulated and unregulated firms, with regulated firms referring to firms whose prices, operations, and financial policies are overseen by a regulatory body. Scott (1977) argued that tax shield was overemphasized in regulated industries, that optimal leverage for regulated firms was lower compared to unregulated and that bankruptcy was more costly for regulated firms due to the service disruption and public funding risk. MacKay and Phillips (2005) examined competitive industries and concentrated industries and look at measures of firms' position in their industry to help explain firm financial structure. They argue that firms' decisions are not made in isolation, rather, they are affected by how their industry peers act in a competitive industry. Kayo and Kimura (2011) analyzes three characteristics of industries, munificence, dynamism, and concentration. Dynamism reflects the business risk of the industry, munificence the capacity of the industry to support sustained growth. The authors find that industry-specific attributes like munificence and dynamism affect the capital structure, though to a lesser extent compared to that of firm-level factors, account for most of the variation in leverage.

Talberg et al. (2008) tested differences in determinants between industries in capital structure using cross-sectional data. The determinants analyzed were profitability, asset tangibility, firm size, age, and growth. The authors found that profitability was negatively related to leverage, supporting the pecking order theory, suggesting that firms prefer internal financing over debt. Asset tangibility was positively related to leverage, suggesting that, as firms increase in size, they will face lower bankruptcy risk and that firms with more assets on their books may use them to leverage the firm. A key contribution of this paper is its industry-level analysis, confirming that there are differences in capital structure across industries. Mature, more capital-intensive sectors generally have higher leverage, while less capital-intensive, technology-based sectors tend

to rely more on equity financing. This can be explained by its lack of tangible assets that they can use as collateral.

This ties into the discussion by Hall et al. (2000), whether determinants truly differ by industry or whether industry effects only reflect firm characteristics. The authors observed that industries with high average asset tangibility also exhibit the highest debt, suggesting that industry could be acting as a proxy for asset tangibility. To discover if determinants differ by industry, an interaction effect between industry and determinants of capital structure can be applied. The authors found that there seemed to be large variations across industries for most determinants, while growth, profitability, and asset structure had the same effect across industries.

Similar to this approach, Daskalakis et al. (2023) applied interaction terms in a panel data framework, demonstrating that the marginal effects of determinants vary across industries. This thesis builds on this literature and extends the work of Daskalakis et al. (2023), adding nuance through interaction terms and focusing on Swedish firms. We aim to clarify whether industry-specific conditions influence the firm-specific capital structure determinants.

The remainder of this thesis is organized as follows. The next section outlines the methodology, including variables and theoretical rationale. The estimation section explains model selection while the following data section provides details on the dataset and descriptive statistics. We then present our empirical results, finally we present the conclusions and implications for future research.

2. Method

This study aims to investigate how determinants of capital structure differ across industries in Sweden. This is done through regressions using panel data to assess the relationship between firm-specific financial variables and leverage. Our independent variables are profitability, tangibility, size, liquidity, and non-debt tax shields.

We utilize panel data, which has become common in empirical corporate finance research since the required data became more available (Song, 2005). However, similar studies to ours by Hall et al. (2000) and Talberg et al. (2008) utilize cross-sectional data. Panel data has the advantage of being able to capture the dynamic relationship of the capital structure and not just a snapshot of it, which is the case with cross-sectional data

(Wooldridge, 2010). According to Frank and Goyal (2024), there are concerns of omitted bias with cross-sectional tests from correlation with unobservable factors. Panel data allow us to control for this unobserved heterogeneity through the use of fixed effects (Wooldridge, 2010).

Given the aim of looking at how our determinants affects the capital structure across industries, we follow Daskalakis et al. (2023) and Hall et al. (2000) by including industry interaction effects between industry dummies and each of the independent variables. This allows us to test whether the determinants affect leverage differently across industries, by allowing slope coefficients to vary by industry.

Another approach would be that of Talberg et al. (2008), which runs separate regressions for each industry and one pooled regression with dummy variables for each industry. We believe the Daskalakis et al. (2023) and Hall et al. (2000) approach offers a better way to compare industries since it gives more nuances to industries by allowing for different slope coefficients for each industry. By using interaction effects, we recognize that a firm's industry belonging will affect firm characteristics, which in turn affect capital structure decisions. Talberg et al. (2008) assumes that the slope coefficients between the determinants and leverage are the same across industries. With interaction effects, we can statistically test if a determinant affects leverage differently between industries.

Year-dummies are included in the model, once again in similarity with the work of Daskalakis et al. (2023). Year dummies were used to capture potential year differences. The period tested in this paper (2018-2023) was a time with heavy fluctuation and turmoil, heavily affecting the economic landscape in Sweden. The historically low interest rates started increasing in 2022, and the COVID-19 pandemic and Russia's war in Ukraine all affected the Swedish economy, leading to the year dummies being included in the model to make sure these differences do not influence the coefficients of interest in this paper.

Furthermore, when using dummies, one category must be omitted and serve as reference category to avoid the dummy variable trap (Hirschberg and Lye, 2001). In our model, Information Technology will be the base industry, and 2018 will be the base year. The base industry and year was chosen arbitrarily.

In Kumar et al. (2017) review of the research field on capital structure determinants in the past 40 years, they observe that among the most common determinants are size, asset tangibility, profitability, liquidity, and non-debt tax shield. These are the independent variables we base our study on, this is also the variables used by Daskalakis et al. (2023). We next define each independent variable and discuss its theoretical expected effect on

leverage.

Profitability is included in the model to take into account the pecking order theory. According to the pecking order theory, profitable firms will borrow less as they have sufficient internal funds and therefore don't need to raise external financing. This is because internal financing avoids the adverse selection costs associated with debt and equity issuance. Therefore, according to the pecking order theory, there will be a negative relationship between profitability and leverage (Brealey et al., 2011). The negative association is found in most empirical research (Hall et al., 2000; Talberg et al., 2008; Daskalakis et al., 2023; Wald, 1999; Rajan and Zingales, 1995).

The trade-off theory predicts that profitable firms benefit from borrowing more since tax shields will have a larger effect, and that profitable firms also have a lower risk of financial distress. Therefore, profitability should be positively related to leverage according to the trade-off theory (Myers, 2001). From the perspective of the free cash flow theory, a profitable firm can use debt as a disciplinary mechanism by limiting managers from using it for empire-building projects. Therefore, the theory predicts a positive relationship between debt and profitability to reduce the agency cost of equity (Delcoure, 2007; Jensen, 1986). The profitability variable in our regression is defined as EBIT/Total Assets, which is a commonly used proxy (Daskalakis et al., 2023; Frank and Goyal, 2009),

$$\text{Profitability} = \frac{\text{EBIT}}{\text{Total Assets}} \tag{1}$$

A large company can more easily borrow as they tend to default less often and is better diversified, therefore, **Size** can be seen as an indirect proxy of bankruptcy (Rajan and Zingales, 1995). Warner (1977) found that bankruptcy costs decreased as a company's size increased in certain industries. The lowered bankruptcy costs should allow firms to take on more debt, and size should therefore have a positive relationship with leverage according to the trade-off theory (Alipour et al., 2015). Huang (2006) argue that large firms might have advantages of economies of scale and bargaining power over creditors, leading to lower costs of issuing debt.

On the other hand, Huang (2006) also argue that firm size could be a proxy for the information outside investors have. Larger firms tend to provide more information to lenders and outside investors compared to smaller firms. According to the pecking order theory, a decrease in asymmetric information should make equity financing more attractive than issuing debt, implying a negative relationship between size and leverage.

According to Kumar et al. (2017), the most common way of measuring size is taking the log of sales or total assets. We follow Huang (2006) and define it as a log of sales to avoid the probability of spurious correlation. This avoids the problem of multicollinearity, which might arise since most of the ratios used are scaled by total assets (Kühnhausen and Stieber, 2014). The log form is common in corporate finance research to mitigate the high skewness of firm size data (Dang et al., 2019). The equation for the size variable is the following:

$$\text{Size} = \log(\text{Sales}) \tag{2}$$

Liquidity is included in the model as a proxy for how well the firm manages its working capital. Liquidity is measured by the current ratio as that is the most common definition used in the literature Kumar et al. (2017).

According to the pecking order theory, there should be a negative relationship between liquidity and leverage since internal financing is preferred over external financing. A high liquidity ensures that there are internal funds for the operations and therefore external financing is not needed (Kumar et al., 2017). While liquidity is generally seen as a financial strength and should facilitate external financing, Myers and Rajan (1998) argues that high liquidity might increase managerial discretion and make it harder for creditors to ensure that the funds are being used decisively, raising agency costs. This paradox of liquidity can lead to lower debt financing. Therefore, agency theory also predicts a negative relationship, if agency costs of liquidity are high, as that will lead to a decrease in access to external financing from creditors (Myers and Rajan, 1998). The negative association is found in most empirical research (Deesomsak et al., 2004; Daskalakis et al., 2023; Sheikh and Wang, 2011). Liquidity is calculated by dividing the current assets by the current liabilities.

$$\text{Liquidity} = \frac{\text{Current Assets}}{\text{Current Liabilities}} \tag{3}$$

Non-debt tax shields are tax deductions for items other than the regular interest tax shield, such as depreciation or investment tax credits. DeAngelo and Masulis (1980) argues that it can be seen as a substitute for the regular debt tax shield and reduce the marginal benefit of debt. They predict that leverage will differ across industries with differing non-debt tax shields. This is in agreement with the trade-off theory, firms with substantial non-debt tax shields gain fewer benefits from debt-related tax-shields.

Therefore, we should see an inverse relationship between leverage and non-debt tax shield. This negative relationship has been confirmed in several empirical papers (Deesomsak et al., 2004; DeAngelo and Masulis, 1980; MacKie-Mason, 1990).

However, there are also numerous empirical studies that show a positive correlation between leverage and non-debt tax shield (Harris and Raviv, 1991; Daskalakis et al., 2023; Bradley et al., 1984; Delcoure, 2007). Bradley et al. (1984) argue that this could be because firms with high non-debt tax shield would also have a large amount of tangible assets, which are predicted to be positively correlated with leverage. Similarly, Wald (1999) states that physical plants have to be included in the regression since they are highly correlated with non-debt tax shield, which we capture in our tangibility variable. (Wald, 1999) finds in his regression that, non-debt tax shield shows a negative correlation with physical plants included and a positive correlation when excluded.

Non-debt tax shield is calculated by dividing depreciation by total assets in accordance with previous research (Daskalakis et al., 2023; Kumar et al., 2017; Deesomsak et al., 2004; Frank and Goyal, 2003; Titman and Wessels, 1988).

$$\text{Non-debt tax shield} = \frac{\text{Depreciation}}{\text{Total Assets}} \tag{4}$$

Firms with plenty of **tangible assets**, i.e., high asset tangibility, will have lower costs of financial distress as it can serve as collateral and there is more value to retain in liquidation. Therefore, according to the trade-off theory, tangibility should have a positive relationship with leverage (Rajan and Zingales, 1995). Agency theory also suggests a positive relationship through the asset substitution problem. A high asset tangibility will lower agency costs in the shareholder and debtholder relation as it provides security for lenders, as it is easier to monitor and liquidate. This decrease in agency costs should facilitate borrowing for companies with high tangible assets as lenders will be more willing to supply loans (Harris and Raviv, 1991).

Harris and Raviv (1991) offers a different perspective on the effect of tangibility through the lens of the pecking order theory. The authors suggest that firms with fewer tangible assets may have difficulty raising equity due to the increased information asymmetry that lower tangibility often leads to. As a result, these firms are more likely to rely on debt financing, implying that lower asset tangibility could be associated with higher leverage.

Empirical studies generally support the view of the trade-off theory and report positive

relationships with leverage. Wald (1999) as well as Rajan and Zingales (1995) find a positive association between tangibility and leverage across several countries. Song (2005) and Hall et al. (2000) find a positive association between tangibility and long-term debt, but a negative relationship to short-term debt.

Tangibility is calculated as the ratio of net property and plant equipment to total assets in accordance with previous research (Daskalakis et al., 2023; Delcoure, 2007; Lemmon et al., 2008).

$$\text{Tangibility} = \frac{\text{PP\&E}}{\text{Total Assets}} \tag{5}$$

Regarding the dependent variable **leverage**, most similar studies use some form of debt ratio. Previous studies have mainly looked at some form of leverage ratio with total liabilities to total assets being the most common one (Alipour et al., 2015). Widely used dependent variables are also total debt, and to split total debt into only short- or long-term debt.

Rajan and Zingales (1995) discusses several different measures of leverage and concludes that total debt over capital is an accurate reflection of past financing decisions and that total liabilities over total assets might overstate leverage. Welch (2011) questions the exclusion of operating liabilities from definitions using total debt. He argues that it underestimates a firm's true leverage by ignoring non-debt obligations. Total liabilities to total assets aligns more closely with the economic reality of leverage by capturing the full extent of a firm's commitments. A creditor will consider total liabilities of a firm, not only long-term debt. Meaning that liabilities will affect a firms debt capacity (Huang, 2006). We follow Welch (2011) and define our leverage variable as total liabilities over total assets, thereby also emulating Daskalakis et al. (2023).

$$\text{Leverage} = \frac{\text{Total liabilities}}{\text{Total Assets}} \tag{6}$$

According to Lööf (2003), the main measures of leverage are those based on either market values or book values. Market value of equity reflects expected future cash flows from the firm, while book value reflects historical performance. Furthermore, Titman and Wessels (1988) argue that both book and market values of debt are valid when examining determinants of capital structure, especially if market data are unavailable.

A study by Graham and Harvey (2001) surveying 392 CFOs indicates that firms don't

rebalance their capital structure based on market equity movements. This aligns with Myers and Majluf (1984), who argue that firms seem to operate with debt ratios based on book values. Many firms have market values far higher than book value, yet we don't see firms with debt levels proportional to their market value. Myers and Majluf (1984) argues that this is because book values serve as a proxy for the value of assets in place, while market values include intangible assets and growth opportunities that can't be used as collateral.

MacKay and Phillips (2005) reference Barclay and Smith Jr (1995), which showed that book leverage is preferable to use in regressions since using market values could spuriously correlate with explanatory variables. The authors run regressions with both market and book leverage and find no notable changes in results. Book-value leverage can be an appropriate stand-in for market-value leverage, according to Bowman (1980), as the cross-sectional correlation between market and book leverage is very large. While Fama and French (2002) and Welch (2004) argue that book and market leverage can yield different results, there is both theoretical and empirical justification for book values.

Studies close to ours, such as Talberg et al. (2008) and Hall et al. (2000), rely on the findings of Myers and Majluf (1984) and Graham and Harvey (2001) and use book values. This study will also use book values for the dependent variable leverage because of the lack of market values, since most of our firms are unlisted.

Table 1: Variable Definitions and Expected Relationships

Variable	**Definition**	**Expected Sign**
Profitability	EBIT / Total Assets	-
Size	log(sales)	+/-
Liquidity	Current Assets / Current Liabilities	-
Non-debt Tax Shield	Depreciation / Total Assets	-
Tangibility	Fixed Assets / Total Assets	+

Definition of variables and expected signs of effects on leverage, based on prior literature and theory.

The model includes the dependent variable leverage (LEV_{it}) for firm i in year t. It includes industry dummies for Manufacturing (M_{it}), Retail (R_{it}), and Construction (C_{it}), with Information Technology (IT_{it}) as the reference category. The explanatory variables used in the model, size ($SIZE_{it}$), profitability ($PROFIT_{it}$), non-debt tax shield ($NDTS_{it}$), tangibility ($TANG_{it}$), and liquidity (LIQ_{it}).

Interaction terms are included to capture whether the effects of these variables on leverage vary across industries. For example, ($M_{it} \times SIZE_{it}$) reflects how firm

size influences leverage within the manufacturing industry, and finally, (d_{2019} to d_{2023}) represents our year dummies.

Thus, the model specification is the following:

$$
\begin{aligned}
LEV_{it} = \beta_0 &+ \beta_{0m} M_{it} + \beta_{0r} R_{it} + \beta_{0s} C_{it} + \beta_1 \textit{SIZE}_{it} + \beta_2 \textit{PROFIT}_{it} + \beta_3 \textit{NDTS}_{it} \\
&+ \beta_4 \textit{TANG}_{it} + \beta_5 \textit{LIQ}_{it} \\
&+ \beta_{1m}(M_{it} \times \textit{SIZE}_{it}) + \beta_{1r}(R_{it} \times \textit{SIZE}_{it}) + \beta_{1s}(C_{it} \times \textit{SIZE}_{it}) \\
&+ \beta_{2m}(M_{it} \times \textit{PROFIT}_{it}) + \beta_{2r}(R_{it} \times \textit{PROFIT}_{it}) + \beta_{2s}(C_{it} \times \textit{PROFIT}_{it}) \\
&+ \beta_{3m}(M_{it} \times \textit{NDTS}_{it}) + \beta_{3r}(R_{it} \times \textit{NDTS}_{it}) + \beta_{3s}(C_{it} \times \textit{NDTS}_{it}) \\
&+ \beta_{4m}(M_{it} \times \textit{TANG}_{it}) + \beta_{4r}(R_{it} \times \textit{TANG}_{it}) + \beta_{4s}(C_{it} \times \textit{TANG}_{it}) \\
&+ \beta_{5m}(M_{it} \times \textit{LIQ}_{it}) + \beta_{5r}(R_{it} \times \textit{LIQ}_{it}) + \beta_{5s}(C_{it} \times \textit{LIQ}_{it}) \\
&+ \beta_6 d_{2019} + \beta_7 d_{2020} + \beta_8 d_{2021} + \beta_9 d_{2022} + \beta_{10} d_{2023} + u_{it}
\end{aligned}
\tag{7}
$$

The interpretation of the independent variables' effect on the dependent variable will differ from a simple regression model due to our interaction effects. For example, when looking at the marginal effect of size on leverage, we must add both the baseline coefficient and the interaction term coefficient. For the manufacturing industry, the marginal effect of size would be $\beta_{2\text{Size}} + \beta_2 m$, where $\beta_{2\text{Size}}$ is the baseline coefficient for size and $\beta_2 m$ the coefficient for the interaction term. The baseline coefficient applies to the reference industry, in our case Information technology, while the interaction term captures the effect in other sectors (Wooldridge, 2016; Daskalakis et al., 2023).

3. Estimation

An ordinary least squares regression in empirical capital structure research is likely to be misspecified as there are unobservable time-invariant components that are likely to be correlated with leverage. Some potential factors could be differences in technologies, market power, or managerial behavior, among others. To deal with this issue, a fixed effect model is commonly employed (Lemmon et al., 2008). According to Allison (2019), a fixed effect model is an econometric method that accounts for time-invariant characteristics of individual entities. Each entity gets its own unique intercept, capturing unobservable heterogeneity. This approach controls for omitted variable bias that could otherwise distort the model estimates. As a result, fixed effect models provide unbiased

estimates of the coefficients. However, it is important to note that the degrees of freedom decrease compared to a random effect model, as one degree of freedom is lost for each entity.

Alternatively the random effect model is also widely used in panel data research and allows researchers to control for unobserved heterogeneity across individuals, firms, or countries (Borenstein et al., 2010). Unlike the fixed effect model, the random effect model assumes that individual-specific effects are randomly drawn from a common distribution and are uncorrelated with the explanatory variables. One advantage is that time-invariant variables can be included in random effect models but not in fixed effect models (Wooldridge, 2010).

To determine which model is most appropriate, a Hausman test is regularly used. The Hausman test checks whether the unique errors in the fixed effect and random effect models are correlated with the regressors. If so, the random effect model is found to be inconsistent, and the fixed effect model is preferred (Wooldridge, 2010).

However, the Hausman test may be too simplistic for model selection. The Hausman test assumes homoskedasticity and no serial correlation, which may not hold in large panel datasets (Wooldridge, 2010). Therefore, we also apply the Mundlak test, originally developed by Mundlak (1978), which serves a similar role to the Hausman test but is valid with clustered standard errors. Rejection of the null hypothesis implies that the random effect assumptions are violated (Wooldridge, 2010).

Even if the Hausman and Mundlak test suggests that fixed effect model is more suitable, some researchers argue random effect may still be informative. Zafar et al. (2019) and Hsiao (2014) suggest that random effect models may provide less biased estimates in the presence of data issues such as measurement errors, which are common in large firm-level datasets (Frank and Goyal, 2003). If classical error assumptions hold, fixed effect has been found to increase measurement errors (Wooldridge, 2010). Based on this argument, we present results from both fixed effect and random effect models.

Clustered standard errors help ensure that the estimators remain unbiased under heteroskedasticity and serial correlation. This adjustment maintains the independence assumption across clusters (Abadie et al., 2023). While alternatives like bootstrapping exist, Abney (2002) notes that with large samples, bootstrapping may introduce bias. We clustered standard errors at the firm level. An alternative would be two-way clustering by firm and time. Petersen (2008) warns that if residuals within a year are correlated across firms, firm-only clustering may be insufficient. However, two-way clustering requires a

sufficient number of time periods. Thompson (2011) suggests that at least 25 time periods are needed for reliable standard errors. Petersen (2008) found that when there are only a few clusters of years, clustering by firm gives almost identical results as if clustering by both.

4. Data

All the data was obtained through the company database Retriever Business, which provides financial information on Swedish firms. A large sample of Swedish firms was collected, ranked by revenue and distributed across our selected industries. These industries were Construction, Information Technology, Retail, and Manufacturing, all industries found in large quantities in the corporate sector. The data was collected through the financial years of 2018 to 2023 and the industries were identified by using Retriever Business's industry classification, which provides high-quality and accurate classifications. Both listed and unlisted firms was included in the data set, this in accordance with Daskalakis et al. (2023).

Banks, insurance, real estate, and utilities sectors were excluded from the data set, as per the methodology adaptod by previous studies (Daskalakis et al., 2023; Talberg et al., 2008; Gaud et al., 2005; Fama and French, 2002), due to these industries being highly regulated regarding their capital structure and financing. 6,000 of the largest unique companies were included in the data set, equally distributed with 1,500 firms per sector, across our four chosen sectors, in accordance with the data selection by Daskalakis et al. (2023).

To avoid some outliers or data errors disproportionately influencing the regression, a decision to winsorize the independent variables has been chosen. Outliers were handled in accordance with the methodology laid out by Frank and Goyal (2009). The authors recommended handling outliers by either truncating the observation using a rule of thumb, winsorizing the variables, or through robust regressions. In this paper, a decision was made to winsorize variables constructed as ratios, all variables except size, to one percentile on both sides. This decision was made to combat the outliers potentially introducing bias into the model, while also acknowledging that the observation exists. Frank and Goyal (2009) argue that this method is more consistent compared to using a rule of thumb to truncate the observations. However, Brownen-Trinh (2019) argues that winsorizing might replace valid extreme observations rather than correcting for data errors, which may introduce bias and decrease the validity of regression. We argue that

this is less of a problem for our dataset since we only winsorize at the 99th and 1st percentile and don't winsorize our size variable since it's log transformed and outliers would be expected.

We construct a balanced panel rather than an unbalanced panel, diverging from Daskalakis et al. (2023)'s approach. This choice is supported by Nickell (1981), who warned that panels with a small number of observations per individual can create bias, especially in fixed effects models. The uniformity that balanced panels provides by including all observations across all time periods for each firm improves the efficiency of estimators, reduce bias and strengthen inference (Baltagi, 2008).

Therefore, only firms with data for all included variables were considered and firms with missing observations for critical variables were completely removed from the data set, thus removing the potential of these observations biasing the results. Frank and Goyal (2009) argues that this is an effective and simple way to deal with such observations, especially since it is rather common in data sets containing financial information. Though the authors argue that it may create bias if the missing data is related to the process being studied, instead suggesting that another alternative to deal with missing data is to use a process of imputation, a method recommended by Little and Rubin (2002). Removing the data is still chosen as this is a straightforward method and common practice in similar studies, and due to the potential biases introduced when using the process of imputation. Furthermore, Frank and Goyal (2003) found that the result in their regression, similar to the one proposed in this paper, was robust both when removing missing observations and when carrying out the process of imputation.

A limitation of this approach is survivorship bias. Bryzgalova et al. (2025) found that around 70% of firms have incomplete data, showcasing the large-scale problem for studies where complete financial information is key. A robustness check is done for checking whether our result is an artifact of using a balanced panel, which can be seen in Appendix B. Another potential limitation of our data selection process is bias towards larger companies, something that Titman and Wessels (1988) also highlighted in their data selection process, opting to only include observations containing all data, running a regression with balanced panels.

Analyzing the summary statistics in Table 2, we observe noticeable discrepancies between industries across nearly all variables. Generally, the Retail and Construction sectors exhibit the highest leverage, with a median of over 50%, while the Information technology and Manufacturing sectors display lower leverage ratios. Generally, leverage was relatively high across all industries compared to prior literature (Frank and Goyal,

Table 2: Descriptive Statistics by Industry (Winsorized Variables)

	ALL	R	IT	C	M
Leverage					
Mean	0.522	0.541	0.488	0.584	0.474
Median	0.519	0.539	0.467	0.593	0.465
Min	0.058	0.058	0.058	0.058	0.058
Max	0.987	0.987	0.987	0.987	0.987
St. Dev.	0.229	0.225	0.248	0.205	0.219
Size					
Mean	10.683	10.742	9.846	10.885	11.258
Median	10.422	10.501	9.478	10.564	10.762
Min	1.099	1.946	1.099	3.401	1.099
Max	20.130	17.837	18.867	18.738	20.130
St. Dev.	1.487	1.134	1.553	1.193	1.627
Profitability					
Mean	0.118	0.124	0.121	0.120	0.109
Median	0.112	0.118	0.132	0.108	0.102
Min	-0.617	-0.617	-0.617	-0.617	-0.617
Max	0.634	0.634	0.634	0.634	0.634
St. Dev.	0.189	0.153	0.273	0.155	0.146
Non-Debt Tax Shield					
Mean	0.028	0.018	0.029	0.033	0.033
Median	0.012	0.006	0.004	0.014	0.025
Min	0.000	0.000	0.000	0.000	0.000
Max	0.206	0.206	0.206	0.206	0.206
St. Dev.	0.040	0.028	0.051	0.044	0.033
Tangibility					
Mean	0.254	0.241	0.218	0.244	0.314
Median	0.186	0.179	0.087	0.158	0.293
Min	0.000	0.000	0.000	0.000	0.000
Max	0.890	0.890	0.890	0.890	0.890
St. Dev.	0.239	0.220	0.264	0.242	0.217
Liquidity					
Mean	2.260	2.181	2.539	1.823	2.495
Median	1.721	1.665	1.821	1.516	1.999
Min	0.346	0.346	0.346	0.346	0.346
Max	13.015	13.015	13.015	13.015	13.015
St. Dev.	1.882	1.806	2.321	1.327	1.852

Industry abbreviations: R = Retail, IT = Information Technology, C = Construction, M = Manufacturing. Variables defined as ratios are winsorized at the 1st and 99th percentiles to mitigate the effect of outliers.

2009). This outcome can be attributed to the method used for calculating leverage, total liabilities divided by total assets. Though, these seemingly high leverage levels are in line with the findings of Daskalakis et al. (2023), using the same leverage equation as us, suggesting that the numbers are reasonable.

The mean and median values for the firm size variable are clustered between 10 and 11, corresponding to approximately 22,000 – 60,000 $(e^{10} - e^{11})$ TSEK. This suggests that the dataset includes a large number of relatively smaller firms. Even though the firms were selected among the 1,500 largest per sector with available data, the inclusion of unlisted companies contributes to the representation of smaller firms.

In terms of profitability, the Information Technology sector shows the highest median profitability, indicating that firms in this sector tend to generate higher EBIT in relation to their total assets. However, mean profitability reveals a different picture, the Retail sector shows the highest average profitability, followed by Construction and Information Technology. The divergence between mean and median values suggests the presence of outliers or skewness, particularly in the Retail and Construction sectors, where a few firms may be driving higher averages.

The non-debt tax shield variable also varies across industries, which aligns with expectations due to differences in asset structures and depreciation rates. Manufacturing firms report the highest non-debt tax shield, implying they have a larger proportion of depreciable assets relative to their total asset base.

A similar pattern is evident in asset tangibility. Manufacturing reports the highest mean and median tangibility, while the Information Technology sector reports the lowest, reflecting the industry's reliance on intangible assets. This supports the notion that capital structure determinants such as tangibility vary systematically by sector.

Finally, liquidity shows substantial industry-level variation. The median liquidity ratio is highest for Manufacturing and lowest for Construction. This once again underscores the diverse capital needs and operational cost structures that distinguish these industries.

Table 3: One-way ANOVA F-statistics for Variable Differences Across Industries

Variable	**F-statistic**	**p-value**
Leverage	452.71	0.000
Profitability	11.80	0.000
Tangibility	278.32	0.000
Size	1660.82	0.000
Liquidity	286.40	0.000
Non-debt Tax Shield	285.24	0.000

The null hypothesis of equal means is rejected for all variables, indicating that the variables differ across industry groups.

To assess whether the firm-level variables differs between the industries a one-way ANOVA test is applied. Table 3 displays the result and reveal that there are statistically significant differences in firm-level characteristics across industries.

5. Results

This section presents the empirical results from both fixed effects and random effects models. By estimating both specifications, we evaluate the stability and reliability of our model. The fixed effects model controls for unobserved time-invariant heterogeneity, while the random effects model assumes no such correlation.

The coefficients for the interaction terms indicate the difference from the base industry. For instance, examining Table 4, in the fixed effects model, the profitability for Retail differs by -0.0848 from the base industry, which has a coefficient of (-0.1865). Hence, the marginal effect of profitability in Retail is -0.2713 (= -0.1865 - 0.0848). The marginal effect for each industry for both the fixed- and random effect regression is presented in Table 6 and Table 7 respectively.

Table 4: Fixed Effects Regression Results

Variable	**All**		**Construction**		**Retail**		**Manufacturing**	
	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val
SIZE	0.0123	0.009***	0.0123	0.060*	-0.0071	0.307	0.0000	0.997
PROFIT	-0.1865	0.000***	-0.1231	0.000***	-0.0848	0.000***	-0.0487	0.018**
NDTS	0.0975	0.291	-0.2338	0.077*	-0.1294	0.454	-0.2696	0.072*
TANG	-0.1630	0.000***	0.0389	0.225	0.0682	0.028**	0.0703	0.025**
LIQ	-0.0502	0.000***	-0.0070	0.193	-0.0047	0.241	0.0033	0.299
CONSTANT	0.5854	0.000***						

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at firm level. The industry columns represents the interaction effects between the industry dummies and variables. The whole regression result with dummies included can be found in appendix C: Full regressions.

Base industry: Information Technology; Base year: 2018.

Table 5: Random Effects Regression Results

Variable	**All**		**Construction**		**Retail**		**Manufacturing**	
	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val
SIZE	0.0126	0.000***	0.0033	0.478	-0.0123	0.012**	-0.0112	0.009***
PROFIT	-0.1915	0.000***	-0.1212	0.000***	-0.0949	0.000***	-0.0442	0.018**
NDTS	0.0860	0.276	-0.1318	0.242	0.0538	0.718	0.1423	0.267
TANG	-0.1900	0.000***	0.0451	0.087*	0.0464	0.074*	0.0619	0.018**
LIQ	-0.0546	0.000***	-0.0099	0.067*	-0.0076	0.035**	-0.0001	0.968
CONSTANT	0.5935	0.000***						

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at firm level. The industry columns represents the interaction effects between the industry dummies and variables.The whole regression result with dummies included can be found in appendix C: Full regressions.

Base industry: Information Technology; Base year: 2018.

Table 6: Marginal effects per industry fixed effect

Variable	Information Technology (Base)	Construction	Retail	Manufacturing
SIZE	0.0123	0.0246	0.0052	0.0123
PROFIT	-0.1865	-0.3096	-0.2713	-0.2352
NDTS	0.0975	-0.1363	-0.0319	-0.1721
TANG	-0.1630	-0.1241	-0.0948	-0.0927
LIQ	-0.0502	-0.0469	-0.0549	-0.0469

*M*arginal effects are calculated by summing main effects and interaction terms from Table 4. The marginal effect represents the total effect of each variable within each industry
Base industry: Information Technology; Base year: 2018.

Table 7: Marginal effects per industry for random effect

Variable	Information Technology (Base)	Construction	Retail	Manufacturing
SIZE	0.0126	0.0159	-0.0014	0.0014
PROFIT	-0.1915	-0.3127	-0.2864	-0.2357
NDTS	-0.0860	-0.0458	-0.1398	-0.0265
TANG	-0.1900	-0.1499	-0.1436	-0.1280
LIQ	-0.0546	-0.0645	-0.0622	-0.0547
Industry Dummy	Omitted	0.0375	0.1769	0.0978

*M*arginal effects are calculated by summing main effects and interaction terms from Table 5. The marginal effect represents the total effect of each variable within each industry
Base industry: Information Technology; Base year: 2018.

Tables 4 and 5 display the regression results from our random effects and fixed effects models, respectively. It should be noted that the fixed effects model removes time-invariant variables; therefore, our industry dummies are omitted and absorbed into the firm-specific intercept (Wooldridge, 2010). However, differences between industries are still captured by the interaction effects between the industry dummies and explanatory variables. The results of the dummies can be found in the full regressions in Appendix C.

In contrast, the random effects model allows for the estimation of both interaction terms and industry dummies. In our random effects model, the industry dummies for Retail and Manufacturing are statistically significant, indicating that average leverage levels in these industries differ from the base industry Information Technology. This contrasts with Daskalakis et al. (2023), who found all industry dummies to be statistically insignificant and thus decided to exclude them from their final model. They interpret

insignificant dummies as "concurrent regressions," where regression lines have identical intercepts but different slopes across industries.

The fixed and random effects models both yield similar results in significance and magnitude of coefficients, except for a few noticeable differences, as showcased in Table 8. The random effects model find that size are statistically significant for the Retail and Manufacturing, and liquidity for Retail which differs from the fixed effect model. The coefficients for the significant variables are also similar, as can be seen in Table 8. Stated by Frank and Goyal (2009), similar results in both random and fixed effect models help explain both cross-sectional variation between firms and temporal variation within firms. This similarity suggests that the variables are generalizable both across time, and across firms, increasing our confidence in our findings. Moreover, it also suggests that the conclusions regarding the underlying theories explaining the effects are strong, and applicable in both cross-industry and multi-period contexts.

Table 8: Coefficient Difference Between Fixed and Random Effects Estimates

Variable	**Fixed Effect**	**Random Effects**	**Percentage Change (%)**
Size	0.0123	0.0126	2.44%
Profitability	-0.1865	-0.1915	-2.68%
NDTS	0.0975	0.0860	-11.79%
Tangibility	-0.1630	-0.1900	-16.56%
Liquidity	-0.0502	-0.0546	-8.76%
Size × Retail	-0.0071	-0.0123	-73.24%
Size × Manufacturing	0.0000	-0.0112	-100%
Size × Construction	0.0123	0.0033	-73.17%
Profitability × Retail	-0.1231	-0.1212	1.54%
Profitability × Manufacturing	-0.0848	-0.0949	-11.91%
Profitability × Construction	-0.0487	-0.0442	9.24%
NDTS × Retail	-0.2338	-0.1318	43.63%
NDTS × Manufacturing	-0.1294	0.0538	141.58%
NDTS × Construction	-0.2696	-0.1423	47.22%
Tangibility × Retail	0.0389	0.0451	15.94%
Tangibility × Manufacturing	0.0682	0.0464	-31.96%
Tangibility × Construction	0.0703	0.0619	-11.95%
Liquidity × Retail	-0.0070	-0.0099	-41.43%
Liquidity × Manufacturing	-0.0047	-0.0076	-61.70%
Liquidity × Construction	0.0033	-0.0001	-103.03%

The percentage change is calculated to express the relative difference in the random effects estimate compared to the fixed effects estimate. Positive values indicate that the random effect model estimate is larger in magnitude than the fixed effect estimate, and negative values indicate it is smaller.

Given that the models provide similar results, and supported by the Hausman and Mundlak tests results rejecting the null hypothesis (See Appendix A.3), the fixed effects model is preferred. Therefore we will mainly focus on the fixed effect results.

Profitability is statistically significantly negatively related to leverage in both models, with a coefficient of -0.18 in the fixed effects model, as seen in Table 5. A 1% increase in EBIT/Total Assets reduces leverage by 0.18%. This finding aligns with the pecking order theory, which posits that profitable firms prefer internal financing over debt. This result is in line with prior findings (Daskalakis et al., 2023; Rajan and Zingales, 1995; Talberg et al., 2008). The interpretation is the same for the rest of the variables except for size as it

is the log-transformed total sales. The size variable is interpreted as a 1% increase in sales would increase the size variable by 0,01. Meaning that with a coefficient of 0.012, a 1% increase in sales would increase the leverage by 0.012*0.01 = 0.00012 (0.012 percentage points).

Profitability is the only variable where the interaction terms are significant across all industries, suggesting that industry context particularly affects this determinant. The interaction terms show that all industries exhibit stronger negative marginal effects compared to the base industry, especially Construction and Retail, suggesting a stronger preference for internal financing in these sectors. Talberg et al. (2008) also found that Construction specifically was highly effected by profitability. The author suggested that this was an effect of construction industry being highly affected by economic downturns, leading them to rely on internal financing during economic downturns. Comparing with our descriptive statistics we notice that Construction firms are highly levered, indicating that the baseline leverage is high but profitable firms in the industry tend to prefer internal financing.

Firm size is positively related to leverage (0.01), as seen in Table 4. This positive relationship suggests that larger firms are more inclined to take on debt, in line with the trade-off theory. Larger firms are generally more diversified, face lower bankruptcy risk and benefits from economies of scale in public financing (MacKay and Phillips, 2005; Daskalakis et al., 2023; Barclay and Smith Jr, 1995). Hall et al. (2000) argues that a positive size coefficient is explained by lenders higher propensity in loaning to larger firms as they generally are perceived as less risky. Our findings contrasts the explanation by Huang (2006), who proposed that in a pecking order context, larger firms suffer less from information asymmetry which should make equity financing more attractive. Our findings suggests that trade-off theory has higher explanatory power compared to pecking-order theory, in the context of size. While the variable is statistically significant, the economic effect is small, suggesting that changes in total sales has limited effect on leverage.

The interaction terms are insignificant in the fixed effect model as seen in Table 4, in contrast to Daskalakis et al. (2023), who found significant effects across all industries. This could suggest that the size benefits, such as easier access to financing from lower perceived risk, does not differ between industries. In contrasts to the findings by Daskalakis et al. (2023), who found size to be significant effects across all industries, our results imply that size is valued equally across industries.

In the random effects model, Table 5 shows the interaction terms for Retail (-0.01) and Manufacturing (-0.01) to be statistically significant, but their marginal effects remain

close to zero as seen in table 7, implying a weak correlation between size and leverage in these sectors while still noting that the main effect is significant and positive, the same as in the fixed effect. This indicates that when we allow for differences between firms we see that, smaller firms tend to take on less debt than larger firms, but changes in size within the firms does not affect leverage, in these industries.

Liquidity shows a statistically significant and negative relationship with leverage (-0.05). This should be interpreted as a one-unit increase in the current ratio being associated with a 5 percentage point reduction in leverage. The findings support the pecking order theory, in which liquid firms prefer internal resources over debt to finance operations due to asymmetric information and no transaction costs (Myers, 1989). It also supports the agency theory, where high liquidity might increase managerial discretion and reduce creditor control, raising agency costs. Our findings align with the findings of Deesomsak et al. (2004) and Daskalakis et al. (2023), although the latter found significant effects across all industries tested, while none of our interaction terms in the fixed effect model were significant, suggesting that this relationship does not differ between industries, contrary to the findings of Daskalakis et al. (2023) in Greek firms.

In contrast, the random effects model in Table 5 finds the interaction term for Retail (-0.01) to be significant, suggesting that firms with higher liquidity is associated with lower leverage in Retail firms to a slightly greater extent than in Information technology.

Non-debt tax shield is statistically insignificant at the $p < .05$ level, illustrated in Table 4 and 5. This suggests that depreciation-related tax benefits are not a major driver of capital structure among Swedish firms. Song (2005) also found non-debt tax shield to be statistically insignificant in Swedish firm, though when disaggregating debt into long-term and short-term the variable became significant. But it contrasts earlier studies which indicated that non-debt tax shield would make firms prefer less debt due to reduced need for tax-shields (Daskalakis et al., 2023; Shah and Khan, 2007; MacKay and Phillips, 2005; DeAngelo and Masulis, 1980).

None of the interaction terms for non-debt tax shield are significant in either model, indicating that non-debt tax shield does not impact leverage differently across industries. This does somewhat align with the findings of Daskalakis et al. (2023), finding that among the variables, non-debt tax shield had the lowest explanatory factor. Though, the authors still found that the interaction terms were significant for some industries, meaning that they found support that non-debt tax shield still had explanatory power and that the determinant differed across industries.

As seen in Table 4, tangibility is significantly negatively associated with leverage (-0.16). Interaction terms for Retail (0.07) and Manufacturing (0.07) are significant, indicating a slightly less negative marginal effect. This contradicts the trade-off and agency theories, which predict a positive relationship. Instead, it supports Hall et al. (2000) and Song (2005), who found a negative relationship between tangibility and short-term debt. This result supports the maturity matching principle (Bevan and Danbolt, 2000), suggesting that firms use short-term debt to finance non-fixed assets.

Our findings align with Daskalakis et al. (2023), who explain the negative correlation in a pecking order context, firms with tangible assets may generate more internal funds, reducing the need for external financing. According to Harris and Raviv (1991), firms with fewer tangible assets face more asymmetric information and may use more debt, explaining the highly negative marginal effect for Information technology and the relatively less negative one for Manufacturing.

In the random effects model shown in Table 5, tangibility remains negatively significant (-0.19). The interaction term for Manufacturing (0.06) is significant at $p < 0.05$, while Retail (0.05) and Construction (0.05) are weakly significant at $p < 0.1$, indicating Manufacturing has the least negative marginal effect.

The year dummies in both models, which can be seen in the full regressions in Appendix C, are negative and statistically significant, suggesting that leverage ratios declined relative to the base year, 2018. This may reflect broader macroeconomic shifts, such as increasing interest rates and economic uncertainty. The signs of the year dummies are in contrast to the findings of Daskalakis et al. (2023), finding that the leverage ratios increased during the time period they tested. The authors explained this as an effect of the economic conditions of Greece improving economy during the time period tested. This further strengthens the argument that our results reflect the broader economic shift during the time period tested.

6. Conclusion

This study examined determinants of capital structure in Swedish firms across four industries, Information Technology, Retail, Manufacturing and Construction, during the period 2018 to 2023. Using firm-level panel data, we analyzed how size, profitability, non-debt tax-shields, tangibility and liquidity influence capital structure. To assess whether these effects vary across industries we applied interaction effects between the

determinants and industry dummies across our chosen industries. Both random effect and fixed model were employed, without any major differences in results.

For the main coefficients, our findings show that profitability, tangibility, and liquidity are negatively and statistically significantly associated with leverage while firm size is positively associated. Non-debt tax shield was not found to be statistically significant. The findings are largely aligned with established theories and literature. The negative relationship for profitability and liquidity are in line with the pecking order theory, indicating that Swedish firms prefer to use internal financing rather than debt as capital. Size is in line with the trade-off theory where larger firms are predicted to have lower bankruptcy cost and therefore can sustain higher levels of debt.

The industry interaction effects show that profitability and tangibility are the only variables that differs meaningfully across our industries. The result of profitability being significantly negative across industries indicates that Swedish firms might be following a pecking order when choosing financing. This suggests that firms prefer to not issue equity as it might send a negative signal. Construction displays the highest marginal effect, indicating that asymmetric information might be a larger problem in this industry. The negative association between tangibility and leverage is contradictory to the predictions by the trade-off theory, but has been found empirically before, particularly for short-term debt (Hall et al., 2000; Song, 2005). This result is explained by the maturity matching principle. Firms finance long-lived assets with long-term debt, while short-term debt is used to finance non-fixed assets. Since our leverage measure includes both short- and long-term debt, this can lead to a negative correlation between tangibility and total leverage if short-term debt dominates. Similar findings have been reported by Bevan and Danbolt (2000); Hall et al. (2000); Song (2005), who showed that tangibility is positively related to long-term debt but negatively related to short-term debt. Our result may therefore reflect not a theoretical inconsistency, but rather a difference in debt maturity structure, supporting the idea that Swedish firms apply the maturity matching principle in their financing decisions.

Further analyzing the industry interaction effects, we notice that the industry effect seem to play a smaller role compared to findings in previous literature (Hall et al., 2000; Daskalakis et al., 2023; Talberg et al., 2008). This in turn has implications for various stakeholders that may use this information in their decision making, taking into account the findings for various applications. Financial analysts and investors may use the findings to evaluate a firm and its management, banks and credit institutions may use the findings to create accurate credit assessment tools and policymakers may take it into account when

designing financial instruments aimed at improving funding opportunities for businesses within targeted industry sectors. Finally, management may use the findings to evaluate financing decision making. Our findings would suggest that these stakeholders should take less into account the industry-specific differences when making these decisions, rather they should focus on the main effects and the underlying theories.

There are several possible explanations why our results differ from previous in terms of the industry effects. One explanation for why our findings differ may be the structural characteristics of the Swedish financial system. Compared to markets like Greece (studied by Daskalakis et al. (2023), Sweden's financial system may be more efficient and industry-neutral. However, this hypothesis is less convincing when considering findings from Talberg et al. (2008) and MacKay and Phillips (2005), who reported pronounced industry effects in more developed markets.

Another explanation lies in methodological differences. Talberg et al. (2008) relied on separate regressions for each industry rather than using interaction terms, which does not allow for formal testing of cross-industry differences. Hall et al. (2000) and Talberg et al. (2008) also employed cross-sectional analyses, while MacKay and Phillips (2005) used a GMM approach. These differences in methodology may account for variations in the findings. Although Daskalakis et al. (2023) used a similar methodology as our paper, their selection of tourism as the base industry sector, which is distinct from the others in their sample-may have accentuated industry effects.

Additionally, the sample period may have contributed to the more muted industry-specific effects. The time frame includes the COVID-19 pandemic, during which widespread subsidies and fiscal interventions may have equalized conditions across sectors. None of the studies referenced in this paper covered this exceptional period, potentially explaining why their industry effects were more pronounced.

Limitations of this paper include the fact that we use both listed and unlisted firms, possibly affecting the results as listed and unlisted firms differ in terms of governance structure and disclosure requirements. Another limitation is related to our data selection process. We selected the firm by size for each industry, possibly introducing selection bias into our data. While both the mixing of listed and unlisted firms, as well as the selection process was in line with the methodology adopted by Daskalakis et al. (2023), it would perhaps have been more prudent to deviate from the methodology laid out by the authors in this case, opting for alternative methods.

Future research could extend this study by applying the same approach to other

markets. At present, only two markets, Swedish and Greek, have been analyzed using this interaction-based panel design, and the divergent findings suggest further exploration could yield valuable insights. By expanding the sample to include additional countries, researchers can determine whether industry-specific differences in capital structure determinants are globally uniform or context-dependent. In addition, Future studies could disaggregate leverage into short-term and long-term components to assess if the firm-level variables differs depending on debt maturity structure. This could offer a more nuanced understanding of the determinants' effect on capital structure.

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Appendix A: Panel Data Assumptions

A.1 Overview of panel data assumptions

Testing for the assumptions underlying the panel data used is key to ensuring valid and robust outputs. Wooldridge (2010) explains that these assumptions must hold in order to produce valid, unbiased, and efficient estimates. Violations of these assumptions may compromise the interpretability and reliability of the regression results. While some violations can lead to biased estimates, others may be addressed using econometric techniques such as clustering the standard errors (Abadie et al., 2023).

Here we present the assumptions for fixed (FE) and random effect (RE) panel data as explained by Wooldridge (2010), and in A.2-A.7 we present results from the assumptions tests.

For fixed effect model:

1. **FE.1** The model is:

$$y_{it} = \beta_1 x_{it1} + \beta_2 x_{it2} + \cdots + \beta_k x_{itk} + \alpha_i + u_{it}, \quad t = 1, \ldots, T$$

2. **FE.2** We have a random sample from the cross-section.

3. **FE.3** Each explanatory variable changes over time for at least some i, and no perfect linear relationships exist among the explanatory variables.

4. **FE.4** Strict exogeneity:

$$E(u_{it} \mid X_i, \alpha_i) = 0 \quad \text{for all } t$$

5. **FE.5** Homoskedasticity:

$$Var(u_{it} \mid X_i, \alpha_i) = \sigma_u^2$$

6. **FE.6** No serial correlation:

$$Cov(u_{it}, u_{is} \mid X_i, \alpha_i) = 0 \quad \text{for all } t \neq s$$

7. **FE.7** The idiosyncratic errors are independent and identically distributed as:

$$u_{it} \sim \text{Normal}(0, \sigma_u^2)$$

For the random effect model:

1. **RE.1** No perfect linear relationships among the explanatory variables.
2. **RE.2** The unobserved effect α_i is uncorrelated with the explanatory variables:

$$E(\alpha_i \mid X_i) = \beta_0$$

3. **RE.3** Homoskedasticity of the unobserved effect:

$$Var(\alpha_i \mid X_i) = \sigma_\alpha^2$$

A.2 Assumptions for Fixed Effects Estimation

A.2.1 FE.1: Model Specification

For each firm i, the model is specified as:

$$y_{it} = \beta_1 x_{it1} + \beta_2 x_{it2} + \cdots + \beta_k x_{itk} + \alpha_i + u_{it}, \quad t = 1, \ldots, T$$

where α_i represents the unobserved firm-specific effect. This captures the panel data structure of the data set used, where firms are observed over multiple years. The unobserved effect α_i accounts for any firm-specific factors that do not vary over time, such as management quality, firm culture, or persistent reputation effects.

Given that our data set follows this setup, we conclude that the assumption holds.

A.2.2 FE.2: Random Sampling

Since a balanced panel of firms with complete data was used, it could introduce survivorship bias, as firms that exited the market or experienced missing data were excluded. To assess whether this bias affected our results, we ran an unbalanced panel with firms that exited the market and with missing data, which can be seen in Appendix B robustness checks.

A.2.3 FE.3: No Perfect Collinearity

No perfect linear relationships may exist between the independent variables. Each explanatory variable must also change over time for at least some firms (Wooldridge, 2010).

Table 9: Correlation Matrix of Independent Variables

	Liquidity	Tangibility	non-debt tax shield	Profitability	Size
Liquidity	1.0000				
Tangibility	-0.2103	1.0000			
non-debt tax shield	-0.1557	0.4695	1.0000		
Profitability	0.1084	-0.2550	-0.2759	1.0000	
Size	-0.1481	0.1798	-0.0200	0.0035	1.0000

Testing for multicollinearity a correlation matrix was calculated. Johannsen et al. (2010) explains that any correlation above 0.7 should be considered high and could damage the robustness of the regression. We notice that we have no such variable with high correlation, suggesting that there exist no such problematic correlation.

A.2.4 FE.4: Strict Exogeneity

The idiosyncratic errors are strictly exogenous:

$$E(u_{it} \mid X_i, \alpha_i) = 0 \quad \text{for all } t$$

meaning that the error term is uncorrelated with the regressors in all time periods.

We assume strict exogeneity of the independent variables, implying that the explanatory variables are uncorrelated with the idiosyncratic error terms in all time periods in the sample. This assumption is a key condition for ensuring consistent and unbiased estimators. Given the nature of our explanatory variables, which primarily reflect firm characteristics and financial ratios determined independently of future shocks to leverage, we make the assumptions that FE.4 holds (Wooldridge, 2016).

A.2.5 FE.5: Homoskedasticity

The idiosyncratic errors have constant variance:

$$\text{Var}(u_{it} \mid X_i, \alpha_i) = \sigma_u^2 \quad \text{for all } t$$

Table 10: Modified Wald Test for Groupwise Heteroskedasticity (FE Model)

Null Hypothesis	$\sigma_i^2 = \sigma^2$ for all i
$\text{Chi}^2(6000)$	7,723,634.49
Prob > Chi^2	0.0000

Table 11: Breusch–Pagan / Cook–Weisberg Test for Heteroskedasticity

Null Hypothesis	**$\text{Chi}^2(1)$**	**p-value**
Constant variance	1496.22	0.0000

Table 12: White's Test for Heteroskedasticity

Null Hypothesis	**$\text{Chi}^2(69)$**	**p-value**
Homoskedasticity	485.47	0.0000

*T*he null hypothesis of homoskedasticity is rejected at the 1% level, indicating the presence of heteroskedasticity.

To test for heteroskedasticity a modified Wald test, Breush-Pagan test and White's test was applied. If there exists heteroscedasticity, ie. that the error term is not consistent across all variables, the error terms may be biased. The null hypothesis of homoskedastic errors was strongly rejected indicating that there is a problem of heteroskedasticity. To combat this we have applied clustered standard errors, combating this problem (Wooldrige, 2020).

A.2.6 FE.6: No Serial Correlation

There is no serial correlation in the idiosyncratic errors:

$$\text{Cov}(u_{it}, u_{is} \mid X_i, \alpha_i) = 0 \quad \text{for all } t \neq s$$

Table 13: Wooldridge Test for Autocorrelation in Panel Data

Null Hypothesis	No first-order autocorrelation
F(1, 5999)	1,033.930
Prob > F	0.0000

*T*he null hypothesis of no first-order autocorrelation is rejected at the 1% level, indicating the presence of autocorrelation in the panel data.

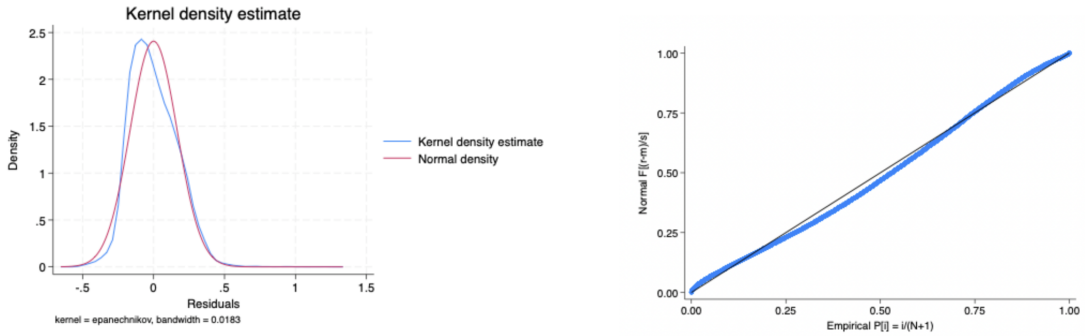
To test for serial correlation, a Wooldridge test was conducted. We notice that the null hypothesis is rejected, suggesting that there exists autocorrelation in the data set. The clustered standard errors ensure that our results remain robust. Clustered standard errors mitigate violations of this assumption by providing consistent standard error estimates even when within-cluster observations are correlated, while still maintaining the assumption of independence across clusters (Abadie et al., 2023).

A.2.7 Fixed effect.7: Normality of Errors

The idiosyncratic errors are independently and identically distributed as normal:

$$u_{it} \sim \text{Normal}(0, \sigma_u^2)$$

This assumption bundles together FE 5 and 6 as well as normal distributed residuals. Seeing as we tested for the first 2 parts earlier we assume that these holds.



Testing for the remaining part of this assumption we use a Kernel density estimate and a Q-Q plot. We use this plot in able to asses if the dataset plausibly comes form a normal distribution, illustrating the normality of the residuals clearly. Analyzing the plot, we notice that the residuals seem to be normally distributed, thus we argue that this assumption holds.

A.3 Specification Tests: Fixed vs Random Effects

To determine which model is the most appropriate, a common approach is using a Hausman test. The Hausman test checks whether the unique errors in the FE and RE model are correlated with the regressors, if the RE is found to be inconsistent one would determine to use the FE model.

The Mundlak test developed by Mundlak (1978) is, like Hausman, a test to determine whether a random effects or a fixed effect model should be used. Unlike a hausman test, Mundlak can be done on a regression with clustered standard errors. A Mundlak test therefore give us a way to test whether our regressors are correlated with unobserved firm effects (Woolridge, 2010). The test is done by meaning the variables and including them in a random effects model with clustered standard errors. Then a joint Wald test was conducted to test if the means was statistically significant. A rejection of the null hypothesis indicates that regressors are correlated with unobserved firm effects and that the assumptions of the random effects model do not hold.

Table 14: Mundlak Specification Test

Test	Mundlak Specification Test
Null Hypothesis	Covariates are uncorrelated with unobserved panel-level effects
Test Statistic	$\chi^2(20) = 388.59$
p-value	0.0000
*F*ixed effects and correlated random effects are consistent under H_0 and H_a. Random effects are efficient under H_0.	

Table 15: Hausman Specification Test Results

Variable	**FE (b)**	**RE (B)**	**(b − B)**	**Std. Err.**
Size	0.01233	0.01262	−0.00029	0.00104
Size × *Retail*	0.01229	0.00335	0.00894	0.00159
Size × *Construction*	−0.00708	−0.01229	0.00521	0.00178
Size × *Manufacturing*	0.00002	−0.01119	0.01121	0.00242
Profitability	−0.18646	−0.19145	0.00499	0.00245
Profitability × *Retail*	−0.12305	−0.12115	−0.00190	0.00330
Profitability × *Construction*	−0.08476	−0.09487	0.01011	0.00388
Profitability × *Manufacturing*	−0.04871	−0.04417	−0.00454	0.00466
non-debt tax shield	0.09753	0.08595	0.01158	0.01822
non-debt tax shield × *Retail*	−0.23381	−0.13178	−0.10203	0.04108
non-debt tax shield × *Construction*	−0.12942	0.05384	−0.18326	0.03897
non-debt tax shield × *Manufacturing*	−0.26959	−0.14229	−0.12730	0.04002
Tangibility	−0.16304	−0.18998	0.02694	0.00418
Tangibility × *Retail*	0.03890	0.04509	−0.00619	0.00798
Tangibility × *Construction*	0.06823	0.04642	0.02181	0.00779
Tangibility × *Manufacturing*	0.07027	0.06193	0.00834	0.00851
Liquidity	−0.05022	−0.05456	0.00434	0.00031
Liquidity × *Retail*	−0.00703	−0.00992	0.00290	0.00059
Liquidity × *Construction*	−0.00470	−0.00763	0.00293	0.00066
Liquidity × *Manufacturing*	0.00335	−0.00012	0.00347	0.00058
Year 2019	−0.01388	−0.01288	−0.00101	0.00012
Year 2020	−0.03291	−0.03077	−0.00214	0.00017
Year 2021	−0.04146	−0.03852	−0.00294	0.00028
Year 2022	−0.04615	−0.04246	−0.00370	0.00039
Year 2023	−0.05379	−0.04960	−0.00419	0.00044

Test of H_0: Difference in coefficients not systematic

$\chi^2(20) = 938.72$ Prob > $\chi^2 = 0.0000$

b = Consistent under H_0 and H_a (from FE model);

B = Efficient under H_0, inconsistent under H_a (from RE model).

The test result suggests rejecting the null hypothesis, indicating that random effects may be inconsistent.

The Hausman and Mundlak tests rejected the null hypothesis, which indicates that a

fixed effect model should be employed as there might be omitted bias from unobservable factors affecting the result. Though due to the arguments made by Hisiao (2010) and Frank and Goyal (2004) we still opt to present both the random and fixed effect outputs.

Appendix B: Robustness Check

B.1 Robustness Checks for Random Sampling

Here we include firms that exited the market and with missing data, which creates a unbalanced panel. The result is highly uniform with the main findings in terms of coefficient signs and significance levels. The only difference being that liquidity for manufacturing is significant. This suggests that survivorship bias is not affecting our results.

Importantly, the similarity between the main and robustness models regression indicate that the potential survivorship bias stemming from the use of balanced panels, is unlikely to affect our results.

Table 16: Fixed Effects Regression Results (Unbalanced Panel)

Variable	**All**		**Construction**		**Retail**		**Manufacturing**	
	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val
SIZE	0.0106	0.008***	0.0084	0.174	-0.0050	0.452	0.0043	0.513
PROFIT	-0.1787	0.000***	-0.1175	0.000***	-0.0862	0.000***	-0.0573	0.005***
NDTS	0.0690	0.486	-0.1293	0.376	-0.2811	0.112	-0.2215	0.140
TANG	-0.1971	0.000***	0.0301	0.377	0.0873	0.006***	0.0762	0.021**
LIQ	-0.0686	0.000***	-0.0028	0.650	0.0027	0.538	0.0141	0.000***
d_2019	-0.0111	0.000***						
d_2020	-0.0282	0.000***						
d_2021	-0.0369	0.000***						
d_2022	-0.0429	0.000***						
d_2023	-0.0522	0.000***						
Constant	0.6307	0.000***						

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at firm level. Industry columns represent interaction effects between industry dummies and variables. Full regression results with dummies are included in Appendix C.

Base industry: Information Technology; Base year: 2018.

Table 17: Random Effects Regression Results (Unbalanced Panel)

Variable	**All**		**Construction**		**Retail**		**Manufacturing**	
	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val
SIZE	0.0110	0.000***	0.0028	0.570	-0.0105	0.039**	-0.0076	0.070*
PROFIT	-0.1586	0.000***	-0.1352	0.000***	-0.1079	0.000***	-0.0658	0.001***
NDTS	0.0894	0.143	-0.0845	0.322	-0.1732	0.168	-0.1312	0.236
TANG	-0.2076	0.000***	0.0392	0.160	0.0701	0.008***	0.0738	0.007***
LIQ	-0.0616	0.000***	-0.0015	0.815	0.0004	0.927	0.0100	0.011**
d_2019	-0.0110	0.000***						
d_2020	-0.0277	0.000***						
d_2021	-0.0349	0.000***						
d_2022	-0.0402	0.000***						
d_2023	-0.0485	0.000***						
Constant	0.6380	0.000***						

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at firm level. Industry columns represent interaction effects between industry dummies and variables. Full regression results with dummies are included in Appendix C.

Base industry: Information Technology; Base year: 2018.

B.2 Alternative size definition (log of total assets)

A commonly used proxy in the literature for size is the logarithm of total assets Frank and Goyal (2009). To test whether our findings for the size variable is sensitive to the chosen definition, we estimate a fixed effect model using the logarithm of total assets instead of sales. The results in table 18 shows that both Construction (p = 0.000) and Manufacturing (p = 0.020), becomes statistically significant, which suggests that this definition may be better in explaining leverage for capital-intensive industries

Table 18: Fixed Effects Regression Results with Alternative Size Definition

Variable	**All**		**Construction**		**Retail**		**Manufacturing**	
	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val
SIZE	0.0029	0.542	0.0349	0.000***	-0.0062	0.419	0.0176	0.020**
PROFIT	-0.1779	0.000***	-0.1299	0.000***	-0.0879	0.000***	-0.0525	0.009***
NDTS	0.1319	0.157	-0.1540	0.244	-0.1546	0.373	-0.2226	0.141
TANGIBILITY	-0.1639	0.000***	0.0173	0.587	0.0689	0.029**	0.0574	0.066*
LIQUIDITY	-0.0511	0.000***	-0.0078	0.143	-0.0044	0.262	0.0033	0.301
CONSTANT	0.5837	0.000***						

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered at the firm level. The industry columns represent interaction effects between industry dummies and the respective variables. Full regression output, including dummies, can be found in Appendix C.
Base industry: Information Technology; Base year: 2018.

Appendix C: Full Regressions

Here we present our full regressions for both random- and fixed effects, including our dummy variables.

Table 19: Fixed effects regression results

Variable	**All**		**Construction**		**Retail**		**Manufacturing**	
	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val
SIZE	0.0123	0.009***	0.0123	0.060*	-0.0071	0.307	0.0000	0.997
PROFIT	-0.1865	0.000***	-0.1231	0.000***	-0.0848	0.000***	-0.0487	0.018**
NDTS	0.0975	0.291	-0.2338	0.077*	-0.1294	0.454	-0.2696	0.072*
TANG	-0.1630	0.000***	0.0389	0.225	0.0682	0.028**	0.0703	0.025**
LIQ	-0.0502	0.000***	-0.0070	0.193	-0.0047	0.241	0.0033	0.299
d_2019	-0.0139	0.000***						
d_2020	-0.0329	0.000***						
d_2021	-0.0415	0.000***						
d_2022	-0.0462	0.000***						
d_2023	-0.0538	0.000***						
Constant	0.5854	0.000***						

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at firm level.
Base industry: Information Technology; Base year: 2018.

Table 20: Random Effects Regression Results

Variable	**All**		**Construction**		**Retail**		**Manufacturing**	
	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val	Coef.	*p*-val
SIZE	0.0126	0.000***	0.0033	0.478	-0.0123	0.012**	-0.0112	0.009***
PROFIT	-0.1915	0.000***	-0.1212	0.000***	-0.0949	0.000***	-0.0442	0.018**
NDTS	0.0860	0.276	-0.1318	0.242	0.0538	0.718	-0.1423	0.267
TANG	-0.1900	0.000***	0.0451	0.087*	0.0464	0.074*	0.0619	0.018**
LIQ	-0.0546	0.000***	-0.0099	0.067*	-0.0076	0.035**	-0.0001	0.968
C (dummy)	0.0375	0.465						
R (dummy)	0.1769	0.001***						
M (dummy)	0.0978	0.035**						
d_2019	-0.0129	0.000***						
d_2020	-0.0308	0.000***						
d_2021	-0.0385	0.000***						
d_2022	-0.0425	0.000***						
d_2023	-0.0496	0.000***						
Constant	0.5935	0.000***						

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at firm level.

Base industry: Information Technology; Base year: 2018.