STOCKHOLM SCHOOL OF ECONOMICS
Department of Finance
4350 MSc Thesis in Finance
Spring 2024

# How Does Cross-Listing in the United States Affect Firm Risk during Periods of Crisis?

*Evidence from a Selection of European Countries during 2017-2022*

Abstract:

This paper seeks to explore the effect of a U.S.-listing on firms' systematic risk performance during times of crisis, using a selection of Swedish, Norwegian, Danish, Finnish, German, and British firms for the period 2017-2022. We analyse how the effect of the COVID-19 pandemic on systematic risk differed between firms with and without access to United States capital markets. To consistently estimate this effect, we deploy a new econometric approach called Synthetic Difference-in-Differences (SDID). The study estimates a significant coefficient of U.S.-listing on equity beta of -0.195 and explores how this coefficient varies based on different types of U.S. listings. While all U.S. listings lead to lower firm systematic risk during the crisis, there is no relationship between magnitude of coefficients and disclosure requirements for different U.S. listings. Furthermore, we explore to what extent changes in beta over the period for the two groups – U.S.-listed and non-U.S.-listed firms – can be explained by accounting fundamentals. We use a set of fixed effects panel regressions on risk and firm fundamentals, combined with proximity score matching. We find inconclusive evidence as to whether the two groups differed in the extent to which the systematic risk could be explained by accounting fundamentals over the period.

Keywords:

Cross-Listing, Firm Risk, Herd Behaviour, United States Equity Markets, Europe, COVID-19 Pandemic, Crisis

Authors:

Mathias Høie (42400)
Rebekka Andersson (24455)

Supervisor:

Mark Sanctuary*, Associate Professor, KTH, INDEK & IVL Swedish Environmental Research Institute

Examiner:

Anders Anderson, Associate Professor, Department of Finance

Master Thesis
Master Program in Finance
Stockholm School of Economics
© Mathias Høie and Rebekka Andersson, 2024

* We would like to thank our supervisor Mark Sanctuary for his guidance, support, and valuable feedback.

# Table of Contents


**1 INTRODUCTION ..... 7**

1.1 Research Motivation & Contribution ..... 7
1.2 Research Framework ..... 8
1.3 Summary of Key Findings ..... 9
1.4 Structure ..... 10

**2 LITERATURE REVIEW ..... 11**

**3 BACKGROUND ..... 14**

3.1 Why do Companies Cross-List? ..... 14
3.2 Cross-Listing in Different U.S. Markets ..... 15
3.3 Selecting The Crisis Period: The COVID-19 Pandemic ..... 16

**4 THEORETICAL FRAMEWORK ..... 17**

4.1 Cross-listing ..... 17
4.1.1 Fundamental covariance ..... 17
4.1.2 Signalling ..... 18
4.1.3 Herding ..... 18
4.1.4 Hypotheses ..... 19
4.2 Market risk ..... 20

**5 METHODOLOGY ..... 21**

5.1 OLS Regression ..... 21
5.2 Synthetic Difference-in-Differences (SDID) ..... 21
5.2.1 SDID Estimator ..... 22
5.3 Systematic Risk and Accounting Fundamentals ..... 23
5.3.1 Two-Way Fixed Effects Panel Regression ..... 23
5.3.2 Propensity Score Matching (PSM) ..... 24

**6 DATA ..... 26**

6.1 Firm Betas ..... 26
6.2 Firm Fundamentals ..... 27

**7 RESULTS ..... 29**

7.1 Market Effects of Crisis ..... 29
7.2 SDID Regression on Systematic Risk ..... 30
7.3 Variation of Coefficient Based on Listing Type ..... 32
7.4 Panel Regressions on Accounting Fundamentals ..... 33
7.5 Regression using Proximity Score Matching (PSM) ..... 34

**8 DISCUSSION ..... 37**

8.1 Discussion of Findings ..... 37
8.2 Limitations ..... 38

8.3 IMPLICATIONS ..... 39
**9 CONCLUSION ..... 40**
**10 REFERENCES ..... 41**
**11 APPENDICES ..... 45**
11.1 APPENDIX A: SDID REGRESSIONS ..... 45
11.1.1 Robustness Test by Time Periods ..... 45
11.1.2 SDID Regression: U.S.-Listings by ADR ..... 45
11.1.3 SDID Regressions by Listing Type ..... 46
11.2 APPENDIX B: PROXIMITY SCORE MATCHING REGRESSIONS ..... 47
11.2.1 Robustness Test: Russia-Ukraine War (Year 2022) ..... 47
11.2.2 Robustness Test: Current Ratio ..... 50
11.3 APPENDIX C: DATA OUTLIERS ..... 53

# List of Tables

**Table 1.** Sample Description: Listing Type by Country. .....27

**Table 2.** Sample Description by ADR Listing Type. .....28

**Table 3.** Sample Description by OTC and non-OTC Listing Type. .....28

**Table 4.** Mean Betas and t-Statistics for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022. .....29

**Table 5.** SDID Regression on Full Sample (2017-2022). .....30

**Table 6.** SDID Regressions on Firms by Listing Type (2017-2022). .....32

**Table 7.** Panel Regression before Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022. .....33

**Table 8.** Summary of Balance before Matching on Panel. .....34

**Table 9.** Summary of Balance after Matching on Panel. .....35

**Table 10.** Panel Regression after Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022. ..36

**Table 11.** SDID Regressions by Time Period (2010-2022). .....45

**Table 12.** SDID Regressions by ADR Type (2017-2022). .....45

**Table 13.** Mean Betas and t-Statistics for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2021. .....47

**Table 14.** Panel Regression before Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2021. ..48

**Table 15.** Panel Regression after Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2021. ..49

**Table 16.** Panel Regression with Current Ratio before Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022. .....50

**Table 17.** Summary of Balance before Matching on Panel including Current Ratio. .....51

**Table 18.** Summary of Balance after Matching on Panel including Current Ratio. .....51

**Table 19.** Panel Regression with Current Ratio after Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022. .....52

**Table 20.** List of Extreme Outlier Betas (2017-2022). .....53

# List of Figures

**Figure 1.** Betas for U.S.-Listed and Non-U.S.-Listed Firms (2017-2022). .....29

**Figure 2.** SDID Parallel Trends Visualization (2017-2022).....31

**Figure 3.** SDID Regressions by Listing Type (2017-2022). .....46

# 1 INTRODUCTION

Since the 1990s, an increasing number of firms cross-list their shares abroad, particularly in the United States (Pagano, Röell, and Zechner, 2002). The decision to cross-list – i.e., to enter a foreign market by listing on an exchange outside the home country – is motivated by access to a broader investor base, stronger corporate governance standards, and enhanced market liquidity. Research has mainly focused on the impact of cross-listing on firm returns (Doidge, Karolyi, and Stulz, 2004; Ghadhab & M'rad, 2018) and to a lesser extent, firm risk (Chira and Marciniak, 2014; Ghadhab, 2019). Literature exploring the direct interaction between systematic risk, cross-listing, and financial crises is notably scarce. Our paper addresses this gap in the literature by being the first to analyse how access to U.S. capital markets during the COVID-19 pandemic influenced firm systematic risk.

We make three significant and novel contributions to the literature. First, by employing the Synthetic Difference-in-Differences (SDID) methodology, we provide a consistent estimate for the interaction effect of the COVID-19 pandemic and a U.S. cross-listing. Second, we construct a comprehensive dataset to assess how different types of U.S. Over-the-Counter (OTC) listings affect firm systematic risk during the COVID-19 pandemic. In addition to a listing on a U.S. regulated exchange, we specifically analyse Pink Current, Pink Limited, and OTCQX International Premier OTC-listings, each defined by their regulatory compliance and information availability. Third, we apply Propensity Score Matching (PSM) and panel regression to explore whether changes in firm systematic risk during the crisis period can be explained by accounting fundamentals.

## 1.1 Research Motivation & Contribution

Following the financial crisis, a prevalent discussion within the financial press has focused on whether the United States will continue to maintain its domicile as the centre of global financial activity, especially within public equity markets (Cetorelli & Peristiani, 2015). U.S. equity markets have consistently secured global equity transactions, drawing significant participation from other notable markets worldwide (Cetorelli & Peristiani, 2015). This dominance has underscored the appeal of the United States as a sophisticated and stable financial destination. Simultaneously, there has been substantial increase in globalization since the early 1990s, and an increasing number of other domiciles have emerged with the necessary sophistication to attract international equity. This shift has potentially reshaped the geography of financial activity and influenced the hierarchy of global financial centres (Cetorelli & Peristiani, 2015). These observed dynamics toward increasing globalization of equity markets are a motivation to analyse the potential effects on companies that choose to list in the United States.

More importantly, the trend of European companies increasingly choosing to cross-list in the United States, contrasted with the diminishing inclination of U.S. firms to list in Europe, marks a significant shift in international business strategies (Pagano, Röell, & Zechner, 2002). Given that cross-listings significantly affect both firm risk and firm returns, such a shift in strategy

can have considerable implications for both European investors and the European corporate sector more widely (Bahlous, 2013). Furthermore, since European firms that cross-list in the United States typically hail from developed countries with more efficient capital markets, this trend necessitates a re-evaluation of findings from the literature on the interaction between cross-listing and crisis in the context of firms from developing (vis-à-vis developed) countries (Bahlous, 2013; Chandar, Patro, & Yezegel, 2009). Additionally, climate change and geopolitical uncertainties enhance the likelihood of crisis events, underscoring the importance of understanding firm risk and its determinants in increasingly diversified global markets (International Panel on Climate Change, 2023).

This paper seeks to address the limited research on the interaction between cross-listing and firm risk during crisis periods. To our knowledge, existing literature has predominantly focused on financial returns, often overlooking risk parameters that become more relevant in times of uncertainty (Saudagaran, 2008). This paper bridges this gap by investigating how cross-listing in the United States affects firm systematic risk during a crisis period. The central research question we address is: *How does cross-listing in the United States affect firm systematic risk during periods of crisis?*

This paper introduces a new methodological approach, Synthetic Difference-in-Differences (SDID), to overcome key challenges to the existing literature on the interaction between cross-listing and crisis. An international cross-listing is not a randomly distributed variable, with Pagano, Röell, & Zechner (2002) showing that European firms that choose to list in the United States have several other characteristics in common. The SDID method allows for the creation of a synthetic control within a Difference-in-Differences (DID) framework, thus facilitating a more consistent and robust approximation of a randomly distributed treatment variable.

## 1.2 Research Framework

Our paper delves into the dynamic interaction between cross-listing in the United States and the systematic risk – measured by equity beta – of European companies during the COVID-19 crisis. This research utilizes a combination of theoretical insights from existing literature on cross-listing effects and herding behaviour, alongside a robust analysis of an extensive dataset comprising daily returns, index returns, risk-free rates, and firm fundamentals.

The study analyses the systematic risk performance of a group of Swedish, Norwegian, Danish, Finnish, German and British companies during the COVID-19 years 2020-2022 and the three preceding years 2017-2020. Due to a considerable body of research showing that cross-listing in different countries affects companies differently, and an increasing trend of European companies replacing European listings with listings in the United States, we compare our sample of U.S.-listed companies to a sample of non-U.S.-listed companies with other cross-listings (Bahlous, 2013; Pagano, Röell, & Zechner, 2002).

The study adopts the Synthetic Difference-in-Differences (SDID) methodology developed by Arkhangelsky et al. (2021), which offers a novel approach to estimate the interaction effects between cross-listing and the pandemic, while adjusting for both observable and unobservable firm attributes. The SDID methodology satisfies the parallel trends assumptions for our pre-crisis period sample (2017-2019). In order to further analyse the causes of any difference in systematic risk, the SDID methodology is then complemented by analysis of results from a panel regression of beta on fundamentals data (accounting variables) pre- and during crisis for U.S.-listed and non-U.S.-listed stocks. These regressions are also estimated using a dataset that has been matched based on proximity score for the variables sector, location, and size, in order to control for covariates.

## 1.3 Summary of Key Findings

Our analysis reveals that both U.S.-listed firms and non-U.S.-listed firms exhibit a statistically significant increase in market risk during the pandemic. However, the firms with a U.S. listing exhibit a lower increase in market risk compared to non-U.S.-listed firms during the pandemic. This finding contradicts Chira & Marciniak (2014), who found that during the European debt crisis, southern European companies with a U.S. listing saw a larger increase in systematic risk relative to those with only a domestic listing.

Moreover, when using propensity score matching on size, company headquarters, and sector, we find that accounting fundamentals influence the systematic risk of both U.S.-listed firms and non-U.S. listed firms during the crisis period. This suggests that investors had a preference for fundamental analysis over sentiment-driven decision-making. This finding also contradicts Chira & Marciniak (2014), who found that accounting fundamentals had a limited effect on systematic risk for firms during the European debt crisis. These findings are, however, only present in the matched regressions, with accounting fundamentals having very little influence over beta for the full sample of non-U.S.-listed companies. This implies that variables other than a U.S.-listing are more important for whether company risk is influenced by accounting fundamentals or not. Accounting fundamentals also become better able to explain firm systemic risk for the entire sample when we drop the year 2022 in order to control for the effect of the Russia-Ukraine war.

Lastly, we explore to what extent the effect of a U.S. listing depends on the type of US listing, in order to ascertain whether signalling plays a role in explaining the difference in systematic risk of U.S.-listed and non-U.S.-listed firms during the crisis. We find that there is no direct association between the cost associated with a certain type of listing, the strength of the signal, and the systematic risk effect of the listing. This contradicts results by Miller (1999), who argued that such signalling could play a major role in stabilizing cross-listing companies during times of crisis; however, it parallels findings by Chandar, Patro, & Yezegel (2009) who find that type of listing does not affect returns.

## 1.4 Structure

This paper proceeds systematically, beginning with a Literature Review (Chapter 2) and Background Section (Chapter 3) that contextualize our findings and contribution to the existing body of research on cross-listing and investor behaviour. We then outline our Theoretical Framework (Chapter 4), which describes what mechanisms can drive differences in systematic risk between U.S.-listed and non-U.S.-listed firms. We also define our main hypothesis for testing in this section. Chapter 5 details the methodologies employed – namely, cross-sectional OLS regression, SDID regression, panel regression, and propensity score matching – describing each method's utility and implementation. In Chapter 6, we outline the data sources used, as well as the criteria for data selection and filtering. Chapter 7 presents the results, illustrating how cross-listing in the United States interacts with systematic risk during the pandemic. The subsequent Discussion synthesizes and integrates these empirical findings into the larger framework of finance theory and practice, interpreting the implications and suggesting potential avenues for future research (Chapter 8). The thesis concludes with a summation of the study's contributions to the field and a reflection on its broader significance (Conclusion).

# 2 LITERATURE REVIEW

The strategic decision for a company to list on a cross-border exchange has garnered substantial attention in financial research, which examines the underlying motives and trade-offs associated with foreign listings (Karolyi 1998, 2006; Pagano, Röell, and Zechner 2002). Advantages of cross-listing include reduced market segmentation (Foerster & Karolyi, 1999; Miller 1999), lower informational asymmetries (Baker et al., 2002), lower capital costs, and greater liquidity (Domowitz et al. 2001). Moreover, cross-listing has been posited to strengthen investor protection (Coffee, 1999; Stulz, 1999). The literature also highlights the role of international trade dynamics, cultural affinities, and geographical proximity. Significantly, empirical evidence shows that listing on U.S. exchanges can lead to notable valuation increases compared to firms without foreign listings (Doidge et al., 2004, 2009; Gozzi et al., 2008). Other research has also emphasized the prestige of the cross-listing destination as a crucial market signal that updates stakeholders about the firm's potential future value, reducing informational frictions and lowering the cost of capital (Merton 1987).

However, despite the large body of empirical research in cross-listing, there is limited research on the direct interaction between systematic risk, cross-listing, and crisis, and, to our knowledge, our paper is the first to analyse this interaction effect using the COVID-19 pandemic. The principal theoretical contribution of our paper is therefore an analysis of how access to U.S. capital markets affects the risk performance of companies during crisis. By exploring this relationship, we make three significant and novel contributions to the literature. First, we apply the Synthetic Differences in Difference (SDID) methodology to find a consistent estimate for the interaction effect of the COVID-19 pandemic and a U.S.-cross-listing. Second, we construct a comprehensive panel dataset that includes various classifications of U.S. Over-the-Counter (OTC) listings — specifically Pink Current, Pink Limited, and OTCQX Premier — to empirically assess how these categories, distinguished by their regulatory compliance and information availability, influence firm systematic risk during the COVID-19 pandemic. Third, we apply Propensity Score Matching (PSM) to analyse how changes in beta during the crisis depend on accounting fundamentals.

Chira & Marciniak (2014) analyse the interaction of cross-listing and firm risk for a series of affected countries during the European debt crisis. The authors use a sample of 64 U.S.-listed firms and 92 strictly domestically listed firms from Portugal, Ireland, Greece, Spain, and Italy to determine how changes in firm risk differed for the two groups during the European debt crisis, and to what extent the risk behaviour of the two groups could be explained by accounting fundamentals. Their results show that the beta of the cross-listed companies increased more on average relative to the non-cross-listed companies. They also found that both beta and standard deviation were poorly tied to accounting fundamentals during the crisis for the cross-listed group, which they argued could be a sign of herding behaviour.

Contrary to our paper, Chira & Marciniak (2014) reach their main results by comparing pre- and post-crisis averages. Consequently, their results do not allow for directly comparing the

two groups: cross-listed and non-cross-listed firms, but only allows for comparing each group with itself pre-crisis. This is because their methodology does not account for differences in covariates between cross-listed and non-cross-listed firms, which might affect the response of the different groups to a crisis. Pagano, Röell, & Zechner (2002) show that European firms that choose to cross-list in the U.S. are more likely to have certain characteristics than those that do not, implying that U.S. listings are not a randomly assigned variable.

We therefore alter Chira & Marciniak's (2014) methodology by deploying an SDID estimation developed by Arkhangelsky et al. (2021). This allows us to compare our group of U.S.-listed companies to a synthetic control created from a group of non-U.S.-listed firms that are more similar in relevant characteristics. When we do this, we find the inverse result of Chira & Marciniak (2014), with U.S.-listings leading to lower systematic risk during crisis. This finding focuses on systematic risk but is in line with a substantial body of cross-listing literature that finds positive effects of cross-listing during times of crisis. Chandar, Patro, & Yezegel (2009), show that companies with ADR listings in the United States outperformed companies with domestic listings during a series of developing world financial crises between 1994 and 2002. Chandar, Patro, & Yezegel (2009) also found no signs of panic selling and excessive herding among foreign investors. Moreover, Ghadhab (2021) deploy a matched pair analysis over 120 matched pairs, finding that cross-listed firms outperformed non-cross-listed firms in terms of return during the great financial crisis in 2008.

We also seek to test to what extent any stabilizing or destabilizing effect of a cross- listing during crisis depends on the kind of listing made. Chandar, Patro, & Yezegel (2009) analyse to what extent the type of U.S. cross-listing affects firm returns during financial crisis for cross listed firms. Using data from J.P. Morgan they separate between those firms traded outside exchanges, in the over-the-counter market and on regulated U.S. exchanges. They find that the type of cross-listing does not significantly affect the return of companies during times of crisis. Abdallah (2008) analyse to what extent type of cross-listing affects research coverage and quality. They deploy data on 584 cross-listed firms that have listed on the LSE, PORTAL, NASDAQ, AMEX, NYSE and OTC. They find that there is no difference in the amount of research coverage between companies cross-listed on U.S. exchanges. This finding also applies to research quality (mean squared forecast error). On the other hand, Hail & Leuz (2009) analyse to what extent cross-listings in the United States reduce the cost of capital of firms, and how this effect varies based on type of cross-listing. Using 1,097 foreign firms listed in the United States they find that the decrease in cost of capital associated with a U.S.-listing is significantly smaller for those firms that list at the OTC market relative to a regulated exchange.

We seek to build further on this literature by analysing how type of cross-listing affects systematic risk during crisis. To our knowledge this combination of variables has not been analysed before. Furthermore, we seek to go further in the extent to which we break down the different listing types. The different forms of OTC listings – namely, Pink Limited, Pink Current and OTCQX International Premier – differ substantially in their disclosure requirements (OTC Markets, 2024). Consequently, we break down the OTC cross-listed companies into these three groups. When we do, we find results most similar to Chandar, Patro,

& Yezegel (2009). While our estimated coefficients differ slightly between the groups, there is no discernible relationship between disclosure requirements and coefficients.

When analysing the extent to which shifts in systematic risk during the COVID-19 pandemic can be explained by firm fundamentals, this paper also interacts with literature on herding. Chira & Marciniak (2014) argue that if fundamentals are poor predictors of firm risk performance during crisis, this is a sign that firm riskiness is not decided by company analysis, and instead relies on sentiment-based pricing or herding. They also find that during the European debt crisis, firm fundamentals are very poor predictors of firm risk for U.S.-listed firms. On the other hand, Chiang & Zheng, (2009) find that investors in U.S. capital markets are less prone to herding than investors in even other developed markets, with herding only happening in the United States in times of crisis. These results imply that binding a company to U.S. capital markets can be a way to reduce herding and make firm risk more analysis-based.

Our paper adds to the debate on herding and cross-listing, by combining Chira & Marciniak's (2014) regression with Ghadhab's (2021) matching methodology. Chira & Marciniak's (2014) control for size, firm domicile and whether the firm is a bank in their regressions. While this approach does control for covariation between the control variables and the accounting fundamentals, it does not adequately control for covariation between the control variables and the extent to which accounting fundamentals can predict firm risk during a time of crisis. It can be theorized that firm size, for example, can have a considerable effect on whether a firm attracts research coverage. Coverage can affect to what extent accounting fundamentals explain firm risk. We therefore deploy proximity score matching to select a sample of U.S.-listed and non-U.S.-listed companies with very similar distributions of the control variables, and rerun Chira & Marciniak's (2014) regressions for improved comparability. When we do, we find that accounting fundamentals have an influence on the systematic risk of both groups, which contradicts Chira & Marciniak's (2014) findings.

# 3 BACKGROUND

## 3.1 Why do Companies Cross-List?

Cross-listing has been extensively studied within financial academia, primarily focusing on its strategic benefits and underlying motivations. Cross-listing is often tied to overcoming market segmentation, which is when barriers between markets cause investors to own a smaller proportion of foreign equities relative to what is optimal (Kang & Stulz, 1995). Consequently, by removing obstacles to foreign investors owning firm shares, the firm can diversify its investor base and reduce its cost of capital. However, there are also other reasons for cross-listing, such as enhancing liquidity, increasing prestige, and committing to regulatory compliance.

***Access to Capital***

The main reason for cross-listings is to overcome market segmentation and increase access to capital. When companies list in larger and more developed markets, they expose themselves to a wider pool of potential investors, which can facilitate new capital inflows at potentially lower costs compared to domestic markets (Doidge, Karolyi, & Stulz, 2004). Cross-listing companies also tend to covary less with other companies in the market they cross-list in, making them a valuable diversification option for investors (Chari & Henry, 2002). Furthermore, by listing in markets with a high presence of institutional investors, companies can tap into more stable sources of capital. Cross-listing increases access to capital by reducing barriers to owning foreign shares, such as regulation, currency risks and lacking information (Bris, Koskinen, & Nilsson, 2009; Chari & Henry, 2002; Barry & Brown, 1985).

***Enhancing Liquidity***

A primary motivation for cross-listing is to enhance the liquidity of shares. Liquidity refers to the ease with which shares can be bought or sold in the market without affecting their price. By making shares available on multiple exchanges, firms can attract a broader investor base, which often results in increased trading activity and reduced-price volatility (Karolyi, 2006).

***Increased Prestige and Regulatory Compliance***

The decision to cross-list can also be driven by a desire to increase international profile and corporate prestige. Listing on prestigious exchanges such as the New York Stock exchange can enhance a firm's visibility significantly. This increased recognition can translate into higher firm valuations as investors are more willing to invest in companies that are perceived as compliant with international standards and regulations (Fernandes & Ferreira, 2008). Moreover, adhering to the stringent regulatory requirements of these major exchanges can signal financial robustness and transparency, further enhancing investor confidence (Coffee, 2002).

***Challenges of Cross-Listing***

Despite these advantages, cross-listing comes with its set of challenges. These include the complexities of complying with multiple regulatory environments, which can lead to higher costs and administrative burdens. Companies must also manage the risks associated with foreign exchange and political instability, which can affect their stock prices and investor interest. Additionally, the benefits of cross-listing may vary significantly depending on the industry, the size of the company, and its home and host markets.

## 3.2 Cross-Listing in Different U.S. Markets

In addition to listing on U.S. regulated exchanges, companies can list in the U.S. Over-The-Counter (OTC) market, which represents a decentralized market where the trading of financial instruments occurs directly between two parties without the oversight of an exchange regulator. Unlike formal exchanges, OTC-trading does not require strict adherence to the disclosure and listing standards of formal exchanges, which can lower costs for issuing companies but may increase risk for investors (Angel & McCabe, 2013). OTC markets allow for trading in stocks that may not meet the stringent listing requirements of bigger exchanges. This flexibility makes it a critical market for smaller, less liquid firms that still seek capital but cannot comply with the stringent listing requirements of larger exchanges (Brüggemann et al., 2018). The regulated OTC market for international firms has several classifications that imply different levels of disclosure and regulatory oversight.

***OTCQX International Premier***

In order to qualify for an OTCQX International Premier listing, the company needs to either have a listing on one of a few accepted foreign exchanges (does not include Stockholm, Copenhagen, or Helsinki), or subject itself to full SEC reporting standards. The company needs to perpetually disclose information following an approved international reporting standard, the SEC reporting standard, or a regulation (OTC Markets, 2024). The firm also requires a qualified OTCQX sponsor, which is another approved OTCQX firm and often includes financial firms with operations in the United States. This sponsor assists companies in meeting the eligibility standards and regulatory requirements for trading on the OTCQX market, ensuring compliance and facilitating the application process. This is the most stringent OTC listing we analyse.

***Pink Current***

In order to maintain a Pink Current status for its listing in the OTC market a company needs to provide a full set of quarterly reports, as well as an annual report and an attorney's letter each year, in addition to maintaining a verified share price. The company also needs to subscribe to the OTC news service and have a verified OTC account. This includes the complete list of current officers, directors, and service providers; outstanding shares; a business description; contact information; and the names of all company insiders and beneficial owners of 10% or more of the outstanding units or shares of any class of any equity security of the issuer (OTC Markets, 2024).

***Pink Limited***

Pink Limited is the least stringent form of listing we analyse. The company needs to disclose one annual report a year, in addition to maintaining a verified stock price quote. The company also needs to maintain an OTC account (OTC Markets, 2024).

## 3.3 Selecting The Crisis Period: The COVID-19 Pandemic

Most of the literature regarding how cross-listing interacts with crisis focuses on regional crisis (Chira & Marciniak, 2014; Chandar, Patro, & Yezegel, 2009; Radelet & Sachs 1998). There is less research on the effects of more global crises, with Ghadhab (2021) being an example. Global crises are interesting because they affect both the home country of the cross-listed firm and the location of the cross-listing simultaneously and might therefore have other implications than a regional crisis. The COVID-19 pandemic is a global crisis and is therefore an interesting subject for research. It was a substantive financial and economic shock that affected large parts of the world, including all the countries in our study and the U.S., relatively simultaneously.

COVID-19, caused by the SARS-CoV-2 virus, was first identified in Wuhan, China, in December 2019, and rapidly escalated into a global pandemic. The pandemic prompted widespread health crises and governmental responses, including lockdowns, travel restrictions, and social distancing measures, significantly altering economic activities and corporate operations worldwide (WHO, 2024). We define the COVID-19 pandemic as the period from the WHO's global health emergency declaration in 2020 to its end in 2022.

The onset of COVID-19 led to unprecedented volatility in global financial markets. Initially, the uncertainty and fear surrounding the COVID-19 impact resulted in sharp declines in stock markets. Yilmaz and Hussain (2021) show how the pandemic caused massive growth in marked volatility in a series of Scandinavian markets during the period. Major indices saw significant drops in March 2020, with the S&P 500 falling by approximately 34% from its peak in February 2020 (Statista, 2024).

Yilmaz & Hussain (2021) also show that the pandemic had widespread effects leading to increased unemployment, a collapse in consumer confidence, as well as a considerable fall in consumer spending, all filtering through to decreased company sales. On the other hand, governments worldwide implemented various fiscal and monetary policies to mitigate the economic impact of COVID-19. These measures, including stimulus packages, interest rate cuts, and liquidity support, were aimed at stabilizing financial markets and supporting economic recovery (Yilmaz & Hussain, 2021). These measures were so strong that in many countries, bankruptcy rates fell considerably during the pandemic (Yilmaz & Hussain, 2021).

Notably, the Russia-Ukraine war started in 2022 during the ongoing COVID-19 pandemic. Consequently, regressions in this paper may be influenced by the effect of the Russia-Ukraine war. We therefore run a series of robustness regressions ending the treatment period in 2021 as opposed to 2022.

# 4 THEORETICAL FRAMEWORK

## 4.1 Cross-listing

There are sound theoretical reasons for why a U.S. cross-listing might increase or decrease firm systematic risk during the COVID-19 pandemic. First, real differences in business performance between European and U.S. companies during the crisis could decrease the co-movement of U.S.-listed European firms with the overall market (Chari & Henry, 2002). Second, listing in the U.S. might signal solid governance and high-quality financial disclosures, stabilizing the equity performance of U.S.-listed firms (Miller, 1999). Third, listing in the U.S. also provides access to sophisticated capital markets, creating a shareholder base that favours fundamentals analysis over sentiment-driven valuation (Chiang & Zheng, 2009).

On the other hand, the investor base in U.S.-listed European firms might be less informed about European firms compared to European investors, possibly leading to herding behaviour. Furthermore, U.S.-listed companies might be subject to U.S. investors shifting their money into U.S. stocks in a flight to safety during a period of crisis (Radelet & Sachs, 1998).

If differences in beta between the two groups are driven by analysis of fundamental returns, then the beta performance of U.S.-listed companies should be well explained by accounting fundamentals. However, if herding behaviour is a key driver, then this should not be the case.

### 4.1.1 Fundamental covariance

Increased covariance between accounting fundamentals of companies in a market should lead to higher betas and vice versa. This is because Sharpe (1964) defines beta as the covariance of the return of an individual asset with the market, and financial theory states that the return on an equity should be based on the fundamental performance of the underlying company (Banz, 1981). This offers one prominent reason for why U.S-listed European companies should have lower betas than non-U.S.-listed European companies during the COVID-19 pandemic. Furthermore, if this is the case, then the development of beta for cross-listed firms during the crisis should be well explained by accounting fundamentals.

Chari and Henri (2002) show that company fundamentals covary substantially less with international firms than they do with firms in the same home market, and that increasing international ownership in a company can thereby decrease its beta. European companies have exposure to different markets than U.S. companies and thereby might have lower covariance with the United States market than with other European markets. Furthermore, the pandemic might also have affected fundamentals in U.S. companies and European companies differently as the two geographies differed in timing and type of pandemic responses. This would have driven down the beta of U.S-listed European firms during the pandemic, as the co-movement of their shares with the overall U.S. market would decrease further. Yilmaz & Hussain (2021) show that the stock markets of different Scandinavian companies reacted differently to different

pandemic response policies from the different countries. This effect might be even stronger on a transatlantic scale.

### 4.1.2 Signalling

The 'bonding hypothesis' posits that by adhering to the governance standards of U.S. markets, firms can signal their reliability and adherence to strong corporate governance. This might positively affect beta during a time of crisis. Stulz (1999), Miller (1999) and Coffee (1999, 2002) show that cross-listing in the United States, where regulatory standards are particularly stringent, serves as a signal of a firm's commitment to upholding minority shareholder interests. Moreover, such a listing also signals that the firm has high standards of governance and can provide high-quality disclosures under the legal framework of another country. This might be particularly salient during COVID-19. Amina (2022) shows that the onset of the COVID-19 pandemic increased the occurrence of and incentives to commit financial fraud, while making fraud prevention considerably more difficult. Consequently, signalling high quality of disclosures and good governance, as well as strong minority shareholder protections, might affect beta during the pandemic. First, by ensuring that investors do not need to worry whether they are being defrauded, a U.S. listing might cause the companies to avoid a rapid withdrawal of funds. Second, investors might be more likely to base their analysis of the company on firm fundamentals if the veracity of these fundamentals is not questioned.

Spence (1973) argued that for a signal to be credible, it should be costly. Moreover, this cost should be lower if the signaller possessed the desirable qualities, such as good governance and accurate financial disclosures, than if they did not. The firms in this study exhibit a range of U.S. listings. Some are listed on regulated U.S. exchanges, while others, forming the majority, are listed in the OTC market. This latter group includes firms with premium international listings, as well as those with Pink Current and Pink Limited level listings. The disclosure requirements, and thus the cost of signalling, vary considerably among these different listing types. If signalling significantly impacts systematic risk, then the effect of a U.S. listing might be more pronounced in those with stringent listing requirements. Therefore, during the pandemic, companies listed under more stringent disclosure conditions should experience greater benefits from their listing.

### 4.1.3 Herding

A considerable body of theory ties herd behaviour to increased systematic risk during crisis. Herd behaviour is defined as “systematic, erroneous decision making by entire populations” and has played a part in several financial crises (Chira & Marciniak, 2014). Zhou & Lai (2009) attribute herding in financial markets to “informational cascades”, where less informed investors choose to follow more informed investors instead of conducting their own analysis (Bikhchandani, Hirshleifer, & Welch 1992). Herding is therefore associated with increased co-movement in markets, i.e., heightened systematic risk. This increase is not tied to any firm fundamentals and therefore penalizes firms for events that do not affect their business performance. Herding has been observed in stock markets globally and grows particularly

intense during periods of either very high or very low returns, such as during a crisis (Corsetti, Pericoli, & Sbracia, 2005; Chiang & Zheng, 2009).

Cross-listing in the United States can have both negative and positive effects on herding behaviours. On the one hand, the U.S. market is the most advanced and deepest capital market in the world, and as such, listing in the United States might increase the amount and sophistication of research coverage on the firm, as well as give the firm a more sophisticated investor base. Both Morck, Yeung, and Yu (2000) and Chiang & Zheng (2009) show that herding varies considerably between markets. Morck, Yeung, and Yu (2000) show that herding is considerably more prevalent in developing markets than in developed markets, while Chiang & Zheng (2009) show that herding is present in all developed markets except the United States, which only sees herding during times of crisis. On the other hand, a rapid withdrawal of funds by foreign investors during times of crisis can exacerbate financial instability in emerging markets. For example, Radelet & Sachs (1998) blame foreign investor panic for a large part of the East Asian financial crisis.

### 4.1.4 Hypotheses

This section formulates several hypotheses to examine the extent to which the previously discussed mechanisms have affected the systematic risk of U.S.-listed firms during the COVID-19 crisis. The first contribution of the paper is to analyse the interaction effect of a U.S. cross-listing and the COVID-19 pandemic on firm systematic risk.

> ***Hypothesis 1:*** *During the COVID-19 pandemic, firms with a U.S. cross-listing saw a smaller increase in systematic risk compared to firms without a U.S. cross-listing.*

Signalling is presented as one of the key mechanisms by which cross-listing can stabilize equity performance during a crisis. If signalling is a key element in driving the effect of a U.S.-listing on the systematic risk of a company during a crisis, then this effect should be more pronounced for listing types that require higher levels of disclosure and regulatory oversight.

> ***Hypothesis 2:*** *Firms with U.S.-listings that have stronger oversight and greater disclosure experience a more significant reduction in systematic risk compared to firms with U.S.-listings that have less oversight and disclosure.*

Herding implies that an increase in systematic risk is decoupled from changes in accounting fundamentals. Conversely, if differences in beta are caused by differences in covariance between the accounting fundamentals, then differences in beta should be explainable by accounting fundamentals. Consequently, we test for the difference between the two causes of fluctuations in beta – herding versus accounting fundamentals – by testing to what extent accounting fundamentals can explain the systematic risk of the group of companies that see the largest increase in their systemic risk in relation to the crisis.

***Hypothesis 3:*** *Firms that have a significant positive coefficient on the interaction of the COVID-19 pandemic and their U.S.-listing status will also see a weaker correlation between beta and accounting variables during the crisis.*

## 4.2 Market risk

Chira & Marciniak (2014) also argue that during times of crisis the overall risk of the market in general tends to increase for all companies regardless of accounting fundamentals. The COVID-19 crisis led to a considerable global recession and financial crisis. Consequently, we expect to see such an increase in market risk during the period. As such, all companies, both U.S. and non-U.S. listed should see an increase in beta during the crisis, and the ability of accounting fundamentals to explain their systematic risk should fall.

***Hypothesis 4:*** *Beta increased during the COVID-19 pandemic for both U.S.-listed and non-U.S.-listed companies.*

***Hypothesis 5:*** *The ability of accounting fundamentals to explain systematic risk fell during the crisis for both U.S.-listed and non-U.S.-listed companies.*

# 5 METHODOLOGY

This section outlines the methodologies applied in this paper to test the hypotheses introduced in Section 4. First, OLS is used to estimate systematic risk of U.S.-listed and non-U.S.-listed firms over the period. *Hypothesis 4* is then tested through the use of a t-test t of differences between average beta values before and during the COVID-19 pandemic. Second, the SDID methodology is applied to test *Hypothesis 1* by estimating the interaction effect of a U.S. cross-listing and the COVID-19 pandemic on the systematic risk of U.S.-listed companies. The SDID methodology is also applied to test *Hypothesis 2*, by re-estimating the model after having created subsets of the treatment group based on type of U.S. listing. Propensity score matching combined with a panel-data regression of beta on accounting fundamentals is used to test *Hypothesis 3* and *Hypothesis 5*. *Hypothesis 3* is tested by comparing the extent to which the beta of the two groups can be explained by accounting fundamentals during the crisis. *Hypothesis 5* is tested by analysing weather this relationship is stronger before or after COVID-19 begins.

## 5.1 OLS Regression

The study estimates equity beta through the standard approach of OLS regression. Beta, or systematic risk, is defined as the beta-coefficient from the below CAPM regression.

$$r^e - r^f = \alpha + \beta(r^m - r^f) + \epsilon \quad (1)$$

In this equation $r^e$ is the return of a given equity, $\alpha$ is a constant term, $r^m$ is the return of the market index for the accessible market and $\epsilon$ is an error term. $r^f$ is the risk-free rate. Beta is thereby the coefficient of the return of an individual equity on the market. We estimate annual betas using daily data. This is to ensure that betas are both estimated on a substantive enough number of observations to be consistent, while also ensuring that they are not estimated over so long a period as to not be responsive to effects from shocks such as the COVID-19 crisis. Average betas for the period before COVID-19 are compared with averages during COVID-19 using a simple t-test to test *Hypothesis 4*.

## 5.2 Synthetic Difference-in-Differences (SDID)

We employ a recently developed estimation methodology called the Synthetic Difference-in-Differences (SDID) estimator in order to analyse the risk differences. The SDID is developed by Arkhangelsky et al. (2021) and the estimator seeks to leverage theoretical insights from both the standard Difference-in-Differences (DID) estimator as well as Synthetic Control (SC) estimators. The SDID simulates randomisation of the treatment variable, which in our paper is the interaction term between the COVID-19 pandemic and a firm being listed in the United States. The SDID approach can thereby overcome problems of identification that come from the fact that a U.S. cross-listing is not a randomly distributed variable among companies.

### 5.2.1 SDID Estimator

The SDID estimator combines theoretical elements from both the Difference-in-Differences (DID) and the Synthetic Control (SC) estimators in order to create an estimator that maintains consistency. We apply SDID to test *Hypothesis 1* and *Hypothesis 2*. An SC approach is estimated from a dataset of treated and control observations. The SC estimator weights the different control observations in order to create a synthetic control observation that closely matches the observed pre-treatment behaviour of the treated observations. This synthetic control then offers a baseline for how the treated variable would have behaved post treatment if it had not been treated (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010, 2015; Abadie and L'Hour, 2021). The SDID approach deploys this SC weighting in a standard two-way fixed effect DID framework, to ensure that the treated group is compared to a non-treated control that matches its pre-treatment trend.

Furthermore, the SDID deploys a set of time weights that weigh different observations such that the post-treatment period is compared to the most similar part of the pre-treatment period (Arkhangelsky et al., 2021). This amounts to an automatization of what is already established practice for DID estimations, where control variables and time periods are chosen to satisfy parallel trends. However, as the SDID estimator is capable of creating a control group based on the outcome variable, it can establish parallel trends in scenarios where the differences between treatment and control group variables are caused by non-observable variables. Moreover, by maintaining the DID setup, the SDID estimator is capable of accounting for two-way fixed effects and can deal with more than one treated observation.

Arkhangelsky et al. (2021) prove the asymptotic normality of the SDID estimator, allowing for a standard construction of confidence intervals, as well as a calculation of t-values and p-statistics. Arkhangelsky et al. (2021) propose three such estimators for the variance. This paper deploys the bootstrap method as it allows for clustering of standard errors and is heteroscedasticity robust.

This paper seeks to compare a treatment effect (COVID-19 * U.S.-listing) on a treatment group (U.S.-listed firms) and a control group (non-U.S.-listed firms) before and after the treatment is deployed. There are several forms of noise that might affect the estimate. The deployed estimator therefore needs to be robust to a series of different scenarios:

1. First, there might be covariates that affect the level of beta differently for the treated and non-treated groups. For example, Chari and Henry (2002) show that access to international capital-markets itself is a variable that affects firm systematic risk. The addition of two-way fixed effects is therefore important to control for this.

2. Second, there are covariate differences between the two groups that affect the change in beta over time. Before the SDID estimation, we find that there is no inherent common trend to the control and treatment groups pre-crisis. We also find that this difference between the two groups proved robust to matching by a set of observable variables.

SDID overcomes this problem: by simulating a common trend, SDID adjusts for both observed and unobserved characteristic differences between the control and treatment group, thereby creating a treatment group with a parallel trend.

3. Third, there might be time shocks that affect the two groups differently in the pre-treatment period. This might make the comparison of pre-treatment and post-treatment outcomes biased. We have sought to address this by defining a pre-treatment period without major shocks, but the time weighting in the SDID adds extra robustness by controlling for such shocks. We also estimate several robustness regressions where we prolong the time period.

This paper employs an SDID estimator to examine the impact of the pandemic on firms with and without a U.S. listing, addressing *Hypothesis 1*. Additionally, the study investigates how this impact varies among several different types of U.S. listings, addressing *Hypothesis 2*.

## 5.3 Systematic Risk and Accounting Fundamentals

### 5.3.1 Two-Way Fixed Effects Panel Regression

The second part of this paper employs a two-way fixed effect panel regression to estimate the explanatory power of accounting fundamentals on equity beta for both U.S.-listed and non-U.S.-listed firms during and pre-pandemic. As such, we estimate four panel regressions: two for the treatment group (U.S.-listed firms) and two for the control group (non-U.S.-listed firms), segmented into pre-pandemic and during-pandemic periods. These regressions allow us to examine *Hypothesis 3* by comparing how changes in beta during the crisis are influenced by accounting fundamentals across both groups. Additionally, the regressions for the pre-pandemic period serve to verify the robustness of the pandemic-period results and address *Hypothesis 5* by analysing how the ability of accounting fundamentals to explain changes in beta varies from the pre-crisis to the crisis period.

The theory of finance dictates that firm fundamental analysis should be applied to establish firm returns and riskiness (Banz, 1981). For instance, Patoukas (2013) shows that firm valuation is affected by accounting fundamentals. Firm capital structure is also decisive for the riskiness of a firm, with use of debt associated with more riskiness (Lazarides & Pitoska, 2009). Debt to equity in particular is shown to have a positive relationship with stock returns (Bhandari, 1988). Moreover, Chira & Marciniak (2014) argue that firm fundamentals should have explanatory power for the systematic risk of firms, and that when risk increases without being based on firm fundamentals, this might imply herding in the market, and unjustly penalise otherwise well-performing firms.

The panel fixed effect regression in this paper includes a series of accounting fundamental variables as well as a control variable for size, defined as the natural logarithm of assets, as per Chira & Marciniak (2014). As the pre-treatment period is longer for this study than for Chira

& Marciniak (2014), who only had one pre-crisis period, a panel regression setup is deployed instead of an OLS setup. Other control variables from Chira & Marciniak (2014), which are company location codes and bank dummies, are removed as they do not vary over time and are therefore handled by the fixed effects term in a panel regression. The exact specification is as below:

$$B = \beta_0 + \beta_1 TDR + \beta_2 DER + \beta_3 PM + \beta_4 ROA + \beta_5 ROE + \beta_6 Size + \alpha_i + \mu_t + \epsilon \quad (2)$$

In this equation $B$ is the systematic risk, $TDR$ is the total debt ratio, $DER$ is the debt-to-equity ratio, $PM$ is the profit margin, $ROA$ is return on assets, $ROE$ is return on equity, $size$ is calculated as the ln of total assets, $\alpha_i$ are firm fixed effects, $\mu_t$ are time fixed effects, and $\epsilon$ is the error term.

The model is specified with data collected at the firm level for (1) U.S.-listed firms during the crisis-period, (2) non-U.S.-listed firms during the crisis-period, (3) U.S.-listed firms before the crisis-period, and (4) non-U.S.-listed firms before the crisis-period.

Due to this setup, a two-way fixed effect regression is only sensitive to variables that vary over time. It is thereby ideal for the sort of testing we are planning to do in this study, as we only observe how changes in beta in the different periods are susceptible to changes in accounting fundamentals, and not level effects.

### 5.3.2 Propensity Score Matching (PSM)

While the specified regressions are internally valid for each group, they do not offer an empirically sound way to compare results between the U.S.-listed and non-U.S.-listed groups. Including controls in the regression specification allows for controlling for covariation between the control variable and the accounting fundamental itself. However, this does not control for variation between the control variable and the extent to which the beta of the firm in question is decided by accounting fundamentals. Propensity score matching mitigates this limitation by selecting a subsample of treated and control observations that are closely matched in terms of distribution of the covariates.

Chira & Marciniak (2014) control for the effect of firm size and firm location, in addition to a bank dummy. Besides affecting beta itself, we argue that all these variables might affect the extent to which a firm's beta is decided by analysis of fundamentals. Furthermore, Yilmaz & Hussain (2021) showed that the effect of the COVID-19 crisis on stock market return differed substantially between sectors, with healthcare and telecoms performing particularly well during the crisis. As some sectors were associated with particularly strong performance during the crisis, we argue that sector type might also affect the extent to which investors analyse the firm and how they trade its shares. Consequently, we choose to match our samples on firm country, firm sector as well as firm size in each of the pre-crisis years 2017, 2018 and 2019. By doing

this we are also matching on the same variables as those deployed by Ghadhab (2021), who match on sector and size when analysing cross-listed versus non-cross-listed firm returns.

# 6 DATA

To empirically analyse the relationship between firm risk and cross-listing in the United States during periods of crisis, we build one principal panel dataset from four types of collected data. First, data on daily returns. Second, data on index returns and risk-free rates. Third, data on firm fundamentals. Fourth, data on OTC listing types. Daily returns for foreign firms cross-listed in the United States are obtained from the Compustat North America Security Daily database. Daily returns for firms not cross-listed in the United States are obtained from the Compustat Global Security Daily database. Daily index returns and risk-free rates are obtained from Refinitiv Eikon Datastream database, where local index benchmarks are used for firms not cross-listed in the United States and the S&P500 is used for firms cross-listed in the United States. Data on firm fundamentals for both U.S.-listed and non-U.S.-listed firms is obtained from the Compustat Global and North America Annual Fundamentals databases. OTC-listing types are collected manually on firm basis from OTC Markets (OTC Markets, 2024).

## 6.1 Firm Betas

We use Compustat North America Security Daily to collect data on daily returns for Swedish, Norwegian, Danish, Finnish, German, and British firms cross-listed in the United States. To avoid confounding effects, we define U.S.-listed firms as firms that are both *headquartered* and *incorporated* in either of the European countries, and that have a listing in the United States. The data is filtered to exclude foreign firms trading on any Canadian exchanges. The data is also filtered to obtain the primary security issuance in the United States for each foreign firm.

We use Compustat Global Security Daily to collect data on daily returns for firms that are not cross-listed in the United States, but are both headquartered and incorporated in either Sweden, Norway, Denmark, Finland, Germany, or Great Britain, respectively. The data is filtered to obtain the primary security issuance of each firm. We do not filter for strictly domestic firms (i.e., firms listed only in their home country) as we aim specifically to test the effect of cross-listing in the United States on firm risk. As such, we include firms with multiple cross-listings within our sample of firms not cross-listed in the United States.

We use data on index returns and risk-free rates from Refinitiv Eikon Datastream to calculate market excess returns. We download data on the Total Return Index for the following indices: OMX Stockholm PI, OMX Copenhagen All Share Index, OMX Helsinki, Oslo Bors All-Share, MDAX, FTSE, and SP500. For the risk-free rates, we download data on Redemption Yield for 10-year government bonds for the different countries in our sample as well as the 10-year Treasury Bill for the United States. We then regress firm excess returns on market excess returns to find measures of beta for each firm for each year in our sample, where local index benchmarks over risk-free rate (i.e., excess market returns) are used for firms not cross-listed in the United States and the S&P500 excess returns are used for foreign firms cross-listed in the United States. Note that for the period in question, Compustat had some problems with their data-provider. This led to some days lacking price data which biased our beta estimates for

some companies. We therefore also downloaded a dataset of monthly data which we used to ensure that we only included firms for which the data was high-quality.

## 6.2 Firm Fundamentals

Once we have estimated the firm betas for firms listed in the Unites States and firms not listed in the United States, we merge data on firm fundamentals. Before we combine firm fundamentals data with our beta estimates, we create the accounting variables used in our regressions, namely the total debt ratio, the debt-to-equity ratio, the profit margin, the return on assets, return on equity, and size (as well as current ratio for robustness regressions).

Once we have all variables in place, we balance our panel so that we only include firms for which we have data for the entire period, as this is required to estimate the SDID. We use the same panel for SDID regressions as for the panel regressions. After this we remove three firms with extreme outlier betas, to keep them from biasing our estimates (see Appendix C). We also drop some companies due to missing, or low-quality data on firm fundamentals.

Below is a table of our balanced panel data for the period 2017-2022.

***Table 1.*** *Sample Description: Listing Type by Country.*

| **Country** | **U.S.-Listed Firms** | **Non-U.S.-Listed Firms** |
|---|---|---|
| Sweden | 23 (12.5%) | 513 (24.4%) |
| Norway | 9 (4.9%) | 128 (6.1%) |
| Denmark | 12 (6.5%) | 100 (4.8%) |
| Finland | 8 (4.4%) | 113 (5.4%) |
| Germany | 44 (23.9%) | 416 (19.7%) |
| Great Britain | 88 (47.8%) | 837 (39.7%) |
| Total | 184 (100%) | 2,107 (100%) |

Below is a table of our balanced panel on firms cross-listed in the United States per ADR listing type for the period 2017-2022.

***Table 2.*** *Sample Description by ADR Listing Type.*

| Listing Type | U.S.-Listed Firms |
|---|---|
| US listed via exclusively non-ADR-listing | 0 |
| US listed via ADR-listing | 90 |
| US listed with both ADR- and non-ADR listing | 94 |
| **Total** | **184** |

Below is a table of our balanced panel on firms cross-listed in the United States per OTC and non-OTC listing type for the period 2017-2022.

***Table 3.*** *Sample Description by OTC and non-OTC Listing Type.*

| Listing Type | U.S.-Listed Firms |
|---|---|
| **Listed on US regulated exchange** | **23** |
| Listed US OTCQX International Premier | 17 |
| Listed OTC Pink Current | 94 |
| Listed OTC Pink Limited | 40 |
| Listed OTC (none of the above/lacking data) | 10 |
| **Listed US OTC** | **161** |
| **Total** | **184** |

# 7 RESULTS

## 7.1 Market Effects of Crisis

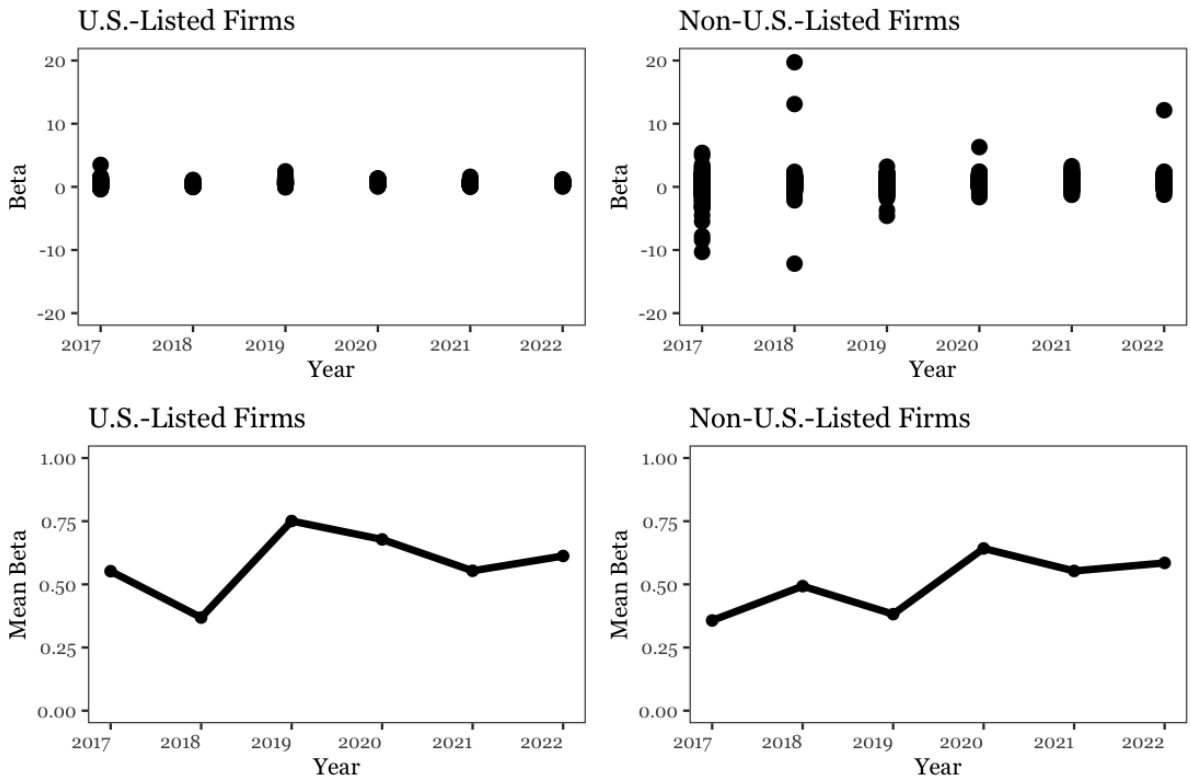
***Table 4.*** *Mean Betas and t-Statistics for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022.*

| | | U.S-Listed Firms | Non-U.S.-Listed Firms |
|---|---|---|---|
| Beta | Before COVID-19 | 0.5571 | 0.4107 |
| | During COVID-19 | 0.6146 | 0.5933 |
| | % Change | 10.32% | 44.46% |
| t-Stat | | -2.99*** | -18.01*** |

*Note:* $^{*}p^{**}p^{***}p<0.01$

We find that both groups have statistically significant increases in average beta in the crisis period, with the non-U.S.-listed firms having a considerably larger increase than the U.S.-listed firms. Consequently, our findings support *Hypothesis 4*; there is a general increase in market risk during the crisis, although there is still considerable difference between the two groups.

***Figure 1.*** *Betas for U.S.-Listed and Non-U.S.-Listed Firms (2017-2022).*




The above graph shows the spread of betas for U.S.-listed and non-U.S.-listed firms for the entire period 2017-2022. Both groups experience some variance, with the U.S.-listed group being considerably more homogenous than the non-U.S.-listed group given that the sample size

for U.S.-listed firms is much smaller. In terms of means, we see that the two groups move quite differently, with the mean of the U.S.-listed group changing slightly more during the period. While the means are in general higher for both groups during the crisis period than before, only the non-U.S.-listed group sees a jump in mean beta associated with the first crisis year 2020. There is no parallel trend between the two groups.

## 7.2 SDID Regression on Systematic Risk

The table below shows the results from the initial SDID regression on the effect of COVID-19 on U.S.-listed companies relative to the non-U.S.-listed companies. We observe a coefficient of -0.195 for the average effect of the treatment on the treated group. This implies that when the COVID-19 crisis hit, those U.S.-listed companies saw a 0.195 lower increase in beta than those companies that were not listed in the United States. Furthermore, based on the bootstrap methodology, deploying 100 iterations, the standard error of the coefficient is 0.047. Consequently, the t-value is -4.14, showing that the finding is highly statistically significant. Our finding thereby supports *Hypothesis 1*.

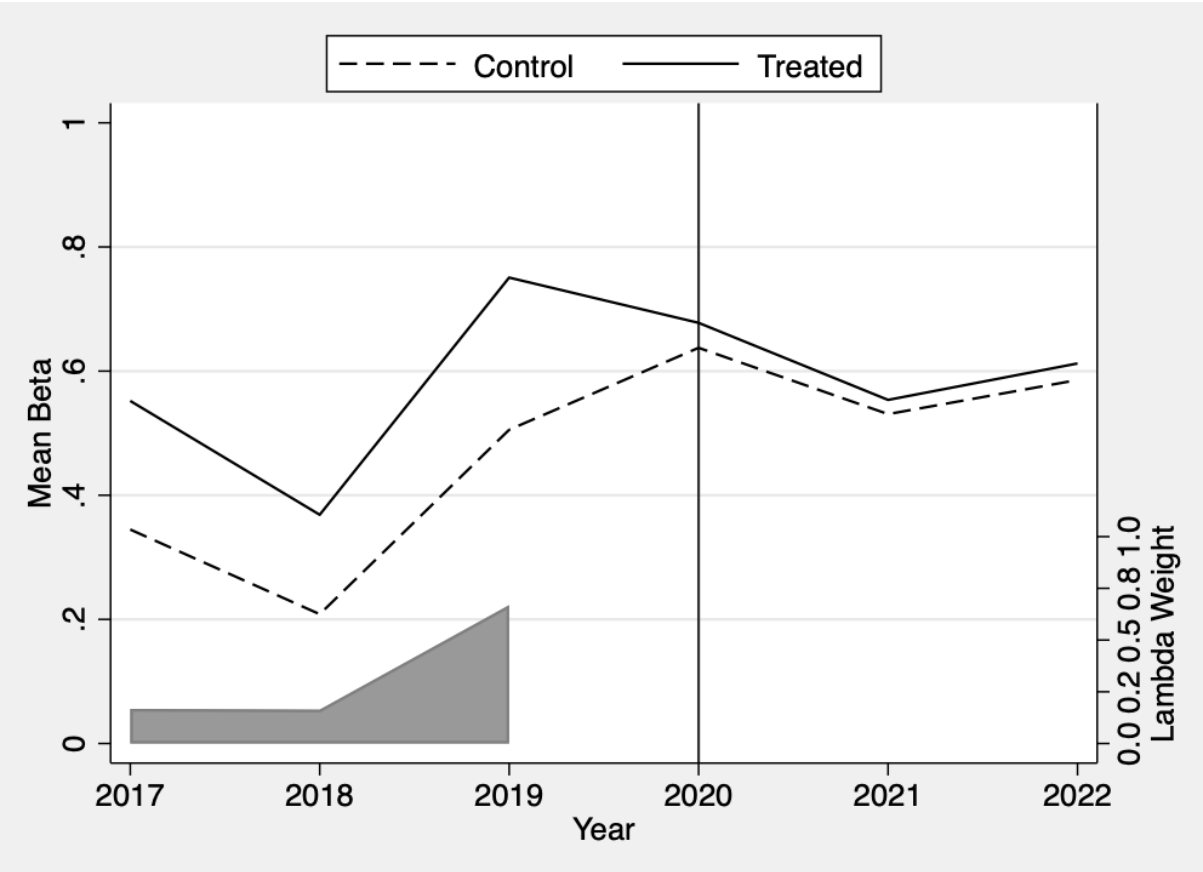
***Table 5.*** *SDID Regression on Full Sample (2017-2022).*

| **Beta** | **ATT** | **Std. Err.** | **t-Stat** | **P>\|t\|** | **95% Confidence Interval** |
|---|---|---|---|---|---|
| Treatment Effect | -0.19500*** | 0.04713 | -4.14 | 0.000 | [-0.28737, -0.10263] |

*Note:* $^{*}p$ $^{**}p$ $^{***}p<0.01$

As the SDID estimator uses the pre-treatment period to estimate the control variable, it might be sensitive to the length of the pre-treatment period. We therefore estimated the SDID regression for each period starting from 2010 onward and found similar and statistically significant coefficients for each time-period (see Appendix A).

***Figure 2.*** *SDID Parallel Trends Visualization (2017-2022).*




The above chart shows the development of both the treated group and the synthetic control in the period leading up to the COVID-19 shock, as well as the three years during the shock. The graph shows point estimates of the mean of the treatment group as well as the weighted combination that is the synthetic control, for the years 2017, 2018, 2019, 2020, 2021 and 2022. Note that the graph only shows one observation for each year, so when the lines start converging before 2020, this is due to the observation in 2020 and not a violation of the parallel trends assumption.

In general, the SDID estimator creates a proxy that tracks the treated group well before the pandemic, and parallel trends seems to be satisfied. There is a difference in levels between the treatment and control group, which is handled by the two-way fixed effect regression when estimating the coefficient. The graph shows that the negative coefficient is almost in its entirety caused by a shock in the first year of the COVID-19 crisis, 2020, where the beta of the control group increases substantially, while that of the treatment group falls slightly. Interestingly, after the shock, the treatment and control groups keep co-moving closely for the rest of the crisis, although with the control group now at a higher level. This implies that the control tracks the treatment group fairly well also in periods outside its estimation period, when there are no shocks present. The graph also shows that the coefficient is predominantly caused by the COVID-19 pandemic, and not the Russia-Ukraine war.

The shaded shape at the bottom of the graph shows how the time weights are distributed between the different years. As can be seen more weight is put on the numbers from 2019 when estimating the coefficient. It is quite common for SDID estimators to put more weight on observations close to the treatment period, as these often behave more similarly to the post treatment movement. All years are however used for calculating the coefficient.

## 7.3 Variation of Coefficient Based on Listing Type

***Table 6.*** *SDID Regressions on Firms by Listing Type (2017-2022).*

| **Beta** | **ATT** | **Std. Err.** | **t-Stat** | **P>\|t\|** | **95% Confidence Interval** |
|---|---|---|---|---|---|
| Regulated Exchange | -0.12993*** | 0.04720 | -2.75 | 0.006 | [-0.22244, -0.03742] |
| OTC: OTCQX | -0.24522*** | 0.09374 | -2.62 | 0.009 | [-0.42894, -0.06149] |
| OTC: Pink Current | -0.19384*** | 0.05050 | -3.84 | 0.000 | [-0.29281, -0.09487] |
| OTC: Pink Limited | -0.23704*** | 0.09374 | -4.26 | 0.000 | [-0.34604, -0.12805] |

*Note:* $^{*}p^{**}p^{***}p<0.01$

The four above regressions re-estimate the SDID regression for subsets of the dataset based on what form of U.S.-listings the companies have. The main finding that having a listing in the US reduced the systematic risk impact of the COVID-19 pandemic is robust for all four kinds of listings, which all have negative and statistically significant coefficients. However, we see that the most disclosure heavy listing, listing on a U.S. regulated exchange, has the coefficient with the smallest magnitude. Furthermore, there is no linear relationship between disclosure requirement for listing and effect of the listing. This weakens the case for signalling as a main driver for the difference in the response of systematic risk to the COVID-19 pandemic for U.S.-listed and non-U.S.-listed firms, implying a rejection of *Hypothesis 3*.

## 7.4 Panel Regressions on Accounting Fundamentals

***Table 7.*** *Panel Regression before Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022.*

| | Dependent variable: Beta | | | |
|---|---|---|---|---|
| | U.S.-listed firms pre-pandemic (1) | Non-U.S.-listed firms pre-pandemic (2) | U.S.-listed firms during pandemic (3) | Non-U.S.-listed firms during pandemic (4) |
| Total Debt Ratio | 0.336 | 0.046** | 0.565*** | 0.001 |
| | (0.389) | (0.021) | (0.165) | (0.001) |
| Debt to Equity Ratio | -0.001 | -0.001* | 0.0003 | -0.001** |
| | (0.001) | (0.0003) | (0.001) | (0.0003) |
| Profit Margin | 0.001** | -0.0001* | 0.005** | 0.0001 |
| | (0.0004) | (0.0001) | (0.002) | (0.0001) |
| Return on Assets | -0.276 | 0.037 | -0.309*** | -0.002 |
| | (0.225) | (0.030) | (0.111) | (0.003) |
| Return on Equity | -0.013*** | -0.008*** | 0.019 | -0.0004 |
| | (0.004) | (0.002) | (0.018) | (0.001) |
| Size | 0.192** | 0.067** | 0.058 | 0.025 |
| | (0.096) | (0.032) | (0.057) | (0.016) |
| Observations | 531 | 6,178 | 530 | 6,223 |
| $R^2$ | 0.095 | 0.006 | 0.078 | 0.002 |
| F-statistic | 6.109*** | 4.307*** | 4.911*** | 1.455 |

*Note:* *p**p***p<0.01

The two regressions of firm equity betas during the COVID-19 pandemic show that the U.S.-listed companies, which saw a lower increase in beta associated with the pandemic, have a beta which is better explained by accounting fundamentals than that of the non-U.S.-listed firms during the time of the crisis. For the U.S.-listed firms, both profit margin, return on equity and return on assets have statistically significant coefficients. On the other hand, we observe that for the non-U.S.-listed companies accounting fundamentals are a much worse predicter of equity beta. Only debt-to-equity ratio has a significant coefficient, and this coefficient is quite low at -0.001. Note, however, that for both groups R-squared falls during times of crisis, implying that for both groups the ability of accounting fundamentals to explain shifts in beta during the crisis falls. Our initial findings are therefore in line with both *Hypothesis 2* and *Hypothesis 5*.

However, it might be that this difference in the ability of accounting fundamentals to predict firm systematic risk is not in fact unique to the pandemic, but rather a consequence of permanent differences between the two groups. Therefore, in order to say if the inability of accounting fundamentals to affect beta is unique to the COVID-19 period, or something that

affects the non-U.S.-listed firms more generally we estimate the same regressions for the pre-treatment control period 2017-2019. We find that for this period accounting fundamentals are better able to predict company betas for the non-U.S.-listed companies. Return on equity, profit margin and total debt ratio all have significant coefficients for the non-U.S.-listed companies in the pre-crisis period compared to only debt-to-equity ratio in the COVID-19 period. On the other hand, we find that several accounting variables are connected to equity betas for the U.S.-listed companies in the pre-treatment period as well, with profit margin and return on equity having significant coefficients. Nevertheless, the model R-squared is lower for the non-U.S.-listed group also before the crisis, and our results do not provide conclusive evidence as to whether the crisis affected the two groups differently.

## 7.5 Regression using Proximity Score Matching (PSM)

As the results from the above regressions might be biased due to the existence of other variables than a U.S.-listing that interact with the extent to which firm systematic risk depends on accounting fundamentals, we seek to control this by deploying proximity score matching, in order to ensure that we have two datasets with similar distributions of key covariates. We match the dataset on the variables location, sector, and size over the three pre-crisis years. We require sector and location to be matched exactly, while seeking as close as possible a match for size without losing to many observations.

***Table 8.*** *Summary of Balance before Matching on Panel.*

| Variable | Means Treated | Means Control | Std. Mean Diff. | Var. Ratio | eCDF Mean |
|---|---|---|---|---|---|
| Distance | 0.4882 | 0.048 | 1.5085 | 6.8796 | 0.4383 |
| Sector | 2983.2386 | 3433.9798 | -0.3519 | 0.9067 | 0.0946 |
| Germany | 0.2443 | 0.2147 | 0.0689 | - | 0.0296 |
| Denmark | 0.0625 | 0.0522 | 0.0425 | - | 0.0103 |
| Finland | 0.0455 | 0.0586 | -0.0631 | - | 0.0131 |
| Britain | 0.4716 | 0.3479 | 0.2478 | - | 0.1237 |
| Norway | 0.0511 | 0.0671 | -0.0726 | - | 0.016 |
| Sweden | 0.1250 | 0.2595 | -0.4066 | - | 0.1345 |
| Size 2017 | 9.5177 | 5.3944 | 2.1442 | 0.6633 | 0.4067 |
| Size 2018 | 9.5588 | 5.4773 | 2.1472 | 0.6454 | 0.4049 |
| Size 2019 | 9.6238 | 5.5687 | 2.1396 | 0.6211 | 0.4025 |

***Table 9.*** *Summary of Balance after Matching on Panel.*

| Variable | Means Treated | Means Control | Std. Mean Diff. | Var. Ratio | eCDF Mean |
|---|---|---|---|---|---|
| Distance | 0.2921 | 0.2763 | 0.0541 | 1.011 | 0.0118 |
| Sector | 2870.4412 | 2870.4412 | 0 | 1 | 0 |
| Germany | 0.1471 | 0.1471 | 0 | - | 0 |
| Denmark | 0.0441 | 0.0441 | 0 | - | 0 |
| Finland | 0.0147 | 0.0147 | 0 | - | 0 |
| Britain | 0.5441 | 0.5441 | 0 | - | 0 |
| Norway | 0.0588 | 0.0588 | 0 | - | 0 |
| Sweden | 0.1912 | 0.1912 | 0 | - | 0 |
| Size 2017 | 8.2710 | 8.0938 | 0.0921 | 1.0126 | 0.0176 |
| Size 2018 | 8.3304 | 8.187 | 0.0755 | 0.9597 | 0.0169 |
| Size 2019 | 8.4128 | 8.2207 | 0.1014 | 0.8881 | 0.0258 |

The two above tables asses the balance of the two datasets in regard to the matching variables. Strong balance implies that there is a very similar distribution of the covariates between the two datasets, which reduces the chance of model misspecification. The goal of a successful matching is to have as similar as possible a distribution, while keeping as large a number of observations as possible (Ho et al., 2007). The balance is assessed by comparing the three variables standardized mean difference, variance ratio and eCDF mean. The standardized mean difference is the difference between the standardized means of the two groups, and for a well-balanced match should be close to or below 0.1. The variance ratio should be between 0.5 and 2 (Greifer, 2024). The eCDF shows the difference between the cumulative distribution function of the two groups and should be small (Greifer, 2024).

Overall, the matched dataset has very strong balance. For most of the variables the matching is exact, and for size, Std. Mean Diff is very close to or below 0.1, variance ratio very close to 1 and eCDF mean very close to zero. This compares favourably to the non-balanced dataset, where there are large differences in particularly size between the two datasets. The goal of a successful matching is to reach good balance without losing to much data. After matching, the dataset is comprised of 68 matched pairs, implying 204 observations per regression, as each company is observed over three years. This equals 37% of the initial treated group.

We can also see from the table that compared to the unmatched dataset, the matched dataset includes more U.S.-listed companies from Sweden and the UK, and fewer companies from the other countries. The matched U.S.-listed companies are also on average smaller than the unmatched sample, as a consequence of large size differences between the two groups in the unmatched sample. There are also some differences in sector. Consequently, results for the matched sample of U.S.-listed companies might be different from the unmatched sample.

***Table 10.*** *Panel Regression after Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022.*

| | Dependent variable: Beta | | | |
|---|---|---|---|---|
| | U.S.-listed firms pre-pandemic (1) | Non-U.S.-listed firms pre-pandemic (2) | U.S.-listed firms during pandemic (3) | Non-U.S.-listed firms during pandemic (4) |
| Total Debt Ratio | 0.563 | 0.287 | 0.644*** | -1.101*** |
| | (0.460) | (0.405) | (0.203) | (0.390) |
| Debt to Equity Ratio | -0.001 | 0.029 | -0.0002 | 0.016 |
| | (0.001) | (0.041) | (0.001) | (0.016) |
| Profit Margin | 0.001** | 0.065 | 0.003 | 0.002*** |
| | (0.0004) | (0.089) | (0.002) | (0.001) |
| Return on Assets | -0.247 | -0.025 | -0.247** | -1.301*** |
| | (0.212) | (0.398) | (0.117) | (0.353) |
| Return on Equity | -0.013*** | -0.018 | 0.039* | 0.060 |
| | (0.003) | (0.099) | (0.020) | (0.057) |
| Size | 0.322** | 0.447*** | 0.193** | 0.166 |
| | (0.124) | (0.164) | (0.091) | (0.143) |
| Observations | 204 | 204 | 204 | 204 |
| $R^2$ | 0.264 | 0.101 | 0.162 | 0.120 |
| F-statistic | 7.756*** | 2.435** | 4.176*** | 2.942** |
| *Note:* | | | | *p**p***p<0.01 |

The matched regression show that the larger non-U.S.-listed firms do depend more on accounting fundamentals than their non-matched peers, with several accounting variables (total debt ratio, profit margin, and return on assets) having significant coefficients during the crisis period. This implies that some of the matched variables have had an effect on the extent to which their systematic risk can be explained by accounting fundamentals. However, in the pre-crisis period only the control variable size is significant.

On the other hand, several accounting variables are significant for describing the systematic risk performance of the US-listed companies in both before and during the pandemic period. Furthermore, the R-squared is still higher for the U.S listed firms in both groups. However, for this group R-squared is not necessarily higher in the pre-crisis period than in the post crisis period. We thereby, find some, but quite limited support for *Hypothesis 3*, and no evidence in support of hypothesis 5. *Hypothesis 5* is further undermined by our robustness regressions, which show that the fall in the ability of accounting fundamentals during the crisis might be more driven by the start of the Russia-Ukraine war in 2022 than the COVID-19 pandemic.

# 8 DISCUSSION

## 8.1 Discussion of Findings

Our study finds that during the COVID-19 crisis, having a listing in the United States decreased the impact of the pandemic on systematic risk by a coefficient of 0.195. This is in line with research by Chandar, Patro, & Yezegel (2009) who found a general outperformance of firms with ADR listings during a series of emerging market crisis. Note however that Chandar, Patro, & Yezegel (2009) analysed return and not systematic risk. We thereby do not find signs of market panic which Radelet & Sachs (1998) argued caused the East Asian financial crisis. Note however, that our sample mainly consists of firms from developed markets, and Radelet & Sachs (1998) argued that panic selling was predominantly a problem for emerging market firms with initially lower investor confidence. Chira & Marciniak (2014) also explore this effect by analysing how the European debt crisis affected a series of U.S.-listed firms from northern more developed European countries and found that they did not experience the same risk increase as the U.S.-listed firms from southern countries. A fertile area for further study might therefore be to re-estimate our models and see how the coefficients differ between our group of developed countries and groupings of developing countries.

We also find that both U.S.-listed and non-U.S.-listed firms experienced statistically significant increases in average systematic risk over the three-year COVID-19 period, relative to the three preceding years. This implies that for both groups, the market risk increased during the time of crisis. However, we found a considerably higher increase of 0.18 for the non-U.S.-listed firms relative to a 0.06 increase for the U.S.-listed firms. This is a very different finding from Chira & Marciniak (2014) who found only statistically significant increases in systematic risk for the non-U.S.-listed group. Consequently, the systematic risk effect of the COVID-19 pandemic on our group of mostly northern European firms differs substantially from the consequences of the European debt crisis on Chira & Marciniak's (2014) group of southern European firms. This might be due to different natures of the crisis, with the European debt crisis being a regional crisis while the COVID-19 pandemic was a global crisis. It might also be due to differing investor behaviour between U.S. investors when investing in southern vis-à-vis northern European equities.

In general, we found that accounting fundamentals remained relevant for explaining firm systematic risk performance for our U.S.-listed firms. This is not in line with Chira & Marciniak (2014) who found that overall accounting fundamentals where poor predictors of market risk during the European debt crisis. Furthermore, when conducting regressions over a matched dataset, we found that accounting fundamentals remained relevant for predicting the systematic risk performance of both our U.S.-listed and non-U.S.-listed groups during the crisis. This implies that the inability of accounting fundamentals to predict systematic risk for our non-U.S.-listed sample in the initial unmatched regression, which is the closest to Chira & Marciniak's (2014) might be due to other characteristics than the lack of a U.S. cross-listing. We therefore find little evidence supporting a sustained difference between the two groups in

regard to whether their systematic risk was based on analysis of accounting fundamentals during the crisis. Consequently, there is limited evidence supporting Chiang & Zheng (2009) who show that both during and outside of periods of crisis the U.S. market is much less prone to herding than even other developed markets. There is, however, also no evidence supporting Radelet & Sachs (1998) theory of panic selling by international investors. Instead, our findings are mostly in line with Abdallah (2008) who found that cross-listing in the United States did not significantly affect analyst coverage of companies, and thereby might not effect the extent to which risk is fundamentals-determined.

Another finding of this study is that the effect of a U.S.-listing on the impact of the COVID-19 pandemic on systematic risk does not depend on the amount of disclosure and regulatory oversight required for the listing. The most regulated form of listing, a direct listing on a U.S. exchange, is associated with a smaller coefficient than a less regulated listing, such as a Pink Limited listing. This finding does not support arguments made by Miller (1999) that signalling might be a reason for cross-listings. If the stabilizing effect of cross-listing was predominantly a result of signalling then the costliest signal should have a stronger effect, and we would expect a very weak effect from listing in the Pink Limited market, as this market requires very little disclosure, and has weak oversight. However, this does not imply that signalling is not a reason for cross-listing for firms in developing markets, where the signal might be more necessary than for our group of developed market firms.

## 8.2 Limitations

There are some limitations to the SDID model that might affect the findings of this study. Predominantly, all DID setups, in particular the SDID, is dependent on the assumption that parallel trends in the pre-treatment period translate to parallel trends in the post-treatment period. We cannot guarantee this. However, the fact that despite the shock in 2020 the treatment group and the control group co-move quite closely in the post-treatment period suggests that the parallel trends assumption is satisfied. Moreover, the triple robustness of fixed effects, time weights and company weights might fail to control for variables that have a strong effect on systematic risk during the crisis, but very weak effect pre-crisis.

The period over which the model is estimated might matter. If there are large shocks that affect the treatment and control groups differently in the pre-treatment period, this might create bias in the synthetic control. In general, the limited number of pre-treatment periods is a weakness to this study. For the SDID estimator to be asymptomatically normal, a fair number of pre-treatment observations is necessary. The exact number of observations required is not known but is likely fewer than what is necessary for a synthetic control approach . We therefore expand our time period considerably during our robustness regressions and find that the results remain qualitatively the same (see Appendix A).

There are also some weaknesses with the exploration of the dependence of beta on accounting fundamentals. While the two regressions offer interesting insights to differences within each

group between the pre-crisis and post-crisis period, differences between the two groups of firms make direct comparisons between the two groups inconsistent. We try to control for this through a regression on a dataset matched using proximity scores, which improves the robustness. However, our matching only matches for the three variables derived from Chira & Marciniak's (2014) controls and that are deployed by Ghadhab (2021). There might be other confounding variables that should be added to the matching. However, considering the data loss associated with the matching, adding more variables would lead to too small a sample in this study. Further research might therefore construct a larger dataset of non-U.S.- and U.S.-listed firms in order to be able to undertake further matching.

## 8.3 Implications

The findings of this paper have implications for further academic study, firm management, and policymakers. The study builds further on research that shows that the risk effect of cross-listing during crisis might vary between developing and developed markets. The exact mechanisms for this might warrant further study. The study shows that if a firm is from a developed economy with efficient capital markets, listing in the United States might still offer a stabilizing effect during times of crisis. However, this effect is not necessarily dependent on the amount of disclosure and regulatory oversight the U.S.-listing requires, and as such it might be better to list in the OTC market where disclosure is lower and less costly. This already seems to be the strategy of the majority of our sample companies listed in the United States. For policymakers, and particularly European policymakers, the study shows that retaining open capital markets, and allowing firms to list abroad, can have a stabilizing effect on the firms in the economy. Furthermore, as the study shows some evidence that the added sophistication of U.S. capital markets has a stabilizing effect on European firms, European governments should concentrate their efforts on increasing the sophistication of European capital markets, in order to extend this advantage also to non-U.S.-listed companies.

# 9 CONCLUSION

This paper provides several insights into the effect of a U.S. cross-listing on firms from several European countries during the COVID-19 pandemic. The paper makes three main contributions to the literature. First, having a U.S. cross-listing during the COVID-19 pandemic decreases systematic risk. Second, the magnitude of this effect does not increase based on the disclosure and regulatory requirements required for different forms of U.S. listings. Third, cross-listing in the United States does not necessarily affect the extent to which a firm's systematic risk is dependent on accounting fundamentals.

Consequently, the overall findings of the study support the attractiveness of U.S. capital markets for European firms, showing that, even when compared to a control group of companies with other cross-listings, a U.S. cross-listing has a positive effect. The paper thereby also shows that the trend of European firms increasingly choosing to cross-list in the United States has a positive and stabilizing effect on European firms and the European economy. This holds true at least for the sample of firms from European countries with a track record of good governance and oversight. However, the study does not indicate whether the results would differ for European firms from countries without these characteristics.

We also show that the positive effect from cross-listing in the United States is not strengthened by undertaking listings with higher requirements in terms of disclosure and regulatory oversight. Consequently, limiting systematic risk during crisis is not a reason for firms from European countries with strong oversight and governance to devote the necessary resources to undertake more demanding listing types. Instead, it is sufficient to get access to U.S. capital markets in less resource intensive ways, such as an OTC listing in the Pink Current or Pink Limited market.

Overall, the study shows that the systematic risk did increase for both U.S.-listed and non-U.S.-listed firms during the COVID-19 pandemic relative to the three years before the crisis. This shows that the pandemic was associated with a general increase in market risk relative to the preceding period, which is in line with theories of increased market risk during times of crisis. However, we found limited evidence that the crisis caused a decrease in the extent to which firm systematic risk depended on firm fundamentals, and what we found was more linked to the Russia-Ukraine war than the COVID-19 pandemic.

# 10 REFERENCES


Abadie, A., & Gardeazabal, J. (2003). The economic costs of conflict: A case study of the basque country. *American Economic Review*, 113–132.

Abadie, A., & L'Hour, J. (2021). A penalized synthetic control estimator for disaggregated. *Journal of the American Statistical Association*, 1817-1834.

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association*, 493–505.

Abadie, A., Diamond, A., & Hainmueller, J. (2015). Comparative politics and the synthetic control method. *American Journal of Political Science*, 495–510.

Abdallah, A.-N. (2008). Do regulations matter? The effects of cross-listing on analysts' coverage and forecast errors: A comparative analysis. *Review of Accounting and Finance*.

Amina, Z. (2022). Detecting Financial Fraud Following The Covid 19 Pandemic: Role of. *Journal of Economics, Finance and Management Studies*, 134-137.

Angel, J. J., & McCabe, D. (2013). Fairness in Financial Markets: The Case of High Frequency Trading. *Journal of business ethics*, 585-595.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., & Wager, S. (2021). *Synthetic Difference in Differences.* Cambridge Mass.: National Bureau of Economic Research.

Bahlous, M. (2013). Does Cross-Listing Benefit the Shareholders? Evidence from Companies in the GCC Countries? *Asia-Pacific financial markets*, 345-381.

Baker, H. K., Nofsinger, J. R., & Weaver, D. G. (2002). International cross-listing and visibility. *Journal of Financial and Quantitative Analysis, 37*(3), 495-521.

Banz, R. (1981). The relationship between return and market value of common stocks. *Journal of Financial Economics*, 3-18.

Barry, C. B., & Brown, S. J. (1985). Differential Information and Security Market. *Journal of Financial and Quantitative Analysis*, 407-422.

Bhandari, L. C. (1988). Debt/equity ratio and expected stock returns: Empirical evidence. *Journal of Finance*, 507-528.

Bikhchandani, S., Hirshleifer, D., & Welch, I. (1992). A Theory of Fads, Fashion, Custom, and. *Journal of Political Economy*, 992-1026.

Bris, A., Koskinen, Y., & Nilsson, M. (2009). The Euro and Corporate Valuations. *The Review of financial studies*, 3171-3209.

Brüggemann, U., Kaul, A., Leuz, C., & Werner, I. (2018). The twilight zone: OTC regulatory regimes and market quality. *The Review of Financial Studies, 31*(3), 898-942.

Cetorelli, N., & Peristiani, S. (2015). Firm Value and Cross-Listings: The Impact of Stock Market Prestige. *Journal of Risk and Financial Management, 8*(1), 150-80.

Chandar, N., Patro, D. K., & Yezegel, A. (2009). Crises, contagion and cross-listings. *Journal of Banking & Finance*, 1709-1729.

Chari, A., & Henry, P. B. (2002). *Risk Sharing and Asset Prices: Evidence from a Natural Experiment.* Cambridge Mass.: National Bureau of Economic Research.

Chiang, T. C., & Zheng, D. (2009). An empirical analysis of herd behavior in global stock markets. *Journal of banking & finance*, 1911-1921.

Chira, I., & Marciniak, M. (2014). Risk change during crises: How do purely local companies differ from cross-listed firms? Evidence from the European crisis of 2010–2012. *Journal of Economics and Business*, 11-23.

Clarke, D., Pailañir, D., Athey, S., & Imbens, G. (2024, May 08). *On Synthetic Difference-in-Differences and*. Retrieved from On Synthetic Difference-in-Differences and: https://www.damianclarke.net/research/papers/SDID.pdf

Coffee, J. (1999). The Future as History: The Prospects for Global Convergence in Corporate Governance and Its Implications. *Northwestern University Law Review, 93*.

Coffee, J. C. (2002). Racing towards the Top?: The Impact of Cross-Listings and Stock Market Competition on International Corporate Governance. *Columbia law review*, 1757-1831.

Corsetti, G., Pericoli, M., & Sbracia, M. (2005). "Some Contagion, some Interdependence":More pitfalls in tests of financial contagion. *Journal of international money and finance*, 1177-1199.

Dittmar, A., & Mahrt-Smith, J. (2007). Corporate governance and the value of cash holdings. *Journal of Financial Economics*, 599-634.

Doidge, C. G., Karolyi, A., & Stulz, R. M. (2004). "Why are foreign firms listed in the US worth more? *Journal of financial economics, 71*(2), 205-238.

Doidge, C. G., Karolyi, A., & Stulz, R. M. (2009). Has New York become less competitive than London in global markets? Evaluating foreign listing choices over time. *Journal of Financial Economics, 91*(3), 53-277.

Domowitz, I., Glen, J., & Madhavan, A. (2001). Liquidity, volatility and equity trading costs across countries and over time. *International Finance, 4*(2), 221-255.

Faulkender, M., & Wang, R. (2006). Corporate financial policy and the value of cash. *Journal of Finance*, 1957-1990.

Fernandes, N., & Ferreira, M. A. (2008). Does international cross-listing improve the information environment. *Journal of financial economics*, 216-244.

Foerster, S., & Karolyi, G. (1999). The effects of market segmentation and investor recognition on asset prices: Evidence from foreign stocks listing in the United States. *The Journal of Finance, 54*(3), 981-1013.

Ghadhab, I. (2019). Does cross-listing in the US mitigate stock crash risk? International evidence. *International Review of Financial Analysis, 63*, 186-197.

Ghadhab, I. (2021). Cross-listing and crisis. *Journal of Asset Management*, 539-558.

Ghadhab, I., & M'rad, M. (2018). Does US cross-listing come with incremental benefit for already UK cross-listed firms? *The Quarterly Review of Economics and Finance, 69*(C), 188-204.

Gozzi, J. C., Levine, R., & Schmukler, S. L. (2008). Internationalization and the evolution of corporate valuation. *Journal of Financial Economics, 88*(3), 607-632.

Greifer, N. (2024, May 5th). *Assessing balance*. Retrieved from Cran: https://cran.r-project.org/web/packages/MatchIt/vignettes/assessing-balance.html#ref-ho2007

Hail, L., & Leuz, C. (2009). Cost of capital effects and changes in growth expectations around U.S. cross-listings. *Journal of Financial Economics*, 428-454.

Ho, D. E., Kosuke, I., King, G., & Stuart, E. A. (2007). Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference. *Political analysis*, 199-236.

Howe, J. S., & Kelm, K. (1987). The Stock Price Impacts of Overseas. *Financial Management*, 51-56.

International Panel on Climate Change. (2023, 04 24). *Climate Change 2023 Synthesis Report.* Retrieved from IPCC.ch: https://www.ipcc.ch/report/ar6/syr/downloads/report/IPCC_AR6_SYR_LongerReport.pdf

Kang, J.-K., & Stulz, R. (1995). *Why Is There a Home Bias? An Analysis of Foreign Portfolio Equity Ownership in Japan.* Cambridge Massachusets: National Bureau of Economics Research.

Karolyi, G. (1998). Why do companies list shares abroad?: A survey of the evidence and its managerial implications. *Financial Markets, Institutions & Instruments, 7*(1), 1-60.

Karolyi, G. (2006). The world of cross-listings and cross-listings of the world: Challenging conventional wisdom. *Review of Finance, 10*(1), 99-152.

Lazarides, T., & Pitoska, E. (2009). Corporate Governance and Debt to Equity Ratio. *SSRN Working Paper*.

Merton, R. C. (1987). A simple model of capital market equilibrium with incomplete information. *Journal of Finance, 42*(3), 483-509.

Miller, D. (1999). The market reaction to international cross-listings: Evidence from depository receipts. *Journal of Financial Economics*, 103-123.

Morck, R., Yeung, B., & Yu, W. (2000). The information content of stock markets: why do emerging markets have synchronous stock price movements? *Journal of Financial Economics*, 215-260.

OTC Markets. OTC Markets Group Inc. (2024, May 6). OTC Markets. Retrieved from https://www.otcmarkets.com/.

Pagano, M., Röell, A. A., & Zechner, J. (2002). The Geography of Equity Listing: Why Do Companies List Abroad? *The Journal of Finance*, 2651-2694.

Palazzo, B. (2012). Cash holdings, risk, and expected returns. *Journal of Financial Economics*, 162-185.

Patoukas, P. (2013). Detecting news in aggregate accounting earnings: Implications for stock market valuation. *Review of Accounting Studies*, 134-160.

Radelet, S., & Sachs, J. (1998). *The onset of the East Asian financial crisis.* Cambridge Mass.: Harvard University.

Sharpe, W. (1964). CAPITAL ASSET PRICES: A THEORY OF MARKET EQUILIBRIUM UNDER CONDITIONS OF RISK. *Journal of FInance*, 425-442.

Spence, M. (1973). Job Market Signaling. *The Quarterly journal of economics*, 355-374.

Statista. (2024, May 6). *Change in performance of S&P 500 during COVID-19 pandemic vs previous major crashes as of August 2020*. Retrieved from https://www.statista.com/statistics/1175227/s-and-p-500-major-crashes-change/

Stulz, R. M. (1999). Globalization of Equity Markets and the Cost of Capital. *Dice Center Working Paper*, 99-1.

Tan, L., Chiang, T. C., Mason, J. R., & Nelling, E. (2008). Herding behavior in Chinese stock markets: An examination of A and B shares. *Pacific-Basin finance journal*, 61-77.

WHO. (2024, 05 06). *World Health Organization Covid interactive timeline*. Retrieved from https://www.who.int/emergencies/diseases/novel-coronavirus-2019/interactive-timeline#!

Yilmaz, E., & Hussain, A. (2021). *The Effect of the Covid-19 Pandemic on the Scandinavian Stock Market.* Copenhagen: Copenhagen Business School.

Zhou, R. T., & Lai, R. N. (2009). Herding and Information based trading. *Journal of empirical finance*, 388-393.

# 11 APPENDICES

## 11.1 Appendix A: SDID Regressions

### 11.1.1 Robustness Test by Time Periods

***Table 11.*** *SDID Regressions by Time Period (2010-2022).*

| Beta | ATT | Std. Err. | t-Stat | P>\|t\| | 95% Confidence Interval |
|---|---|---|---|---|---|
| Time Period: 2010-2022 | -0.36040*** | 0.03870 | -9.31 | 0.000 | [-0.43625, -0.28456] |
| Time Period: 2011-2022 | -0.35245*** | 0.03445 | -10.23 | 0.000 | [-0.41997, -0.28493] |
| Time Period: 2012-2022 | -0.32547*** | 0.03684 | -8.84 | 0.000 | [-0.39766, -0.25327] |
| Time Period: 2013-2022 | -0.26714*** | 0.03065 | -8.72 | 0.000 | [-0.32721, -0.20708] |
| Time Period: 2014-2022 | -0.27537*** | 0.03127 | -8.81 | 0.000 | [-0.33666, -0.21409] |
| Time Period: 2015-2022 | -0.27887*** | 0.03444 | -8.10 | 0.000 | [-0.34638, -0.21137] |
| Time Period: 2016-2022 | -0.21633*** | 0.03771 | -5.74 | 0.000 | [-0.29024, -0.14243] |
| Time Period: 2017-2022 | -0.19500*** | 0.04713 | -4.14 | 0.000 | [-0.28737, -0.10263] |

*Note:* $^{*}p$ $^{**}p$ $^{***}p<0.01$

### 11.1.2 SDID Regression: U.S.-Listings by ADR

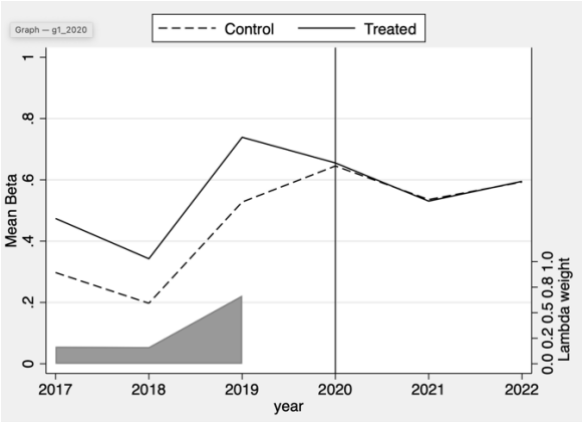
***Table 12.*** *SDID Regressions by ADR Type (2017-2022).*

| Beta | ATT | Std. Err. | t-Stat | P>\|t\| | 95% Confidence Interval |
|---|---|---|---|---|---|
| Only ADR | -0.19229*** | 0.05195 | -3.70 | 0.000 | [-0.29410, -0.09047] |
| ADR and non-ADR | -0.19997*** | 0.04965 | -4.03 | 0.000 | [-0.29730, -0.10265] |

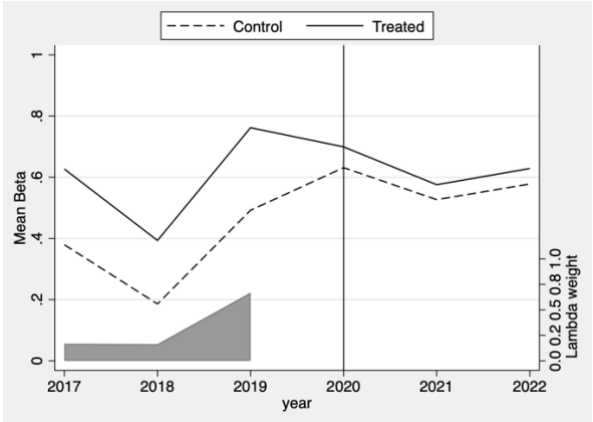
*Note:* $^{*}p$ $^{**}p$ $^{***}p<0.01$

### 11.1.3 SDID Regressions by Listing Type

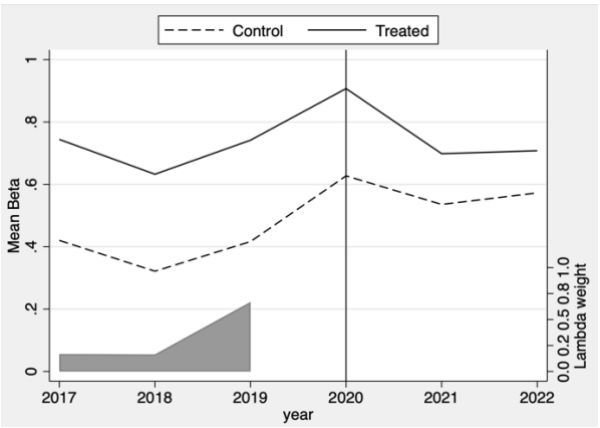
***Figure 3.*** *SDID Regressions by Listing Type (2017-2022).*



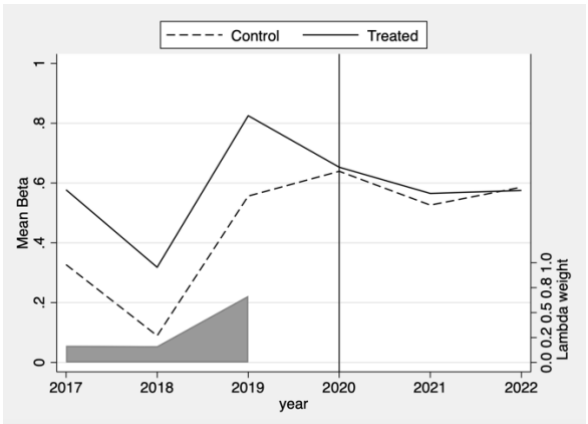

***(a)*** *Only ADR*



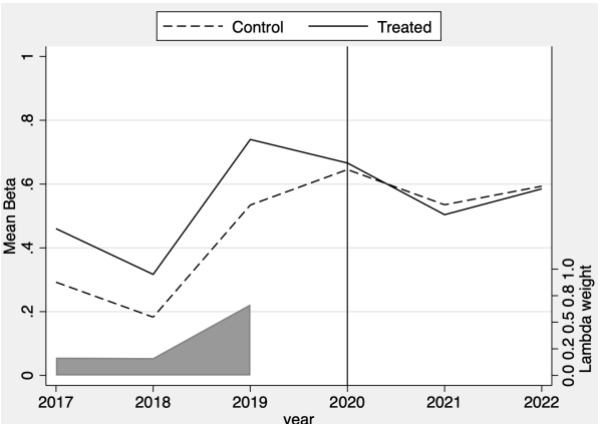

***(b)*** *Both ADR and non-ADR*



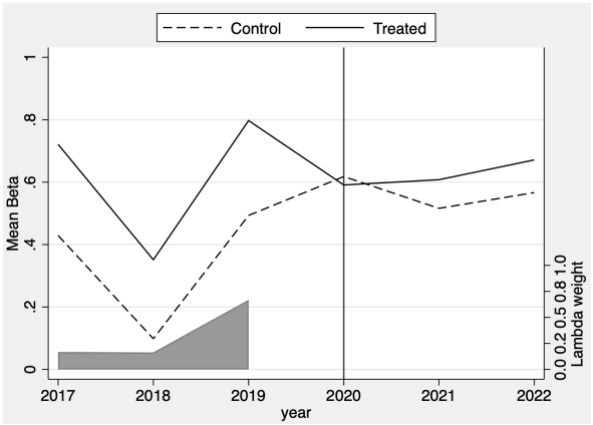

***(c)*** *Regulated Exchange*




***(d)*** *OTCX Premier International*




***(e)*** *Pink Current*




***(f)*** *Pink Limited*

## 11.2 Appendix B: Proximity Score Matching Regressions

### 11.2.1 Robustness Test: Russia-Ukraine War (Year 2022)

The COVID-19 pandemic overlaps with the start of the Russia-Ukraine war, which began on February 24th, 2022. To maintain the robustness of our results, we run regressions ending the treatment period in 2021 as opposed to 2022. We find that our results are robust with and without including the year of the conflict.

***Table 13.*** *Mean Betas and t-Statistics for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2021.*

| | | U.S-Listed Firms | Non-U.S.-Listed Firms |
|---|---|---|---|
| Beta | Before COVID-19 | 0.5571 | 0.4107 |
| | During COVID-19 | 0.6157 | 0.5976 |
| | % Change | 10.52% | 45.51% |
| t-Test | | -2.80*** | -17.25*** |

*Note:* $^{*}p$ $^{**}p$ $^{***}p<0.01$

The first regression is a standard panel regression with fixed firm and time effects before matching. We apply the same dataset pre-matching as in Table X, only removing year 2022. We find that when removing the year 2022 a considerably higher amount of accounting fundamentals become statistically significant for the non-U.S.-listed companies during the pandemic, implying that some of the differences found in the main results might be driven by the Ukraine war, as opposed to the COVID-19 pandemic. However, R-squared is still much higher for the U.S.-listed group.

***Table 14.*** *Panel Regression before Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2021.*

| | Dependent variable: Beta | | | |
|---|---|---|---|---|
| | U.S.-listed firms pre-pandemic (1) | Non-U.S.-listed firms pre-pandemic (2) | U.S.-listed firms during pandemic (3) | Non-U.S.-listed firms during pandemic (4) |
| Total Debt Ratio | 0.336 | 0.046** | 0.870*** | 0.002 |
| | (0.389) | (0.021) | (0.231) | (0.002) |
| Debt to Equity Ratio | -0.001 | -0.001* | 0.0005 | -0.001* |
| | (0.001) | (0.0003) | (0.001) | (0.0003) |
| Profit Margin | 0.001** | -0.0001* | 0.001 | 0.001** |
| | (0.0004) | (0.0001) | (0.009) | (0.0003) |
| Return on Assets | -0.276 | 0.037 | -0.165 | -0.002 |
| | (0.225) | (0.030) | (0.136) | (0.002) |
| Return on Equity | -0.013*** | -0.008*** | -0.016 | -0.003*** |
| | (0.004) | (0.002) | (0.021) | (0.001) |
| Size | 0.192** | 0.067** | -0.117 | 0.022 |
| | (0.096) | (0.032) | (0.085) | (0.022) |
| Observations | 531 | 6,178 | 354 | 4,151 |
| $R^2$ | 0.095 | 0.006 | 0.145 | 0.007 |
| F-statistic | 6.109*** | 4.307*** | 4.815*** | 2.477** |

*Note:* *p**p***p<0.01

The second regression is a standard panel regression with fixed firm and time effects after matching. We apply the same dataset post-matching as in Table X, only removing year 2022. This robustness check also shows that when removing the Ukraine war year 2022, accounting fundamentals become quite able to explain beta for also the non-U.S.-listed firms, implying that this finding might be driven by the Ukraine war.

***Table 15.*** *Panel Regression after Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2021.*

| | Dependent variable: Beta | | | |
|---|---|---|---|---|
| | U.S.-listed firms pre-pandemic (1) | Non-U.S.-listed firms pre-pandemic (2) | U.S.-listed firms during pandemic (3) | Non-U.S.-listed firms during pandemic (4) |
| Total Debt Ratio | 0.563 | 0.287 | 0.804*** | -1.269* |
| | (0.460) | (0.405) | (0.238) | (0.681) |
| Debt to Equity Ratio | -0.001 | 0.029 | 0.00001 | 0.084** |
| | (0.001) | (0.041) | (0.001) | (0.032) |
| Profit Margin | 0.001** | 0.065 | -0.030** | -0.094 |
| | (0.0004) | (0.089) | (0.015) | (0.202) |
| Return on Assets | -0.247 | -0.025 | -0.033 | -1.305* |
| | (0.212) | (0.398) | (0.143) | (0.724) |
| Return on Equity | -0.013*** | -0.018 | 0.004 | 0.217** |
| | (0.003) | (0.099) | (0.021) | (0.097) |
| Size | 0.322** | 0.447*** | 0.143 | -0.094 |
| | (0.124) | (0.164) | (0.115) | (0.269) |
| Observations | 204 | 204 | 136 | 136 |
| $R^2$ | 0.264 | 0.101 | 0.292 | 0.187 |
| F-statistic | 7.756*** | 2.435** | 4.265*** | 2.380** |
| *Note:* | | | | *p**p***p<0.01 |

### 11.2.2 Robustness Test: Current Ratio

Liquidity measures and cash have been shown to have a considerable effect on firm returns (Faulkender & Wang, 2006; Dittmar & Mahrt-Smith, 2007; Palazzo, 2012). We therefore estimate a robustness regression with current ratio as a measure of liquidity. Current ratio is estimated as current assets divided by current liabilities and is appropriate for most industry types excluding financial institutions. Financial institutions measure liquidity risk by measures such LCR or NSFR, which are not accounted for in the Compustat data for fundamentals. To test whether current ratio impacts the results, we run the following robustness regressions, which exclude financial firms and include current ratio.

The first regression is a standard panel regression with fixed firm and time effects before matching including the Current Ratio. We find that including current ratio does not noticeably change the results from our main paper, with current ratio not being statistically significant for non-U.S.-listed firms before or during the pandemic.

***Table 16.*** *Panel Regression with Current Ratio before Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022.*

| | Dependent variable: Beta | | | |
|---|---|---|---|---|
| | U.S.-listed firms pre-pandemic (1) | Non-U.S.-listed firms pre-pandemic (2) | U.S.-listed firms during pandemic (3) | Non-U.S.-listed firms during pandemic (4) |
| Current Ratio | 0.124** | 0.00004 | -0.028 | 0.0001 |
| | (0.051) | (0.0002) | (0.018) | (0.0001) |
| Total Debt Ratio | 0.797* | 0.054** | 0.516*** | 0.001 |
| | (0.431) | (0.021) | (0.181) | (0.002) |
| Debt to Equity Ratio | -0.001 | -0.0005 | -0.002 | -0.001* |
| | (0.001) | (0.0003) | (0.003) | (0.0003) |
| Profit Margin | 0.001** | -0.0001* | 0.004* | 0.00001 |
| | (0.0004) | (0.0001) | (0.002) | (0.0001) |
| Return on Assets | -0.234 | 0.051* | -0.277** | -0.0003 |
| | (0.228) | (0.030) | (0.113) | (0.003) |
| Return on Equity | -0.012*** | -0.007*** | 0.025 | -0.0002 |
| | (0.004) | (0.002) | (0.019) | (0.001) |
| Size | 0.231** | 0.059* | 0.067 | 0.045** |
| | (0.099) | (0.036) | (0.060) | (0.021) |
| Observations | 471 | 4,473 | 470 | 4,506 |
| $R^2$ | 0.122 | 0.009 | 0.094 | 0.003 |
| F Statistic | 6.077*** | 3.663*** | 4.534*** | 1.346 |
| *Note:* | | | | *p**p***p<0.01 |

We match on these variables and receive the following results.

***Table 17.*** *Summary of Balance before Matching on Panel including Current Ratio.*

| Variable | Means Treated | Means Control | Std. Mean Diff. | Var. Ratio | eCDF Mean | eCDF Max |
|---|---|---|---|---|---|---|
| Distance | 0.5623 | 0.0464 | 1.6711 | 7.6198 | 0.4486 | 0.7852 |
| Sector | 2798.9103 | 3102.7989 | -0.2507 | 0.9813 | 0.0766 | 0.1687 |
| Germany | 0.2372 | 0.2072 | 0.0705 | - | 0.03 | 0.03 |
| Denmark | 0.0705 | 0.0448 | 0.1003 | - | 0.0257 | 0.0257 |
| Finland | 0.0385 | 0.0666 | -0.1462 | - | 0.0281 | 0.0281 |
| Britain | 0.4744 | 0.3342 | 0.2806 | - | 0.1401 | 0.1401 |
| Norway | 0.0513 | 0.0577 | -0.0293 | - | 0.0065 | 0.0065 |
| Sweden | 0.1282 | 0.2894 | -0.4822 | - | 0.1612 | 0.1612 |
| Size 2017 | 9.2855 | 5.1213 | 2.296 | 0.7109 | 0.4245 | 0.7127 |
| Size 2018 | 9.3251 | 5.2027 | 2.3062 | 0.6855 | 0.4228 | 0.7172 |
| Size 2019 | 9.3968 | 5.2896 | 2.2956 | 0.6604 | 0.421 | 0.7114 |

***Table 18.*** *Summary of Balance after Matching on Panel including Current Ratio.*

| Variable | Means Treated | Means Control | Std. Mean Diff. | Var. Ratio | eCDF Mean | Std. Pair Dist. |
|---|---|---|---|---|---|---|
| Distance | 0.2787 | 0.2613 | 0.0564 | 1.0457 | 0.0162 | 0.0901 |
| Sector | 2643.6364 | 2643.6364 | 0 | 1 | 0 | 0 |
| Germany | 0.1455 | 0.1455 | 0 | - | 0 | 0 |
| Denmark | 0.0182 | 0.0182 | 0 | - | 0 | 0 |
| Finland | 0 | 0 | 0 | - | 0 | 0 |
| Britain | 0.5636 | 0.5636 | 0 | - | 0 | 0 |
| Norway | 0.0545 | 0.0545 | 0 | - | 0 | 0 |
| Sweden | 0.2182 | 0.2182 | 0 | - | 0 | 0 |
| Size 2017 | 7.7774 | 7.6278 | 0.0825 | 0.9291 | 0.0188 | 0.1444 |
| Size 2018 | 7.8457 | 7.7268 | 0.0665 | 0.8709 | 0.0216 | 0.2313 |
| Size 2019 | 7.9286 | 7.8116 | 0.0654 | 0.9098 | 0.0209 | 0.2539 |

The second regression is a standard panel regression with fixed firm and time effects after matching. We apply the same dataset post-matching as in Table X, only removing year 2022. We find that including current ratio does not noticeably change the results from our main paper, with current ratio not being statistically significant for non-U.S.-listed firms before or during the pandemic.

***Table 19.*** *Panel Regression with Current Ratio after Matching for U.S.-Listed and Non-U.S.-Listed Firms pre- and during COVID-19 for the Period between January 2017 – December 2022.*

| | Dependent variable: Beta | | | |
|---|---|---|---|---|
| | U.S.-listed firms pre-pandemic (1) | Non-U.S.-listed firms pre-pandemic (2) | U.S.-listed firms during pandemic (3) | Non-U.S.-listed firms during pandemic (4) |
| Current Ratio | 0.176** | 0.007 | -0.029 | -0.039 |
| | (0.081) | (0.018) | (0.020) | (0.024) |
| Total Debt Ratio | 1.307** | -0.030 | 0.583** | -0.656 |
| | (0.535) | (0.314) | (0.232) | (0.558) |
| Debt to Equity Ratio | -0.001 | 0.004 | -0.007 | 0.003 |
| | (0.001) | (0.041) | (0.004) | (0.012) |
| Profit Margin | 0.001** | -0.0004* | 0.002 | -0.001 |
| | (0.0004) | (0.0002) | (0.002) | (0.001) |
| Return on Assets | -0.198 | 0.065 | -0.247** | -1.659*** |
| | (0.218) | (0.254) | (0.124) | (0.391) |
| Return on Equity | -0.012*** | -0.077 | 0.062** | 0.049 |
| | (0.003) | (0.064) | (0.025) | (0.070) |
| Size | 0.386*** | 0.269** | 0.200* | 0.250 |
| | (0.133) | (0.111) | (0.101) | (0.174) |
| Observations | 165 | 165 | 165 | 165 |
| $R^2$ | 0.324 | 0.140 | 0.217 | 0.217 |
| F Statistic | 7.060*** | 2.403** | 4.072*** | 4.075*** |

*Note:* *p **p ***p<0.01

## 11.3 Appendix C: Data Outliers

Below is a list of companies we remove with extreme outlier betas for the regressions we run on the period 2017-2022.

***Table 20.*** *List of Extreme Outlier Betas (2017-2022).*

| | Beta | | | | | |
|---|---|---|---|---|---|---|
| **Company Name** | **2017** | **2018** | **2019** | **2020** | **2021** | **2022** |
| Gerry Weber International | 0.95 | 1.01 | -0.70 | -1201.91 | 0.37 | 0.84 |
| Horus AG | -0.08 | -998.59 | -45.40 | -0.51 | 0.04 | 0.25 |
| Xtract Resources Plc | -49.73 | 0.37 | -0.98 | 0.59 | -0.63 | 0.47 |